EXPERIMENTAL AND THEORETICAL
STATICS OF LIQUIDS
SUBJECT TO
MOLECULAR FORCES ONLY,

BY

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TO MONSIEUR A. QUETELET

DIRECTOR OF THE OBSERVATORY OF BRUSSELS , ETC.

You, who were one of the active promoters of the intellectual regeneration of Belgium, and whose work contributed so much to the illustriousness of this country; you, who guided my first steps in the career of science, and who taught me, by your example, to excite among young people the love of research, who did not cease being for me a devoted friend, allow me to dedicate this work to you, in testimony of recognition and constant affection.

J. PLATEAU.

This work is formed primarily of the contents of eleven Series of Memoires that I published, from 1843 to 1868, in the Memoires of the Academy of Belgium, under the title: Research experimental and theoretical on the equilibrium shapes of a liquid mass without gravity[1]. But the revision of the whole of these Series enabled me to follow, in the current work, an order a little more methodical, to rectify some passages, and to fill gaps, especially in the histories; until the end of 1869, I could extend I did of them two those new, one concerning the surface viscosity of liquids, the other with the constitution of liquid streams; I introduced several additions which appear worthy to me of interest, such as the theory of exploding laminar bubbles; on the other hand, I removed, as concerning purely dynamic phenomena, the majority of the results of my first Series; finally I have indicated, in a last paragraph, the titles of the articles which appeared after 1869 on subjects in connection with those that I treat.

[1]The first Series carries the a little different title: Phenomena shown by a liquid mass free and withdrawn from the action of gravity, first part.

STATICS OF LIQUIDS
SUBJECT
TO SOLELY MOLECULAR FORCES

CHAPTER ONE.

Preliminary concepts. General conditions which must be satisfied, in the state of equilibrium, free face of a liquid mass supposed without gravity. – Freeing a liquid from the action of gravity, leaving it free, either on all its surface, or on part of it to obey its own molecular attractions. Use of the first process: liquid sphere; experimental checks of the principles of the theory of capillary action. – Shapes bounded by plane surfaces: liquid polyhedra.

§ 1. It is known that, in ordinary circumstances, the free face of a liquid, in rest, when it has a rather large extent, is plane and horizontal, except towards its edges; but it is known, at the same time, that this form plane and horizontal is an effect of gravity, and that, between fixed boundaries, the free face can take very different forms, because then the action of gravity becomes of the same order as the molecular actions, for example, at the top of a liquid column raised or lowered in a capillary tube. One understands, accordingly, that if the liquids were deprived of gravity, their free face could have, with the at-rest state, and to an unspecified extent, forms very different than the flattened horizontal form, and it is, indeed, at what one arrives by calculation, as we will see.

It results from the work of Laplace on capillary action, that the liquids exert on themselves, under the terms of mutual attraction their molecules, a normal pressure on the surface at each point; that this pressure can be viewed as emanating from a surface layer having a thickness equal to the sensitive range of activity of the molecular attraction and is, consequently, extremely small; finally that this same pressure depends on the curvature of surface at the point considered, and that if one indicates by P the pressure, the force per unit area exerted on a plane surface, by A a constant, and by R and R' the principal radii of curvature, i.e. those of maximum and minimum curvature of surface at the same point, the pressure corresponding to this point has as a value, always with respect to the unit of area,

$$P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right) \tag{1}$$

In this formula, the radii R and R' are positive when they belong to convex curves, or when they are directed inside the mass, and they are negative when they belong to concave curves, when they are directed outside; as for the quantities P and A, they change only with the nature of the liquid.

That being so, let us conceive a liquid mass in the absence of gravity; the pressures of the various points, its surface layer transmitting one to the others by the interior liquid, it will be necessary, so that there is equilibrium, that all these pressures are equal to each other; it will thus be necessary that the expression above has a constant value with regard to all the surface; and as the quantities P and A are themselves constant, one will express the general condition of equilibrium as simply:

$$\frac{1}{R}+\frac{1}{R'}=C, \tag{2}$$

the quantity C being constant, and being able to be positive, negative or null.

Let us specify more for people who are not very familiar with the theory of surfaces. At a point taken arbitrarily on an unspecified surface, let us imagine a normal there, and

I pass a plane through this normal; it will cut surface according to a certain line, and the radius of curvature of the latter, at the foot of the normal, will be one of those of surface in this point. Now let us turn the plan around the normal; in each one of its positions, it will determine a new line, and, consequently, a new radius of curvature; however, among all these radii, there will be, in general, one which will have a greater curvature, and one with a curvature lower than all the others; they are our two principal radii R and R', and the quotients $1/R$ and $1/R'$ are the two principal curvatures at the point considered. Let us add that, according to a result curious from analysis, cross-sections to which these principal curves belong are always at right angle one on the other.

Thus, under the terms of formula [2], a liquid mass supposed without gravity must assume a shape such that the algebraic sum of the two principal curvatures has the same value at all the points of its surface. Each one of these principal curvatures is besides, according to what precedes, positive or negative according to whether it is convex or concave.

Note furthermore that when there is, at the same point of surface, convex curveatuer and concave curvature, the principal curvature of this last species is lowest algebraically; it is thus that which has the greatest negative value, and consequently the smallest negative radius.

Let us quote finally a second curious result from analysis: If one considers two unspecified sections at right angles containing the same normal, the radii of curvature which correspond to them, radii which we will name ρ and ρ', will be such that the quantity $\frac{1}{\rho}+\frac{1}{\rho'}$ will be equal to the quantity $\frac{1}{R}+\frac{1}{R'}$. It results from it that one can substitute the first of these quantities for the second, and that, consequently, the equilibrium equation in its greater general formulation will be $\frac{1}{\rho}+\frac{1}{\rho'}=C$.

If all these concepts still leave some darkness in the spirit of the reader, they will be cleared up by the many applications which we will make.

Only Poisson, I think, before my research, has touched directly on the general question of the shapes of equilibrium of a liquid mass without gravity, arrived[2] in 1828, at formula [2]: after having treated in all its general formulation the problem of the interior equilibrium of a fluid under the action of its own molecular forces and of exernal forces given, he seeks the equilibrium equations relating to the common surface of two fluids in contact; then he supposes that one of them is a liquid, and the other a gas exerting a uniform pressure; finally he removes the external ones, except the pressure, and thus finds, with regard to the surface of the liquid mass, the formula concerned.

It shows then that there is only one shape of possible equilibrium among the spheroids of revolution very little different from a sphere, and that this shape is that of the sphere itself.

§ 2. It is obvious, indeed, that a spherical surface satisfies formula [2] since all its radii of curvature are equal. It is in the same way obvious for the plane, for which all the radii of curvature are infinite, which makes everywhere the first member of the formula equal to zero, only in this case the constant C is zero. I cite these two surfaces because they are immediately obvious; but, as we will learn hereafter, they are far from being alone.

The constancy of the algebric sum of the two principal curves is not the only interpretation which formula [2] has:

In the first place, geometricians know that the quantity $\frac{1}{r}+\frac{1}{r'}$ is proportional to the average curvature, i.e. the average among all the curvatures of the surface at the point considered; formula [2] thus indicates also that our surfaces of equilibrium are those

[2] Mémoire sur l'équilibre des fluides. (Mém. de l'Acad. des Sciences de Paris, vol. IX, year 1830, p. 1)

which have the same average curve in all their points.

the invariability of the average curve includes/understands besides easily: since the pressure in each point depends on the curves, it is clear that if the average between all those does not vary a point with another, the effect of their unit will not vary either, and that thus the pressure will be the same one in all the points.

Now, the geometricians also know that surfaces with constant average curvature are in an unlimited number; a liquid supposed without gravity is thus likely of an infinity of shapes of equilibrium.

In the second place, if the constant C is equal to zero, and where consequently the mean curvature is zero, formula [2] gives

$$R = -R'$$

thus surfaces with zero mean curvature are such that at each one of their points the two radii of principal curvature are equal and of contrary signs; in other words, if, at an unspecified point of a similar surface, one conceives the two normal sections which have at this point the principal curvatures, sections which, we know, form between them right angles, one of these two curves will be convex, and the other will be equal, but concave. The plane alone makes a kind of exception, because all its curvatures are zero.

It follows, moreover, from what is known from the paragraph preceding, that if ρ and ρ' indicate the radii of curvature of two unspecified sections at right angles through same normal at a point of a surface with mean curvature zero, one has always also $\rho = -\rho'$, and that, consequently, if, at a point of a surface of this kind, the curvature be convex in a certain direction, it is necessarily concave, and with same degree, in the perpendicular direction.

In the third place, on the basis of a result of the Calculus of Variations, geometers have shown that surfaces satisfying formula [2] have minimum area. According to this principle, which we will consider again, when the constant C has a finite value, the surface of the shape would be always less than all nearby surfaces containing the same volume and with the same boundary, and when constant C is null, surface would be always, in an absolute way, i.e. without taking account of volume contained, less than all nearby surfaces with the same boundary; in this last case, such surfaces are often called *minimal surfaces.*

Under consideration from the purely mathematical point of view, the surfaces with which we occupy ourselves were the object of research of several geometers; we will profit from further results they obtained; only I will say, as of now, that the radii R and R' can be expressed using differential coefficients, which converts formula [2] into a differential equation of the second order; by making $\frac{dz}{dx} = p$, $\frac{dz}{dy} = q$, $\frac{d^2z}{dx^2} = r$, $\frac{d^2z}{dx^2} = t$, and $\frac{d^2z}{dy^2} = t$, this equation is:

$$(1+p^2)t + (1+q^2)r - 2pqs = C(1+p^2+q^2)^{3/2}. \qquad (3)$$

It thus represents all surfaces with constant mean curvature, and, consequently, all the shapes of equilibrium which would be appropriate to liquid without gravity; but it could not, up to now, be integrated except in certain cases, and, among the infinitely many surfaces which satisfy it, there is only one score, to my knowledge, which one has either the equation in final co-ordinates, or the determination by elliptic functions, or by geometrical generation.

Let us say however that methods that will be found would certainly make it possible, if calculations were carried out, to exceed much this number.

Let us furthermore present a note: it is indifferent to place the liquid on one side or other of a surface of equilibrium; in other words, any shape of equilibrium in relief has its corresponding identical in hollow; for example; just as a full sphere formed by a liquid without gravity would be in a state of equilibrium, at least in a vacuum, in the same way a hollow sphere in a liquid without gravity would be also in a state of equilibrium. Indeed, while passing from the shape in relief to the shape in hollow, the radii of curvature R and R' obviously only take, at each point, contrary signs, without changing absolute values, which gives simply also a contrary sign to the constant C, and consequently does not destroy the equilibrium condition.

§ 3. The results found by geometers applying to liquids with the seemingly impossible condition of null gravity, seemed to have remained in a state of clever speculations, without real physical significance; but I made known two processes by means of which one can place a liquid in circumstances so it behaves as if it did not weigh anything.

The first consists of introducing an olive oil mass into a mixture of water and alcohol whose density is exactly equal to that of the oil employed. Then, indeed, the action of gravity on the oil mass is complétely neutralized, and as the two liquids cannot mix, the immersed mass remains free to obey the pressures which emanate from its surface layer; also, when it is not in contact with any solid, it forms, whatever its volume, a perfect sphere, which remains suspended within the ambient liquid. I made in this manner exact spheres having up to 14 centimetres diameter. [3]

The second process makes it possible to produce shapes of equilibrium in the air; it rests on a principle which we will show rigorously later, but that we can render comprehensible as of now: let us design a very thin liquid film, a soap water film, for example; because of its tenuity, this film has an extremely small mass, so that the action of gravity is negligible in the presence of the capillary pressures which emanate from the surface layers (§ 1) of the two faces; each one of these faces conseqently takes a shape satisfying formula [2]; and as one of them in hollow is what the other is in relief, it follows from the remark at the end of the preceding paragraph that second follows that of the first, and that thus the form of equilibrium of a similar film is necessarily, without appreciable difference, one of those which would affect the free face of a full

[3] Since the publication of my first Series, I recognized that two old scientists had already closely approached this process.

In 1676, Boyle (Philos. Transact., vol. XI, pp. 775 and 779) on the basis of the fact that drops of rain and dew have a round shape, and noticing that these drops, surrounded by air, are made of a fluid in another fluid, proposes to test what happens to drops of a liquid immersed in another liquid with which they do not mix. To this end, he introduces into a bottle a layer of potash a concentrated carbonate solution, and, over, a sufficient quantity of also concentrated alcohol; then he drops in this last liquid drops of spirits of turpentine, and he sees these drops, which do not dissolve immediately in the ambient liquid, descend through this and to come to rest, with an appreciably spherical shape, on the surface of the alkaline liquid; he notes that sphericity starts to appear faded when a drop formed by the meeting of several others reached approximately a third of an inch in diameter. He then pours drops of water in essence of clove, a liquid whose density is very little higher than that of water, and these drops, which reach the top of the ambient liquid, show him in the same way a form very spherical when they are small, and slightly flattened when they reach about double the size of a pea.

In the second place, Segner, in a memoire published in l751 (Commentar. Gotting., vol. I), and entitled *De figuris superficierum fluidarum*, says as follows:

" If a liquid falls freely into a nonresistant medium, the action due to the weight of the higher portions on those which precede them is completely null. And if two immiscible liquids are given with perfectly equal densities, and one pours a small quantity of one of them in a vessel containing a larger quantity of the other, the weight of the first will be constant - by the pressure of the second in such a way that this one will not be able to exert any action to preserve or change the shape of the immersed liquid. The drop which falls, or the immersed drop as we have just said it, will thus take identically the same shape as a drop would without gravity."

Segner only states this principle, he does not test the application of it, and doesn't draw any part.

Let us add that, in same work, he is led, by a theory about which we will further speak, to the consequence that a liquid mass finite and without gravity should take the spherical shape liquid mass deprived of gravity. Soap bubbles, for example, when they float insulated in the air, have, each one knows it, a spherical shape, as the oil mass immersed in our diluted alcohol.

§ 4. Let us return to the first process. Before employing it with the realization of the shapes of equilibrium, let us describe the apparatus and the preparations necessary to the experiments. If one operated in a bottle, the refraction would deteriorate the appearance of the produced shapes, it would show them widened in the horizontal direction; it is thus necessary to get a vessel with plane walls made out of plate glass. What appeared to me most suitable is to give to this vessel the form of a cube of 20 centimetres on a side; the glasses which constitute the side faces and the bottom are glued in an iron frame. As for the lid, it consists of another plate of glass of the same dimensions, and than one simply sets it on the top of the vessel. This plate lid is bored, in its middle, with a broad circular opening furnished with an iron neck which one closes by means of a stopper of the same metal; this is crossed along its axis, held by soft friction, by a cylindrical stem in iron[4] approximately 15 centimetres high and 5 to 6 millimetres thickness, whose higher end carries a button which makes it possible to seize it easily, and whose lower end is hollow threaded in the direction of the axis, so that one can screw there parts which we will indicate. The higher edge of the vessel is polished with emery; in this way, when the lid is placed, the alcohol of the interior mixture cannot evaporate. Finally the apparatus is provided with fixing screws, and a tap to let the liquids run out.

We must announce, as of now, two causes which tend to bring disorder in the experiments, and against which it is necessary to be warned as much as possible.

§ 5. We initially concern ourselves with the first. Let us suppose that, by an operation about which we will speak soon with detail, one obtained a beautiful oil sphere in perfect equilibrium density in the alcoholic mixture. This equilibrium is not long in deteriorating: after a few minutes, one sees the sphere leaving its place, and going up with an extreme slowness. If one then adds to the ambient liquid a little alcohol to restore the equilibrium, it is still broken in the same manner at the end of a certain time; it would be necessary to continue for two or three days to maintain it by the successive addition of small quantities of alcohol, to make it finally appreciably permanent. It is that oil absorbs in rather great proportion the alcohol of the mixture in contact with it, and becomes less dense, while the mixture, which loses its alcohol thus, increases, on the contrary, in density. Let us add that, on its side, the alcoholic mixture dissolves a little oil, or rather of the olein which belongs to oil.

§ 6. One conceives, according to that, that it is essential to employ only two liquids rendered, by mutual saturation, inert with respect to each other. Here, to achieve this goal, the means which I believe the best:

One starts by making, in a large bottle, a mixture of alcohol and distilled water measuring 22° with the hydrometer of Beaumé; one adds to it, if that is necessary, a certain quantity of alcohol, so that while introducing into the liquid, using a funnel, a small oil mass, it settles slowly to the bottom. One then pours, by the same funnel, a quantity of oil such as could constitute a sphere from 7 to 8 centimetres in diameter, then, after having stoppered the bottle, one rather quickly turns it over a great number of times, but without shaking it, until the oil is divided into spherules a few millimetres in diameter, and one then lets rest the whole. After a time more or less long, the spherules are, for the most part, reunited together in one single mass. When the merging is partially produced, when the whole is reduced to a small number of rather bulky

[4] I indicate iron and not copper, for all remaining metal of the apparatus, because it is a question of employing oil, and this liquid does not remove anything from iron, while, in prolonged contact with copper, it attacks it slightly, takes a green color, and, which is a serious disadvantage, increases density.

masses, one can stimulate the operation: for this purpose, one introduces the end of a wire into one of these masses, and the adherence which it contracts with metal makes it possible to lead it to bump against another, if it were not there already; but that is not enough so that both merge; they remain separated by a thin film of alcoholic liquid, which seems to oppose it with a certain resistance[5], and that one pokes with the wire; one is soon thus able to have but one single mass, assembled from very small spherules disseminated in the ambient liquid.

If then the single mass occupies the bottom of the bottle, one again subdivisides it by reversals; if it occupies the top of the liquid, one adds a little alcohol, then one turns over in the same way several times the bottle, but with great precaution, in order not to divide the mass or to separate it only in a very small number of parts, and it arranges itself in such a manner that it still ends up going down slowly at the bottom, after which one subdivides it, like previously, in small spherules which one again lets meet; finally one adjusts the densities so that the mass does not express any more a tendency pronounced to go up or to go down.

It can happen, in consequence of a defect of homogeneity of the oil, that after a subdivision, part of the spherules gather at the bottom and another part at the top of the liquid; when that takes place, and each group of spherules is converted into a mass, one leads the smallest towards the other using the wire, and one obliges them to form but one.

In the operations of adjustment, when the proportion of alcohol is too strong, one should not add pure water to the mixture, because having dissolved oil, water would precipitate the latter, and the liquid would become milky; one avoids this disadvantage while employing, in place of pure water, a mixture of alcohol and water at 16° Beaumé. Let us say here that, when one is very close to the equilibrium of densities, the quantities of alcohol or liquid at 16° added should be extremely small.

When, after these operations, oil is, almost totally, constituting a single mass without appreciable tendency to go up or to go down, one taps it by means of a siphon started by a side tube, and one receives it in another bottle.

As one must get kind a quantity of inert mixture alcoholic more than sufficient to fill the vessel with plane walls, and a quantity also ogf inert oil greater than that which one considers necessary for the experiments, it is necessary to carry out the preparation above in several large bottles at the same time. When finished, one puts all the oil masses in the same bottle so that their sum must almost entirely fill it and then one stoppers it carefully, without which the oil would gradually lose the alcohol which it absorbed. It appears initially strewn with spherules of alcoholic mixture; but these spherules yield little by little to oil a portion of their alcohol, and, having an excess of density consequently, end up gathering at the bottom of the bottle in one or more masses which one removes with a pipette. Finally it happens sometimes that oil thus prepared loses its limpidity, when this circumstance arises, one does not worry, it does not harm the experiments.

As for the alcoholic mixture left in the large bottles, it is, on its side, strewn with a multitude of very-small oil spherules; but one disencumbers it by a filtration, keeping the funnels covered to avoid the evaporation of alcohol.

§ 7. The second disturbing cause lies in the variations in the temperature, and it is difficult to imagine how much our immersed oil masses are sensitive thereto: for example, a large sphere of oil being well in equilibrium in the ambient liquid, if one carries the vessel to a room somewhat hotter or colder than that where it was initially, the sphere does not delay, in the first case, to go down, and, in the second, to go up; it is

[5](1) Boyle had already observed this phenomenon by carrying out the experiments which were in question in the note of § 3.

even enough to apply hands to the outside of the vessel to see it, after a few moments, to start to go down. It is understood, according to that, that when one wants to carry out the shapes of equilibrium, it is necessary to operate in a place whose temperature remains appreciably constant throughout the experiment.

This condition is impossible to fill when giving a public lesson: if one regulates the densities before the arrival of the listeners, the presence of those necessarily raises the temperature, and equilibrium deteriorates. We will indicate however, below, a means of countering this disadvantage.

§ 8. Now that we have prepared an oil and alcohol mixture, and that we know the influence of the variations in the temperature, we can begin the experiments.

But before that, I believe it necessary to insist on a point; I am, for a great number of years, afflicted with complete blindness; the reader could thus conceive some doubts about the whole exactitude of the facts that I lay out, and I must reassure you in this respect. All the experiments of this first chapter, as well as part of those of the remainder of the work, were carried out by myself, when I still enjoyed the plenitude of my sight; the others were always done, except very rare exceptions, in my presence, under my direction, with all the precautions which I could imagine to avoid errors, and by people accustomed to observation. These people, which it will be enough for me to quote here the names to dissipate any uncertainty, are: Mr. Duprez professor of physics at the Athenaeum and the Industrial School of Ghent, Mr. Donny professor of chemistry, Mr. Lamarle professor of construction, and Mr. Van der Mensbrugghe in charge of the course of mathematical physics, at the University of the same city; finally my son Felix, well-known as experimental naturalist.

§ 9. Let us pass finally to the description of the experiments. That is to say initially simply to make a large sphere freely suspended in the alcoholic mixture. One starts, for a reason which one will know soon, by covering with a cotton fabric square the bottom of the vessel with plane walls, then one pours in this vessel, by the opening of the lid, the alcoholic mixture until a suitable height is reached, and one agitates it there with a spatula of glass to ensure the homogeneity of it. The oil which one must introduce also not being homogeneous, one also mixes it with the assistance of a glass rod , having care to place a plug of wadding in the neck of the bottle, so that oil does not lose alcohol appreciably. One then sets in the neck of the lid of the vessel a small funnel whose collar penetrates a few centimetres into the alcoholic liquid, and one pours oil there, after having taken the precaution to surround by a thick fabric the bottle which contains it, so that the heat of the hand cannot modify the density of it.

As, is likely, the temperature at which one operates not identically the same as during the preparation of the liquids, the oil poured by the funnel rises up to the surface of the ambient liquid, or goes down to the bottom of the vessel. In the first case, one stops after having introduced a mass of little volume, one removes the funnel and one absorbs, by means of a small glass syringe with a long nozzle, the floating mass; one adds then a little pure alcohol, which one mixes carefully, and one repeats, if that is necessary, the same operations, until the oil poured in the funnel descends with slowness, and will settle on the cotton fabric. I do not need to say that if too much alcohol had been added, one would resort to the mixture with 16°. Finally one continues to pour oil, to give the mass the desired volume.

Let us say here that the piece of fabric is essential in all the experiments: impregnated with alcoholic liquid it is not able to be wetted by oil, while, without its presence, when the mass goes down, it can contract adherence with the bottom of the vessel, be spread out, and make thus impossible the later operations there.

It can happen that the oil, instead of accumulating on the fabric in just one large mass, is subdivided in several separate masses; one then joins those together using the wire (§ 6), then one adjusts the densities. For this last operation. for mixing closely with the remainder of the ambient liquid the small quantities added of pure alcohol or alcohol to l6° one does not introduce the spatula by the opening of the lid; it is more convenient to leave this opening closed and to slide the top plate so as to open a small portion of the top of the vessel, by which one inserts the spatula; one agitates this gently and long enough in the liquid, taking great precautions so that the oil mass does not divide and so that it will not adhere to the side walls of the vessel.

When equilibrium is definitively established, one thus has the curious spectacle of a large perfectly spherical, and motionless liquid mass within the surrounding liquid.

§ 10. If one wants to show the result to an audience (§ 7), one ends the handling in a little different way: instead of making the two densities equal, one intentionally establishes a certain heterogeneity in the alcoholic liquid; one makes it initially so that this liquid contains a small alcohol excess, then one slowly pours in a suitable quantity of mixture at 16°. This, due to its excess of density, goes down to the bottom of the vessel, where it spreads in a horizontal layer. Then one introduces the oil, which, because of the small alcohol excess which contains the higher mixture, goes down through this last, and comes, either in only one mass, or in several partial masses, to be poised on the denser layer of the lower mixture. That being done, one joins together, if it is necessary, the isolated masses into just one; then one agitates the liquid with care so as to imperfectly mix the layer at the bottom with higher layers, but without dividing the oil mass, and one then lets the system rest. One sees that there must result, in the alcoholic liquid, a state of increasing density starting from the layers less dense than oil, to the sub-layers denser than this same oil, and that, consequently, the oil mass will have to be held in steady equilibrium vertically, in a certain layer of which the average density is equal to its.[6]

Then, if the temperature has risen or has suddenly dropped a little, the oil sphere will go down or go up a certain amount; but, if the ambient liquid is sufficiently heterogeneous, it will soon meet a layer having on average the same density that it has, and will stop there.

In truth, under these conditions, the mass cannot constitute a sphere rigorously any more; it must be flattened by a small quantity in the vertical direction; but, unless the heterogeneity of the ambient liquid is not too strong, this flatness is insensible to the eye, and the mass appears spherical.

§ 11. I described the precautions thoroughly to be taken to ensure the success of the current experiment; they are the same ones with regard to all the other experiments which are done by the process of immersion, and they are essential. One soon acquires practice, and then all becomes very easy; but if they are neglected, only extremely imperfect results are obtained.

Let us furthermore describe a circumstance which could be embarrassing: in the course of an experiment, it happens rather often, either by a fall in temperature, or because one has introduced a too great quantity of mixture at 16°, that the mass used rises to the surface of the ambient liquid and flattens out more or less, but leaves above it a thin film of this liquid, a film which seems resistant, as between two juxtaposed masses (§ 6); then, after some time, the mass presents a portion of plane surface at the level of that of the surrounding liquid; and, which is strange, it has, so to speak, contracted an adherence with this last surface. For detaching it completely, the only means is to pour with care a little pure alcohol, which spreads on the set of two surfaces,

[6] The various liquid layers thus superimposed admittedly tend to mix themselves; but as they are placed in the order of their densities, this spontaneous mixture is carried out only with extreme slowness, and one needs a great number of days for the liquid to become homogeneous. It thus does not result in any disadvantage for the experiments.

and thus destroys the adherence in question; one mixes then this pure alcohol with the subjacent alcoholic mixture.

It is needless to add that after each series of experiments, one taps, as we said (ibid), the oil mass using a siphon, and that, if the alcoholic mixture is strewn with small oil spherules, one filters it by keeping the funnel covered.

§ 12. Before going further, let us insist on a remark. The attraction concerned in my experiments is molecular attraction, which, one knows, is exerted in a significant way only up to an excessively small distance, and the forces which determine the shapes of our immersed oil masses emanate only from a surface layer whose thinness is extreme. On the contrary, in a large presumedly fluid celestial mass, the action of the surface layer is insensible, and the effective attraction is gravitation, under the terms of which all the parts of the mass act on each other whatever their mutual distances. These two species of attraction must thus produce different results; if both give the sphere, it is because of the perfect symmetry of this shape, symmetry which would make it a form of equilibrium under all conceivable laws of attraction; but, besides this unusual special case, one would be mistaken if one wanted to draw from my experiments some induction with regard to astronomical facts.

13 Now let us apply our process to new experiments: it will enable us, initially, to check the most significant principles of the theory of the capillary action.

Let us start with that which we pointed out above, and whereby capillary actions emanate very from a surface layer excessively tiny thickness.

Let us conceive an unspecified solid system plunged into the interior of our oil sphere, and give to this sphere a volume such that it can wrap the solid system completely without touching the surface at any point of it. Then, if the principle in question is true, the presence of the solid system will not have any influence on the shape of equilibrium, since, in these circumstances, the surface layer, from where the shaping actions emanate, remains entirely free; while if these actions emanated from all the points of the mass, a nonsymmetrical modification made to the interior parts of this one would necessarily make one in the external form.

It is confirmed by experience: the state of a solid system completely wrapped by an oil mass would be rather difficult to realize; but one can approach it very nearly by introducing into the liquid sphere an arbitrarily shaped iron plate suspended by a very thin iron wire which runs through it as well as the oil mass; as long as no point of the edge of this plate reaches the surface of the liquid mass, it preserves its spherical form. In mathematical rigor, sphericity must be, as we will see below, somewhat degraded by the wire which pierces the surface in two points; but, in consequence of the smoothness of this wire, the deterioration is completely inappreciable.

Here is another fact of a similar nature. In the course of the experiments, it happens sometimes that portions of the alcoholic liquid are imprisoned in the interior of the oil mass, and there form many isolated spheres. However these spheres can be placed in an arbitrary way in the interior of the mass, without resulting in the least modification of its external shape.

Let us furthermore insert in the liquid mass an unspecified solid system; but now let us give to the mass too small a volume for it to constitute a sphere which wraps this system completely. Then this last will reach necessarily the surface layer, and, if the principle is true, the shape of the liquid mass will have to change, or, in other words, will not be able to remain spherical any more. It is what takes place indeed, as one would expect: the liquid mass spreads on portions of the solid system which stick outside its surface; it ends up occupying either the totality of these portions, or only part of their extent, according to the form and dimensions of the solid system, and thus forms another shape of equilibrium. We will see examples of them later.

Instead of inserting the solid system into the interior of the liquid mass, simply let us put it in contact with the external surface. Then an action being established on a point of the surface layer, equilibrium will have to be broken and the shape of the liquid mass will have to change. It is what happens: the mass spreads on the surface which is offered to it, and takes consequently a new shape. One could believe that this case repeats one of those of which we have just spoken; because it seems that the liquid mass, while spreading on the solid system to reach the new shape of equilibrium, must end up occupying or wrapping this system in the same way as if one had originally inserted it in the interior. There are indeed circumstances in which things must occur thus; but the experiments which we will report show that there are other circumstances for which the result is very different.

§ 14. Let us take for the solid system a thin circular plate of iron[7] carried by a wire of the same metal fixed perpendicularly at its center (fig. 1). The free end of this wire is threaded, so as to be able to screw tightly in the end of the cylindrical stem (§ 4); when the lid of the vessel is in place, the small solid system is thus immersed in the alcoholic liquid.

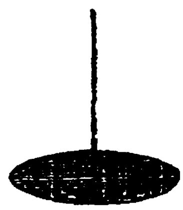


Fig. 1

Let us inform here the reader, to avoid repetitions, 1° that all those of our solid systems which must be suspended in the alcoholic liquid, attach in the same way; 2° that they are all made of iron, for the reason stated in the note of § 4; 3° that before introducing into the vessel any of these solid systems, it should be entirely wetted with oil, and for this reason it would not be enough to simply soak it in this liquid, it is necessary to rub some slightly with the finger.

Now, an oil sphere being realized beforehand in the alcohol mixture, let us create adherence between its higher part and the lower face of our solid plate[8]. Once contact is well established, oil spreads quickly on the surface which is offered; but, which is remarkable, though one took the precaution to rub oil all over the system, i.e. the two faces of the plate as well as its edges, oil stops clearly on the edge without passing to other side of the plate, and thus presents an abrupt interruption in the curvature of its surface.

In the present case, the new shape which the mass takes is a portion of sphere. This portion will be all the larger relative to the complete sphere, when the volume of the oil mass is larger; but always the curvature will stop clearly with the contour of the plate (see fig. 2, which represents the section of the solid system and the adherent mass, for three volumes different from this one).

As for the cause of this singular discontinuity, one understands it without difficulty: the plate reaching along its contour the surface layer, it is natural that there appears along this contour something particular, and that continuity in the shape ceases where it exerts, on the surface layer, an outside attractive action.

§ 15. We still use the plate as above, but instead of presenting one of its faces outside the oil sphere, now let us insert the plate by its edge in the interior of this

[7] The diameter of that which I used myself was 4 centimetres. I mention this diameter to fix the ideas; in our experiments, dimensions of the solid systems are completely arbitrary, and have as other limits only those which are imposed by the size of the vessel.

[8] So that this operation can be carried out with facility, it is necessary initially that the oil sphere is held, in the ambient liquid, below the opening of the lid; then, the plate being introduced into the vessel, one has but to lower it using the stem which goes through the stopper, to bring it towards the mass. If the latter did not occupy the proper position, one would lead there beforehand by pushing it by means of a glass rod.

We must point out that the real contact between the plate and the sphere of oil is usually not established immediately: there is a certain resistance to overcome, similar to that in question in § 6 and 11; but, to surmount it, it is enough to push a little the liquid sphere with the plate; the light pressure which results from it soon ruptures the obstacle and produces adherence.

sphere[9]. Then the liquid will necessarily spread on the two faces of the solid, and if the diameter of the original sphere was less than that of the plate, one will see oil forming, on the two faces, two spherical segments whose curvatures will stop clearly at the contour of the plate.

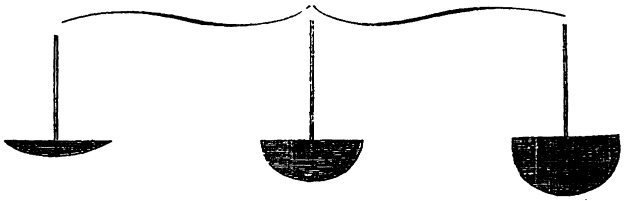


Fig. 2

These two segments can be equal or unequal, according to whether one introduces the edge of the plate into the liquid sphere so that the plane of the plate passes, or not, through the center of the sphere. The higher segment will be slightly deformed by the action of the supporting wire; but this effect would be insensible if the plate were supported by a very fine wire. Fig. 3 represents the result of the experiment with two unequal segments.

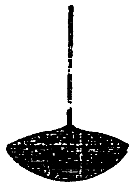


Fig. 3

Discontinuity in the curves is a very general fact, which we will see frequently reproduced in the course of our experiments; it will lead us further with consequences of a great interest.

§ 16. Here is an important remark. When a liquid shape stops thus at a solid contour, one must regard it as a portion of one of equilibrium more extended, which can be imagined continuing beyond the solid contour. Indeed, the molecular attraction exerted by this contour extending only up to an excessively small distance, all the remainder of the surface layer of the liquid mass produces its pressures freely, and is consequently worked so as to satisfy formula [2] of § 1; however the geometrical law to which its form is subjected cannot cease abruptly with the contour in question. Thus, in the experiments of the two preceding paragraphs, the shapes which are pressed either on the lower face, or on the two faces of the plate, are segments of a sphere, always ignoring the small deformation that the solid wire subjects the higher segment to. We will see soon that using suitable solid systems, one can make very varied partial shapes of equilibrium.

§ 17. Here is another fact which shows in a rather curious way that the surface layer alone is the seat of the shaping actions. If one lets it sit for two or three days, a fresh oil mass in the alcohol liquid will in time establish equality of the densities (§ 5), then when one taps the major part of the mass by means of a siphon, one sees, when this mass is sufficiently reduced, that its surface forms folds, and so when one removes the siphon, the remaining mass, which remains suspended in the ambient liquid, does not take again a spherical shape; it preserves an irregular aspect, and appears indifferent to all the forms. It is that, in consequence of the unequal solvent action exerted by the alcoholic mixture on the elements of oil (ibid), that solid principles become prevalent on the surface itself, and, condensing out of the mass progressively, end up constituting

[9]Here is how this operation is carried out. One supports at some distance above the neck of the lid the stopper which carries the system of the plate, in such a manner, however, that this plunges to a sufficient depth in the alcohol mixture. There is thus freedom to make with the plate rather wide movements, and one brings it towards the liquid mass. The latter must, for that, occupy beforehand a suitable position. Once the liquid mass starts to touch, one holds the plate at rest until the action is finished, after which one sets with care the stopper in the neck.

a kind of film. Thus when the surface layer loses part of its liquidity, the mass loses at the same time its tendency to take a definite equilibrium shape.

§ 18.Now et us check a second principle, that of the pressures exerted by the surface layer on the mass.

The solid system that we will employ is a pierced circular plate (fig. 4). It is placed vertically, and is attached by a point of its circumference to the wire which supports it. Let us give to the oil sphere a diameter less than that of the plate, and insert the plate by its edge in the mass, in a direction which does not pass through the center of the sphere. The oil will form initially, as in the experiment of paragraph 15, two unequal spherical segments; but things will not persist in this state: one will see the most convex segment decreasing gradually by volume, and consequently by curvature, while the other will increase, until they became perfectly equal between them. Part of the oil thus passes through the opening of the plate to go from one of the liquid segments towards the other, until the equality above is reached.

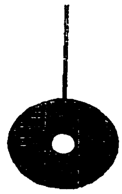


Fig. 4

But, let us note, once the oil spreads on the two faces of the plate, so that the surface layer reaches all the circumference, the action of the solid system is supplemented, and the movements which occur then in the liquid mass to reach the form of equilibrium, can consequently be due only to an action emanating from the free part of the surface layer. It is thus the latter which drives out the liquid through the opening of the plate from one curved segment towards the other, from which one can conclude that the liquid is subjected to a pressure at least on behalf of the surface layer of the curved segment. But it is easy to see that the surface layer of the other segment exerts also a pressure, which, only, is less than the preceding one; indeed, if for the curved segment one came to substitute a segment which was, on the contrary, less curved than the other, oil would then be driven out in the opposite direction. It follows from all that that the total surface layer of the mass exerts a pressure on the liquid which it contains, and that the intensity of this pressure depends on the curvature of the free surface. Moreover, since the liquid goes into the the curved segment for which is curved less, one sees that, for a convex surface of spherical curvature, the pressure is all the more strong as the curvature is pronounced, or that the radius of the sphere to which surface belongs is smaller.

This influence of the curve was, as well as the existence even pressures, indicated by the theory when it is about a surface of spherical curve convex, formula [1] of § 1, which represents the pressure exerted by the surface layer in an unspecified point of this one, becomes $P+\frac{A}{R}$, since, in this case, R' is equal to R; and like, for the same liquid, P and A are constant, it is seen that the pressure of which it all is is all the more energetic as the ray R is smaller, or, in other words, that surface is curved.

Thus our experiment confirms fully the theory, not only the existence of capillary pressures, but also their dependence on curvature.

The relation between the formula above and our experiment suggests a remark: the term P, which indicates the pressure corresponding to a plane surface, is the same one for our two liquid segments; however, the pressures being transmitted throughout the mass, it results that the portion P of the pressure due to the surface layer of the one of the segments is neutralized by the same portion P arising from the other segment; consequently, the pressures really concerned in the phenomenon which we have just described, are simply expressed, for each segment, by the term $\frac{A}{R}$, i.e. it depends on the curvature.

§ 19. The principle checked in §§ 13 to 17 lead us to modify the preceding experiment so as to obtain a significant result. Equilibrium once produced, it only by its external edge than the pierced plate acts on the surface layer of each of the two spherical segments; all the remainder of this plate is thus then without influence on the total shape. However it follows therefrom that it would be still the same if one made the opening larger; only, the larger the diameter of the latter, the less time will be necessary until equality between the two curvatures is established. Lastly, one must be able, without changing the equilibrium shape, to increase the opening until close to the edge of the plate, or, in other words, to reduce the solid system to a simple ring of thin wire.

It is what the experiment confirms; but, to put it into execution, one cannot limit oneself, as previously, to inserting the solid system into an oil sphere of a diameter less than that of this system, and to then let the molecular forces act; because the wire, because of its little thickness, would not oblige the liquid to spread so as to adhere to the totality of the ring. The mass would then remain impaled by a part of it, and its spherical form would not be appreciably degraded if the wire is thin: to obtain the sought shape, one starts by giving to the oil sphere a diameter a little higher than that of the metal ring; then one introduces the latter in the mass in such a manner that it is completely enclosed; finally, using the small glass syringe (§ 9), one gradually removes liquid from the mass[10]. Then decreasing in volume, its surface is soon supported on all the contour of the ring, and, continuing the volume decrease, a lenticular form appears. One then can, by new subtractions of liquid, reduce the curvatures of two surfaces to the degree one considers suitable. One obtains in this manner a beautiful biconvex lens, entirely liquid except for its circumference. Moreover, due to the considerable excess of the index of refraction of the olive oil over that of the alcoholic mixture, the lens has all the properties of lenses of convergence: for example, it enlarges the objects at which one looks through, and one can vary this enlargement at will, by removing or by adding liquid to the mass.

Our liquid shape thus carries out what one could not obtain with lenses of glass, i.e. it constitutes a lens with variable curvature and enlargement.

The one that I formed had a diameter of seven centimetres, and the metal wire diameter was approximately a half-millimetre. One could employ with the same success a wire much thinner; but then the apparatus would become inconvenient by its too great ease of becoming deformed.

While acting with care, one can decrease the curvature of the lens until making it almost zero: I could reduce, for example, the lens which I formed and whose diameter was, as I said, seven centimetres, to have no more than two or three millimetres thickness.

§ 20. We have just shown the agreement of experiment with theory in the case of convex surfaces of spherical curvature; we will see that the agreement remains in the same way in the case of plane surfaces and concave surfaces of spherical curvature.

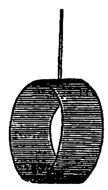


Fig. 5

we use a solid system formed of a broad band of iron circularly curved so as to constitute a hollow roll, and attached to the suspension wire by a point of its external surface (fig. 5). Not to bring into the experiment the production of additional phenomena, we will suppose that the metal band's width is smaller than the diameter of the cylinder formed by this same band, or that it is at most equal to it. Let us make the oil mass adhere to the interior surface of this system, and suppose that the liquid is enough that it sticks outside the ends of the cylinder. In this case, the mass will have, on each side, a convex surface of spherical curvature, and the curvatures of these two surfaces will be equal. This shape is a consequence of what we saw previously, and we should not stop there; but it will be useful to us as a starting point, to arrive at the other shapes which

[10]One introduces the nozzle of the instrument by the opening which one spared with the top of the vessel while making slip (§ 9) the plate lid.

we need.

Let us apply the nozzle of the syringe to one of the convex surfaces above, and remove liquid gradually. Two surfaces will then decrease in curvature at the same time, and, operating with care, we will thus be able to make them perfectly plane; however we know (§ 2) that a plane surface also satisfies the equilibrium condition.

Let us apply then the nozzle of the instrument to the one of these surface layers, and remove a small quantity of liquid. Then both surfaces will grow hollow simultaneously, and will constitute two concave surfaces of spherical curvature, whose edges rest on those of the metal band, and whose curvatures are the sames. Lastly, by new extractions of liquid, the curvatures of the two surfaces will become increasingly strong, while remaining always equal to each other.

This experiment leads obviously to the following consequences: since the other free surface layer becomes concave spontaneously as soon as that to which one applies the instrument becomes concave, it should be concluded from that that the surface layer belonging to the first exerted a pressure, which was counterbalanced by an equal force arising from the opposite surface layer, but which ceases being it and which drives out the liquid, as soon as this opposite layer starts to grow hollow. Moreover, since new extraction of liquid determines new departure from equilibrium, so that surface opposite to that on which one acts directly shows spontaneous hollowing when the other surface increases curvature, it follows that the concave surface layer pertaining to the first still exerted a pressure, which, initially, was neutralized by an equal pressure coming from the other concave layer, but which becomes dominating and drives out the liquid again, when this other layer increases curvature.

The experiment thus makes visible: 1° that a surface layer determines a pressure on the liquid; 2° that a concave surface of spherical curvature determines also a pressure; 3° that the latter is lower than that which corresponds to a plane surface; 4° that it is of as much less than concavity is pronounced, or than the radius of the sphere to which surface belongs is smaller.

But all these results were already announced by formula [1]: since they are always surfaces of spherical curvature, one has, as previously, $R' = R$; moreover, surfaces considered being concave, the radius R is negative (§ 1); the formula becomes consequently $P - \frac{A}{R}$. But it is known, according to the theory of Laplace, that the term P is always very large relative to the following; the total pressure expressed by this formula is thus necessarily positive, and less than P; moreover, one sees, it is all the more weak as the radius R is smaller, or, in other words, that surface is more concave; it is reduced besides to P when the radius R is infinite, i.e. when surface is plane.

In the application of this same formula to the experiment, one can, just as in the case of § 18, notice that the portion P of the pressure is neutralized, and to look at thus as forces acting with each of two surfaces only that which is represented by the $-\frac{A}{R}$ term; however this term being negative, the force ut represents is a suction, and is all the more energetic as the curvature is stronger.

§ 21. The shape that we have just obtained constitutes a biconcave lens with equal curvatures, and it enjoys all the properties of lenses of divergence, i.e. it reduces objects that one looks at through it, etc. Moreover, as one can increase or to decrease the curvatures of the two surfaces in degrees as small as wanted, it follows that one thus has a lens of divergence of variable curvature and strength.

After having formed a liquid lens of convergence and a liquid lens of divergence, it made me curious to combine these two species of lenses, in order to form a liquid spyglass. For that, I initially substituted for the ring of wire of paragraph 19, a circular plate of the same diameter pierced with a large opening (fig. 6); this plate being turned on a lathe, I was certain to have it perfectly circular, while it would be quite difficult to meet the same condition with a simple wire of curved iron. In the second place, I took, for the solid part of the biconcave lens, a band of approximately 2 centimetres width and curved into a cylinder of $3\frac{1}{2}$ centimetres diameter. These two systems were assembled as represented in fig. 7, so that all the apparatus being suspended vertically in the alcoholic mixture by the wire has, and the two lenses liquid being formed have, their two centers at the same height, and 10 centimetres apart. In this situation, one cannot adjust the spyglass by modifying the distance between the objective and the eyepiece; but one arrives at the same goal by varying the curvatures of these two lenses. Using some trial and error, I easily managed to obtain thus excellent Galilean spyglasses, enlarging approximately twice objects far away, like ordinary spyglasses, and giving perfectly clear images with very little iridescence. Fig. 8, which represents across-section of the system, shows both lens. It is understood that the concave lens must brought very close to the face of the vessel through which one looks; this is why the suspension wire is more distant from this lens than other.

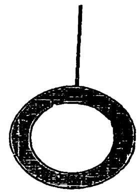


Fig. 6

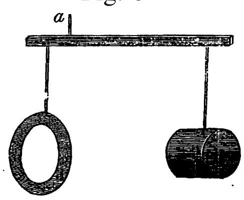


Fig. 7

§ 22. Here finally is a checking of a very different kind, relating to yet another point. As is known, the rise of heavy liquids in tubes of which they can wet the walls has some significance only when these tubes have very small internal diameters, from whence came the term capillary phenomena, and gravity always establishes a limit to the level of the raised column. But if the action of gravity is neutralized, these restrictions must disappear, and the liquid must be able to fill indefinitely a tube of an unspecified diameter.

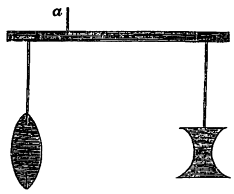


Fig. 8

I was curious to test this application of my process. It is conceived initially that it is necessary to maintain the higher opening of the tube below the surface of the alcoholic liquid which fills the vessel, so that, all occurring within this liquid, gravity cannot exert any influence on the phenomenon.

With this condition, the experiment appears very simple; it seems that it is enough to proceed in the following way: 1° to form, in the alcoholic liquid, an oil sphere of a suitable volume, two liquids having the same density; 2° to bring this sphere close to the bottom of the vessel; 3° to take a tube of glass of an unspecified diameter, but a length such that supposing its lower end in contact with the top of the oil mass, its higher end does not reach the surface of the alcoholic liquid; 4° to wet oil perfectly in the interior of this tube; 5° finally to introduce this same tube vertically into the vessel, there to insert until it touches the oil mass, and to keep it, by some means, motionless in this position.

§ 23. Scarcely any experience is necessary to do it well, in fact; but it is necessary to employ some additional care which facilitates the operations, and without which one would seldom obtain a complete result.

In the first place, considering the nature of the phenomena which one wants to observe, one understands that it is advantageous to replace the vessel with plane walls which was useful to us up to now, by an apparatus having less width and more height; that which I made use is a large test-tube made out of glass, 10 centimetres broad and 55 high.

In the second place, a perfect equality between the densities of oil and the ambient liquid takes a rather long time to obtain; therefore it is more convenient to make it so that the oil has a very small excess density so it sinks very slowly to the bottom of the vessel, where one will have placed beforehand a piece of cotton fabric (§ 9); if the excess density is extremely small, it will have little influence in the presence of the action of the molecular forces.

In the third place, so that the tube once introduced into the apparatus was supported there in a suitable position, I employed the simple means stated here: close to the higher end of the tube, I rolled up a wire of copper, by then twisting it with a grip to tighten it, but in a manner that there remained two free ends of sufficient length; these two free ends were raised obliquely, and I folded up the ends in the shape of hooks; those being set on the higher edge of the cylindrical vessel, are thus used to suspend the tube in the interior of this vessel. Moreover, to prevent that the tube did not swing inconvenienlyt, I attached, about its middle, and by the same means, a second wire of copper whose ends were not folded up in hooks and made springlike contact with the interior wall of the vessel[11] in this way, the tube is kept motionless on the axis of the system. Let us add that the length of copper wire with hooks must be such that when the tube is placed, its lower opening reaches the top of the oil sphere which rests on the bottom of the vessel.

In the fourth place, it is necessary to furnish the lower end of the tube with an iron collar, giving its lower contour a thin edge approximately 3 millimetres wide. This addition aims to oppose the oil mass rising in an irregular way up the external surface of the tube; here, as in the experiment of § 14, the small edge stops any external rise of oil, and obliges the mass to take a perfectly symmetrical position under the tube.

In fifth place, to be able to appreciate the speed of the movement of the ascending liquid column and the variations in this speed, one marks, with ink, on the tube, features perpendicular to its length and also spaced decimetre by decimetre, for example, starting from the lower opening; these features are traced all around glass, in order to make the observation easier.

In sixth place, if, before plunging into the vessel the tube provided with all the above accessories, one restricts oneself to wetting with oil its interior surface, one sees soon appearing a phenomenon which embarrassed me a long time ago: the oil film withdraws irregularly to accumulate in certain places and to trade places, elsewhere, with the alcoholic liquid, so that the continuity of the oily layer is destroyed. It is in vain that one takes all possible care to oil perfectly the tube inside; one can even boil the oil beforehand, but the above effect does not occur any less. After several unfruitful tests to counter this problem, I thought of the following process, which gives a complete success. One closes one of the ends of the tube with a cork stopper, and one fills it with very hot molten lard, that one lets remain there for a few minutes; one then empties the tube, uncorks it, and, suspending it vertically, one lets it drain, until the light layer of lard which remains adherent there is entirely cooled; care is taken to turn it over from time to time, in order to make a more uniform thickness of the lubricating layer. The tube being thus prepared, one stoppers it again, fills it with oil, empties it immediately, uncorks it and plunges it at once in the vessel; the oil film retained then by its adherence with the lard does not separate any more.

§ 24. When things are organized well, the experiment has every success: as soon as adherence is established between the oil sphere and the edge which furnishes the opening of the tube, one sees oil rising gradually in the interior of the latter until reach-

[11]It is necessary to employ copper wire here, and not iron wire: because this last could scratch the vessel inside, and one can often enough make an imperrceptible line on the internal wall of a glass vessel, causing it to break.

ing the higher end of it, although this tube has a large diameter, and that one gave it all the length which the provision of the apparatus allows. During its rise, the oil column is shown bounded by a concave hemispherical surface. Its movement is a delayed movement; we will soon know the reason for it.

In one of my experiments, the tube had an internal diameter of 14 millimeters, and a length of 42 centimetres; the diameter of the oil sphere was approximately 7 centimetres, and this mass had so small an excess of density that it took more than fifteen minutes to descend to the bottom of the vessel.

Here, under these conditions, are the times taken by the top of the liquid column to traverse the successive decimetres along the tube:

1 decimetre	$1'47''$
2 decimetre	$3'37''$
3 decimetre	$6'37''$
4 decimetre	$9'0''$

One sees that these times are growing, and that thus, as I mentioned above, the movement is delayed.

§ 25. Let us seek the reason for this characteristic, and, for that, examine which are the actions brought into play in the upward movement of the liquid.

During the achievement of the phenomenon, the tube is occupied by the whole of two columns, one lower made of oil and which is increasing in length, the other higher made of alcoholic liquid and which is, on the contrary, decreasing; but the densities of the two liquids being to very close to equal, one can look at the sum of the masses of these two columns, or the total mass to drive in the interior of the tube, as not varying appreciably. On the other hand, the force which produces the rise of oil, is a continuous force, and, moreover, increasing. Indeed, it results from the difference of the pressures respectively exerted by the convex surface of the mass attached to the lower opening of the tube and by the concave surface which constitutes the top of the column; however this last surface preserves the same curve, and determines, consequently, the same pressure, throughout the phenomenon, while the first, by the gradual reduction in the mass which it encloses, takes an increasingly strong curvature, and thus determines an increasingly intense pressure.

Keeping up the force exerted on a constant mass, necessarily tends to produce an accelerated movement; but there is in the system a resistance which grows with the height of the oil column, and which, consequently, tends on the contrary to make the movement delayed. This resistance is born from the friction of the double column which occupies the tube against the oil film adhering to the interior wall; the friction of the oily part of the total column is obviously much more extreme than that of the alcoholic part, and as resistance due to the first grows with the length of the oil column, one understands that the total resistance which comes from the two frictions is also growing. The movement thus tends, on the one hand, to be accelerated, and, on the other, to be delayed, and one will easily admit that the second influence can override the first.

Rigourously, in appreciation of the force which produces the upward movement, it would be necessary to consider, in addition to the molecular action of oil on itself, that of the alcoholic liquid on itself, and finally the mutual action of the two liquids. But it is here only about the effect of the curvatures; however, on the surfaces of separation of the two liquids, the curves have opposed directions, according to whether one views them as pertaining to one or the other of these liquids, from where it results that the actions which come from it are of the same direction: for example, to the adherent mass under the tube, the surface of oil being convex, so that its radius of curvature is positive, the action due to the curvature is directed inside the oil (§ 1), and the surface of the alcoholic liquid being concave, which makes its radius negative, the action which is determined by the curvature is directed outside the liquid in question and, consequently, still inside the oil. It is clear, moreover, that these two actions which are added are, at each point, in the same ratio; it is thus enough, for simplicity of reasoning, to consider only one of them, and one can at will take that of oil on itself or that of the alcoholic liquid on itself. As for the mutual action of the two liquids, it does nothing but decrease, in an obviously constant ratio also, the sum of the two preceding ones, and consequently one can neglect it.

§ 26. As one saw above, in the experiment such as I described it, there is a cause which prevents one from seeing the simple effect of the capillary forces, an effect which should give rise to an accelerated movement of the column. But one can attenuate the influence of this disturbing cause; it suffices to carry out the experiment under conditions opposite of the preceding ones, i.e. by filling the vessel with oil and substituting for the oil sphere a sphere of alcoholic liquid. Indeed, it will be necessary whereas the interior wall of the tube is wetted with a layer of this last liquid, and the two parts of the ascending column rubbing against this layer, one will thus avoid the resistant friction of oil against itself. In this version of the experiment, the frictions exerted by the two parts of the column, in the lower part that of the alcoholic liquid against itself, and, for the higher part, that of oil against the same liquid, the difference between these two frictions, whatever is the direction, will be necessarily much less than in the preceding case; from which it follows that total resistance will approach much more to be independent the height of the lower part of the column; one can thus expect to obtain, in this case, an accelerated movement.

§ 27. Let us try to subject these conclusions to experimental proof. But let us note beforehand that the change of the conditions of the phenomenon also requires some preliminary operations. And initially so that the alcoholic sphere, after having sunk slowly in the ambient oil, cannot contract adherence with the bottom of the vessel, one deposits in advance on this bottom an iron disc which one carefully rubbed with oil.

In the second place, the interior wall of the tube must obviously be wetted with a layer of alcoholic liquid; but, if one does not use particular means, this layer divides, as the oil film divides in the preceding experiment when one does not have recourse to the lard coating. The means that I will indicate is completely effective: after having stoppered the tube at one of its ends, one fills it with a rather thick solution of gum arabic, then it is emptied, it is uncorked, and one lets it drain, while turning it over several times so that the gummy layer takes an equal thickness everywhere, until this layer is perfectly dry[12]; then after having stoppered it again one fills it with alcoholic liquid so that some run out immediately, one uncorks it and one plunges it right away into the oil in the vessel.

Finally it is still necessary to prevent the alcoholic mass from partially rising on the external surface of the tube, and, for that, it is enough that the lower end of it is furnished, as in the preceding experiment, with an iron collar; only this collar must be without edge, and it should be carefully prevented that small portions of the above gummy solution do not stick to its face; it is wise, moreover, to rub it with oil, which one will do when one finishes the creation of the gummy layer.

It is needless to add that the tube must be provided with a copper wire intended to ensure its position and with the ink features which are used to observe the progress of

[12] I have encountered certain tubes which did not require this preparation, and which it was enough to wet directly inside with the alcoholic liquid, after having cleaned them perfectly. It will be, of the remainder, more needful to make use of the gummy coating.

the top of the column.

§ 28. When all these provisions are taken, that the tube is in place and is reached at its lower by the alcoholic sphere, the liquid which forms this sphere starts at once to rise in the tube, and its movement accelerates. Here are the results obtained with a tube 15 millimetres in interior diameter and 42 centimetres long, the alcoholic sphere having about the same diameter as the oil sphere of the preceding experiment and having also only a very little higher density, the tube was partitioned in half-decimetres

1 half-decimetre	54″
2 half-decimetres	48
3 half-decimetres	46″
4 half-decimetres	43″
5 half-decimetres	42″
6 half-decimetres	41″
7 half-decimetres	39″
8 half-decimetres	37″

It was many years ago that I carried out these experiments on capillary rise; however I have a vague memory of a fact which I did not take note of then: I believe I also tested with a tube of similar length, but whose internal diameter was only about 5 mm, and to have noted that the liquid did not go up there, or stopped soon. The slowness of the upward movement, in the preceding experiments, shows that the forces which produce this movement are weak, and, in a narrow tube, resistance undoubtedly become sufficient to prevent their action. If thus my memory does not mislead me, it is necessary, to be successful, use broad tubes, as those about which I spoke.

§ 29. Let us return to the equilibrium shapes. In the experiment of paragraph 20, we obtained a shape which had plane surfaces. There were two of them, parallel to each other, and bounded by circular peripheries; but it is obvious that these conditions are not necessary for plane surfaces to belong to a liquid mass in equilibrium. It is understood that the forms of solid contours must be indifferent, provided that they constitute plane shapes. It is understood, moreover, that the number and the relative directions of the plane surfaces can be unspecified, since such circumstances do not have any influence on the pressures which correspond to these surfaces, pressures which will remain always equal between them. Finally the experiment of § 19 showed us that a simple wire is enough to establish discontinuity between the two portions of surface which rest on it. We will be able to thus carry out the abrupt passage between a plane surface and another by means of a wire representing the edge of the angle of intersection of these two surfaces.

All that leads us to this curious consequence, that one must be able to form entirely liquid polyhedra except for just their edges. However, experiment fully verifies it: if one takes for the solid system a wire frame representing all the edges of an unspecified polyhedron, and one makes adhere to this frame an oil mass of a suitable volume, one obtains, indeed, in a perfect way, the polyhedron in question, and one thus has the curious spectacle of parallelepipeds, prisms, etc, formed of oil, and whose only solid parts are their edges alone. Fig. 9 represents the frame of a cube with its handling wire, which must naturally be larger than those which constitute the edges. As for dimensions, I will say that the edges of my cubic frame were 7 centimetres long; here, the rest of the, dimensions, as always are completely arbitrary.

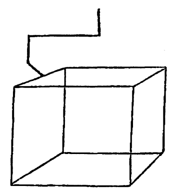


Fig. 9

§ 30. To create one of these liquid polyhedra, one gives initially to the oil mass a rather large volume so that it can enclose the frame, and one inserts this latter; if necessary, one pushes the mass in one direction or in another with a glass spatula well cleaned beforehand. Then, one removes oil by means of a small syringe, and one first sees the surface of the mass supported at that time on all the solid edges; all the faces of the shape are then convex. Continuing the action of the syringe, one sees this convexity decreasing everywhere at the same time, and, while operating by suitable degrees, one is able to render all the faces flat[13].

I must insist in this respect on the exact adjustment of the densities: the slightest difference between them is enough to degrade the surfaces in a significant way. Moreover, in order to reach perfect regularity, it is often useful, not only in the current case, but, in general, in that of any little bulk shape, to employ a precaution about which I did not speak yet. It can happen, in consequence of small solvent actions resulting from the additions of pure alcohol or of mixture at 16° always necessary at the beginning of a series of experiments for the adjustment of the densities, that the oil which constitutes the mass loses its homogeneity; then the portions charged with alcohol and consequently less dense, go to the top of the shape, and tend to go up, while the denser portions go to the bottom and tend to go down. In this case, with a cubic frame, for example, the side faces of the shape could appear appreciably flat, and the faces higher and lower being still slightly convex.

To counter this problem, one introduces into the mass an iron spatula that one stirs in there in all directions for a long time and with care, so as not to deform the frame by shocks; the operation must be carried out before all the oil excessis removed. When the spatula is withdrawn, it takes with it a small quantity of oil which it gives up while leaving the alcoholic mixture, and which one then absorbs with the syringe. Furthermore let us say that the operation of the spatula sometimes causes the introduction of alcoholic bubbles into the interior of the oil mass; one also extracts them with the syringe.

Finally, when the experiment must be made in front of a large audience, one establishes a small heterogeneity in the alcoholic mixture (§ 10), and, if the temperature rises or falls, it is enough to make the liquid polyhedron descend or rise by a certain amount by inserting or withdrawing the cylindrical stem; one thus brings this polyhedron to a layer for which the average density is equal to that of oil.

§ 31. Let us present a last note: the polyhedra that one wants to create must not have re-entrant plane angles; a solid edge of an angle of this kind cannot keep apart two surfaces which attach to it, because, unless the solid wire is very large or the angle very wide, these two surfaces would communicate one with the other, and consequently, all along the line whereby they would join, the curvature, in the transverse direction, would be extremely strong, which would determine an energetic suction. It is easy to see, indeed, that, in the small portion of surface which would connect the two plane faces, the principal curvatures would be, on the one hand, zero curvature along the length of the wire, and, on the other hand, strong curvature transversely, the first would have consequently an infinite radius, and the second, because of its concave form, a negative radius; the term depending on the curvatures in formula [1] of § 1 thus reduces to $-\frac{A}{2R}$, which represents (§ 20) a suction, as I have said, and, in consequence of the smallness of R, this suction would be very strong; the liquid then would cross the edge, and would accumulate in more or less great quantity in the re-entrant angle.

Also is this this last result which the experiment shows: when one tries to make

[13]To have to remove less oil and thus shorten the operations, one can do as follows: one gives to the oil mass a volume just a little larger than that of the polyhedron than it must form, then one brings it into the frame, and, using an iron spatula that one inserts, one easily obliges it to stick successively to all the lengths of the solid edges; finally one extracts the oil excess.

such a polyhedron, one easily makes all the faces flat except the pair which should form the re-entrant angle, and this remains partially filled by oil which has a single curved surface. This surface, which must determine the same pressure asthe plane faces, necessarily has zero mean curvature, and, consequently, at each one of its points, the two principal curvatures are equal, with one convex and the other concave (§ 2).

§ 32. Just as with our oil lenses we made a Galilean spyglass, in the same way a triangular oil prism can be used to produce the phenomenon of dispersion: one thus obtains a beautiful solar spectrum using a prism with liquid faces. Only, as the effect is due to the excess of the index of refraction of oil over that of the alcoholic liquid, one needs to have a spectrum well spread out, that the angle of refraction of the prism is obtuse: an angle of 110° gives a very good result. Moreover, it is obviously necessary that the faces of the prism are perfectly plane, which requires a carefully frame worked.

To finish what relates to liquid polyhedra, let us say that Mr. Mach, in a conference described in § 210a, advances that one can make similar polyhedra in air, provided that they are given tiny dimensions: in his opinion, if one builds, for example, out of very fine wire, a cubic frame of approximately $1\frac{1}{2}$ lines (3mm) on a side, that one plunges in water, then withdraws it, and then with a piece of filter-paper removes the excess of liquid, one obtains a small water cube.

CHAPTER II.

Considerations relating to the shapes of equilibrium in general. shapes of equilibrium of revolution: sphere, plane, cylinder, unduloid, catenoid, and nodoid; experimental study using the first process; results of the geometers.

§ 33. Before occupying ourselves with particular shapes of equilibrium other than the sphere and the plane and with their creation by our first process, let me generalize a remark already made, in the preceding chapter, under particular circumstances. For clarity, let us recall the expression (§ 1) for the pressure that an element of a surface layer exerts,

$$P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right).$$

If, at the point considered, the principal curvatures $1/R$ and $1/R'$ are both convex, and, consequently, positive, or if they are in opposite directions, but the convex one overrides the concave one, then the second term of the formula above is positive, and consequently the total pressure, at the point in question, is higher than P, i.e. than that of a plane surface. The shape of equilibrium then has finite and positive mean curvature.

If the principal curvatures are both concave and thus negative, or if, one being concave and the other convex, and the first is in excess, the second term of the formula is negative, and, consequently, the total pressure at the point considered is lower than that of a plane surface. In this case, the shape of equilibrium has finite and negative mean curvature. Moreover, since the pressure P always finds itself neutralized as I remarked (§§ 18 and 20), one can look at the action of the surface layer as constituting a suction.

Finally if the two principal curvatures are equal and of opposite directions, which cancels the term in question, the pressure is reduced to P or that of a plane surface; that is the case of surfaces with zero mean curve.

It is needless to add that if one of the principal curvatures is zero, the sign of the term is determined, as well as the direction of the action, by the sign of the other principal curvature.

§ 34. All that being understood, we can continue. Judging only by experiment, the sphere is the only possible equilibrium shape in a complete state, with a finite mass of liquid; indeed, an entirely free oil mass in the alcoholic liquid is formed invariably into a sphere, and if one deforms it by an unspecified means and one then leaves it to itself, it always takes again the spherical shape. In other words, the sphere is only surface with constant mean curvature which is closed, and thus all others have infinite dimensions in certain directions. However this deduction, although extremely probable, is not completely conclusive: a closed surface different from the sphere and with constant mean curvature could exist, without the shape which it represents being realizable by experiment; if, for example, this shape, in its entirety, were in a state of unstable equilibrium, the kind of equilibrium which we will note in many cases, it is clear that our oil mass would never take it. The geometers have not, that I know of, solved the question completely; only, initially, it is easy to show that if there is a closed surface, other than the sphere, with constant mean curvature, it should not be sought among surfaces with zero mean curvature. Indeed, a closed shape is necessarily such that one can conceive a plane which is entirely external to it and does nothing but touch it in a point; however it is clear that at this point all the curvatures are of the same direction; the supposed shape could not thus satisfy the condition that at each one of its points the two principal curvatures are in opposite directions. It follows therefrom that at least surfaces with zero mean curvature all are indefinitely extended, of which a plane already offers us an example.

In the second place, Mr. Jellett has shown[14] that, of all closed surfaces such that a ray emanating from an interior point intersects them in only a single point, the sphere is the only one whose mean curvature is constant.

Lastly, among surfaces completely given about which I spoke in § 2, six, including the sphere, have finite mean curvature, and the five different from the sphere are indefinitely prolonged.

§ 35. Now let us consider any of the shapes of equilibrium of infinite dimensions in certain directions. We will not be able to create any in its totality; but let us imagine this creation carried out, and insert, by thought, in the infinite shape, a solid system which completely bounds a portion. If the contours of this solid system are exactly on the surface of the mass, it is obvious that the shape will not be modified, because solid contours exert their attraction only up to an excessively tiny distance, and all the remainder of liquid surface will be under the same conditions as before. Consequently if the solid system is composed of two thin sections including between them the portion of the shape in question, we can imagine the two parts which extend indefinitely beyond being removed; the intercepted portion, the portion to be kept, will not have undergone any change, and its surface will stop clearly, as in the experiment of § 14, at the contours of the two plates. Nothing thus prevents us from creating in reality this isolated portion, which leads us to the general conclusion, that with the aide of appropriate solid systems, we can create portions of all equilibrium shapes.

And here, let us note, experiment goes further than theory; indeed, because of the difficulties of calculation, there is, as we said, only a small number of perfectly known surfaces with constant mean curvature; however, by varying the forms of the solid systems, one can obtain as many partial shapes of equilibrium as is wanted; they are there under our eyes, and we can observe them at leisure; only, as, in general, we do not have their equations in finite co-ordinates, we are unaware of what they become beyond the limits of the solid systems.

But a question is presented: let us suppose one of these partial shapes is created in my apparatus; one has the right to wonder whether the alcoholic mixture in which it is immersed does not modify its form. However, to the surface of separation of the two liquids, the alcoholic mixture offers, in hollow, the same shape which oil offers in relief; if thus the shape of the oil mass did not satisfy formula [2] of § 1, the shape in hollow of the alcoholic mixture would not satisfy it either (§ 2), and consequently equilibrium could not exist; thus, since there exists the condition expressed by the formula which must be necessarily met with regard to the shape of the oil, it is the same with regard to that of the alcoholic mixture.

Let us say finally that, for the creation of the majority of partial shapes of equilibrium, the solid systems can be formed from simple iron wire; one will understand this soon.

36 Let us start with the shapes of equilibrium of revolution, among which we know already the sphere and the plane. The discussion, from the purely mathematical point of view, of surfaces of revolution of constant mean curvature is almost complete; we will further expound on those results; but, for now, we propose to arrive without calculation at the general forms of these surfaces, with all their modifications and with all their details, by relying on experiment and by using simple reasoning applied to the formula $1/R + 1/R' = C$. In this research, experiment and theory will always go hand in hand, and the first will thus provide us a great number of checks of the second.

[14] *Sur la surface dont la courbure moyenne est constante* (Journ. of Mr. Liouville, vol. XVIII, p. 103, year 1853).

It is well-known to geometers that one of the principal radii of curvature at an unspecified point of a surface of revolution is the radius of curvature of the meridian curve, and that the other is the portion of the normal to this line ranging between the point considered and the axis of revolution, or, more simply, the normal at this point. According to that, to avoid any ambiguity, we will replace, in the formula above, the letters R and R' by the letters M and N, the first indicating the radius of curvature of the meridian curve, and the second indicating the normal; thus, for shapes of revolution, the formula of equilibrium will be

$$\frac{1}{M} + \frac{1}{N} = C \tag{4}$$

§ 37. This notation agreed upon, we initially will show that, in addition to the plane, the sphere is the only shape of equilibrium of revolution whose meridian curve meets the axis.

Let us suppose there is a shape of equilibrium of revolution other than the plane and the sphere, and whose meridian curve reaches the axis. I say, initially, that this line can meet the axis only perpendicularly. Indeed, if it cut it obliquely or if it were tangent to it, the normal would be zero at the contact or point of intersection, and the quantity $1/M + 1/N$ would become infinite at this point[15], while it would have finite values at nearby points, so this quantity would thus not be constant along the curve as required by the equilibrium equation.

Let us imagine now that the liquid shape meets the condition that we have just established, and consider, starting from the axis, an arc of the meridian curve. Since, by assumption, this line is neither straight nor circular, the curve of the arc will vary from one point to another; it will start consequently by being either increasing or decreasing, and we will be able to take a small enough arc so that the curve is always increasing, or

[15] Rigourously, however, there is a case to which this reasoning would seem not to be applicable. One can design a curve such, that at the point where it meets the axis, the radius of curvature is zero, and that around this point the radius of curvature and the normal are of opposite signs; then the quantity $1/M + 1/N$ would constitute a difference, whose two terms would become at the same time infinite at the point located on the axis, and one does not see, at first glance, why this difference cannot remain finite. We have thus to show that the thing is impossible.

For that, but only in this case, we will be obliged to make use of the known expressions of the radius of curvature and the normal according to the differential coefficients.

If we take the axis of revolution as the x-axis, we will have, as one knows, p and q being respectively the differential coefficients of the first and the second order of y with respect to x,

$$M = \frac{(1+p^2)^{3/2}}{q} \tag{a}$$

$$N = y(1+p^2)^{1/2}, \tag{b}$$

from which we deduce, for the ratio of the two terms of the first member of the equilibrium equation,

$$\frac{\frac{1}{N}}{\frac{1}{M}} = \frac{1+p^2}{qy}. \tag{c}$$

Now $y = f(x)$ is the equation of the meridian curve. Let us take for the origin of co-ordinates the point where this line meets the axis, so that, for $x = 0$, one has $y = 0$; we are able to then suppose the function $f(x)$ expanded in a series of ascending and positive powers of x; and if we want that the curve meets the axis at an angle other than a right angle, which requires that, for $x = 0$, the first differential coefficient be finite or zero, it will be necessary that the exponent of x in the first term of the series is at least one. Let us notice here that, having to consider the curve only at the point where it reaches the axis and at very close points, we can always suppose x extremely small, so that, relative to this portion of the curve, our series will be necessarily convergent. Thus let us pose:

$$y = ax^m + bx^n + \dots, \tag{d}$$

an equation in which the exponents $m, n, \dots$ are positive and the smallest, m, is at least equal to one. One will have consequently:

$$\begin{aligned} p &= max^{m-1} + nbx^{n-1} + \dots \\ q &= m(m-1)ax^{m-2} + n(n-1)bx^{n-2} + \dots \end{aligned}$$

According to that, if it is wanted that at the point located on the axis the radius of curvature is zero, one sees, by the formula [c] that at this same point, q must be infinite, and, under the terms of the second of the expressions above, this condition will be satisfied if the first at least of the exponents m, n is smaller than 2.

Now let us substitute in formula [c] these same expressions for p and q and that for y; it becomes:

$$\frac{\frac{1}{N}}{\frac{1}{M}} = \frac{1 + (max^{m-1} + nbx^{n-1} + \dots)^2}{m(m-1)ax^{m-2} + n(n-1)bx^{n-2} + \dots)(ax^m + bx^n + \dots)},$$

and one sees easily that, for $x = 0$, this ratio becomes infinite. Let us notice, in passing, that this result is independent of the condition $m < 2$, so that it is true as well for a radius of curvature finite or infinite at the point located on the axis, for a zero radius of curvature; what was to be shown, moreover, according to what we saw higher. Now if, at this same point, the radius of curvature is zero, the two quantities $1/M$ and $1/N$ both take, surely, infinite values; but since their ratio becomes at the same time infinite, their difference becomes also infinite, which was to be shown.

We note finally that a line whose radius of curvature would be zero at a point located on the axis, could not satisfy the formula of equilibrium, nevertheless it would meet the axis perpendicularly; indeed, it is easy to see that then, in the vicinity of the point of meeting, the radius of curvature and the normal would be of the same direction, and that thus, at the point concerned, the quantity $1/M + 1/N$ would be the sum, and not the difference, of two infinities.

always decreasing, starting from the point located on the axis until the other end. Let us suppose that the curvature is growing, and let *abd* (fig. 10) be the arc in question. At the point the normal is along the axis, and, as one moves away from this point, it forms with the axis an increasingly large angle; but we will limit the length of the arc so that, between *a* and *d*, this angle remains acute. Through the two points *a* and *d* let us pass an arc of circle *acd* which has its center on the axis, and which, consequently, meets it perpendicularly.

Since the arc *abd*, along which the curvature is always increasing, starts at the same point and with the same tangent direction as the arc of the circle, and, after having separated from it, joins it at *d*, it is obvious that its curvature must initially be lower than that of this second arc, and to become then higher than it, so that at the point *d* the radius of curvature of the arc *abd* is smaller than the radius of the arc of circle. But from the common initial direction of the two arcs, and this relative progess of the curvature of the arc *abd*, it necessarily results that this last is, as the figure shows, external to the other, and that at the point *d* it must cut it, and not just touch, if thus one froms, at this point *d*, the normal *df* with the arc of curve and the radius *dg* of the arc of circle, the first will be less oblique on the axis than the second, and consequently it will be shorter. Thus, at the point *d*, the two quantities M and N will both be the less than the radius of the arc of the circle. Now let us take, in the part of the arc *abd* where the curvature is less than that of the arc of circle, an unspecified point *m*, and take, on the second of these arcs, a point *n* such that the portion *an* is equal in length to the portion *am*. Under these conditions, the point *m* will be obviously further away from the axis than the point *n*, and, in addition, the normal at *m* will be more oblique to the axis than the extended radius of *n*; by this double reason, the normal in question will be thus larger than the radius of the arc of circle; but, in consequence of the inferiority of the curvature at*m*, the radius of curvature at this point will be larger than the radius of the arc of circle.

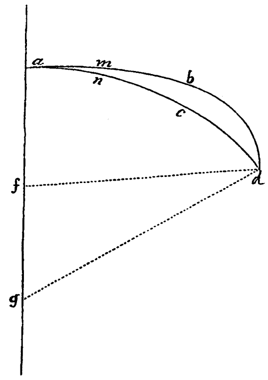


Fig. 10

It results from all this that the values of M and N corresponding to the point *m* are both the higher than those which correspond to the point *d*, but it is clear that M and N are of the same sign over the entire length of the arc *abd*, and that thus, at the point *m* as at the point *d*, since the quantity $1/M + 1/N$ constitutes a sum, this same quantity is thus smaller at *m* than at *d*, and consequently the equilibrium of the generated liquid shape is impossible.

If it is supposed now that the curvature of our meridian arc is always decreasing, as one sees along *a'b'd'* (fig 11), it is clear that then this arc will be interior to the arc of circle *a'c'd'* having its center on the axis, that its curvature will start by being higher than that of this last, then become lower, and that at the point of one of the arcs one will intersect the other without being tangent; from which it will be concluded, by the mode of reasoning employed in the preceding case, that the quantity $1/M + 1/N$ is larger at a point closer to *a'* than to *d'*, so that the equilibrium of the generated shape is also impossible.

Thus when the meridian curve meets the axis, the equilibrium condition cannot be satisfied unless this line is the circumference of a circle having its center on the axis, or is a line perpendicular to ththe axis; thus finally the generated shape is necessarily a sphere or a plane.

From there rises the truth of what I have advanced by describing the experiments of

§§ 14 to 20, the knowledge that the portions of surfaces which are based on the contour of a circular plate, a ring or a hollow band, are segments of a sphere. Otherwise, one would need that the curved cap was not a surface of revolution, which the eye would easily realize.

§ 38. The meridian curves of other equilibrium shapes of revolution which cannot have any point in common with the axis, will extend ad infinitum, or close upon themselves away from the axis. The first will generate shapes which extend themselves ad infinitum. The second would give annular shapes; we will later find out if the existence of such shapes is possible.

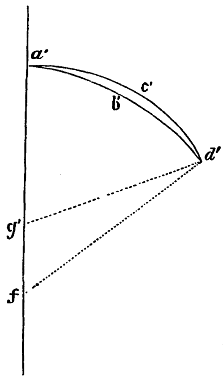


Fig. 11

To simplify the research on the lines in question, we will show that they do not contain any cusp point. Let us suppose the existence of a point of this nature; we have to consider three cases: 1° that where the tangent at the cusp point, the tangent which is there common to both branches of the curve, is not perpendicular to the axis of revolution, having some other direction; 2° that where this common tangent is perpendicular to the axis and where the two branches approach the axis while going towards the cusp point; 3° finally that where, the common tangent being still perpendicular to the axis, the two branches, while going towards the cusp point, move away from the axis

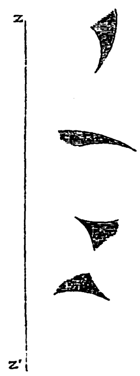


Fig. 12

First case. – By looking at fig. 12, which represents, by meridonal cross-sections, portions of the liquid shape for various positions of the cusp point compared to the axis of revolution ZZ', one will easily recognize that around this point, the normal is always, for one of the branches, directed inside the liquid and consequently positive, while, for the other, it is directed outside and consequently negative, but the equation $1/M + 1/N = C$ could not include this change of sign of the normal N while passing from one branch to the other because it would require that at the cusp point it must be zero or infinite. In the current case, the normal in question is obviously finite, since the tangent is not perpendicular to the axis, and so the cusp point cannot be like this.

Second case (fig. 13). – If the cusp point is of the second kind, i.e. if the two branches which end in it are located on the same side of the common tangent, it is seen that, for one of these branches, the normal and the radius of curvature are both positive, while, for the other, they are both negative; the quantity $1/M + 1/N$ would thus change sign passing from the one to the other, and thus would not be constant in all the extent of the liquid shape.

If the cusp point is of the first kind, i.e. if the two branches are located on the two opposite sides of the common tangent, the radius of curvature is there, as one knows, zero or infinite; but a null radius of curvature would make the quantity $1/M + 1/N$ infinite, so that we have to examine only the assumption of an infinite radius of curvature. Then, since, due to the direction of the tangent, the normal is also infinite, the quantity $1/M + 1/N$ would be reduced to zero at the cusp point; it would thus be necessary, for equilibrium, that this quantity is also zero at all the other points of the meridian curve, but that is impossible in particular, since, as soon as one deviates from the cusp point, the radius of curvature and the normal take, on each of the two branches, finite values of the same sign.

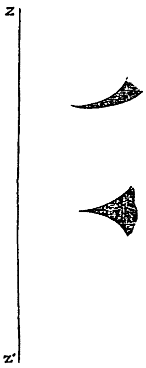


Fig. 13

Third case (fig. 14) – If the cusp point is of the second kind, the radius of curvature has opposite signs on the two branches, and consequently it must be zero or infinite at the point in question; but, as we already pointed out, we do not have to concern ourselves with the assumption of a zero radius of curvature, so there remains the case of an infinite radius of curvature. Then, the normal at the same point being on its side infinite, equilibrium requires, as above, that the quantity $1/M + 1/N$ is zero at all the points of the meridian curve. This seems possible at first glance, since, around the cusp point, the radius of curvature and the normal are, on each branch considered separately, of opposite signs, but we will see hereafter that this possibility is not possible.

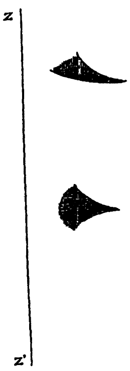


Fig. 14

If the cusp point is of the first kind, the radius of curvature is necessarily zero or infinite there, as already pointed out; and, since we must reject the zero radius of curvature, the quantity $1/M + 1/N$ is still equal to zero at the point in question, and it must be also at all the other points, which appears possible as previously, and for the same reason.

But so that at all the points of the meridian curve the quantity $1/M + 1/N$ is zero, it is obviously necessary that at each one of these points the radius of curvature is equal and opposite to the normal; however it is well-known to geometers that only one curve enjoys this property, and this curve is the catenary, which does not have any cusp point.

§ 39. The principles established in the two preceding paragraphs having eliminated some potentially awkward complications of our meridian curves, we can approach our subject more directly.

It clearly follows from formula [4] that the cylinder is an equilibrium shape of revolution; in it, indeed, the meridian curve being a straight line parallel with the axis, M is infinite everywhere, which reduces the formula to just the term $1/N$, and this term is constant, since the normal N is the radius of the cylinder.

The cylinder, in its complete state, extends indefinitely in the direction of the axis; but, according to what we expounded (§ 35), we will be able to create a portion of it.

Before describing this creation, as well as that of later shapes, I insist again on the whole of the precautions about which I already spoke, and which are absolutely needed, especially in experiments of this kind; I thus point out that it is necessary: 1° only employ two liquids, alcoholic mixture and oil, that are mutually inert, or nearly so (§ 6); 2° when one pours the oil, surround with several layers of fabric the bottle which contains it, in order to eliminate the influence of the heat of the hand (§ 9); 3° to give great care to the adjustment of the densities (§ 6 and 9); one would believe with difficulty, without

having
tried it out, how much tiny differences affect fairly large shapes; 4° to look after in the same way the homogeneity of the oil which constitutes the immersed mass (§ 30); 5° finally to operate in a place whose temperature is, as much as possible, constant (§ 7); if it varies a little throughout an experiment, and if the regularity of the shapes is affected, restore the equality of the densities. I also repeat that with these precautions, which quickly become familiar, the experiments are easily carried out, and give perfectly regular results.

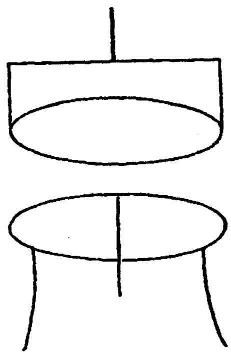


Fig. 15

§ 40. Let us proceed now to the creation of the cylinder. The solid system that we will employ to this end, consists of two horizontal circles of wire, with equal distance between them, and having, for example, 7 centimetres diameter (fig. 15). One of these rings rests, by three small feet, on the piece of fabric which covers the bottom of the vessel (§ 9), and the other is fixed, by the tail of its fork, to the bottom of the cylindrical stem.

After having raised as much as possible the upper ring, let us slide the lid plate a bit to make an opening to the top of the vessel, and, by this, obliquely introduce the nozzle of a small funnel, whose nozzle must be rather long; then gently introduce oil in a quantity such as forms a sphere approximately a decimetre in diameter. Let us remove the funnel, and lead the liquid sphere towards the lower ring, so as to make it adhere to all its contour; slide the lid back, to close again the vessel, and bring the upper ring exactly above the other[16]. Let us lower then this upper ring until it comes in contact with the oil mass, and it also adheres to it. The mass being thus attached to the two rings, let us raise the upper slowly: then the illustrated portion of liquid between them will lengthen while narrowing, and one will reach a point where this portion of the shape will be perfectly cylindrical; there will be an exact cylinder 7 centimetres in diameter and 12 to 14 in height (fig. 16).

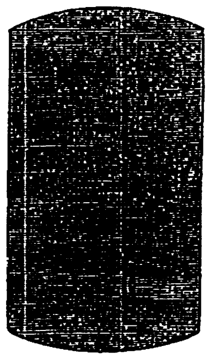


Fig. 16

Only the two bases of the shape are not plane; their surfaces constitute convex segments of a sphere, and this is a necessary consequence of the theory. Indeed, according to the notation that we adopted, the pressure corresponding to an unspecified point of cylindrical surface has as a measurement $P+\frac{A}{2}\frac{1}{N}$, and as the curvature $1/N$ is positive, since the normal N is directed inside the liquid, the pressure concerned is higher than that of a plane surface; however it is obviously necessary, for the equilibrium of the mass, that surfaces of the bases exert, at each point, a pressure equal to the preceding one, and, consequently, higher also than that of a plane surface, a condition which requires that they be convex.

In this experiment, one sees, the solid system consists of a simple iron wire. One could make use of discs instead of rings; but, with the latter, the result is much more curious, and it offers besides, as I have just shown, a new check of the theory.

If, in this same experiment, the densities of the two liquids are not completely equal, one is informed of it by the fact that the shape takes, in a more or less pronounced way, one of the two forms in fig. 17; the small defect of adjustment is then corrected.

[16]Let us say here, once and for all, that it is with the assistance of similar operations which one carries out the creation of the majority of the shapes with which we will concern ourselves.

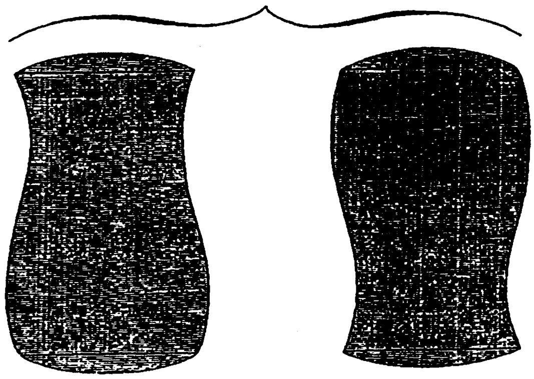


Fig. 17

Lastly, if one proves to have some difficulty forming an oil sphere of one decimetre without it touching the side wall of the vessel, nothing prevents one from stopping with a smaller diameter; the cylinder will then be less high, but one will be able, if one wants, to add oil, and to then raise the upper ring until the cylindrical shape is reproduced.

§ 41. The result that we have just obtained will enable us to arrive at a numerical check. It is easy to deduce from the theory the radius of the spheres to the which bases belong; indeed, if we represent it by x, the pressure corresponding to a point of the surface of these bases will have (§ 18) as a value $P + A \cdot \frac{1}{x}$, and since this pressure must be equal to that which corresponds to a point of cylindrical surface, we will be able to pose:

$$P + A \cdot \frac{1}{x} = P + \frac{A}{2} \cdot \frac{1}{N}$$

from which we deduce:

$$X = 2N.$$

Thus the radius of the spheres of which the bases form part, is equal to the diameter of the cylinder.

According to that, knowing this diameter, which is the same as that of the rings, one can calculate the theoretical height of the caps, then measure, in the shape, by means of a cathetometer, the real height, and compare the results. It is what I did, as one will see.

§ 42. If one imagines the liquid shape cut by a meridian plane, the section of each cap will be an arc pertaining to a circle whose radius will be, according to what precedes, equal to 2N, and the breadth of this arc will be the height of the cap. If one supposes the metal wire which forms the rings to be infinitely thin, so that each cap is supported on the circumference of an even cylinder, the chord of the arc above will also be equal to 2N, and if one indicates by h the height of the caps, one will have:

$$h = N(2 - \sqrt{3}) = 0.268 \cdot N$$

But the exact external diameter of my rings, or the value of 2N corresponding to my experiments, was 71mm, 4, which gives $h = 9.57mm$.

But metal wire having a certain thickness, and the caps not being supported on the external circumference of the rings, it happens that the chord of the meridian arc is a little less than 2N, and that, consequently, the real theoretical height of the caps is a little smaller than given by the preceding formula. To determine it exactly, let us indicate the chord by $2c$, which gives:

$$h = 2N - \sqrt{4N^2 - c^2}.$$

Now, let us notice that the meridian plane cuts each ring in two small circles to which the meridian arc of the cap is tangent, and on each one of these the chord of the arc intercepts a small segment. However, the meridian arc being tangent with the sections of wire, it happens that the small segments are similar to the chord of the cap; and as the chord of this last differs very little from the radius of the circle to which the arc belongs, the chords of the small segments could be regarded as equal to the radius of the small sections, a radius which we will indicate by r. It is clear, moreover, that the excess of the external radius of the ring over the half-chord c is just the excess of the radius r over the half-chord of the small segments, a half-chord which, according to what precedes, is equal to $\frac{1}{2}r$. One deduces therefrom, $N - c = \frac{1}{2}r$, whence $c = N - \frac{1}{2}r$, and one only needs to substitute this value in the preceding formula, to get the real theoretical value of h. The thickness of the wire which formed my rings was of 0.74mm, so $\frac{1}{2}r = 0.18mm$, which gives, for the real theoretical height of the caps in these circumstances,

$$H = 9.46mm.$$

I will point out that it is difficult to distinguish, in the liquid shape, the precise limit of the caps, i.e. the circumferences of contact of their surfaces with the rings. To eliminate this problem, I measured the height of the caps only starting from the external planes of the rings, i.e., for each cap, starting from a plane perpendicular to the axis of revolution, and resting on the surface of the ring on the side which looks at the top of the cap. The quantity thus measured is obviously equal to the total height minus the breadth of the small segments which we considered above, and, consequently, according to the similarity between these small segments and the chord of the cap, one has, to determine this breadth that we will indicate by f, the proportion $\frac{h}{c} = \frac{f}{r/2}$, which gives, for our liquid shape, $f = 0.05mm$, from which

$$h - f = 9.41mm.$$

Such is thus, ultimately, the theoretical value of the quantity that it was a question of measuring.

§ 43. Before reporting the results which measurements gave me, I must present some significant remarks here.

If the densities of the alcoholic mixture and oil are not rigorously equal, the mass tends slightly to go up or go down, and the height of one of the caps is then a little too large, while that of the other is a little too small; but it is understood that if their difference is tiny, one will still obtain an exact result by taking the average between these two heights. One avoids part of the fiddling that would be required by the establishment of perfect equality between the two densities.

But a thing to which it is necessary to give great care, is the homogeneity of each of the two liquids. To more easily stir the oil using the iron spatula, one does this operation when the mass is still attached only to the lower ring.

§ 44. In order to further lessen the influence of a possible tiny difference between the two densities, one only gives to the liquid shape a rather small height. One initially measures, with the cathetometer, the distance between the tops of the two caps, then one measures, by the same means, the distance between the external planes of the two rings. The difference between the first and the second result gives obviously the sum the two heights of which the average should be taken, and, consequently, this average, or the sought quantity $h - f$, is thus half of the difference.

The measurement of the distance between the external planes of the rings requires some particular precautions. First, since the points of the rings for which it is necessary to aim are not in fact on the external surface of the shape, the oil interposed between these points and the eye must produce effects of refraction which would introduce a small error into the value obtained. To eliminate this problem, it is enough to expose the rings, by running out the liquids of the vessel by the tap (§ 4), or by means of a siphon, then to remove the small oil portions which remain adherent to the rings, by wiping on those a small strip of filter-paper; it is also necessary to absorb in the same manner the drops of alcoholic liquid which remain attached to the interior of the front wall of the vessel. Needless to say that, to carry out these operations, one opens a portion of the top of the vessel, and that after having finished, one replaces the lid plate exactly in its original position.

Here now are the results which I obtained: the distance between the tops was found initially, on average, by four successive measurements, equal to 76.79mm. But the alcoholic liquid having been then agitated again for some time, so that its perfect homogeneity was more certain[17], two new measurements were taken immediately after, which gave, on average, 77.02mm.

The distance between the external planes of the rings was on average, on the right side of the system, 57.73mm, and, on the left side, 57.86mm; thus taking the average of these two results, one has, for the distance between the centers of the external planes, the value 57mm,79.

According to that, if one uses the first of the two values obtained for the distance from the tops, 76.79mm, one will find:

$$h - f = \frac{76.79 - 57.79}{2} = 9.50mm;$$

and if one uses the second result, 77.02mm, one will find:

$$h - f = \frac{77.02 - 57.79}{2} = 9.61mm$$

These two heights differ very little, as is seen, from the height 9.41mm deduced from theory (§ 42): for the first, the difference does not exceed a hundredth of this theoretical value, and, for the second, it exceeds hardly two hundredths.

These slight differences undoubtedly came from weak heterogeneity remaining in the liquids; it is probable that, in the first case, neither of the two liquids was absolutely homogeneous, and that the two contrary effects which resulted therefrom (§ 30 and notes § current), partly neutralized each other, while, in the second case, the alcoholic liquid being made completely homogeneous, the effect of the small heterogeneity of the oil appeared in its entirety.

At all events, these same differences are both rather tiny so that one can regard the observation as agreeing with the theory, of which it offers, as one sees, a quite remarkable confirmation.

§ 45. With rings of 7 centimetres, one can hardly, in our vessel, make a cylinder whose height exceeds twice the diameter. To have a larger ratio, one could use smaller rings, of 3 centimetres, for example, but, in this case, one observes a singular phenomenon: if the height that one wants to give to the shape notably exceeds triple of the diameter of the rings, one does not succeed any more, despite the care which one takes to obtain the cylindrical form; before it is reached, the shape always separates spontaneously into two unequal portions which remain respectively attached to the two rings.

[17]If the alcoholic liquid is not quite homogeneous, and the shape is in a layer whose average density is equal to that of oil, the two caps can be equal, but their height is too small; in effect, the oil which forms the upper cap is then in contact with a liquid less dense than it, and consequently tends to go down, while the reverse takes place for the oil which forms the lower cap. By establishing a very marked heterogeneity intentionally, and by employing the suitable precautions, one can even form an appreciably regular cylinder whose bases are completely plane

To make these experiments, the most convenient means is to give to the oil sphere a too large diameter, so that, when the upper ring is raised to the height that one wants, the meridian curve of the portion of shape between the two rings is rather strongly convex, then to try to absorb the excess oil. Only, as the introduction of the nozzle of the syringe requires that one make an opening at the top of the vessel, one makes it so that the liquid shape is not completely on the axis of this last, and that thus the plate lid is moved back just a little. I note, as of now, that, for all the shapes which remain to be described requiring the use of the syringe, it is necessary, in their regard, to resort to the same provision.

Now, let us specify better what happens when the distance between the two rings is notably over triple their diameter: always, before the meridian convexity disappears, one sees the liquid shape being thinned on one of the halves of its height, while it is swollen on other half (fig. 18); these modifications grow more and more, until the neck of the narrow part becomes very thin; then the shape is divided at this point, and is thus divided into two portions.

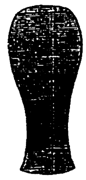


Fig. 18

§ 46. One could suspect that the phenomenon is due to a tiny remaining difference between the densities of the two liquids; but, in addition to it inevitably occuring, as I said, despite the care that one brought to equalizing the densities, it occurs even when things are laid out so that the axis of the shape is horizontal, i.e. when one includes this shape between two rings or two vertical discs, placed facing one another (fig. 19); however, in this last case, if the axis is quite horizontal, a difference between the densities obviously cannot tend to determine a transport of oil in greater abundance towards one of the bases.

From all that one must conclude that a liquid cylinder in which the ratio of length to diameter is notably greater than 3, constitutes a shape of unstable equilibrium. I limit myself, for the moment, to announcing this remarkable fact, which will be studied in detail in chapter IX.

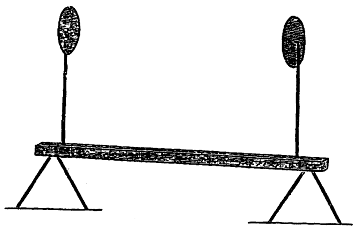


Fig. 19

Thus, when one wants to create a liquid cylinder, make it so that the ratio of the distance between the rings or the discs and their diameter does not exceed 3. Let us add that the experiment is much easier for a horizontal cylinder than for a vertical cylinder, because, with regard to the first, a slight difference between the densities does not have an appreciable effect on the regularity of the shape. To carry out this in the apparatus of the fig. 19, one attaches initially to the internal face of the one of the discs, a mass of oil of a volume greater than that which the cylinder must include, then, using a ring of wire of the same diameter as the discs and carried by a thicker wire of which one holds the free end by hand, one easily stretches the mass towards the second disc, to which one makes it also adhere, and then one absorbs the excess oil, until the shape is exactly cylindrical.

Lastly, in the case of an audience, one will make use of a solid system similar to the preceding, but without feet, and held in a reversed position, like that of fig. 7 of § 21, by a suspension wire; in this manner, one can lower or raise the whole in the alcoholic mixture made slightly heterogeneous.

§ 47. In the experiment of § 40, if the upper ring is stopped before the cylindrical form is reached, the portion of the shape between the two rings, a portion which is always perfectly symmetrical around the axis, does not belong to the sphere, and yet does not belong to the cylinder; it thus forms part of a new equilibrium shape of revolution. Likewise, if, after having obtained an exact cylinder, one further raises the upper ring a bit, the shape is narrowed more or less in the middle of its height, and, if the ring then is stopped, it remains in this state, without ceasing being a surface of revolution; then also, consequently, it constitutes a portion of an equilibrium shape different from the sphere and cylinder.

To manage to determine what must be, in their complete state, the liquid shapes to which the portions in question belong, I first describe an experiment.

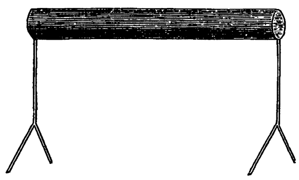


Fig. 20

Let us take for the solid system an iron cylinder of a rather considerable length relative to its diameter, carried on two feet made out of wire of the same metal (fig. 20); let us suppose, for example, it has a length of 14 centimetres, and a diameter of 2. This cylinder being carefully rubbed with oil and introduced into the vessel, let us bring in contact with the middle of its length an oil sphere of a suitable volume. Once adherence is established, the liquid mass spreads on surface along the cylinder so as to wrap a portion of the length of it, loses its spherical form, and constitutes finally a shape of revolution whose meridian curve changes curvature going towards its two ends, to become, at these two points, tangent to the generator of the cylinder. Fig. 21 represents the meridian cross-section of the liquid shape and the solid cylinder.

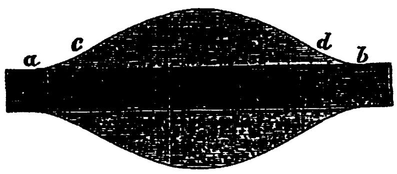


Fig. 21

But we know that when the liquid mass adheres to a solid system which modifies its form, the only parts of this system which affect the produced shape, are the very narrow lines whereby it is met or touched by the surface layer of the mass, so that it can in general be reduced to simple iron wire representing these same lines. However, for the shape in question, the free face of the liquid mass touches our solid cylinder along two circumferences perpendicular to the axis, and passing through the points *a* and *b*; nothing thus prevents one from replacing the whole cylinder by two rings representing these circumferences, i.e. having an external diameter equal to that of the cylinder, placed vertically facing each other, and at distance apart the interval *ab*. Only it will be necessary that the quantity of oil is larger, in order to compensate for the volume of the portion of the cylinder removed from the interior of the mass, and one will even need yet a little more oil, to provide the volume of the two bases which are poking out of the rings, bases whose surfaces are, as we will soon see, convex segments of a sphere. Finally, to avoid these last, which unnecessarily complicate the shape, one can use discs instead of rings; in both cases, the shape then will be entirely made of oil; it is represented in this state, as a meridian cross-section or vertical projection, by the fig. 22; *am* and *bn* are the sections or projections of the discs.

We will say soon why we indicated the use of a cylinder rather than discs or rings.

§ 48. The shape that we have just obtained, and in which the meridian line stops at the points *a* and *b* where it touches the solid cylinder (fig. 21), or meets the edges of the discs (fig. 22), constitutes obviously only one portion of the complete shape of equilibrium. Thus let us try to continue the meridian curve, starting from these same points *a* and *b* where its elements are parallel to the axis.

It is easy to show that the points *a* and *b* are not points of inflection. At such points, the radius of curvature is zero or infinite; but since, in our meridian curves, it cannot be question of a zero radius of curvature, which would make infinite the first member of the equilibrium equation, it would be necessary to suppose an infinite radius at the points which we consider, and the equation would be reduced to $\frac{1}{N} = C$; however the points c and d (fig. 21) are really points of inflection of this kind, as the aspect of the shape shows, so that the equilibrium equation is necessarily reduced to $\frac{1}{N} = C$; the normal N should thus have, at the points a and b, the same length as at the points c and d, which obviously it does not: because firstly the points c and d are further away from the axis than the points a and b, and, secondly, the normals which leave the first are oblique with the axis, while those which correspond to the second are perpendicular to it.

Beyond the points a and b the curve thus starts by keeping its curvature of the same sense as on the inside, so it is a concave curve towards the outside (fig. 23). However let us suppose that, on the prolongation beyond a, for example, this curvature is initially either increasing, or decreasing less than it does on the other side of a; we will always be able to take, on the prolongation in question, a rather small portion am so that at each one of its points, the curvature is stronger than at the corresponding point of a portion an of the same length taken on the first part of the curve. Under the terms of the superiority of curvature at all the points of

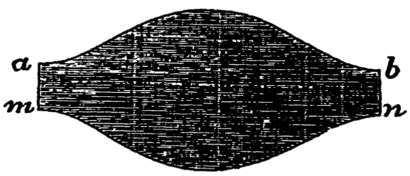


Fig. 22

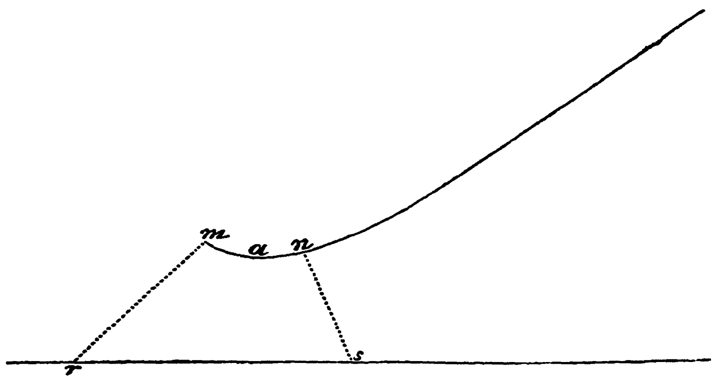


Fig. 23

the arc am, the point m is necessarily further away from the axis than the point n, and, moreover, the normal mr at the first is more oblique with the axis than the normal ns at the second; the normal at m is thus, by this double reason, larger than the normal at n. On the other hand, according to the same assumption relating to the curvatures, the radius of curvature at m is smaller than at n. Therefore, when jumping from the point n to the point m, the first term of the quantity $\frac{1}{M} + \frac{1}{N}$ will increase and the second will decrease. However, in the parts of the curve that we consider, the radius of curvature and normal are opposite to each other, and consequently have opposite signs, so that the quantity $\frac{1}{M} + \frac{1}{N}$ constitutes a difference, thus if one of the terms of this quantity grows while the other decreases, it cannot keep the same value, and equilibrium is impossible.

On the contrary, if it is supposed that the curvature of the arc am decreases more, starting from a, than that of the arc an one will conclude, by the same mode of reasoning, that the quantity $\frac{1}{M}+\frac{1}{N}$ would also change in value in passing from one of the parts of the curve to the other.

The assumption of higher or lower curvatures in the arc *am* than in the arc *an* is thus incompatible with the equilibrium equation; it is needed consequently, to satisfy this equation, that, for a small distance *am*, the curvature is identically the same as on the arc *an* of the same length taken on the other side of *a*. However it is clear that this identity involves all the part of the curve located beyond the point *a* with the part located on the inner side. The portion of curve between *a* and *b* (fig. 21 and 22) will thus reproduce beyond *a*, then, by the same reasoning, will reproduce further, and so on indefinitely, and it will be the same on the other side of the point *b*, so that the meridian curve will be an undulating curve extending ad infinitum along the axis, which it approaches and moves away from periodically at equal intervals.

The complete shape of equilibrium is thus prolonged ad infinitum along the axis, and is composed of a regular succession of equal bulges and necks; fig. 24 represents a meridian cut along a certain extent.

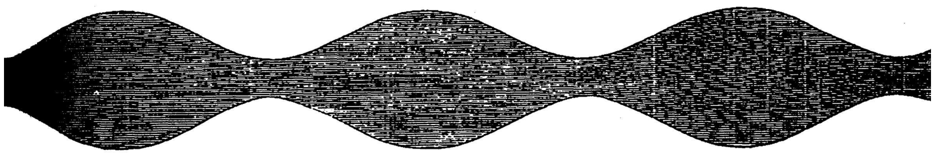


Fig. 24

In order to shorten the language, we will give to this shape the name of *unduloid*, drawn from the shape of its meridian curve.

§ 49. It is easy to understand how equilibrium can exist in such a shape, although, in the bulging parts, the two principal curvatures are convex, while, at the narrow parts, one of the principal curvatures, the meridian one, is concave, and the other convex: it is that, on these last parts, the convex or positive curvature is stronger than the concave or negative curvature, so that the mean curvature at each point is positive (§ 33), and equal to that which corresponds to the various points of the bulging parts.

Because, in the unduloid, the mean curvature is positive, whenever one creates an arbitrary portion of unduloid between two rings, the bases which rest on those must be segments of a convex sphere.

§ 50. If, in the experiment of § 47, the volume of oil remaining the same, one employs a solid cylinder of a larger diameter, the liquid mass extends further from the axis, and the meridian curvature decreases, so that, in the corresponding complete shape, the bulges and necks are less marked. The meridian curvature is thus diminished in the partial shape, and, consequently, in the complete shape, the diameter of the solid cylinder is larger; from which it is seen that, in these variations, the complete shape tends towards the cylindrical form, which can be regarded as the limit of these same variations.

If, the volume of oil remaining always the same, one employs, on the contrary, a solid cylinder of a smaller diameter, the liquid mass is shortened in the direction of the axis, the meridian curvature increases, and the shape approaches a sphere more and more; when, for example, for an oil mass originally constituting a sphere 6 centimetres in diameter, one takes, as the solid cylinder, a wire of two millimetres thickness, the mass has already almost exactly a spherical form, and, if one uses a very fine wire, the difference from the spherical form becomes insensible. Now, the complete shape varying in this same manner in all its parts, the bulges and necks will be pronounced

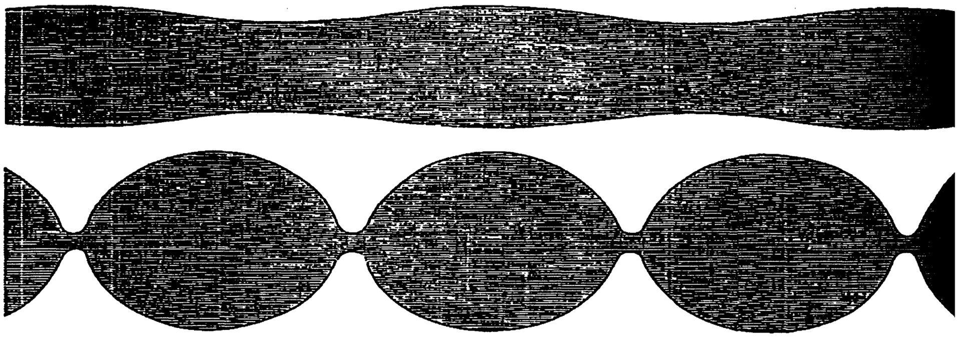


Fig. 25

more and more, and, in the ultimate limit, one understands that it will consist of a succession of tangent spheres equal to each other on the axis.

The complete unduloid can thus vary in form between two very different limits, which are, on the one hand, the cylinder, Fiq. 25 shows two unduloids of which one differs little from the cylinder, and of which the other approaches a series of spheres.

§ 51. But the unduloid is subject to another kind of variation, which gives a third limit. Let us suppose a vessel similar to ours and of much larger size; there let us place horizontally, within the alcoholic liquid, a solid cylinder two centimetres in diameter, for example, of a considerable length, and carried on sufficiently high feet. Let us make adhere to this cylinder a mass of oil which produces a portion of unduloid similar to that of fig. 21, then add a new quantity of oil; the shape will then increase in length at the same time as in thickness, but push to its side slightly, so that one of its ends is brought back to the same place as before, and the other end moves out. If we add thus successively new quantities of oil, while always bringing back the first end of the shape to the same place it was, this shape will take on more and more thickness, and its second end will move back more and more; and as we can imagine the large vessel and the cylinder as lengthened as we want, nothing puts a limit on the theoretical possibility of the increase of the shape in thickness and length. If thus we suppose this increase carried out until it is infinite, the top of the convex meridian arc and the second end of the shape will not exist any more, so that the meridian curve will be moving away indefinitely from the axis starting from the first end; and since this constitutes, in the complete shape, the circle of a neck, and that on the two sides of a neck all is perfectly symmetrical (§ 48), one sees that the complete meridian curve will be reduced to a simple curve with two infinite branches, like the parabola, having its axis of symmetry perpendicular to the axis of revolution; consequently, the generated complete shape will be reduced to a single neck extending indefinitely on both sides from its central circle. We will know soon the precise nature of this third limit of the unduloid.

§ 52. Let us return now to the use of two discs for the creation of the portion of unduloid between the middles of two neighboring necks (§ 47). When this creation is tested, by attaching to the two discs an oil mass larger than that which must constitute the shape, then gradually removing excess by means of the small syringe, the operation goes without difficulty as long as the elements of the meridian curve which end at the borders of the discs deviate notably from parallelism with the axis; but when they are close reaching this parallelism, or, in other words, at the time one approaches the portion of unduloid which one wants to obtain, it is necessary to act with more care, without which the shape could deteriorate spontaneously and be divided. By conducting the operation with care, and by removing, towards the end, oil only by extremely small quantities, one arrives, as far as the eye can judge, at the sought portion (fig. 22), a portion which varies in form while approaching or while moving away from the cylinder, according to whether the diameter of the discs is larger or smaller compared to their distance; but then the slightest disturbance, such as a small movement given to the mass by the nozzle of the syringe, is enough to induce gradual deterioration, then the destruction of the shape: one sees it being thinned close to the one of the discs, this thinning becoming more and more, and oil goes in greater quantity to the side of the other disc (fig. 26), and the mass ends up separating in two parts.

Since, in the shape thus obtained, a deterioration caused by a tiny disturbance then progresses spontaneously, one must infer that the portion of the unduloid between the middles of two neighboring necks is at the limit of stability.

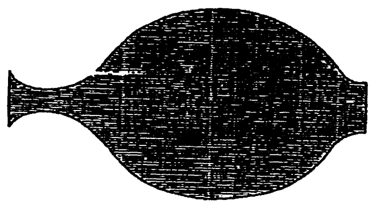


Fig. 26

One understands, according to what we have just expounded, why, in § 47, we prescribed the adoption of a cylinder as the solid system: with discs, one needs particular fiddling and care to arrive without accident at the point where the last elements of the meridian curve are or appear parallel to the axis, while with the cylinder, the shape is perfectly stable, and the desired parallelism is established by itself. But it remains to be explained how the stability of the shape can depend on the two circumferences whereby the surface layer of the mass touches the cylinder (ibid.). The thing is very simple in the case of the discs, when it happens, as we have said, that the shape is spontaneously thinned on one side side, the elements of the surface layer which end at the edge of the disc closest to where this effect takes place, incline themselves towards the axis (fig. 26); however, in the case of the cylinder, the last elements of the surface layer could not be inclined thus, since they are applied to the surface of the solid.

This explanation naturally suggests the idea of substituting thick discs for the thin discs, or rather portions of a cylinder: because, by initially giving to the mass a sufficient volume so that oil reaches the edges of the faces of these last discs as opposed to those which were looked at, then by removing liquid until the circumferences of contact are on the thickness of these same discs, stability as indicated above will exist obviously just as easily as with a cylinder. And it is fully confirmed by experiment: the discs which I used myself each had 15 millimetres diameter, and 8 thickness, and they were kept 90 millimetres apart; the entire system is shown in fig. 27. By making adhere to them an oil mass initially too large, then removing excess, and slightly pushing the mass to the right or to the left with the nozzle of the syringe, so that the points where the meridian curve appeared tangent were about at equal distance between the two bases of each disc, the produced shape showed itself perfectly stable; one could continue to absorb small quantities of oil, to bring the ends of the meridian curve slightly nearer the edges of the facing solid bases, without the shape losing its stability, and it is only when they appeared to reach these same edges, that instability appeared.

§ 53. Since the portion of unduloid which we considered already finds the limits of stability when it is formed between two thin discs, when it is free in all its extent except for its bases, it would be useless to seek to create an also free portion of unduloid which would extend on both sides of the middles of two necks, and one concludes that the indefinite unduloid is, as is the indefinite cylinder, a shape of unstable equilibrium.

§ 54. It is easy to see now that the convex shapes which are obtained (§ 40) when, after having attached an oil sphere to two horizontal circles, equal in diameter and placed one above the other, one raises the upper ring by a quantity less than that which gives the mass a cylindrical form, are only portions of bulges of unduloid; only, when one produces these convex shapes by the process which we have just pointed out, they are placed so that their axes are vertical.

Indeed, let us conceive an unduloid created by means of two thick discs (§ 52), and consequently in a state of steady equilibrium, and imagine that one places, at equal distances on the right and on the left from the middle of this shape, between the bulging middle and the discs, two vertical solid rings having their centers on the axis and their external contours precisely on the surface of the mass; it is clear that these rings will not destroy the equilibrium of the shape; however, if it is supposed that the parts of the shape located beyond the rings are replaced by convex segments of a sphere being based on the latter, and whose curvature is such that it determines a pressure equal to that which belongs to the remainder of the shape, equilibrium will obviously still exist, and it will still be perfectly stable, since the distance between the rings is less than that which corresponds to the limit of stability. But then, if the rings are not isolated enough so that the portion of the meridian curve which extends from the one to the other contains the points of inflection, it is visible that the unit will constitute one of the convex shapes in question; because, according to the various forms of the unduloid, the meridian curve of the portion between the rings can vary from an arc of circle having its center on the axis, to a straight line, as in these same convex shapes. So that those were not portions of unduloid, one would need that between the same rings placed at the same distance apart, and with the same oil mass, there would two possible shapes of equilibrium there, both in a stable state; however this is refuted by experiment: if, after having transformed a sphere of oil into one of the convex shapes in question, either by increasing the spacing of the rings, or by the subtraction of a certain quantity of liquid, one agitates the alcoholic mixture so as to perturb the oil mass with considerable movements, but, not to divide it, and that then one lets it return at rest, it always regains the same shape identically.

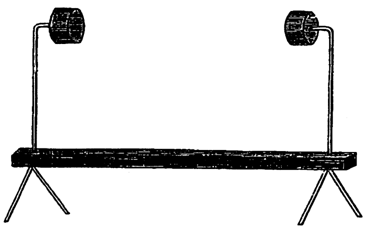


Fig. 27

One also sees that, in the experiments of § 52, the liquid shape, when oil is still in excess, also already constitutes a portion of unduloid.

§ 55. Let us pass to the shapes which the mass takes when the spacing of the rings exceeds that which corresponds to the cylindrical form.

If, after having formed between two rings a vertical cylinder whose height is much less than that which would correspond to the limit of stability, and one raises a little the upper ring, then one sees the cylinder growing slightly hollow in the meridian direction, so that the shape presents a neck; if the ring is further raised, the necking deepens more, and it is always perfectly symmetrical on both sides of the circle of the throat, which is consequently located at the middle of the interval of the rings. If, in the starting cylinder, the relationship between the height and the diameter was suitable, one can, continuously thus, make the necking very pronounced; the meridian curve changes the direction of its curvature while going towards the rings, so that it presents two points of inflection located at equal distances from the two sides of the circle of the throat, and also the bases of the shape preserve their convex form, and their curvature even increases somewhat. In this experiment, there is always, one understands, a limit of spacing of the rings, beyond which equilibrium is not possible any more, if one passes it, and the neck spontaneously thins until it breaks, and the shape separates into two portions, but, for any spacing less than the limit, the equilibrium is stable. The cylinder which appeared to me to give in the most marked way the results above, is that of which the height to the diameter has nearly the ratio of 5 to 7: in employing, for example, rings 70 millimetres in diameter, it is necessary to form a cylinder of approximately 50 millimetres in height; the upper ring can then be raised until it is almost 110 millimetres distant, and one thus obtains a shape in which the circle of the throat is approximately only 30 millimetres in diameter.

The experiment done by this process requires great care: the equality of the densities of the two liquids and the homogeneity of the oil must be perfect, and, at the time one approaches the limit of spacing of the rings, it is necessary to act with much care. But one succeeds without difficulty by laying out things so that the axis of revolution is horizontal: the rings of 70 millimetres, which are then vertical, must be placed in advance at a distance of 110 millimetres apart; each one of them is fixed, by its lower part, with a vertical wire, and these wires are fixed themselves, by their lower ends, with a small iron plate which supports all the system; finally these same wires are surrounded by cotton, so that the oil does not stick to them (§ 9). One first forms a cylinder between the two rings[18], then one gradually decreases the volume of the mass using the small syringe. When the circle of the throat nears 30 millimetres, take care then to remove oil only by very small portions. One manages to reduce this diameter to 27 millimetres, and one thus obtains the result represented fig. 28.

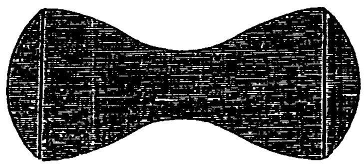


Fig. 28

But it is obvious that all these necked shapes that have convex bases, shapes which can, as well as those which we studied in the preceding paragraphs, deviate from a cylinder as little as is wanted, are still portions of unduloid, but taken differently in the indefinite unduloid: while the middle of the earlier is the equator of a bulge, and the middle of the latter is occupied by a neck; the maximum extent of the first us composed of a whole bulge between two half-necks (fig. 21 and 22), and that which is shown in fig. 28 is composed of a whole neck between two portions of bulges.

§ 56. Now let us take again our horizontal rings, in order to be able to place at will the upper ring nearer or further from the other; further let us form a cylinder between them, then, without changing their distance, remove gradually oil from the mass. If the ratio of the distance between the rings to their diameter is much less than in the last experiment of the preceding paragraph, the curve of the bases, instead of increasing as necking deepens, goes, on the contrary, down; and if this ratio does not exceed approximately $2/3$, one manages to make the bases absolutely flat. For a still smaller ratio, one can even go further: continuously withdrawing liquid, one sees the bases becoming concave: let us form, for example, between rings 70 millimetres in diameter, a cylinder of 35 millimetres height (fig. 29); by the gradual withdrawal of oil, we will see the bases subsiding more and more at the same time as the neck grows hollow, and finally losing all their curvature, and we will thus have the result shown in fig. 30. If we

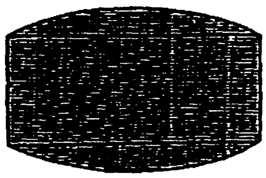


Fig. 29

[18]One carries out this operation by the means indicated in § 46, i.e. by initially attaching the mass to the one of the fixed rings, then stretching it towards the other using a mobile ring of the same diameter, and finally absorbing the oil excess.

continue to use the small syringe, the bases would take a concave curvature; but, for the moment, we stop when they are plane.

With such bases, the neck between the rings cannot (§ 49) belong any more to the unduloid, and we consequently come to a new shape of revolution. Thus let us seek what this new shape is in its complete state.

Fig. 30

The bases of our partial shape being flat, and consequently with zero mean curvature (§ 2), equilibrium requires that the surface of the portion ranging between the two rings, and, consequently, that of all the remainder of the complete shape, also have zero mean curvature; it is necessary thus that $\frac{1}{M} + \frac{1}{N} = 0$ throughout, from which it follows that $M = -N$; and since the quantities M and N can be regarded as both belonging to the meridian line (§ 36), this line must be such that at each one of its points, its radius of curvature is equal and opposite to the normal. However the geometers have shown that the only curve which enjoys this property is the *catenary*[19]. The curve turns its apex towards the axis to which one directs the normals, the line which symmetrically divides it in two equal parts is perpendicular to this axis, and the distance of the apex of the curve from the axis is equal to the radius of curvature at the apex.

Our shape is thus, in its complete state, that which would be generated by a catenary thus placed with respect to the axis. We will give it, accordingly, the name of *catenoid* ; fig. 31 shows a rather wide meridian cross-section of it, in which the axis of revolution is ZZ'.

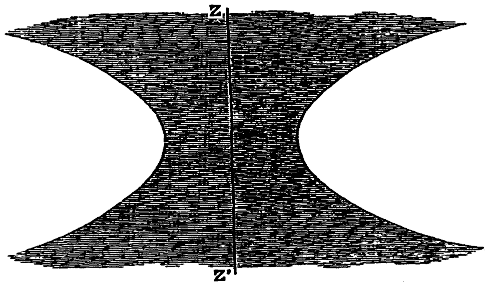


Fig. 31

The catenary being a curve with infinite length, the catenoid thus extends to infinity, as the cylinder and the unduloid, but not only along the axis.

It is hardly necessary to point out that, in our partial catenoid, the curved surface between the rings exerts the same pressure as the flat bases, so it is that, in this curved surface, the mean curvature, i.e. (§ 2) the average between all the positive and negative curvatures around the same point, being zero, its influence on the pressure is also zero, so that this pressure remains the same one as if there were no curvature. We already saw a case similar at the end of § 31.

§ 57. Under the terms of the principle at the end of § 2, one can design two catenoids of different aspects, shown in fig. 31, in which the liquid fills space inside the catenary while, and another in which the liquid occupies the space outside the curve. Fig. 32 shows a meridonal cross-section of this last.

§ 58. In the experiment of § 56, one can, as we said, only make shapes with flat bases when the spacing of the rings does not exceed approximately 2/3 of their diameter. We will reconsider (§ 62) this experiment; but we will insist as of now on a significant consequence which immediately results: one must conclude, indeed, that,

[19]the catenary is, as one knows, the curve formed, in the state of equilibrium, by a perfectly flexible heavy chain suspended at two fixed points.

for rings of a given diameter, there is a maximum of spacing beyond which no portion of a catenoid is possible between them. We will show that this result agrees with theory, and we will be led at the same time to a new result.

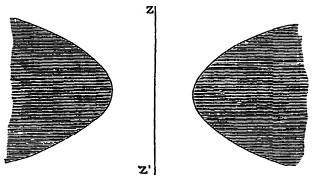


Fig. 32

One saw that the generating catenary must satisfy this condition, that the radius of curvature of its apex is equal and opposite to the line segment which measures the distance from this apex to the axis of revolution. That being so, let us conceive, in a meridian plane, a line perpendicular to the axis of revolution, and representing the axis of symmetry of the catenary, then a second straight line parallel to the axis of revolution and distant from it by a quantity equal to the radius of the rings. In the various spacings of the latter, their centers will remain on the axis of revolution, and their contours will always be based on the second line above, that, to shorten, we will call the line of the rings. Finally let us imagine, in the same plane, a generating catenary having its apex at the point where the line of the rings is cut by the axis of symmetry about which we spoke. This catenary will be tangent at this point to the line in question, and will be able consequently to be on the rings only when contours of those pass through the point of tangency, or, in other words, when the mutual distance between the two rings is zero[20] the catenary in question thus corresponds to the case of a zero spacing of the rings. Now let us suppose that the curve leaves this position and goes gradually towards the axis of revolution, while changing so as to always satisfy the condition of equality between the radius of curvature of its apex and the distance from this apex to the axis; in each one of its new positions, it will cut the line of the rings at two points, which we will indicate by A and B. The distance between those will thus represent, in each one of these same positions, the spacing of the rings, and the corresponding catenary will represent the meridian curve of a catenoid whose rings would include the portion between them. That posed, let us examine the progess of points A and B.

In the initial position of the catenary, i.e. when its apex is tangent to the line of the rings, points A and B are merged at the point of tangency; then, when the apex of the curve starts to go towards the axis of revolution, these two points separate and gradually move apart. However I say that their mutual distance will reach a maximum, after which it will decrease, on the contrary. Indeed, according to the condition to which the catenary is subject, when its apex arrives very near the axis of revolution, the radius of curvature of this apex will have become very small, from which it follows that the two branches of the curve will be brought closer, and that, consequently, the two points A and B will be also be closer neighbors of one another; finally when the apex is on the axis, these same points are again joined together in only one, since then the radius of curvature of the apex will be zero, and that thus the two branches of the curve will make only one straight line merged with the axis of symmetry. The points A and B, starting in coincidence, are initially widening, then approaching, until coinciding finally a second time; from which it necessarily follows that the mutual distance between these two points reaches a maximum; moreover, it is easily seen, according to the nature of the curve, that this maximum must be finite, and even that it cannot be considerable relative to the diameter of the rings.

It is obvious that, on its way to the axis of revolution, the curve passes through all the cases which, with the rings given, can be appropriate for equilibrium; the maximum

[20] We consider here, to simplify, rings made of wire without thickness.

above thus constitutes a limit of spacing of the rings, beyond which there is no longer a possible catenoid between them. But what precedes also provides us another remarkable consequence. Since, during the journey of the apex of the catenary, points A and B move away initially one from the other before later approaching, they necessarily pass by again by the same distances, so that, for each distance lower than the limit, they have two catenaries at the same time, so it results therefrom that with any spacing of the rings less than the maximum spacing, there are always two distinct catenoids supported on these rings, but penetrating unequally between them. One easily sees that the apices of the two generating catenaries, apices which, for a null spacing, are one with the common contour of the rings in contact, and the other on the axis of revolution, approache each other more and more as the spacing increases, and coincide finally, as well as the two whole curves, when this spacing reaches its maximum. The two catenoids will thus differ less as the spacing of the rings is larger, and will be only one in the limit.

§ 59. All catenaries are, as one knows, similar to each other; however if one conceives a succession of complete catenoids generated by catenaries of various sizes, all these catenaries will be also, according to the condition which they must satisfy (§ 56), similarly placed compared to the axis of revolution, and consequently all catenoids will be similar shapes.

The complete catenoid is thus not suitable for variations of form like the unduloid, but constitutes a single shape, like the sphere and the cylinder.

Thus the two complete catenoids which, theoretically, pass through the same rings, when the spacing of those is below the limit, differ from each other only in their absolute corresponding dimensions.

§ 60. Of the two partial catenoids belonging to these two possible complete catenoids, according to the theory, between the rings, our process necessarily gives the outer one; and if one then tries to arrive at the inner one by removing new quantities of oil from the mass, it is always, as we will see soon, another shape of equilibrium which is produced; however, of the impossibility of creating this inner partial catenoid, one can legitimately conclude that it would constitute an unstable shape of equilibrium.

As for the outer one, it obviously forms a larger portion of the complete catenoid as the spacing of the rings is closer to the maximum; because, as the rings are more isolated, the arc of the catenary which they intercept between them is (§ 58) a more considerable portion of the curve. To have a partial catenoid more extended compared to the complete catenoid, it would be necessary that the catenary penetrate between the rings; but consequently, by some small quantity which the apex of the curve advanced, the spacing of the rings would decrease (ibid.), there would be another possible catenary, less sunken, and being supported on the same rings, and the partial catenoid generated by the first having sunken more, it would be unstable. The catenoid of greatest height thus constitutes the most extended portion of the complete catenoid which one can form between two equal rings.

Let us announce here another consequence to which what precedes seems to lead, and which would be in opposition with the facts: for any spacing lower than the maximum, the outer catenoid always shows perfectly stability, and, as one saw above, the inner must be viewed as always being unstable; however the catenoid of greatest height forms, as one has just seen, the passage between the catenoids of the first category and those of the second, and, consequently, between the stable catenoids and the unstable catenoids; one can thus believe oneself in the right to admit that the catenoid of greatest height is the limit of the stability of this kind of shape; however, when one creates it with an oil mass (§ 62), it expresses a very decided stability. We will discover, in the chapter where we will treat the questions of stability, to what this apparent contradiction is due.

§ 61. – It is easy to see that the third limit of the variations of the unduloid, the limit about which we spoke in § 51, is just the catenoid. Indeed, by varying the partial unduloid in the manner indicated in this same paragraph, it is clear that as the volume of the mass is increased, the normal and the radius of curvature relating to the apex of the convex meridian arc are growing, and become infinite at the same time as the volume; from which it follows that with this limit the quantity $\frac{1}{M} + \frac{1}{N}$ is zero, which we know to be the character of the catenoid.

The quantity $\frac{1}{M} + \frac{1}{N}$ thus converging towards zero as the unduloid approaches the catenoid, the pressure exerted by the surface layer converges at the same time towards that of a plane surface; if thus one conceives, between two rings, a neck pertaining to an unduloid, and if one imagines that this unduloid goes by degrees towards the catenoid, the bases of the shape, bases whose pressure must always be equal to that of the neck, will become necessarily less and less convex, and will be finally completely plane. However that is obviously what the experiment of § 56 carries out: when, after having formed between two rings a cylinder of which the height does not exceed 2/3 the diameter, one gradually removes liquid from it and the bases subside little by little until losing all their curvature, the necking which occurs and deepens progressively, belongs, one understands, to an unduloid which tends towards its third limit, and the experiment in question thus makes us aware of the progressive passage of the unduloid to the catenoid.

If we join together what precedes with the contents of § 55, we will legitimately be able to deduce this conclusion from it: any neck supported on two rings and having convex bases is the neck of an unduloid, whether the curvature of the bases is higher, equal or lower than that of the bases of the cylinder which would lie between the same rings.

§ 62. I sought to determine by experiments, at least the manner of its approach, the maximum relationship between the height and the diameter of the bases. The external diameter of the rings used was 71 millimetres. In each test, one started by forming a cylinder between these rings, then one removed oil from the mass by full syringes initially, and then by small portions; one stopped the operation from time to time to observe the shape. One found, in this manner, that the greatest spacing of the rings for which one could obtain a shape with plane bases, was 47 millimetres, which is about 2/3 of 71. We can conclude that the maximum height of the partial catenoid is, either exactly, or is very close to, 2/3 of the diameter of the bases. This catenoid is shown in fig. 33. These experiments introduced curious characteristics, of which description will find its place in chapter IX.

We finish here the study of the unduloid and the catenoid, and we will now pass to that of another shape.

Fig. 33

§ 63. This other shape, we already foresaw a portion of: it is necking with concave bases of which we have spoken in § 56, necking which, by the nature of these bases, is different from the unduloid and the catenoid. To create it, it is necessary, as we said, that the distance between the rings be lower than the 2/3 of the diameter; fig. 34 shows, by meridian cross-section, a similar necking, for a distance between the rings equal to approximately a third of the diameter, and with the bases already strongly grown concave; the dotted lines are the sections of the planes of the rings. Let us test now, as we did with regard to the two preceding shapes, to determine the complete shape of the meridian curve.

Let us present some initial remarks. First, let us suppose that the relationship between the distance between and the diameter of the rings is sufficiently low to make it possible to extract a great quantity from the liquid without fear of causing rupture. In this case, with necking and the bases growing concave at the same time, one understands that there must come the moment after which their surfaces could not coexist any more without mutually intersecting. One will discover, in chapter VI, what happens then; but one sees, as of now, that if one wants to observe necking in all the phases where it belongs to the new shape of equilibrium, one must put an obstacle in the way of the hollowing of the bases, and it is easy to substitute discs for the rings; one can then remove oil until the shape spontaneously divides at the middle of its height. The discs which I employed are, like the rings, 70 millimetres in diameter; the lower is connected to three feet more solid than those of the rings, with the points of contact located between the edge and the center; the upper is supported by a sufficiently thick wire fixed perpendicularly at its center.

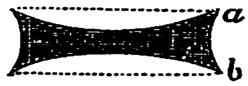


Fig. 34

In the second place, necking, which happens between rings or even further between discs, always shows perfect symmetry on both sides of its mid-circle. It is besides what the theory wants, because the mode of reasoning of § 48 is independent of the nature of the meridian curve, and applies thus to the necking in question here as well as with a necking of an unduloid. If thus, in a meridian plane, one conceives a line perpendicular to the axis of revolution and passing through the center of the mid-circle, all that the complete meridian curve will offer on one side of this line, it will also offer, in an exactly symmetrical way, on the other side, so that this same line will constitute an axis of symmetry.

In the third place, since, by using rings, the bases of the partial shape are concave segments of a sphere and have consequently a negative mean curvature, it must be the same for the necked surface, and, consequently, of all the remainder of the complete shape; thus, in this complete shape, the quantity $\frac{1}{M} + \frac{1}{N}$ is negative everywhere.

§ 64. These preliminaries posed, I say that the points a and b (fig. 34), where the partial meridian curve stops, cannot, in the complete meridian curve, be points of inflection. One sees, indeed, according to the direction of the tangent at these points, that if the meridian curve took from there a contrary direction of curvature (fig. 35), the radius of curvature, in this part of the shape, would be directed inside the liquid like the normal, and that thus the quantity $\frac{1}{M} + \frac{1}{N}$ would become positive.

Beyond the points a and b, the meridian curve thus starts by keeping a concave curvature; and the same direction of curvature is maintained obviously for the same reason, as long as the curve is moving away at the same time from the axis of revolution and the axis of symmetry. But the curve cannot indefinitely continue to move away from these two axes; indeed, if such were its progress, it is clear that the curvature should decrease so as to become zero, in each of the two branches, at the point located at infinity, so that at this point the radius of curvature would have an infinite value; and as it would obviously be the same with the normal, the quantity $\frac{1}{M} + \frac{1}{N}$ would become zero at this limit.

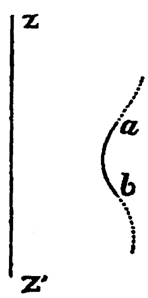


Fig. 35

It is thus necessary that at a finite distance from its apex, the curve has two points at which its tangents are parallel to the axis of symmetry, and that is what the experiment confirms, as we will see.

§ 65. If discs are employed, and one places them at a distance equal to approximately a third of their diameter, and one withdraws sufficient liquid, the angle between the end tangents of the surface of the mass and the plane of each disc decreases until being cancelled completely, so that this surface is then tangent to the disc planes (fig. 36), and that thus the end tangents of the meridian curve are parallel to the axis of symmetry. It is extremely difficult to consider the precise point where this result is reached; but one makes sure that it really occurs, by continuously removing liquid one does not delay, indeed, to see the circumferences which bound the surface of the mass leave the edges of the discs, withdrawing, while decreasing in diameter, at a certain distance inside of these edges, and to leave free a small zone of each solid plane; however, as these zones remain necessarily wet with oil, although an excessively thin layer, it is clear that the surface of the mass must connect to it tangentially. If the spacing of the discs is less still, a result of comparable nature is obtained; only one can, before there is spontaneous disunion in the middle of the shape, get more narrow circumferences of contact, or, in other words, to increase the width of the free zones (fig. 37).

Fig. 36

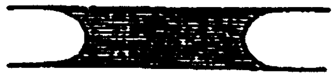


Fig. 37

§ 66. The reason given in § 64 to establish the absence of an inversion of curvature as long as the curve moves away at the same time from the axis of revolution andthe axis of symmetry, obviously still applies at the points which we have just considered, i.e. at those where the tangents are parallel with this last axis; from which it follows that the curve approaches it, preserving the same direction of curvature, as fig. 38 shows, where the curve is drawn on a greater scale than the portion included in fig. 36, and where the axis of symmetry is represented by line XX'.

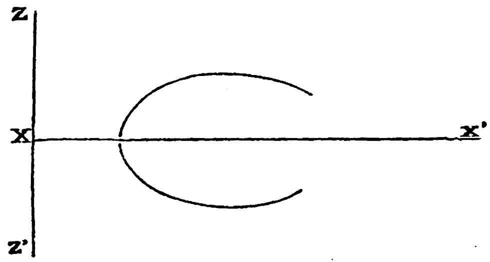


Fig. 38

And as long as these prolongations of the curve continue to move away from the axis of revolution, the direction of the curvature must still remain the same one. Let us suppose, indeed, that it changes at f and g for example (fig. 39); then, from the point f to a point such as m located a little beyond that, the radius of curvature and the normal would have, one sees, opposite directions, so that the quantity $\frac{1}{M}+\frac{1}{N}$ would be a difference; however, from f to m, the normal would obviously be growing, since, on the one hand, the distance to the axis of revolution increases, and that, in addition, this normal would have an increasingly large obliqueness; it would thus be necessary, so that the difference above remained constant, that the radius of curvature was also growing from f to m; but it is precisely the opposite which would take place; because, due to the inflection, the radius of curvature would be infinite at f, and consequently could only decrease from there. It is needless to point out that what we have just said also applies

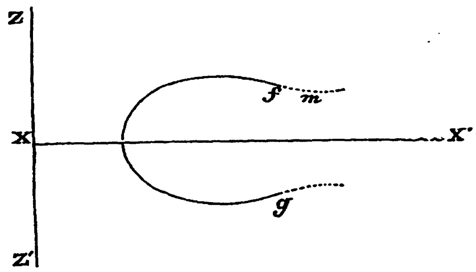


Fig. 39

to the point g.

Let us see now if, before reaching the axis of symmetry, the curve can present two points such as h and k (fig. 40) where its tangents are perpendicular to this axis. For that, let us examine what conditions the curve must satisfy from the apices to the points h and k, and it will be enough to consider the arc snh. That is to say the point where the tangent of the curve is parallel to the axis of symmetry. Between s and n, the radius of curvature and the normal obviously have opposite directions, and the quantity $\frac{1}{M}+\frac{1}{N}$ constitutes a difference; therefore, from a point to another of this arc, the quantities M and N must vary in the same direction; and as the normal is increasing from point s to the point n, the radius of curvature must be in the same way increasing; from which it follows that, from s to n, the curvature is decreasing. Further, i.e. from n to h, one sees that the radius of curvature and the normal are directed to the same side, in a manner that the two terms of the quantity $\frac{1}{M}+\frac{1}{N}$ are of the same sign, and that thus, from the one point to the other, the quantities M and N must vary in opposite direction; however, as soon as one leaves the point n on the arc nh, the normal starts decreasing, since, at the point n, it is infinite, therefore the radius of curvature starts increasing, or, in other words, the curvature is initially decreasing, and, whatever its later progress, it will always be, at all the points of the arc nh, less than at n, since at all these points the normal is finite, and, consequently, less than at n. But we know that the curvature is growing from n to s; therefore, in all the extent of the arc nh, the curvature is less than at any point of the arc ns.

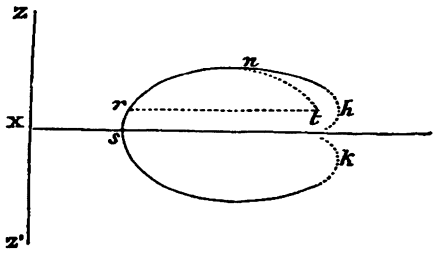


Fig. 40

That posed, draw the line hr parallel to the axis of symmetry, then build, from the point n an arc nt exactly symmetrical to the arc nr. Over the entire length of the arc nh the curvature will be, under the terms of what precedes, less than at any the points of the arc nt; from which it follows that this last arc will be entirely interior to the first, but the arc nt ends in t on the line hr with a tangent which necessarily makes, with the part tr of this line, an acute angle; therefore, so that the arc nh, which starts from n in the same direction as the arc nt, ends perpendicularly, at h, to the line hr, one would need that after having moved away from the arc nt, it then becomes approaching, which is obviously impossible in consequence of the inferiority of the curvature at all of its points; one even sees that it must cut the line hr at an angle acuter than it does the arc nt.

Thus the curve, towards the axis of symmetry, cannot cease moving away from the axis of revolution; and since it also cannot change the direction of its curvature, it must cut the axis of symmetry; finally it is understood that consequently there is another condition which governs the curve: it must cut this axis obliquely, so that we arrive at this conclusion, that it forms a node (fig. 41).

We will check the existence of this node with the aid of experiment; if we did not start there, it would be necessary initially to show that for a neck for which the mean curvature is negative, there is not any other possible shape of the meridian curve.

§ 67. The necks formed in the experiments of § 65 being generated by a portion of the node of the complete meridian curve, it is clear that the shape generated by the entire node, from its apex to its point, would be concave in the interior of oil; but, as we know (§ 2), it is indifferent, for equilibrium, whether the liquid is located on one side or the other of a surface; one can thus equally well suppose the shape generated by the node is in relief, and it is in the latter state that our experiment will happen. Only when the liquid is transported to the other side of the curve, the quantities M and N change their sign at the same time, and consequently the quantity $\frac{1}{M}+\frac{1}{N}$, which was negative, becomes positive.

One forms, in a ring of iron wire, a biconvex liquid lens (§ 19), of which the thickness is very nearly equal to the sixth of the diameter; for example, with a ring 70 millimetres in diameter, the thickness of the lens must be approximately 12 millimetres. If one perpendicularly bores this lens in its middle by a means which we will indicate below, one obtains a regular annular shape, limited outside by the solid ring, and which persists for two or three seconds, after which one sees the central opening going towards a solid point of the ring, then the mass and all the liquid ebbs towards the opposite part of the ring, to form a large appreciably spherical mass there. But the temporary annular shape which is formed in these circumstances is, although unstable, a shape of equilibrium, since it is maintained for a few moments, and its duration is long enough so that one can note that its cross-section meridian has the form shown in fig. 42, in which the dotted line is the cross-section of the plane of the ring. This meridian cut shows obviously that the surface of the produced shape is generated by a node having its apex turned towards the axis of revolution and its point on the solid ring.

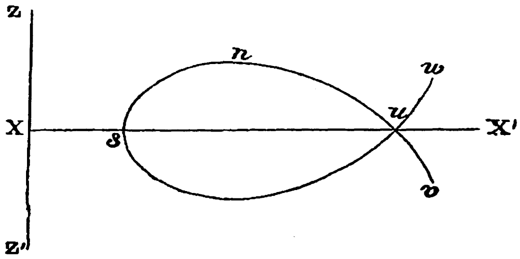


Fig. 41

We dwell somewhat on the details of the experiment which we have just described, and on certain modifications of this same experiment. To bore the lens, it is necessary to employ a small wooden rod finished with a point at one of its ends, and fixed, at its other end, to a wire which one obliquely bends in a manner so that by holding it with the hand, one can introduce the small rod into the vessel and perpendicularly bore the lens. If the diameter of the solid ring is, as we supposed above, 70 millimetres, then that of the small cylinder must be approximately 16 millimetres; the rodr and its point must be covered with cotton fabric, in order to prevent any adherence of the oil.

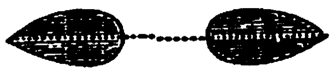


Fig. 42

If one gives to the lens a thickness which notably exceeds a sixth of the diameter of the solid ring, the liquid returns once that the small crod is withdrawn, and the mass regains its lenticular shape; but one can make the thickness less than the limit above; then the central opening takes larger dimensions, and, consequently, the node of the meridian curve is smaller.

When the thickness of the lens is sufficiently lower than the limit in question, the mode of spontaneous destruction of the unstable shape is no longer the same: the central opening does not go then any more towards a point of the solid ring, but the annular liquid mass is necked and is divided in several places at the same time, which it converts it into a series of small isolated masses which remain adherent with various parts of the metal ring. We will reconsider later this last phenomenon.

§ 68. The liquid ring thus being able to take, in the same solid ring, dimensions very different according to the thickness of the lens, or, in other words, according to the volume of the liquid of which it is formed, it follows that, for the same distance from the point of the node of the meridian curve to the axis of revolution, the length of the node can vary between wide limits: in the experiments described above, these variations are shown to be between a small fraction of the distance in question and nearly 3/4 this same distance. The complete shape concerned is thus not always similar to itself like the sphere, the cylinder or the catenoid; it is, like the unduloid, suitable for variations of form. The comparison of the liquid shapes shown in fig. 36 and 37 leads us to the same conclusion.

§ 69. Before further going, let us announce a remarkable characteristic. If the node in relief is supposed, the liquid which occupies it sits in the concavity of the curve, and it must obviously be still on the same side of this curve beyond the point u (fig. 41); it thus fills the spaces between the prolongations uv, uw and the node, so that this node is immersed, either completely, or partially, in the interior of the mass. If the node in hollow is supposed, one easily sees that then the prolongations uv and uw are immersed in the liquid.

There follows this singular consequence: that, although the general condition of equilibrium is satisfied, one can represent the complete shape only with the state of a simple surface, and not with that of a liquid mass. In this last state, it is possible to consider only isolated portions of the shape, such as, for example, that the portion generated by the node alone.

§ 70. Now let us try to discover the progress of the curve beyond the points v and w (fig. 41). We know already, by the reasons explained in § 66, and to which fig. 39 is referred, that as long as the branches of the curve continue to move away from the axis of revolution, the curvature cannot change direction, and consequently remains concave towards this axis

That being so, there are obviously only three possibilities: either the branches in question move away from the axis of revolution so that their distance to this last converges towards infinity; or they tend towards an asymptote parallel to this axis; or each one of them presents, at a finite distance from the point u of the node, a point where the tangent is parallel to this same axis.

We must immediately exclude the first of these assumptions: it would require, as we already pointed out (§ 64), that at the points located at infinity on the two branches, the radius of curvature and the normal were both infinite, and that thus the quantity $\frac{1}{M}+\frac{1}{N}$ is equal to zero.

Thus let us examine the second assumption, that of an asymptote parallel to the axis of revolution. At the point n (fig. 41), the normal is infinite and the radius of curvature is finite; at the point where the prolonged branch nuv would reach the asymptote, on the contrary, the radius of curvature would be infinite and the normal, which would measure the distance from this point to the axis, would be finite. Therefore, while going from the point n to this limit point, the normal, initially greater in length than the radius of curvature, would become then shorter than it; from which it follows that there would be on the curve a point where the normal and the radius of curvature would be equal, and for which, consequently, the center of curvature would be on the axis of revolution. Let α be this point (fig. 43), O the corresponding center of curvature, and $\alpha\beta$ a small arc of circle with point O as center. Our branch of curve would leave the point α with the same direction and with the same curvature as the arc $\alpha\beta$, then would separate immediately from it. Now let us suppose that starting from α the curvature is initially decreasing; the curve will necessarily start by being external to the arc of circle. Let $\alpha\gamma$ be a small arc of this curve, along which the curvature decreases, and let the length of the arc $\alpha\beta$ be equal to that of the arc $\alpha\gamma$. The point γ will be fur-

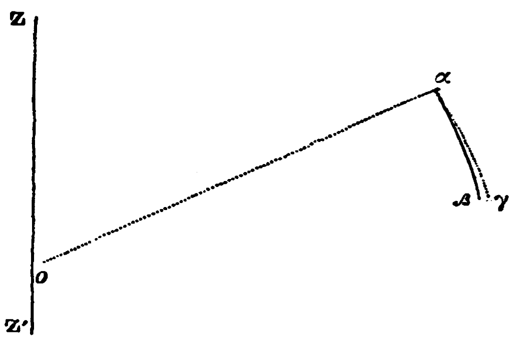


Fig. 43

ther away from the axis than the point β, and, moreover, because of the inferiority of the curvatures, the tangent at γ will make with this same axis an angle larger than the tangent at β; the normal at the point γ will be thus, by this double reason, longer than the normal at the point β.

On the other hand, still because of the inferiority of the curvatures, the radius of curvature at the point γ will be longer than the radius of curvature at the point β; but, at this last point, these two quantities have the same value, as at the point α; therefore, while passing from α to β, the radius of curvature and the normal will both increase. However that is incompatible with the equilibrium equation; indeed, the curve, in all the part that we study, turning its concavity towards the axis, the radius of curvature and the normal are of the same sign everywhere, and consequently when one increases, the other must decrease, and vice versa. If it is supposed that starting from α the curvature is growing, the arc of the curve will be interior to the arc of circle, and the same mode of reasoning would show as, from one end to another of the first, the radius of curvature and the normal will both decrease. The assumption of an asymptote parallel with the axis of revolution leading thus to an impossible result, one sees that it must be rejected like the first.

It is thus the third assumption which is true; i.e. that the curve presents two points p and p' (fig. 44), where the tangent is parallel to the axis of revolution.

§ 71. Experiment fully confirms this deduction of the theory, and it moreover provides data which will reveal to us the later progress of the curve.

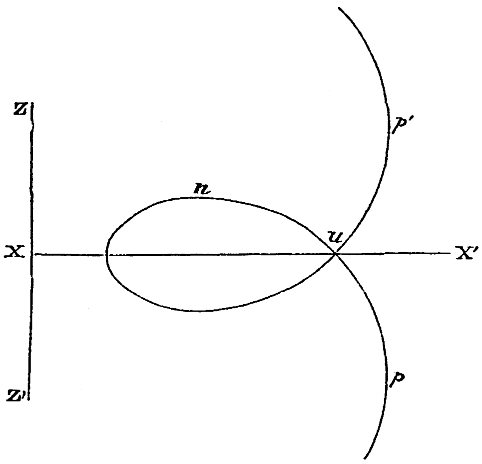


Fig. 44

The two discs being placed at an arbitrary distance from one another, at a distance equal to their diameter, for example, one forms between them a cylinder, and one lowers the upper disc then gradually: the shape, we know, becomes then an unduloid, and it bulges more and more, until it constitutes a portion of sphere (fig. 45). But if one continues to lower the upper disc, the convexity of the meridian increases further, and exceeds consequently the point above; one obtains thus, for example, for a certain separation of the discs, the result shown (fig. 46), and the liquid shape is always perfectly stable. However, in this state, it cannot belong to the unduloid any more, since one exceeded the sphere, which is (§ 50) one of the limits of the variations of this last. Finally one can lower the upper disc until at the points where the meridian curve leads to the edges of the discs, the tangents are close to being perpendicular to the

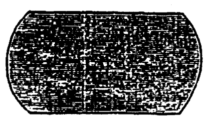


Fig. 45

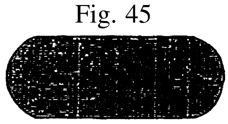


Fig. 46

axis of revolution, as one sees in fig. 47, and, for a smaller oil mass, in fig. 48; but, around this degree of separation of the discs, the liquid shape loses its stability: if one lowers a little too much the upper disc, one sees oil going in a large mass on one side of the axis of the system, so that the shape ceases being of revolution, then this same oil crosses the edges of the discs and extends partly on the outsides of those.

Now, under the terms of what was explained in the preceding paragraph, as long as the curve, starting from *n* (fig. 44), is moving away from the axis of revolution, the radius of curvature cannot become equal to the normal; and, since it starts lower, it must remain lower as long as one does not reach the point *p*; therefore, in all the extent of the arc *nup* except at the point *n* and perhaps at the point *p*, to which the demonstration does not extend, the center of curvature is always located between the curve and the axis, and consequently the curvature is increasingly stronger than that of the circumference of circle which would have its center on the axis. But, as we have just seen, in the partial liquid shapes shown in fig. 46, 47 and 48, the meridian curvatures are stronger than when the shape is a portion of sphere, or, in other words, they are stronger than that of a circumference of a circle passing through the edges of the discs and having its center on the axis. It is clear, according to that, that these same partial shapes form portions of the complete shape generated by an arc of the meridian curve extending on both sides from the point *p* (fig. 44); only they refer obviously to different cases of this complete shape, which we know to be susceptible to variations as is the unduloid.

§ 72. Let us take a last step in the continuation of our meridian curve. In the experiments above, when the densities of the two liquids are made quite equal, the oil shape always appears perfectly symmetrical compared to its equatorial circle. In truth, it is the eye which judges thus, and one could think that this symmetry is perhaps only approximate; but we will show that it is exact. In the absence of any accidental cause of irregularity, there would be no obvious reason why an excess of curvature existed on one side of the equator rather than the other, since the two discs are equal and parallel; from which follows that there is necessarily an equilibrium shape where symmetry is exact. But if, in our partial shapes, shapes which are stable, symmetry were only approximate, it would have to be admitted that the exactly symmetrical form of equilibrium of which we came to speak would be unstable. If thus all the liquid shapes that one can obtain in the experiments described above, i.e. those which give all degrees of lowering of the disc from the case of fig. 45 until that of fig. 47 and all masses greater and smaller with the same discs, if, I say, all these shapes were symmetrical only seemingly, to each one of them would correspond another equilibrium shape very little different, and which would be unstable. But the existence of two extremely close equilibrium partial shapes, one stable and the other unstable one, can well happen in a particular case of the variations of two complete shapes, or, at least, one of them; but, it is understood, it is impossible that the same thing recurs in all the extent of the variations of the partial shapes. Let us conclude therefore that, in the liquid shapes of the preceding paragraph, symmetry is real, and that thus, in our complete meridian curve, there is, in addition to the axis of symmetry of the node, another axis of symmetry also perpendicular to the axis of revolution, and passing through the point *p* (fig. 44).

Fig. 47

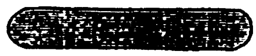


Fig. 48

But consequently all that the curve presents on one side of this point, it must present symmetrically other side; the node which is above p must thus have its counterpart below; and since these two nodes themselves respect their axis of symmetry, it necessarily follows, initially, that they are perfectly identical, and, in the second place, that all that there is on one side of the one of them must reproduce on the other side identically; from which it follows finally that above the higher node there is another similar, then above this one another still, and so on indefinitely along the axis of revolution, that the same thing takes place below the lower node, and that all these nodes are connected by likewise identical arcs between them. Fig. 49, in which the axis of revolution AB is placed horizontally, shows a wide portion of the curve.

The shape generated by this curve is thus prolonged indefinitely in the direction of the axis, as are the cylinder and the unduloid; we will also give this shape a name, we will call it the *nodoid*. Let us notice only that, this shape being, just as the unduloid, suitable for variations between some limit, fig. 49 must be viewed as offering only one case of its meridian curve.

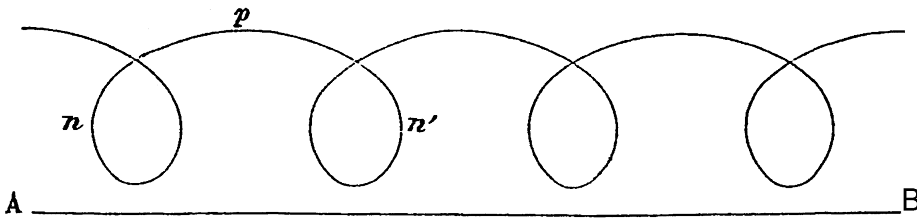


Fig. 49

Recall here the observation that we presented in § 69, and that one will understand much better now due the appearance of the curve, the observation that one can represent the complete shape only by a simple surface, because supposing it filled, it would have parts embedded in the mass.

§ 73. Before studying the nodoid in its variations, we must answer a question suggested by the results of the experiments of § 71. Now that we know the shape of the meridian curve, we see that these experiments form the portion of the nodoid generated by a more or less large part of the one of the arcs convex towards the outside, such as *npn'* (fig. 49); but one can wonder whether that does not require that with discs of a given diameter, the volume of oil must be between certain limits, so that, for larger or smaller volumes, the shape formed would not belong any more to a nodoid. To decide, let us take one of these formed shapes, continue the meridian arc beyond the point where it contacts the edge of one of the discs, the upper disc, for example, and see whether it is possible to arrive at a curve other than the meridian curve of a nodoid.

Let us suppose initially that, in the part of its way where it continues to approach the axis of revolution and to move away from the axis of symmetry, the curve has a point of inflection, so that it turns its convexity towards these two axes. If, while it still approached the first, it changed a second time the direction of its curvature, the normal corresponding to this second point of inflection would necessarily be shorter than the normal corresponding to the first, so it would have less obliqueness and would start from a point closer to the axis, but that is incompatible with the equilibrium equation: because this equation being reduced to $\frac{1}{N} = c$ at all points of inflection, the two normals above should be equal.

The existence of this second point of inflection being thus impossible, one sees that beyond the first, the curve, which cannot (§ 37) reach the axis of revolution, would necessarily have or to tend towards an asymptote parallel to this axis, or to present, at a finite distance, a point where the tangent was parallel to this same axis.

One understands immediately that the first of these two cases must be rejected: because, at the limit point where the curve would touch the asymptote, the radius of curvature would be infinite, which would still reduce, at this point, the equilibrium equation to $\frac{1}{N} = c$, and the normal would obviously be shorter there than at the point of inflection.

In the second case, the point where the tangent would become parallel to the axis of revolution could not itself, always because of the obvious inequality of the normals, be a second point of inflection; it would thus constitute a minimum of distance to the axis, and consequently a small arc extending on both sides from this minimum would generate a neck, which could be formed between two rings or equal discs. However we discussed all the possible partial shapes of this nature; we saw that any neck belongs either to the unduloid, or to the catenoid, or to the part of the nodoid which surrounds the apex of a node; but we know that the convex partial shape which we started with is not a portion of unduloid, since its convexity exceeds the sphere; it is clear, in the second place, that it is not a portion of catenoid, and finally one sees, according to what precedes, that the necking above would not be a portion of node.

Thus our original assumption, that of a point of inflection in the part of the curve which is moving away from the axis of symmetry and approaching the axis of revolution, inevitably leads to impossibilities, and consequently the curve keeps the same direction of curvature until it leaves these conditions.

But, to leave these, it is obviously necessary that it initially ceases moving away from the axis of symmetry, or, in other words, that it presents a point where the tangent is parallel to this last axis. And this point is also not a point of inflection, because the normal and the radius of curvature would be both infinite there, which would cancel the quantity $\frac{1}{M} + \frac{1}{N}$. Therefore, beyond this point, the curve goes down again towards the axis of symmetry and preserves the direction of its curvature. Moreover, the same direction is still maintained, as we will show, as long as the curve continues to go down; indeed, the liquid of the partial shape formed which was used by us as starting point being placed in the concavity of the curve, one initially easily sees that at all the points of our downward branch, the normal is negative; however if this branch contained a point of inflection, the quantity $\frac{1}{M} + \frac{1}{N}$ would be reduced, at this point, to the term $\frac{1}{N}$, and, consequently, because the sign of the normal would be likewise negative, while, on the meridian arc of the partial shape formed, the radius of curvature and the normal being both positive, the quantity $\frac{1}{M} + \frac{1}{N}$ is positive.

But the branch in question cannot go down indefinitely while always approaching the axis of revolution, or, in other words, cannot tend towards an asymptote parallel with this axis: because, at the point located at infinity on the asymptote, the quantity $\frac{1}{M} + \frac{1}{N}$ would be still reduced to the term $\frac{1}{N}$, and, consequently, would be still negative; thus our branch must pass through a minimum of distance from the axis of revolution, and thus forms the generating arc of a neck; and as this neck could belong neither to the unduloid, nor to the catenoid, it constitutes necessarily the apex of a node of a nodoid.

We thus are invariably brought back to the meridian curve of the nodoid, and we must conclude that all the shapes which one obtains in the experiments of § 71 are partial nodoids when spherical curvature is exceeded, whatever the degree of separation of the discs, and whatever the volume of oil compared the diameter of the discs.

§ 74. We can now examine what is the nature and what are the limits of the variations of the nodoid. Since, in the experiments of § 71, one passes through a portion of a sphere after which, as we have just seen, is born immediately the partial nodoid, and since this varies in a continuous manner until the phase where instability starts, it is clear that a portion of a sphere constitutes one of the limits of these variations, and that thus the limit of the corresponding changes of the complete nodoid is an indefinite succession of equal spheres having their centers on the axis. But, thinking a little about it, one will recognize that the only possible mode of continuous variation tending towards this limit is as follows: as the complete nodoid approaches the succession of spheres, the dimensions of the nodes as well as the distance from their apices to the axis decrease more and more, while the curvature of the arcs which connect these nodes converges towards that of the circumference of a circle having its center on this same axis; finally, in the limit, the nodes entirely disappear, and the arcs above become mutually tangent semicircles. The spheres generated by these semicircles are thus also tangent, and it follows that one of the limits of the variations of the nodoid is, as we said, an indefinite succession of equal spheres which are tangent on the axis.

We know already (§ 50) that a similar succession of spheres constitutes one of the limits of the variations of the unduloid; this limit is thus common to both shapes, and consequently forms the transition from the one to the other; it is besides what the experiments of § 71 show, since, going from the cylinder to the portion of sphere, the shape formed always belongs to the unduloid.

Fig. 50 represents the meridian curve of a nodoid not very far away from the limit which we have just found.

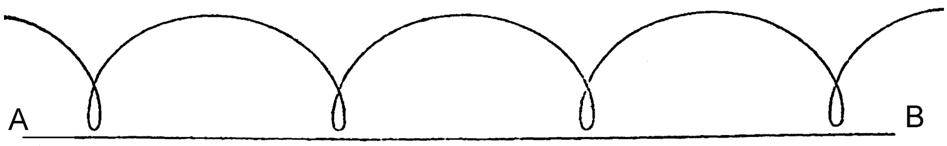


Fig. 50

§ 75. The variations of the nodoid have an extremely remarkable second limit. Let us suppose that one creates, by the process described in § 67, the portion of a nodoid generated by an isolated node; let us suppose, moreover, that one successively repeats the experiment using increasingly large solid rings and by modifying the volume of oil in such a way that the length of the meridian node, i.e. the distance from the apex to its point, remains the same. When the diameter of the solid ring is very considerable, the normals corresponding to the various points of the node will be all very large, so that in all these points the $\frac{1}{N}$ term of the equilibrium equation will be very small, and it is seen that this term will converge towards zero as the diameter of the solid ring converges towards infinity; but it cannot be the same for the $\frac{1}{M}$ term, because if this last also tended to zero, the liquid shape would have as a limit of its variations the catenoid, which is obviously impossible under the conditions where we placed ourselves, i.e. with a node of constant length; one will be able always to thus design the rather large solid ring so that at all the points of the meridian node the term $\frac{1}{N}$ is extremely small relatively to the term $\frac{1}{M}$. Then that which expresses the meridian curvature, will have, under the terms of the equilibrium equation, to vary very little on all the contour of the node, and consequently this last will approach a circumference of circle. It is clear that, in this case, the curvature of the arcs which connect the consecutive nodes of the complete meridian curve will be constant and of about the same order as that of these nodes, because the $\frac{1}{N}$ term will also be very small on the arcs in question. One understands, accordingly, that the consecutive nodes of the meridian curve will overlap each other, and that thus, for a certain large diameter of the solid ring, this line has the form partially shown in fig. 51. In this drawing, one did not indicate the axis of revolution, because it is placed too far away.

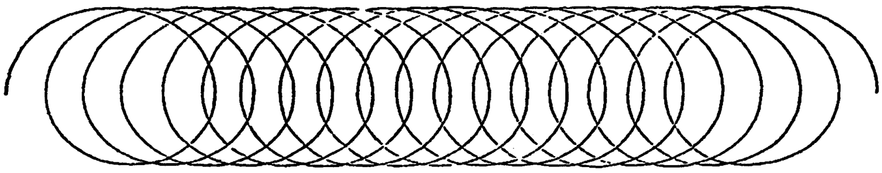


Fig. 51

If it is imagined that the diameter of the solid ring receives a new increase, the meridian curve will approach uniformity even more, the nodes will be closer to circular, and they will overlap more; finally, in the limit of these increases, i.e. when the diameter is infinite, the $\frac{1}{N}$ term will disappear completely for all the points of the meridian curve, which will reduce, for this whole line, the equilibrium equation to $\frac{1}{M} = c$; the radius of curvature will thus be then rigorously constant, and we arrive at this singular result, that the totality of the meridian curve will have condensed into only one circle; and as this will be located at an infinite distance from the axis of revolution, it is seen that the generated shape will be simply a cylinder.

Thus the second limit of the variations of the nodoid is a cylinder; but this cylinder is placed transversely compared to the axis of the nodoid from which it derives, and this axis is infinitely far away, while the cylinder which forms the second limit of the variations of the unduloid (§ 50) has as an axis the same as the unduloid.

§ 76. For the partial creation of a nodoid whose complete meridian curve is like fig. 51, it is not necessary that the solid ring have a large diameter in absolute terms; it is enough that this diameter is large relative to the length of the meridian node. Indeed, if one remembers that, in this, the curvature is (§ 66) decreasing from the apex to the points where the tangents are parallel to the axis of symmetry, and where, from there to the point, it is less than at these last points, one will understand that if the length of this same node is small compared to the radius of the solid ring, its width will be smaller still, and that at its apex the radius of curvature will be tiny in comparaison to the distance from this apex to the center of the ring, the distance which constitutes the normal; at the apex thus the $\frac{1}{N}$ term will be small compared to the $\frac{1}{M}$ term, and the value of the quantity $\frac{1}{M} + \frac{1}{N}$ will depend especially on that of this last; but it is at the apex that the normal is the least large; therefore, the remainder of the node and the arcs which link this node with the nodes close to the complete meridian curve, the $\frac{1}{N}$ term will influence less still, and consequently, in all the extent of this line, the curvature will vary only slightly.

We will describe later the best means of creating the conditions which we have just indicated; moreover, it is obviously a nodoid of this species of which one obtains a portion in hollow in the experiments of § 65 when the discs are brought very close and one stops the extraction of oil at the point where the extreme elements of the meridian arc are lying on the faces of the discs at their edges. It is also the same in the experiments of § 71, when the distance between the discs is very small and the extreme elements of the meridian arc are also as close as possible to lying on the prolongations of the solid faces; only here the meridian arc does not belong any more to only one node: it is formed, as one will see it by looking at fig. 51, of the arc which links two consecutive nodes and of two portions of the latter.

§ 77. Finally the variations of the nodoid have, like those of the unduloid, a third limit; it is revealed to us by the same experiments which led us to knowledge of the nodoid. In these experiments (§§ 56 and 63), when, after having formed a cylinder between two rings placed at a distance less than 2/3 of their diameter, liquid gradually is removed, the partial shape, we know, becomes initially an unduloid, then reaches by degrees the catenoid, after which it passes immediately to the nodoid; from which it follows obviously that the catenoid is one of the limits of the variations of the nodoid, and, moreover, that this constitutes a new transition from it to the unduloid; we already saw another (§ 74) consisting of the indefinite continuation of spheres.

The third limit of the variations of the nodoid is thus the catenoid, and it is easy to render comprehensible how the shape reaches that point. If we remember that the experiments of which we speak form the portion of the nodoid generated by an arc pertaining to a node and turning its concavity outside, we will conclude that the portion of the nodoid which passes to the catenoid is that which is generated by one of the nodes, whose apex becomes that of the meridian catenary. That said, let us imagine that each node of the complete meridian curve changes gradually to arrive at the catenary, and imagine, to fix ideas, that, during all these modifications, the distance from the apices to the axis of revolution remains constant. As the nodes approach the catenary, the quantity $\frac{1}{M}+\frac{1}{N}$ will converge towards zero, but, on all the arcs which link the nodes between them, the quantities M and N are of the same sign, and consequently the quantity $\frac{1}{M}+\frac{1}{N}$ relating to these arcs cannot converge towards zero unless M and N converge at the same time towards infinity; all the points of these arcs will thus move away indefinitely from the axis of revolution, at the same time as their curvature will become indefinitely lower; from which it follows that the points of the nodes will move away more and more from the axis, while, by the growing development of the intermediate arcs, which, according to the nature of their curvature, can obviously only decrease in curvature without extending more, the nodes will deviate more and more from each other, until, in the limit, they are all infinitely distant and are infinitely lengthened. If thus we consider one of them in particular, all the curvature will be eliminated, and, in addition, its point will have disappeared, and it will be transformed into the meridian curve of a catenoid, i.e. a catenary.

§ 78. It remains to answer a last question: are there other shapes of equilibrium of revolution than those whose existence we have recognized up to now? All these last are such as one can always include portions of them between two equal and parallel discs; however our experiments exhausted all the combinations of this kind; from which one must conclude that if there were still other shapes, they would not be able to meet this condition, and it would be necessary obviously, for that, that their meridian curves did not present any point of which the distance to the axis of revolution was a maximum or a minimum. As these lines also could not reach the axis, they should be always moving away, from a first point located at infinity on an asymptote parallel to this axis, up to another point also located at infinity. That said, at the first of these two limit points, the radius of curvature would be necessarily infinite, while the normal would be finite, and the equilibrium equation would be reduced to $\frac{1}{N}=c$; but it follows that the curve could not anywhere change direction: because if there were a point of inflection, the equation of equilibrium would be also reduced to $\frac{1}{N}=c$, and consequently the normals at first extreme point and this point of inflection would be equal, which is obviously impossible. Consequently the curve being free from any undulation, the curvature would necessarily converge towards zero, or, which is the same, the radius of curvature would converge towards infinity, while approaching the second extreme point, so that at this point the $\frac{1}{M}$ term would disappear as would the first, which would require, just as previously, the impossible equality of the two normals.

The only equilibrium shapes of revolution of a liquid mass without gravity are thus those to which we have arrived knowledge of: the sphere, the plane, the cylinder, the unduloid, the catenoid and the nodoid.

Thus there is no equilibrium shape of revolution having, in its complete state, an annular form, and the sphere is the only one which is closed; we can consequently restrict the possibility that a closed surface, other than the sphere, has constant mean curvature, and to add as we said in § 34, that if there is such a surface, it is not a surface of revolution.

§ 79.Let us see now results of the geometers. In his *Nouvelle théorie de l'action capillaire*[21] published in 1831, Poisson, seeking to determine the equation of the free face of a small quantity of a heavy liquid between two horizontal solid planes, arrives initially at a second-order differential equation, containing a term into which the quantity g, or gravity, enters implicitly as a factor, an equation which, if one made $g = 0$, would reduce, as it must, to that of our equilibrium shapes of revolution, put in a differential form. He shows then that, on a certain assumption relating to the volume of the liquid, an assumption which comes down to taking a rather small volume so that the influence of gravity is very tiny compared to that of the molecular forces, one can, to a first approximation, neglect the term concerned, which is consequently the same thing as to suppose the liquid without gravity. Poisson obtains the first integral of the equation thus reduced, then he notices that one will be able to always obtain the second by means of elliptic functions, and, in some cases, by means of arcs of circles and logarithms.

§ 80. In 1831 also, Goldschmidt studied analytically[22] the surface generated by the catenary, that is to say the catenoid. The conclusions to which we have come agree fully with those which he deduces from his formulas. He finds the precise value of the limiting ratio between the radius of the bases and the spacing of those, and from the number which he gives, one draws, for the limiting ratio between this spacing and the diameter of the bases, the value 0.6627, value which is indeed, one sees, very close to 0.6666, or 2/3.

Goldschmidt indicates an elegant geometrical construction to determine the position of the respective apices of two meridian catenaries, in the case for a spacing of the bases less than the limit. He deduces from this the consequence, that in the limit, if, at the point where the axis of symmetry of one catenary cuts the axis of revolution, one extends a tangent to the curve, the point of contact is with the one of the ends of the intercepted arc. One concludes from that if one also extends a tangent to the other end of the same arc and that these two tangents are prolonged, they will also touch at its ends the opposite meridian arc. This property characterizes in a simple way the limiting catenoid.

Goldschmidt shows that beyond the limiting spacing, this does not hold any more, as the surface of revolution with minimum area based on the bases consists of the two planes occupying these bases.

Finally he arrives at the following result: the surface area generated by the revolution of an arbitrary portion of the meridian catenary is equal to half of the surface area of the cylinder having for its base the neck circle and for its height the portion of the axis between the normals led to the two ends of the generating arc.

§ 81. Lindelöf and Moigno again dealt with, in 1861, the problem of the catenoid[23]; they find, by a shorter method, several of the results of Goldschmidt. They arrive, moreover, at this other result: for an arc of catenary whose end A is taken arbitrarily, it is the minimum area surface of revolution only up until the second end B, moving away along to the curve, reaches a position such that the two tangents from A and B meet

[21] Chap. VI of this work.

[22] *Superficiei minimae rotatione curvae data duo puncta jungentis circa datum axem ortæ* (Goettingen, 1831).

[23] *Leçons de calcul des variations.* Paris, 1861, No. 102 to 105.

at a point on the axis of revolution. This property thus provides the general condition which all limiting catenoids must satisfy, whether their bases are equal or unequal.

§ 82. Delaunay has given[24], in 1841, an extremely simple and quite remarkable generation of the meridian curves of surfaces of revolution of constant mean curvature. He showed, by means of calculation, that these curves can be described by one of the foci of a conic section rolling on a straight line. This straight line is then the axis of revolution of the surface.

Later Mr. Lamarle[25] geometrically showed the same principle with new and fruitful methods.

These two geometers are satisfied with the demonstration of the principle in question, without seeking the shapes of the lines thus traced; only Delaunay recalls that the catenary can be generated in this manner by the focus of a parabola; but it is easy to see that the lines resulting from this mode of generation present all the characteristics of form and all the modifications which I have concluded, as one saw, by experiment and reasoning, and which Beer had, on his side, partially deduced from calculation, as I will say soon.

Indeed, one recognizes that when the rolling curve is an ellipse, the curve described by any of the foci is a sinuous curve reproducing periodically along the line, and alternately presenting a maximum and a minimum of distance from this line; it will be, consequently, the meridian line of an unduloid.

When the ratio of the axes of the ellipse approach unity, the sinuosities of the described line will be less marked, and if these two axes are equal, i.e. if the rolling curve is a circle, the described curve becomes a straight line parallel with the first, and the shape of revolution becomes a cylinder.

On the contrary, the more the ratio of the axes of the ellipse depart from unity, the stronger the sinuosities. If, the major axis keeping a fixed value, the minor axis decreases until vanishing, so that the ellipse is reduced to its major axis, with the foci at the ends, the described curve will consist of a succession of semicircles which are tangent on the line; the shape of revolution is thus, in this case, a succession of tangent spheres along the axis equal to each other, and which nothing prevents us from conceiving as isolated.

If, the ellipse being reduced to its major axis, one supposes that it becomes increasingly long, but that, for each one its lengths, its tracing end departs from the same point on the straight line, then the semicircle traced by a rotation will be increasing more and more in dimensions, and, in the limit, that is to say when the other end of the major axis will have moved away to infinity, the described curve will be a line perpendicular to the fixed straight line; it will be the meridian curve of a plane.

If one again takes nondegenerate ellipses with their major axes increasingly large, but such that the distance from the tracing focus to the end of the major axis is the same for all, the nearer parts of the traced curves will all approach this same distance from the straight line; but the convex parts towards the outside will have increasingly wide dimensions, and finally, in the limit of the increases in the ellipse, i.e. when it is transformed into a parabola, the described line will have only one near part, all the others being moved away to infinity: it will be a catenary, and the generated shape will be the catenoid.

Remaining is the case where the rolling curve is a hyperbola. At first glance, one does not easily see that one can make a hyperbola roll on a straight line so that one

[24] *Sur la surface de révolution dont la courbure moyenne est constante* (JOURN. DE M. LIOUSVILLE, vol. VI, p. 309).

[25] *Théorie géométrique des rayons et centres de courbure* (BULLET. DE L'ACAD. BELGIQUE, 1857, 2nd series, vol. II, p. 33).

of the foci describes a continuous curve; but let us notice that when one of the halves of the hyperbola completes a roll on the line, so that the line, originally asymptotic with one of the branches of the half, will have become asymptotic with the other half, the second half of the hyperbola will at the same time have the straight line for an asymptote, which could be regarded as a common tangent at an infinite distance; there will thus be mathematical continuity in the rolling if we then make roll on the line this second half of the hyperbola, and if, when it will have in its turn rolled entirely so that the line is become again a common tangent, we start again to roll the first half, and so on. But one will be convinced easily that the curve traced in these circumstances by the same focus is a continuous curve which presents, along the line, an infinite succession of nodes; it is thus the meridian curve of a nodoid, and while varying the ratio of the axes of the rolling hyperbola, one will produce all the variations of this shape.

When the hyperbola passes to the parabola, the described curve will be reduced to a catenary, and the generated shape will be a catenoid. When, the real axis of the hyperbola keeping a finite value, the secondary axis vanishes, which will place the foci at the ends of the first and will reduce the hyperbola to two rays outward from the foci, one easily sees that the nodes will disappear, and that the described curve will become a succession of semicircles, so that the generated shape will be a succession of spheres. Finally, when, the real axis of the hyperbola still keeping a finite value, the secondary axis grows indefinitely, which will open more and more the two halves of the hyperbola while moving the foci away from the vertices, one easily sees in the same way that the nodes of the described curve will widen, then encroach on each other, i.e. this curve will exhibit the modifications about which I spoke in § 75, only the limit of these variations, the condensation of the whole curve into only one circle, is, from the point of view of rolling, a mathematical limit which cannot be reached in reality, since the foci would be at an infinitesimal distance from the line, and that half of the hyperbola which rests on it is incident with it in all its extent.

This discussion of the layout of the meridian curves naturally has its place after the discussion of the principle of Delaunay; but I must say here that a similar discussion was already published by Mr. Lindelöf, in a Report about which I will speak in § 89.

§ 83. The calculations of Delaunay and the demonstrations of Mr. Lamarle mentioned above do not exclude the possibility of surfaces of revolution of constant mean curvature other than those whose meridian curves are traced as above; but Mr. Lamarle later returned[26] to the same subject, and showed, always by means of his own particular methods, that this mode of generation is the only one which can give surfaces of revolution of constant mean curvature. This conclusion is, one sees, entirely in agreement with that which we deduced from reasoning (§ 78).

§ 84. Beer devoted to the at-rest state of the liquid mass part of the second of two Memoires, where he did me the honor of applying calculations to the experiments of my first Series[27]. It deals only with equilibrium shapes of revolution, and it seeks, by means of elliptic functions, the complete integral of the equation of their meridian curves. As I will have occasion to make use of these results, I will indicate, in a few words, how Beer arrives at the first and the second integral.

He takes the axis of revolution as the y axis. Then, p and q respectively indicating the derivatives of first and second order, one has, as one knows, for the normal, the expression $\frac{x\sqrt{1+p^2}}{p}$, and, for the radius of curvature, $\frac{(1+p^2)^{3/2}}{q}$; by setting equal to a constant the sum of the inverses of these two quantities, one has consequently, for the

[26] *Exposé géométrique du calcul differentiel et intégral*, 3rd part, 1863, p. 247, (Mem. of Acad. of Belgium, collection in-8°, vol. XV).

[27] *Tractatus de theoriâ mathematicâ phoenomenorum in liquidis actioni gravitatis detractis observatorum.* Bonn, 1857.

equation of the shapes in question,

$$\frac{q}{(1+p^2)^{3/2}} + \frac{p}{x\sqrt{1+p^2}} = C. \tag{1}$$

Now, if one multiplies the two members by xdx, and then one replaces qdx by its equivalent dp, it becomes:

$$\frac{xdp}{(1+p^2)^{3/2}} + \frac{pdx}{\sqrt{1+p^2}} = Cxdx,$$

but it is easy to see that the first member of the equation thus transformed is the differential of $\frac{px}{\sqrt{1+p^2}}$; there will be thus, by integration,

$$\frac{px}{\sqrt{1+p^2}} = \frac{Cx^2}{2} + C', \tag{2}$$

C' being an arbitrary constant. Such is the sought first integral.

Representing then by α_1 and α_2 the respective minima and maximum x-coordinates of the curve, Beer transforms this equation into the following

$$dy = \frac{x^2 \pm \alpha_1\alpha_2}{\sqrt{(\alpha_1^2 - x^2)(x^2 - \alpha_2^2)}} dx; \tag{3}$$

then he passes to integration with elliptic functions. He gets

$$x^2 = \alpha_1^2 \sin^2\phi + \alpha_2^2 \cos^2\phi,$$

and, placing the origin of the co-ordinates at the foot of the minimum x-coordinate α_1, he obtains, ultimately, the represention of the meridian curve by the equation

$$y = \alpha_2 \left\{ E\left(c, \frac{\pi}{2}\right) - E(c,\phi) \right\} \pm \alpha_1 \left\{ F\left(c, \frac{\pi}{2}\right) - F(c,\phi) \right\}, \tag{4}$$

in which the letters F and E respectively indicate the elliptic functions of first and second kinds. The modulus c and the amplitude ϕ are defined by the relations

$$c = \sqrt{\frac{\alpha_2^2 - \alpha_1^2}{\alpha_2^2}}, \qquad \phi = \arcsin\sqrt{\frac{\alpha_2^2 - x^2}{\alpha_2^2 - \alpha_1^2}}$$

§ 85. Beer deduces from these formulas the following results: The equation [4] represents two kinds of curves, according to whether one takes the upper or lower of the two signs which affect one of the terms of the second member.

To the upper sign corresponds a wavy line similar to the sinusoid, and made of identical parts which reproduce indefinitely along the axis. The x-coordinate of the points of inflection is the proportional mean between the minimum and maximum x-coordinate. The slope at these same points is equal to $\frac{2\sqrt{\alpha_1\alpha_2}}{\alpha_2-\alpha_1}$. The distance between the minima and maximum x-coordinates has the value

$$\Delta y = \alpha_2 E\left(c, \frac{\pi}{2}\right) + \alpha_1 F\left(c, \frac{\pi}{2}\right);$$

by doubling this expression, one has, in the generated shape, the interval between the mid-circle of a neck and that of the following neck. The limits of the variations which this shape tests when one varies the ratio between α_1 and α_2 are the sphere and the cylinder. One will be able to create this same shape by making adhere an oil mass to a solid cylinder in the middle of alcohol with the same density as the oil.

When taking the lower sign in equation [4], one also has a curve made up of identical parts which reproduce along the axis. If one considers one of these parts starting at its top with a minimum x-coordinate, it goes down initially while turning its concavity upward, reaches a lower point where its tangent is parallel to the x axis and whose x-coordinate is the proportional mean between the minimum and maximum x-coordinates, then goes up, always preserving the same direction of curvature, until the maximum x-coordinate, beyond which it continues symmetrically until a new x minimum. The distance between the minimum and maximum x-coordinates has the expression

$$\Delta y = \alpha_2 E\left(c, \frac{\pi}{2}\right) - \alpha_1 F\left(c, \frac{\pi}{2}\right);$$

The limits of variation of the generated shape are, on the one hand, the sphere, and, on the other hand, a circle having its center on the axis and its plane perpendicular to this one. The meridian curve above will be that of an alcoholic mass surrounding, within oil of the same density, a metal cylinder.

§ 86. Let us present here some remarks about this part of the work of Beer. The two meridian curves that he discusses are those of the unduloid and the nodoid; but, it is seen, another meridian curve, that of the catenoid, escaped him completely; he would have found it by making, in the first integral (eq. [3] of § 84), the quantity α_2 equal to infinity.

In the second place, consequently, of this singular omission, Beer assigns to the variations generated by the two shapes only two limits, instead of the three which they actually have.

These gaps are filled in a posthumous work[28] by same author, published in 1869.

In the third place, he finds, for the second limit of the second shape, a simple circle, while I found a cylinder placed transversely compared to the axis of the nodoids from which it derives. This difference occurs because Beer considers what becomes of the shape as the maximum and minimum x-coordinates of the meridian curve approach equality, but while preserving finite values, while, making on the contrary constant the difference in these two x-coordinates, I supposed that they both converged towards infinity, or, to speak more exactly, that the axis of revolution moved away indefinitely from the curve (§ 75). I did not speak about the limit consisting of a simple circle, because it cannot constitute a liquid shape; it is a mathematical and nonphysical limit.

Lastly, as for the means indicated by Beer for the partial creation of the second shape, i.e. the nodoid, it is exact theoretically, but it would be quite difficult in practice; one can be convinced, indeed, by what I explained in § 71, that a similar liquid shape would go far beyond its limit of stability; one could thus obtain it only by maintaining it with suitable obstacles.

We thus sought, Beer and me, primarily by different means, forms of equilibrium shapes of revolution. As one saw (§ 82), one could arrive at these forms on the basis of the principle of Delaunay; but Beer, by the simplicity and the elegance which he knew how to give to the first order differential equation and elliptic integrals, facilitated the analytical study and the exact construction of the meridian curves of the unduloid and the nodoid, and, on my side, by physically forming all the equilibrium shapes of revolution, with their variations and their limits, I provided theory the support of experimental checks. Moreover, while only employing, jointly with experiment, reasoning about geometrical constructions, I made clearly accessible to the mind the relations

[28] *Einleitung in die mathematische Theorie der Elasticität und Capillarität.* Leipzig.

between the forms in question and the general condition of equilibrium, and I put the search for these same forms within the grasp of people who are not familiar with higher mathematics.

§ 87.In the same Memoire, Beer determines the expressions for the volume and surface area of a portion of his first shape, or, in other words, the unduloid, this portion lying between two cross-sections perpendicular to the axis. For that, in the general formulas which represent the volume and the surface area of an unspecified shape of revolution, he replaces dy by its equivalent (eq. [3] of § 84), then integrates by elliptic functions, the modulus and the amplitude being the same ones as previously.

One easily recognizes that the substitution above would also make it possible to integrate by elliptic functions in the case of the nodoid.

Furthermore, one will easily verify that one obtains by an ordinary integration the volume and the surface area of a portion of catenoid; one will be able to thus evaluate, for all the equilibrium shapes of revolution, the volume and the surface area of a portion limited by two sections which are perpendicular to the axis.

§ 88. Mr. Mannheim presented, in 1858, at the Société Philomatique de Paris[29], on the theory of *roulettes*, a Note in which he states initially this theorem:

When a plane curve ABC rolls on a fixed line EF, the roulette described by a point M related to the rolling curve has the same length as the curve GPH, the locus of projections of the point M on the tangents of ABC[30].

Then he deduces several corollaries, among them the following:

The curve traced by the focus of an ellipse which rolls on a straight line has the same length as as a circle whose diameter is the major axis.

One knows, indeed, that the locus of projections of a focus of an ellipse on its tangents is a circle having the major axis for its diameter.

Mr. Mannheim does not speak about the curve traced by a focus of a rolling hyperbola; but it is known as well that the locus of projections of a focus of a hyperbola on its tangents is a circle having for its diameter the real axis of the hyperbola, from which it follows that the curve plotted by one of the foci has the same length as the circle in question. This extension to the meridian curve of the nodoid, moreover, was already announced by Mr. Lindelöf (see the next §).

The principle of Mr. Mannheim allows, one sees, the evaluation of the length of given portions of the meridian curves of the unduloid and the nodoid, when one has the major axis of the rolling ellipse and the real axis of the rolling hyperbola; however these axes are known when one knows the minimum and maximum distances of the traced curve from the fixed straight line; indeed, if one represents the rolling ellipse in the position where its major axis is perpendicular to the straight line, one sees that the describing focus is then closer or further from this line, and that thus the minimum and maximum distances above are those of this focus to the two vertices of the ellipse; the major axis is thus equal to their sum. One will find likewise that the real axis of the rolling hyperbola is equal to the difference of the maximum and minimum distances from the traced curve to the fixed straight line.

The statement of Mr. Mannheim relating to the meridian curve of the unduloid refers to the whole development of the rolling ellipse on the fixed line, but, it is understood, the general theorem also applies to an arbitrary lesser portion of this develop-

[29] Journal l'Institut, n° 1260.

[30] Later, Mr. Mannheim recognized (JOURN. DE L'ECOLE POLYTECHNIQUE, vol XL) that this theorem had been stated in 1840 by Steiner.

Some time after the publication of the article of Mr. Mannheim, Mr. Lamarle gave (BULLET. DE L'ACAD. DE BELGIQUE, 188, 2nd series, vol. IV, p. 239), with the aid of his new methods, an extremely simple demonstration of this same theorem.

ment and consequently of the traced curve.

Furthermore, by substituting, in the general expression $\int dx\sqrt{1+\left(\frac{dy}{dx}\right)^2}$ for the length of a curve, the value of $\frac{dy}{dx}$ relating to the meridian curves of the unduloid and nodoid (eq. [3] of § 84), one will verify that the integration is carried out easily by ordinary means.

Since the meridian curve of the catenoid, or the catenary, is rectifiable, one sees that one will be able to always evaluate, either by a geometrical construction, or by calculation, the length of a given arc of the meridian curve of any equilibrium shape of revolution.

§ 89. In 1863, Mr. Lindelöf revisited[31] all the questions about surfaces of revolution of constant mean curvature. On the basis of the theorem of Delaunay (§ 82), he seeks in particular the relations between the properties of the surfaces in question and the elements of rolling conic sections. He deduces from his calculations a succession of new results, almost all of which I will mention.

Let us first note that the author names "complete unduloid" and "complete nodoid" the portions of the infinite unduloid and nodoid whose meridian curves correspond respectively to a whole revolution of the ellipse and hyperbola generators. Here now are his results:

1° The sum of the principal curvatures at each point of the surface is, for the unduloid, equal to the curvature of the circle circumscribing the generating ellipse, and, for the nodoid, equal to the curvature of a circle having as diameter the transverse axis of the generating hyperbola.

2° Rectification (finding the lenght) of the meridian curve of the complete unduloid, given earlier by Mr. Mannheim, with extension to the meridian curve of the complete nodoid.

3° Measure of the surface area of the catenoid generated by an arbitrary portion of the meridian catenary, found already by Goldschmidt (§ 80).

4° The volume of the catenoid bounded by two arbitrary planes perpendicular the axis is equal to half that of the cylinder having as base the circle of the neck, and for height the part of the axis ranging between the extreme normals to the meridian.

5° The volume of the catenoid is also obtained by multiplying the surface area of this catenoid by the half-radius of the circle of the neck.

6° The volume of the limiting catenoid is half of that of the cylinder with the same bases and of the same height[32].

7° The surface area of a complete unduloid is equal to that of a cylinder whose base is the circle circumscribing the generating ellipse of the meridian, and whose height is the circumference of an ellipse having for axes the diameters of the largest and the smallest parallel circles.

8° The surface area of a complete nodoid is equal to that of a cylinder whose diameter is the transverse axis of the generating hyperbola of the meridian, and whose height is the circumference of an ellipse having for axes the diameters of the largest and the smallest parallel circles.

9° The volume of a complete unduloid exceeds that of the cylinder whose base is the circle circumscribing the generating ellipse of the meridian, and whose height is

[31] *Théorie des surfaces de révolution courbure moyenne constante* (MÉM. DE LA SOCIÉTÉ DES SCIENCES DB FINLANDE).

A detailed summary of this Memoire is inserted in the newspaper *Les Mondes* (vol. III, pp. 394, 414 and 431).

[32] I had found this result a long time before, and I had communicated it to Mr. Lindelöf, as he declares it in his Report. This same result is not reproduced in the summary un the newspaper *Les Mondes*.

equal to the circumference of the ellipse having for axes the diameters of largest and of the smallest parallel circles, by a third of the excess of this same cylinder over a second cylinder of the same height as the unduloid, and whose base is the circle inscribed in the generating ellipse.

10° The volume of the complete nodoid exceeds that of the cylinder whose diameter is equal to the transverse axis of the generating hyperbola of the meridian, and whose height is equal to the circumference of the ellipse having for axes the diameters of largest and of the smallest parallel circles, by a third of the sum of this same cylinder and a second cylinder of the same height as the nodoid, and whose diameter is equal to the combined axis of the generating hyperbola.

§ 90. I submitted to experiment result 6° of the preceding paragraph. For that, it was necessary to create, in the alcoholic liquid, a limiting catenoid, then to convert it into a cylinder by bringing together the bases. But as all the oil of the catenoid was to be contained in this cylinder, it was necessary to avoid the formation of convex bases, and consequently to include the liquid mass between discs and not between rings. Under these conditions, the only means to be used to obtain the limiting catenoid was to draw apart the discs by a distance equal to the height of the limiting catenoid corresponding to their diameter, then to make adhere to their faces a mass of oil in excess, and finally to withdraw liquid until the circle of the neck had the diameter pertaining to the limiting catenoid.

The diameter of the discs of which I made use was 71.49 mm; the height of the corresponding limiting catenoid with this diameter is (§ 80) thus 71.49mm × 0.6627 = 47.38mm.

Lindelöf and Moigno have found[33], for the ratio of the radius of the bases to that of the circle of the neck, the value 1.81017, of which the inverse is 0.5524 (see also the table of the next §); with my discs, the diameter of the circle of neck was thus 71.49mm × 0.5524 = 39.49mm.

One regulated the spacing of these discs by means of the cathetometer, and, to regulate in the same way the diameter of the circle of the neck of the liquid shape, one laid down the cathetometer horizontally on suitable supports.

An accidental difficulty prevented reaching the precise spacing of 47.38mm; that at which one stopped was, on one side of the shape, 46.85mm, and on the opposite side 47.05mm, and consequently, on average, 46.95mm, a quantity lower than the theoretical value of a little less of the hundredth of this one.

With regard to the circle of the neck, one withdrew oil until the diameter of this circle was very near 39.60mm, a quantity which barely exceeds the theoretical value by three thousandth of it.

The shape thus obtained thus approached very near the limiting catenoid, and consequently its volume should have been very close to that of a cylinder with the same bases and a height equal to half of the distance between the discs, i.e. equal to 23.47mm; however, after having lowered the upper disc until the shape appeared to be exactly cylindrical, the levelling of this cylinder gave, on one side 23.00mm, and, on the opposite side, 23.07mm, average 23.03mm. The difference between this result and the theoretical height 23.47mm is 0.44mm, which is not two hundredths of the theoretical height; it undoubtedly comes, mainly, from the small uncertainty there always is about the exact point where the cylindrical form is exactly reached.

Although this experiment leaves something to be desired as far as precision, one however can, I think, look at it as providing a sufficient check of the theoretical princi-

[33] See the note of § 81.

ple.

§ 91. In a article[34] published in 1869, Mr. Lindelöf recalls a principle stated in the work written jointly by him and Mr. Moigno, a principle which I mentioned in § 81; he draws on the general formulas and the exact numbers relating to the limiting height of the partial catenoid between bases whose diameters have between them an arbitrary ratio, and he displays his results in two tables.

The first of these tables gives the diameters of the two bases, the height of the limiting catenoid and the distance from the smallest base to the circle of the neck, when one supposes the diameter of this last invariable and taken as unity, and that one varies by a tenth of a unit, from 1 to 0, the ratio of the diameters of the bases.

The second takes the diameter of the lower base as a constant quantity and equal to unity, and varies that of the higher base from 1 to 0 ; it contains, under these conditions, the limiting heights of the corresponding catenoid, diameters of the circle of throat, and distances from this one at the upper base: I reproduce this second table here:

Diameter of the lower base = 1.

DIAMETER OF THE UPPER BASE.	HEIGHT LIMIT OF THE CATENOID.	DIAMETER OF THE NECK	DISTANCE FROM NECK TO UPPER BASE.
1.0	0.66274	0.55243	0.33137
0.9	0.62835	0.52313	0.29765
0.8	0.59116	0.49009	0.26280
0.7	0.55064	0.45271	0.22696
0.6	0.50609	0.41028	0.19037
0.5	0.45654	0.36199	0.15341
0.4	0.40057	0.30693	0.11668
0.3	0.33595	0.24415	0.08107
0.2	0.25878	0.17268	0.04795
0.1	0.16059	0.09165	0.01941
0.0	0.00000	0.00000	0.00000

§ 92. To finish what relates to equilibrium surfaces of revolution, I will add here that I sought the expressions of the radii of curvature of the respective meridian curves of the unduloid and the nodoid at the minimum and at the maximum distance from the axis, and that I found these expressions extremely simple.

Let us first see those which relate to the unduloid. To arrive there conveniently, start with the general formula of equilibrium according to the radius of curvature and the normal. Let us indicate by ρ_1 and ρ_2 the radii of curvature respectively corresponding to the two points in question, points for which, preserving the notations of Beer (§ 84), the normals are α_1 and α_2; we will have thus the two equations

$$\left.\begin{aligned} \frac{1}{\rho_1}+\frac{1}{\alpha_1} &= C \\ \frac{1}{\rho_2}+\frac{1}{\alpha_2} &= C \end{aligned}\right\} \qquad [a]$$

In addition, if we take, like Beer, revolution centered on the y axis, and if we notice that then, at the two points considered, the tangent is infinite, equation [2] of § 84, replacing

[34] *On the limits between which the catenoid is a minimum surface* (Yearly mathematics of MISTERS Clebsch and Neumann, vol. II, p. 160).

there successively x by α_1 and by α_2 will give us the following:

$$\left.\begin{aligned} \alpha_1 &= \tfrac{C\alpha_1^2}{2} + C', \\ \alpha_2 &= \tfrac{C\alpha_2^2}{2} + C', \end{aligned}\right\} \qquad [b]$$

from which, by the elimination of C', one gets

$$C = \frac{2}{\alpha_2 + \alpha_1};$$

finally, substituting this value of C in the two equations [a], one finds

$$\left.\begin{aligned} \rho_1 &= -\alpha_1 \tfrac{\alpha_2+\alpha_1}{\alpha_2-\alpha_1}, \\ \rho_2 &= \alpha_2 \tfrac{\alpha_2+\alpha_1}{\alpha_2-\alpha_1}, \end{aligned}\right\} \qquad [c]$$

the value of ρ_1 is negative, because, at the minimum point of the meridian curve of the unduloid, the curvature of this curve is concave.

From these two expressions one also deduces the extremely simple relation

$$\frac{\rho_2}{\rho_1} = -\frac{\alpha_2}{\alpha_1};$$

thus the absolute values of the radii of curvature respectively corresponding to the maximum and minimum distances to the axis, have the same ratio between them.

Let us apply the same considerations to the nodoid. If one looks at the constant C as positive, which amounts to supposing the liquid is in the concavity of the curve, the normal at the minimum point will obviously be negative; for this shape, it will thus be necessary to replace α_1 by $-\alpha_1$ in the first of the equations [a].

As for the substitution of α_1 for x in equation [2] of § 84, I must present a remark. According to the shape and the position of the curve, the coordinate x is always positive in the equation concerned, and, consequently, it will be the same for α_1, which plays here simply the role of x-coordinate; but, in the first member of this equation, x is multiplied by the quantity $\frac{p}{\sqrt{1+p^2}}$, for which it is necessary to determine the sign at the minimum point where its absolute value becomes unity, but if we refer to the general expression $\frac{x\sqrt{1+p^2}}{p}$ or $\frac{x}{\frac{p}{\sqrt{1+p^2}}}$ for the normal, and if we remember that at the minimum point the normal is negative in spite of the primarily positive sign of x, we will conclude that at this same point the quantity $\frac{p}{\sqrt{1+p^2}}$ is equal to -1, and that thus, when one replaces x by α_1, the first member of the equation will become $-\alpha_1$.

It will thus be enough, for the nodoid, to change, in equations [a] and [b], α_1 to $-\alpha_1$ and consequently, to have the values of ρ_1 and ρ_2 corresponding to this shape, one will have only to make the same change in the expressions [c] which will give

$$\left.\begin{aligned} \rho_1 &= \alpha_1 \tfrac{\alpha_2-\alpha_1}{\alpha_2+\alpha_1}, \\ \rho_2 &= \alpha_2 \tfrac{\alpha_2-\alpha_1}{\alpha_2+\alpha_1}, \end{aligned}\right\} \qquad [d]$$

from where one deduces also

$$\frac{\rho_2}{\rho_1} = \frac{\alpha_2}{\alpha_1}$$

so that here still the two radii of curvature are in ratio as the distances to the axis. I did not speak, in connection with the unduloid, of the question above relating to the sign; because in this shape the normal is positive everywhere.

The second of the expressions of [c] shows that at the equatorial bulges of the unduloid, the meridian curvature is always less than that of a circle which has its center on the axis, and the second of the expressions of [d] shows that, in the nodoid, at the equator of the portions convex towards outside, the meridian curvature is, on the contrary, stronger than that of a similar circle.

§ 93. Before passing to the second method for creating equilibrium shapes, let us present a remark with regard to the first. It is clear that one could substitute for oil and for the alcoholic mixture two others liquids which do not mix and which are likely to be brought to have equal densities. That is what Mr. Dufour[35] did in 1861: he made large spheres of water suspended in a mixture of chloroform and oil; chloroform being heavier and oil lighter than water, one easily arrives at a mixture of suitable density. Mr. D'henry, former preparator of Delezenne, proposed to me, in a letter in 1869, a mixture of carbon bisulphide and spirits of turpentine, in which one immerses a water mass; or of diluted sulphuric acid, with a carbon bisulphide mass immersed.

These new systems of liquids would offer, over mine, the advantage of avoiding the small mutual solvent action (§ 5) of oil and the alcoholic liquid; but, on the other hand, are dangerous to breathe; the dilute sulphuric acid would attack the small iron apparatuses, and, moreover, droplets of this corrosive liquid could be splashed on the clothing of the experimenter. Finally I much doubt that one obtains, with these liquids, an adherence with the solid parts as perfect as with oil. I thus think that, up to now, the best liquids for the performance of my experiments are those which I indicated.

If Mr. Dufour himself used chloroform, it was because the goal of his experiments required that the immersed liquid was water: he proposed to seek how water would behave apart from any solid contact and during a prolonged cooling below 0°; he saw water spheres thus, when they were small, remaining liquid sometimes until -18°, and even -20°; the mixture in which they swam composed of sweet almond oil, chloroform and petroleum oil. Let us say, in passing, that these experiments have led Mr. Dufour to a theory, which appears extremely probable to me, of the formation of hail.

Mr. D'henry, in the letter about which I spoke above, tells me about an experiment which is not without interest: after having created, in diluted sulphuric acid, a large carbon bisulphide sphere, he dropped in the acid, above this sphere, a drop of carbon bisulphide coloured by iodine; it went down through the acid to the sphere, in which it was incorporated; but then it transformed into a ring, which moved towards the center of the sphere while remaining parallel; as it advanced, it widened, while melting little by little in the liquid which surrounded it.

§ 94. In the same letter still, Mr. D'henry asks me to test the action of a powerful electromagnet on equilibrium shapes made with liquids of different nature. The known experiments that I will point out show that this action is probably not without influence.

It remained in my memories that a physicist, after having made a great number of oil spherules disseminated in a mixture of water and alcohol of the same density, subjected this system to the action of an electromagnet, and noted, in the spherules, certain given movements. The article where this experiment is described was published, I think, about 1844, but I could not find it.

In 1853, Matteucci presented at the Academy of Science of Paris a Memoire[36]

[35] *Sur la congélation de l'eau et sur la formation de la grêle.* (bibl. univers., arch. des sc., new period, volume X, page 346).

[36] *Sur les figures d'équilibre et sur les mouvements de certaines masses liquides et gazeuses,* quatrième Mémoire, (Comptes-Rendus, volume XXXVI, page 917).

in which he expounds results of experiments of the same kind: the two liquids of equal density which he employed were olive oil and a solution of iron proto-chloride in alcohol; one of these liquids was distributed uniformly in more or less large drops suspended in the other. While placing the vessel which contained them between the poles of a powerful electromagnet, Matteucci saw these drops perform pronounced movements, and arrange themselves in patterns.

§ 94bis. Another note: our first process can be useful to carry out on a large scale ordinary capillary phenomena, i.e. those where gravity intervenes. It is enough, for that, that the alcoholic mixture contains a very small alcohol excess; then, indeed, oil is no longer completely withdrawn from the action of gravity, but what remains of this action has only a very weak intensity, and is thus, on large-sized masses, of the same order as the action of the molecular forces.

Under these conditions, for example, when the oil mass is entirely free, it will be flattened somewhat on the piece of fabric which covers the bottom of the vessel, and then presents, on the whole, the same form as a droplet of mercury of a suitable volume deposited on a horizontal solid level. Under the same conditions, the mass suspended from the solid disc of the experiments of § 14 lengthens, and takes a shape identical to that of a water drop suspended from the lower end of a solid rod, etc.

I am unaware of where the three first Memoires were published, but the way in which the author begins this one leads me to believe that, in the precedents, they were not two liquids of the same density.

CHAPTER III.

Second process; demonstration. – Liquid giving films of very long persistence. – Creation, by means of films, of equilibrium shapes of revolution. – Pressure exerted by a spherical film on the air which it contains. – Search for a very small upper limit of the sensible radius of activity of molecular attraction.

§ 95. Let us show, as we announced at the end § 3, that if the very weak action of gravity is neglected, then the equilibrium shapes of a thin liquid film created in air are identically the same ones as those of a neutrally buoyant liquid mass.

If, at a point of one of two surfaces of such a film, one considers a normal line to this surface, it is clear that, considering the small thickness of the film, the line in question could be regarded as being also normal to the other surface. Moreover, if, through this common normal one passes a plane, it cuts the two surfaces following curves which will be able, without appreciable error, to be viewed as identical. Consequently, at the points where normal intersects the two surfaces, the curvatures of the two curves will be the same; only, compared to the liquid which forms the film, one of these curvatures will be convex and the other concave. If thus ρ indicates the radius of the first, that of the second will be $-\rho$; and as this result is general, it also applies to the principal curvatures, so that, if R and R' represent the two principal radii of curvature at one of the two points considered, the two principal radii of curvature at the other point will be $-R$ and $-R'$. Accordingly, the capillary pressures respectively corresponding to these two points, and referred to the unit of area, are (§ 1), for the first,

$$P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right),$$

and, for the second

$$P-\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right).$$

But these two pressures being opposite, they gives a resultant equal to their difference, that is,

$$A\left(\frac{1}{R}+\frac{1}{R'}\right).$$

Now, if the film shape is such that, in all its extent, this resultant is zero, it is clear that equilibrium will exist. If this condition is not met, the resultants respectively corresponding to the various points of the shape will tend to drive out these points in one direction or the other; but, in this case still, equilibrium will be possible if the film shape is closed, and thus imprisons a volume of air; because then, if it has a form such that the resultants in question have the same intensity everywhere, these forces obviously will be counterbalanced by the resistance of interior gas or that of the atmosphere. One will thus express the general equation of equilibrium of film shapes by establishing the condition that the resultant is zero or constant; and, for that, as the coefficient A is constant and finite, it will be enough to have

$$\frac{1}{R}+\frac{1}{R'}=C,$$

the constant C being zero or finite.

However this general equation being also that of the equilibrium of the filled masses, it follows that the films take, as I have said, identically the same shapes as these masses.

Thus one must be able to form in the air, with thin liquid films, such as those of soap water, all the shapes which we obtained with filled oil masses. As I already pointed out (§ 3), soap bubbles offer, with regard to the spherical shape, a first example of this realization in a film state.

Only the identity between the film shapes and those of our filled oil masses is not mathematically exact, because, while the films are very light, they are not absolutely weightless; but the effect of gravity on the forms which they take, is, in general, completely insensible to the eye.

§ 96. Here a remark is necessary relative to the sign of the constant C and the interpretation of this sign. According to the way in which I have just arrived at the general equation of equilibrium of film shapes, it is clear that, in this equation, the quantity $\frac{1}{R} + \frac{1}{R'}$ can indifferently, as for its absolute value, be referred to one or the other of the two surfaces of the film. If we agree to refer it to the surface which is on the outside of the shape, then, when this same quantity, or, which amounts to the same, the constant C, is positive, the pressure corresponding to surface in question will be higher than P, i.e. than that of a plane surface, and the pressure corresponding to other surface will be less than that of a plane, and, consequently, less than the first; consequently the resultant, which necessarily acts in the direction of largest of the two forces, will be directed, like this one, towards the interior of the shape. With the same convention, when C is negative, the larger of the two pressures will belong to the surface which us on the interior of the shape, from where it follows that the resultant will be directed towards the outside.

Thus when C is positive, the film shape will exert a pressure on the gas mass which it imprisons, and when C is negative, the film shape will exert a pressure on the surrounding air; but these actions will be counterbalanced, in the first case, by the excess pressure of the interior air, and, in the second, by the relative excess of the pressure of the surrounding air; finally, when C is zero, the film shape will exert action neither in one direction nor in the other.

§ 97. Thus, when the film shape is closed, the equilibrium condition has all its generality, C can be positive, negative or zero; but if the shape is not closed, equilibrium can obviously only happen for $C = 0$. From there this remarkable consequence follows, of which we will see further a great number of applications: that when a thin liquid film has its two faces in contact with the open atmosphere, it represents necessarily a surface with zero mean curvature.

§ 98. The films which one obtains with a simple soap solution have only a very short existence, unless they are enclosed in a vessel: a soap bubble one decimetre in diameter formed in the open air of a room, seldom lasts two minutes; generally it bursts after one minute, or even after one half-minute. It was thus significant, for the creation of equilibrium shapes, to seek some better liquid; however, on the basis of an idea that suggested to me by Mr. Donny, I was happy enough to arrive, after several attempts, at the composition of a liquid which provides films of a remarkable persistence: one forms it by mixing, in suitable proportions, a soap solution and glycerin; I name it *glyceric liquid*. I will give, hereafter, the details of its preparation; but as the result varies more or less with the quality of the substances employed, I will indicate first the means which I adopted to test it.

I took as the criterion of comparison the persistence of a bubble of diameter of approximately one decimetre deposited, in open room air, on a wire ring 4 centimetres in diameter. This ring is supported by three small feet, like of those of § 40; it had been permanently slightly oxidized by weakened nitric acid (§ 110). When one wants to perform the experiment, one immerses it in the liquid so that it is well wet by it, one withdraws it, and one sets it on its feet on a table opposite the door; it is then occupied by a plane film that one leaves there; then, by means of a common clay pipe, which one has beforehand soaked its opening in the liquid for a few minutes, one blows the bubble, and one deposits it on this film, with the totality of which its lower part is linked at once; finally the pipe is removed, and the bubble remains on the ring. That done, one leaves the room, closes the door with care, and observes the bubble from time to time through the keyhole.

Here is why I assign the bubble a given diameter: all things being equal, the films last, in general, much less when they are larger; it is thus necessary, when one wants to compare persistences of bubbles coming from various samples of glyceric liquid, to give to these bubbles the same diameter.

§ 99. This admitted, let us describe the preparation of the liquid concerned. Let us say initially that this preparation must be carried out in the summer, and at a time when the temperature of the room does not go down, at least during the day, below 20^o; at temperatures notably lower, one obtains only bad or poor results.

The most suitable soap is that of Marseilles, and the glycerin which always appeared the best to me is that which is manufactured in England, and which is known under the name of glycerin of Price; one gets it by the intermediary of a pharmacist. I will thus suppose, in what follows, that one makes use of these substances, and that one chooses a sufficient temperature. It is not impossible to succeed with other soaps and other glycerins, but then the proportions must change, and I cannot say more in general.

The process of preparation varies according to the goal which one proposes. Initially, if one holds more with simplicity of handling than with the excellence of the result, one operates in the following way:

One takes household soap bought recently, so that it preserves all its moisture; one cuts it in very small fragments, and one dissolves, at a moderate heat, a part in weight of soap in forty parts of distilled water. When the solution cools to about room temperature, one filters it through a paper which is not too permeable, in order to get it limpid; then one pours in a bottle three volumes of this solution and two volumes of glycerin of Price; one strongly agitates it long enough so that the mixing is quite thorough, after which one leaves it at rest until the following day. Then, according to the quality of the household soap, it can happen that the mixture has remained appreciably limpid, or it can be quite cloudy.

In the first case, one will be able to immediately use the liquid for experiments: the maximum persistence of the test bubbles will be one hour and a half approximately; but the liquid will lose its properties day by day, and, after a fortnight, persistence will be reduced to ten minutes.

In the second case, the precipitate which degrades the transparency of the liquid remains initially in suspension throughout the mass, but floats up then with an extreme slowness, and, after a few days, forms a definitely separate layer in the upper part of the liquid; one then collects the limpid portion by means of a siphon which starts by a side tube, and the preparation is finished. I must point out that when one introduces into the liquid the short branch of the siphon, a portion of the precipitate forms around the external surface of the tube a kind of reversed cone; it is thus necessary, before starting the siphoning, to remove this coating. For that, one initially leaves the whole at rest for a quarter hour, then one agitates a little from side to side the immersed branch of the siphon; the cone of precipitate is detached as small blobs which float up little by little and will join the upper layer. The liquid collected under these conditions is much better than the preceding; one can employ it in experiments at once after the operation of the siphon; the test bubbles which it provides have a maximum three hours of persistence; finally it is preserved sufficiently well for about a year.

Such are the facts that I observed by making use of the simple processes above; but it is probable that one would meet household soap samples giving intermediate results.

§ 100. In the second place, if one agrees to resort to a more complicated operation, but whose result is quite better, here is what it is necessary to do. After having prepared the soap solution as previously, one thoroughly mixes 15 volumes of it with 11 volumes of glycerin of Price, or, which amounts to the same thing, 3 volumes with 2.2 volumes of glycerin, then one leaves the mixture to itself for seven days. In this interval, the liquid can, according to the quality of the soap, get cloudy or remain limpid, but one does not worry any. The morning of the eighth day, one plunges the bottle in water which is cooled by agitating pieces of ice there so as to lower its temperature to 3° approximately, and one maintains this same temperature for six hours by suitable additions of ice. If the mixture of soap, glycerin and water is in considerable quantity, it should be split between several bottles, so that its temperature goes down sooner to that of the bath. During this prolonged action of cold, the liquid is cloudy. The six hours having passed, one filters it through a paper sufficiently permeable[37], and, if it is gathered into one large mass, one distributes it in several filters placed on separated bottles, and functioning simultaneously. But it is necessary to prevent the liquid contained in the filters from being heated, lest the precipitate which the cold made could be partly redissolved; for that, before pouring into the filters, one sets with care in each one of them a small elongated bottle filled with pieces of ice and provided with a glass stopper to give it more weight; this bottle must be tilted so as to rest by its side part against the filter; finally one surrounds by pieces of ice the base of each bottle which carries a funnel, then, withdrawing the liquid from the bath, cold, one fills the filters immediately. The first portions of liquid which pass are turbid; one filters them again, and it is enough to repeat this last operation two or three times so that the liquid collected then is absolutely limpid.

I do not need to add that if filtration lasts long enough, it is necessary to renew the ice of the small bottles; as for that which one laid out around the base of the bottles, and which is intended to prevent the reheating of the portions which pass initially while involving precipitate, one understands that it is not necessary any more as soon as the liquid is limpid.

Having finished filtration, one lets the liquid sit for ten days; then the preparation is complete.

With a liquid thus prepared, the test bubbles can, under the best conditions, persist 18 hours, i.e. six times as long as with the second liquid of the preceding paragraph.

§ 101. The substances which enter the liquid are products of industry, and are, moreover, of an organic nature; however, similar products almost always vary either with the times, or with the factories from where they come; also I obtained only rarely the extraordinary result mentioned above. Moreover, to make physicists capable of appreciating the degree of confidence which the process I have detailed deserves, I have joined together in a table all the results that it provided me; only it will be seen that, for some of the liquids tested, the proportions were slightly different.

These tests were carried out during four successive summers, and consequently with various glycerin samples of Price and household soap; the quantity of each prepared liquid was 100 to 200 grams approximately. The third column of the table gives the number of volumes of glycerin for three volumes of soap solution, and the fourth the proportion of soap compared to water; the fifth contains persistences in whole numbers of hours; the fraction there was not added, because one was going in general to observe the bubble only hour by hour. When persistence was of less than one hour, to shorten, it is there indicated by the character $\bar{1}$; finally some bubbles had to be aban-

[37] I use the filter papers of Prat-Dumas.

doned in the evening, because of the advanced hour, and burst during the night, so that one is unaware of the total number of hours of their persistences; this circumstance is indicated by a + sign placed above the number of hours observed; thus $\overset{+}{7}$ signifies the bubble was observed for seven hours, but persisted beyond for an unknown time.

Before each, the liquid was made as homogeneous as possible by strongly agitating it, then letting it rest for ten minutes.

YEARS.	Nº OF ORDER OF LIQUIDS	VOLUMES OF GLYCERIN	PROPORTION OF SOAP	PERSISTENCES IN HOURS.
1862	1	2,0	1/40	4, 9, 4.
	2	2,0	1/40	5,9,11.
	3	2,0	1/35	7, $\overline{1}$, 8.
	4	2,2	1/35	10, 10
	5	2,2	1/40	17, 16, 12, 11, 18
1863	6	2,2	1/40	10, 12 4, $\overset{+}{7}$, 6.
	7	2,2	1/40	10, 6.
	8	2,2	1/40	$\overline{1}$, 4, 6, 9, 7.
1864	9	2,2	1/40	2, 3, 4, 4, 5, 3, 4, 4.
	10	2,2	1/40	2, 4, 5, 3, 5.
	11	2,0	1/40	$\overline{1}$, 4, 8, 4.
	12	2,2	1/40	2, 6, $\overline{1}$, 7, 7.
	13	2,0	1/40	3, 3, 7.
	14	2,2	1/40	4, 6, 9, 1, 2.
1865	15	2,2	1/40	5, 5, 2.
	16	2,2	1/40	4, 7, 5, 5, 5, 4, 5.
	17	2,2	1/40	10, 7, 1, 8, 6, 6, 7.
	18	2,2	1/40	9,6,3,5,3,$\overset{+}{6}$,9,6,7,$\overline{1}$,4,5.
	19	2,2	1/40	$\overset{+}{10}$, 4, 3, $\overset{+}{5}$, 9, 7, 8.

One sees that the liquids of 1862, of 1863 and 1865 were in general very good, but that those of 1864 were lower, without being bad; I must add that liquids 11, 12 and 13, especially the last two, had the characteristic that bubbles were formed with difficulty: several burst either before even being deposited on the ring, or immediately afterwards.

I prepared, in 1864, with another glycerin sample, two more liquids which are not in the table; they were definitely bad, but I have certain reasons to suspect an error in the weighing of the soap, of which the quantity would have been too strong; it is for this reason that I did not register them; however the error concerned is not proven to me, and that makes me admit the possibility of a very exceptional failure.

Finally, what committed me to regard as the most effective proportions those that I prescribed in § preceding, is that, among the liquids of 1862, the most excellent, that which gave me persistences of 17 and 18 hours, i.e. the fifth, was prepared in these same proportions.

§ 102. The glyceric liquid obtained by the process of § 100 starts to be slightly cloudy after approximately a month, then the cloudiness increases imperceptibly, and, at the same time, the persistence of the films which the liquid provides decreases little by little. The precipitate which is formed thus does not float up, or floats up only with excessive slowness, and if one tries to clarify the liquid by filtration, the precipitate passes with it through the filter. This same precipitate, which results from a gradual deterioration of the liquid without the intervention of cold, is probably of another nature that that which appears during cooling to 3°.

The various samples which I prepared having been successively used by my experiments or having been joined together in a single mass of which the parts thus had very unequal ages, I cannot say with certainty how long a liquid obtained by the process in question serves sufficiently for the creation of film shapes. I could ensure however that this time would go far beyond a year: I had preserved by chance a small quantity of a rather good liquid, though prepared in circumstances which were not the most favorable; I took it again, and again tested it approximately two years and half later; it still, without too much difficulty, inflated in bubbles of one decimetre; some of those burst immediately, but a last remained on the ring, and persisted hours. One can thus believe that a liquid made under normal conditions would be maintained better still.

§ 103. Household soap consists, one knows, of a mixture of oleate, stearate and soda margarate, and I assured myself, by some experiments, that it is the first of these salts that must give the soap solution the property of blowing large bubbles. I was led, moreover, to view as probable that the precipitate made by the cold in the preparation of the glyceric liquid (§ 100), and of which separation using the filter improves this liquid so considerably, is made of stearate and soda margarate. I concluded therefrom that by substituting pure oleate of soda for household soap, one could make, by a process much simpler, a liquid better even than the best liquids prepared with the soap; and experiment confirms it fully: it was enough for me, indeed, to dissolve simply, at a moderate heat, soda oleate in distilled water, then to mingle glycerin with this solution.

As of the following day or two days later, the liquids were used in experiments, and they gave me bubbles (always of a decimetre and in open air), whose maximum persistence exceeded 24 hours. As far as I could see, the glyceric liquid with the soda oleate is not clouded by the cold; moreover, I have strong reasons to believe that it is preserved much longer than the liquid with the soap; however it also clouded after a more or less long time.

The liquid with the soda oleate is thus the true glyceric liquid, it acts much like the liquid with the soap, and its preparation is easier. Unfortunately, pure soda oleate is not in the trade, and it is necessary, to get some, to resort to a chemist.

I owe the samples of which I made use to the kindness of Mr. Rottier, preparator of chemistry at the University of Ghent; he had obtained them by the process described in the *Précis of organic chemistry* of Mr. Gerhardt. Let us insist, with regard to this preparation, on a significant point: if it is wanted that oleate is fit for making good glyceric liquid, is needed that the chemist employs, to precipitate it and to have thus isolated it, sea salt perfectly purified either by successive crystallizations, or by another means; with commercial salt, collected oleate gives, dissolved in water, a turbid liquid which one cannot clarify sufficiently.

§ 104. I employed the soda oleate in two states: 1° wet and having the consistency of household soap, 2° completely dry. I had at my disposal two different samples of wet oleate; both as well as dry oleate gave perfectly limpid solutions; those of wet oleates were cloudy, in truth, after a few hours, but they clarified themselves in two or three days by depositing precipitate, so that one had only to elutriate them.

The proportions of oleate which appeared to me most effective are, for wet oleate, a dissolved part in weight in fifty parts of distilled water, and, for dry oleate, a part in weight in sixty parts of distilled water; as for glycerin, I obtained excellent results by mingling it with the solution of the second wet oleate, and with that of dry oleate, in the same proportion as for the liquids with the soap, i.e. 2,2 volumes of glycerin for 3 of oleate solution; but, with the first wet oleate, I had to reinforce the quantity of glycerin a little. As one will see by the table below, this same oleate also gave me very long persistences with rather different proportions, being the solution made with a larger quantity still of glycerin.

I must present a remark with regard to dry oleate: that which was useful for the liquids above had undergone its drying in bulk. Mr. Rottier dried another portion in a coarse powder state, and this was shown much worse; it appears, accordingly, that the desiccation deteriorates oleate on its surface; this deterioration has little influence when the substance is in large pieces, because then the surface is small relative to the volume, but it produces a considerable deterioration when this same substance is in a state of great division.

Here now the table of the results; it is laid out like that of § 101, and contains only the results corresponding to good proportions; all were obtained in the summer of 1863.

STATE OF the OLEATE	N° Of ORDER OF LIQUIDS	VOLUMES DE GLYCERIN	PROPORTION OF SOAP	PERSISTENCES IN HOURS.
Wet	1	2,4	1/50	14, 23, 5, 8.
same	2	2,7	1/40	$\overset{+}{13}$, 22, 12, 9, 7, 10, 6, 12, 7.
same	3	2,6	1/50	3,7,12,5,23,20,$\overset{+}{12}$.
same	4	2,4	1/50	$\overset{+}{12}$, $\overset{+}{13}$.
Dry	5	2,2	1/60	19, 23, 10
same	6	2,2	1/60	23, 17, 10, 13
Moist	7	2,2	1/50	24, 3, $\overset{+}{10}$, 4, 17, 4.

Although these liquids gave me, one sees, very long persistences, I am convinced that it is possible to go still much further. Indeed, one can notice that persistences of the same liquid are in general extremely unequal, which leads me to believe that my tests relating to the proportions were not rather numerous, and that, for several of the liquids, those indicated in the table are not the best.

§ 105. I believe it my duty to insist on the great importance of the proportions, so much for the liquid with the soap as for the liquid with the soda oleate; the experiments which led me to those that I indicated, give me the certainty that if one randomly made the oleate or soap solution and its mixture with glycerin, one could have a liquid not very much better than or even worse than simple soap water. For example, I prepared, at the end of the summer of 1865, under good conditions of temperature, a liquid with soap where the solution was, as for the others, at a proportion of $\frac{1}{40}$, but where there were only 1.8 volume of glycerin for 3 volumes of this solution; however, of seven bubbles of this liquid, only one reached one hour duration, and, for several of the others, persistence was not even half an hour.

§ 106. It is known that soap bubbles persist much longer in closed vessels than in open air; and it is the same for the bubbles of glyceric liquid; only, so that the experiment has complete success with regard to these last, it is necessary that the vessel in which one encloses the bubble has considerable dimensions relative to the volume of the bubble.

I used as a vessel a cubic cage made out of glass 30 centimetres on a side; the liquid was formed of the mixture of Nos 17 and 18 of the table of § 101; it gave in open air, persistences of 5, 3, 4, 5, 9, $\bar{1}$, 5 and 6 hours, and, in the glass cage, 33, 15, 27 and 21 hours.

One much increases persistence by placing in the closed vessel a substance which absorbs the moisture of the air: after having obtained the results above, I started again the experiments by depositing beforehand calcium chloride fragments on the bottom of the cage; but it was necessary to avoid the great quantity of aqueous vapor in the interior of the bubble when blown means of the mouth; for that, I attached the stem of a pipe to a hollow rubber ball, then, after having compressed it to expel the air, I introduced the head of the pipe into a bottle containing a certain quantity of calcium chloride, and let the ball be reinflated by its elasticity, thus aspiring the dry air of this bottle; that done, I used this instrument to form a bubble; this one persisted 7 hours.

I then subjected to the same experiments a liquid prepared with soda oleate, but with an oleate which was not very good[38]. It gave, in open air, persistences of 10, 10, 12, $\overset{+}{10}$ and $\overset{+}{8}$ hours, and could consequently be compared to the best liquids prepared with soap. In the glass cage, without calcium chloride, a first bubble lasted 10, another 24, and a last 41 hours; with the use of calcium chloride, the persistence was 54 hours.

I must say here that the glass cage had its opening turned to the bottom, and was poised simply on a shelf, so that, to blow the bubble, the cage had initially to be removed, and that thus, during the operation, the bubble was exposed to the open air of the room; moreover, the quantity of calcium chloride was insufficient to cover the bottom of the cage; finally the edge of the cage joined only imperfectly to the shelf in certain places. It follows that the air in which the bubble remained immersed was not well desiccated; but, while trying to produce a more complete drying, I obtained only less persistences; likewise, when, to inflate the bubble with perfectly dry air, I interposed a glass tube full of calcium chloride fragments between the stem of the pipe and the rubber the ball.

It thus appears that, to reach the greatest persistences, one should desiccate the atmosphere of the closed vessel and the air which inflates the bubble only up to a certain point. We will further see (§ 303) that the need for a large-sized vessel and for imperfect drying of the air can be explained completely.

§ 107. I said (§ 99) that with other soaps and glycerins of other sources, it was not impossible to get a liquid good, but that then the proportions must be modified; here are examples:

Initially, by employing household soap and a French glycerin which had been sold to me in a bottle bearing the names *Lamoureux and Gendrot*, the liquid, prepared in the proportions which the glycerin of Price requires, gave only persistences of one quarter of an hour; but I tested different proportions then, and I succeeded by dissolving one part of soap in 30 parts of distilled water, with an equal volume of glycerin; persistences were 2, 4, $\overset{+}{6}$, 9 and 8 hours.

In the second place, I also succeeded with the soap of Windsor and glycerin of Price; but I must say that this soap, like the majority of other toilet soaps, presents a serious disadvantage: when one dissolves it in distilled water, the solution forms, in the cold, a gelatinous mass. I however came, after many trials, to use it for the preparation; but the process is too complicated to indicate it here, and I mention the use of this soap only to show the possibility of obtaining a good liquid with soaps other than that of Marseilles.

Here, moreover, is a preparation recommended as easy by Mr. Böttger[39]. I translate the essential part of the article:

"One introduces, in a large bottle containing cold distilled water, the palm oil soap cut in very small fragments; one strongly shakes, and one thus gets a solution as saturated as possible; one filters it through sufficiently permeable paper, and one mingles with it approximately a third of its volume of chemically pure and concentrated glycerin. . . .

Bubbles of a foot diameter and more, suitably protected from agitations of air and shocks, often persist 5 to 10 minutes, but bubbles from 1 to 3 inches (26mm to 78mm)

[38] For this reason it is not registered in the table of § 104.

[39] *Leichte Anfertigung einer Flüssigheit zur Erzeugung der Plateau'schen Gleichgewichtsfiguren ohne Schwere* (Jahres-Bericht des physikalischen Vereins zu Frankfurt am Main, 1868-1869, p. 10).

last whole hours and even frequently 10 to 20 hours."

I am unaware if it is easier to get palm oil soap than of pure soda oleate; but, undoubtedly, it must be extremely difficult to get chemically pure glycerin. It is seen, moreover, that the liquid of Mr. Böttiger is inferior to the liquid with soda oleate (§ 104).

I will add here that I unnecessarily tried to prepare a suitable liquid by employing German glycerins of the trade; perhaps I did not vary the proportions sufficiently. I thus insist still that people who want to prepare my liquid use the glycerin of Price.

§ 108. The bubbles of glyceric liquid always express, when they persist long enough, two quite remarkable phenomena:

Initially, the film which constitutes a bubble takes, after one hour or two, an appreciably equal thickness in all its extent, except, of course, the small lower portion intercepted by the metal ring. One recognizes this by the distribution of colors: indeed, when one observes one of these bubbles by holding the eye at the height of the center, one sees, in the middle of its surface, a broad circular space of a uniform color, and, around this, one or two concentric rings of different colors. The color of the central space is obviously that of the film under normal and nearly normal incidence; as for the colors of the rings, they result from the stronger obliqueness of the lines of vision. I must add that often the top of the external Newton's ring offers, to a certain extent, a shade a little different from the remainder, and which indicates, at the top of the bubble, a thinness that is the smallest of the film.

In the second place, the colors in question go up initially slowly towards the first orders, then go down again, slowly also, until the red and the green of the last, and even sometimes almost to the white; from which follows this singular consequence that the thickness of the film, after having decreased up to a certain point, is then increasing.

§ 109. The uniform thickness to which the film arrives can, it seems to me, be explained in the following way: when our bubble is deposited on the solid ring, the gradual thinning of the film must go with an increasing slowness, because, all things being equal, the thinner the film, the less the mass of the portions of liquid which go down without delay towards the metal ring, and consequently the more their movement must be blocked by the resistance which viscosity opposes to it; however, by the same the reason obviously, if the film has an unequal thickness, the descent of the liquid will be less slow in the thicker portions, from where it follows that the film will tend towards a uniform thinness.

It is needless to recall that I always disregard the small cap intercepted by the solid ring, a cap which this ring makes independent of the remainder.

In truth, in the first moments of the existence of a bubble, the thickness is larger closer to the solid ring, since, as one approaches it, the slope of the film is increasingly strong, and gravity acts by an increasingly weak component; but it suffices to admit that the influence of the increase in mass overrides that of the diminution of the component in question.

To make the explanation complete, it is necessary still to give the reason for the circumstance mentioned above, that the thickness is initially larger at the bottom of the bubble; however it is understood that, as long as the film has everywhere a rather notable thickness, the influences of differences in mass are less felt, and thus the liquid which goes down towards the lower half of the bubble can, for some time, remain there more or less accumulated. It is supported by the following experiment: one produces a plane film in a wire ring 7 centimetres in diameter, for example, which is done by plunging this ring horizontally in the liquid and withdrawing it in the same way. If the ring is kept then in a horizontal position, the plane film in question is colorless, at least for a certain time; but if, as soon as it is formed, the ring is inclined and thus kept, it is divided soon in many colored horizontal bands, whose colors indicate an increase of thickness from the highest part to the lowest part. These films never have a very long persistence, but they would probably arrive little by little at a plain color form. One will see, moreover, in suitable circumstances, a tilted plane film ends up becoming entirely black.

As for the retrogradation of the colors, it comes from the glyceric liquid absorbing the moisture of the ambient air; indeed, when a bubble is formed in a closed vessel, if one makes it so, by wetting with water the interior walls of this vessel, that the bubble is surrounded by a very wet atmosphere, the colors do not leave the red and the green of the last orders; and if, on the contrary, one deposited at the bottom of the vessel an absorbing substance, the colors go up without retrogressing. It is that, in a very wet atmosphere, the absorption of water by the film continuously repairs the reduction in thickness due to the descent of the liquid, while, in a desiccated atmosphere, this repair cannot take place. Lastly, for open air, it should be admitted that the effect of the descent of the liquid initially overrides that of absorption, and that then it is the reverse which happens. I will return to this phenomenon, and I will explain it then more clearly.

§ 110. Now that we are in possession of a liquid which makes very durable films, let these films be used for the creation of the equilibrium shapes of revolution. The apparatuses necessary are as follows: 1° a system of wire rings 7 centimetres in diameter, similar to those of § 40; only the feet of the lower ring are fixed on a solid plate, so that the unit has a certain weight. 2° a second similar system, but whose rings are only 3 centimetres in diameter. 3° the system of discs of 7 centimetres of § 63. 4° a shelf with screw fixing. 5° a support consisting of a vertical stem along which slips, with soft friction, a horizontal arm, and at the end of it one can fix either one of the upper rings by the end of the tail of its fork, or the upper disc by the end of the wire which supports it. I took, for this support, a cathetometer, the ring or the disc attached, using an intermediate piece, to the end of the lens; one thus had, in addition to the other conditions, the ability to read, on the graduation of the instrument, the distance the ring or disc is raised or lowered.

When the rings and the discs are new, the glyceric liquid adheres to them badly, and the film shapes burst while one tries to form them, or almost immediately after their formation; but one removes this difficulty in the following way: one immerses the apparatuses in question in nitric acid diluted with about four times its volume of water, one keeps them there until their surfaces are notably oxidized, which requires only about two minutes, then one carefully washes them in pure water, one wipes them with filter paper, and one lets them dry; they are then clean enough to be useful indefinitely, and always give quite persistent shapes.

Here now is how the experiments are prepared. One initially makes the cathetometer quite vertical, and one attaches the upper ring or disc to it; if this ring or disc does not appear completely horizontal, one corrects its position by slightly curving with a pliers the wire which supports it. One then places the lower part on the shelf, the lower ring or disc, in such a manner that it is about vertically under the other, then, by means of the adjustment screws and by small displacements of the lower part, one easily manages to make it so that when lowering the upper, the two rings or the two discs overlap exactly. Then, after having mounted the upper, one carefully wets each of them with glyceric liquid. For the lower ring a well soaked brush is useful for this purpose, and, for the upper, one lifts up to it a capsule containing the same liquid, in which one immerses it. After one withdraws the capsule, the ring is occupied by a plane film, but it is burst. As for the discs, one spreads the liquid with the brush on the totality of the two facing sides, then one brings in contact with the wet face of the upper disc the liquid contained in the capsule, finally one removes it.

§ 111. Now let us suppose that it is a question of making the catenoid. The system of rings of 7 centimetres is taken, and, after having laid out the things as I indicated, one lowers the upper ring until it is separated from the other by not more than a fraction of a millimetre; then strokes all along both rings several times the brush well soaked with glyceric liquid, in order to fill the small space left between them. The upper ring is then raised, and one sees a film catenoid extending from the one to the other. I will point out here that between two equal rings whose spacing is less than the limiting spacing, there are (§§ 58 and 80) two possible catenoids unequally necked, and that when one creates, with oil within the alcoholic liquid, a filled catenoid, it is always (§ 60) the least necked which occurs, from which I concluded that the more necked is unstable; however, as one was to expect, the film catenoid of our experiment always also necked the least.

Continuously gradually raising the ring, one soon reaches a point where equilibrium ceases: one sees the catenoid being narrowed quickly in its middle, and being converted into two plane films occupying the two rings respectively. The reading with the cathetometer gives then, for the interval between the two rings, approximately 46 millimetres, i.e. very close to two thirds of the diameter of the rings, or the height of the limiting catenoid (§ 80).

Being an open shape, and being consequently made of a film in contact on its two faces with the open air, it must necessarily (§ 97) represent a surface with zero mean curvature, or, in other terms, one of those which the geometers called minimal surfaces; however, according to the demonstration of Goldschmidt (§ 80), beyond the limiting spacing, there is no longer any surface of revolution of minimum area supported on the two rings, except the whole of two planes occupying the latter respectively. Let us notice, while passing, that the films which, after the transformation, fill the two rings, offer an example of a film realization of the plane.

I already drew attention (§ 60) to the fact that a limit catenoid, formed with oil in the alcoholic liquid, far from deteriorating spontaneously, is, on the contrary, very stable; I will show, in its place (§ 387), to what this difference between the filled shape and the film shape is due.

§ 112. To carry out the cylinder with the same rings of 7 centimetres, one proceeds as follows: after having mounted the upper ring at a sufficient height, one blows a bubble of approximately 10 centimetres diameter, and one deposits it on the lower ring, to which it adheres immediately; then one lowers the upper ring until it comes to touch the bubble, which also sticks to all its contour; it is needless to recall that the two rings must be wetted beforehand with glyceric liquid (§ 110); finally one gradually raises the upper ring, and the bubble which, thus vertically stretched, loses its side meridian curvature more and more, converts, for a certain spacing of the rings, to a perfectly regular cylinder, presenting convex bases like the filled oil cylinders.

One can give to the bubble a little larger diameter; but when it is too considerable, one does not arrive any more at the cylindrical form, either because the cylinder which one would like to obtain exceeds its limit of stability, or because, if it is still inside this limit, it starts to approach it: in this last case, indeed, the stabilizing forces becoming very weak, the slight weight of the film exerts an appreciable effect, and the shape becomes more or less inflated in its lower half and narrowed in its higher half. The highest cylinder that one can create in a regular way with the rings indicated, has a 17 centimetres height approximately, and one sees that it is inside limit of stability, since this corresponds to a height a little larger than triple the diameter (§ 46).

§ 113. Does one want to obtain a partial unduloid necked in its middle (§ 55)? One deposits on the lower ring a bubble only approximately 9 centimetres in diameter, one touches it, as previously, with the upper ring, then in the same way this last is raised, but one goes beyond the point where the shape becomes cylindrical; it then appears narrowed in its middle, all the more deeply the more one raises the ring, and thus constitutes the sought unduloid. This arises, like the cylinder, with perfectly regularity, and its bases are also convex segments of a sphere.

By further raising the upper ring, one reaches a point where equilibrium cannot exist any more, and then the shape is narrowed quickly in its middle, where it is divided to be transformed into two spherical bubbles respectively attached to the two rings.

If it is a partial unduloid bulging in its middle (§§ 47 , 52 and 54) that one proposes to make, one makes use of the system of rings of 3 centimetres. One forms a bubble approximately 8 centimetres in diameter, and, after having deposited it on the lower ring, then touches it with the upper ring, and one raises this last; the bubble passes thus by degrees to a shape made up of a bulge between two portions of neck and having still for bases of the convex segments of a sphere; it is consequently the unduloid in question.

In this experiment, it is necessary to stop with a degree of spacing of the rings for which the tangents at the extreme points of the meridian curve are still notably tilted on the axis, and, with this condition, the shape appears regular like the preceding ones. If one goes until approaching the point where these tangents would be vertical, the shape borders its limit of stability (§ 52), and, as happened with the cylinder, the reduction in the stabilizing forces leaves the weight of the film a significant action; the bulge then appears a little below the middle height of the shape.

§ 114. Finally the creation of the nodoid requires the use of the system of discs. One starts by inflating a bubble from 3 to 4 centimetres in diameter, one brings it in contact with the wet face of the lower disc, to which it adheres at once while being spread out more or less, and one continues to inflate it until it belongs to a sphere approximately 10 centimetres in diameter, then one removes the pipe; the film attaches then to the edge of the disc. One lowers then the upper disc until it comes to touch the top of the bubble; this one opens immediately in this place, and the film also gaining the edge of the last disc, forms, from one edge to another, a portion of bulge of unduloid. Things being in this state, one continues to lower the upper disc, and, when one passes the point where the shape would constitute a spherical zone, one has the desired partial nodoid (§ 71). If the disc is lowered further, one reaches, absolutely as with the filled oil nodoid, a point with beyond which the shape ceases being of revolution, and bulges laterally more as the disc drops more.

§ 115. All these experiments are extremely curious; there is a particular charm to contemplate these thin shapes, almost reduced to mathematical surfaces, which are adorned with brilliant colors, and which, in spite of their extreme brittleness, persist for so a long time.

These same experiments are carried out promptly and in the most convenient way. There are not here any more the problems which, in the experiments with the filled oil masses, result from the equalization of the two densities, the variations in the temperature, and the small mutual action of the two liquids. There are only certain experiments which imperiously require the use of oil and alcoholic liquid: such is, for example, the creation of the shape generated by a whole node of the meridian line of the nodoid (§ 67).

When one finished a series of experiments with the glyceric liquid, one washes the rings or the discs by agitating them in rainwater, then, to dry them, one deposits the first On filter paper, and one wipes the second.

Let us indicate a useful precaution: when one continually carries out a large number of experiments, it is wise to moisten the upper ring again from time to time with glyceric liquid.

§ 116. Before passing to the equilibrium shapes which are not shapes of revolution, we will deal with two questions having to do with film spheres.

The external surface of such a sphere having all its radii of curvature equal and positive, the film presses (§ 96) on the air which it imprisons. However the pressure corresponding to a point of a film shape has, as we saw (§ 95), as an expression:

$$A\left(\frac{1}{R}+\frac{1}{R'}\right);$$

but, in the case of a spherical shape, one has $R = R' =$ the radius of the sphere; if thus we indicate by d the diameter of the bubble, the value of the pressure will become simply $\frac{4A}{d}$, while neglecting, of course, the small thickness of the film; from which it follows that the intensity of the pressure exerted by a spherical film bubble on the air which it imprisons, is inversely proportional to the diameter of the bubble.

The fact of this pressure has been known for a long time, and, in 1844, Mr. Henry, in a verbal communication made at the American Society[40] *On the cohesion of liquids*, had deduced, from considerations other than the preceding ones, the law which we have just stated.

Now, let us take again the general expression of the pressure corresponding to an arbitrary point of a liquid surface, getting:

$$P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right).$$

For a surface of convex spherical curvature, if one indicates by d the diameter of the sphere to which this surface belongs, the expression above becomes:

$$P+\frac{2A}{d},$$

and, for a spherical surface of concave curvature belonging to a sphere of the same diameter, one will have:

$$P-\frac{2A}{d}.$$

Thus, in the case of convex surface, the total pressure is the sum of two forces acting in the same direction, forces of which one indicated by P is the pressure which a plane surface would exert, and of which the other represented by $\frac{2A}{d}$ is the action which depends on the curvature. On the contrary, in the case of concave surface, the total pressure is the difference between two forces acting as opposed directions, and which are still one the action P of a plane surface, and the other $\frac{2A}{d}$ which depends on the curvature. One sees therefrom that the quantity which represents, as we showed above, the pressure exerted by a spherical film on the air which it imprisons, is equal to the double of the action which comes from the curvature of one or the other surface of the film.

Now, when a liquid rises in a capillary tube and its diameter is sufficiently small, one knows that the surface atop the raised column does not differ appreciably from a concave half-sphere, whose diameter is consequently equal to that of the tube. Let us recall, moreover, part of the reasoning by which one arrives, in the theory of capillary action, at the law which relates the height of the column raised to the diameter of the tube. Let us suppose a very thin channel from the low point of the hemispherical surface

[40] Philos. 1845, vol. XXVI, page 541.

in question, going down vertically until below the lower opening of the tube, bending then horizontally, and finally rising to lead vertically to a point of the plane surface of the liquid external with the tube; the pressures corresponding to the two openings of this small channel will be, on the one hand, P, and, other, $P-\frac{2A}{\delta}$, by indicating by δ the diameter of the concave half-sphere, or, which amounts to the same thing, that of the tube. However the two forces P offset each other, leaving only the force $-\frac{2A}{\delta}$, which having a contrary sign with that of P, acts consequently upwards at the lower point of the concave hemisphere, and it is that which supports the weight of the molecular net contained in the first branch of the small channel between this same point and a point located at the height of the external level.

That said, let us notice that the quantity $\frac{2A}{\delta}$ is the action which comes from the curvature of the concave surface. The double of this quantity, or $\frac{4A}{\delta}$ will thus express the pressure which would be exerted on the air contained a film sphere or hollow bubble of diameter δ and formed of the same liquid. It follows therefrom that this pressure constitutes a force able to support the liquid at a height double that to which it rises in the capillary tube, and to which, consequently, it would rise having equilibrium with the pressure of a column of the same liquid this double height. Let us suppose, to fix ideas, δ equals a millimetre, and indicate by h the height at which the liquid stops in a tube of this diameter; we will have this new result, that the pressure exerted on the air contained by a formed hollow bubble of a given liquid and having 1mm diameter, would make equilibrium with that which would exert a column of this liquid a height equal to $2h$.

But, the pressure exerted by a bubble being inversely proportional to its diameter, it follows that the liquid column which would make equilibrium with the pressure exerted by a bubble of an arbitrary diameter d, will have a height equal to $\frac{2h}{d}$.

It seems initially that this last expression should apply equally well to the liquid which drops in the capillary tube, h always in a tube of 1 mm diameter; but it is not completely thus, because that requires, as one easily sees from the nature of the reasoning which precedes, that the top surface the column depressed in the capillary tube was appreciably a convex hemisphere; however it is known that, in the case of mercury, this surface is curved according to the observations of Mr. Bède[41], its height is only approximately half of the radius of the tube; from which it follows that the evaluation of the pressure given by our formula would be too small with regard to a similar liquid. One will be able, moreover, to regard it as a first approximation.

§ 117. Let us take for the measure of the pressure exerted by a bubble the height of the water column with which it would be in equilibrium. Then, if ρ indicates the density of the liquid of which the bubble is formed, that of water being 1, heights of the water columns and the liquid in question which would make equilibrium with the same pressure will be between them in the inverse proportion of the densities, and consequently, if the height of second is $\frac{2h}{d}$, that of the first will be $\frac{2h\rho}{d}$. Thus by designating by p the pressure exerted by a film sphere on the air which it imprisons, we obtain ultimately:

$$p=\frac{2h\rho}{d},$$

ρ being, as we have seen, the density of the liquid which constitutes the film, h the height to which this liquid rises in a capillary tube of 1 mm diameter, and d the diameter of the bubble.

If, for example, the bubble is made of pure water, one has $\rho=1$, and, according to the measurements taken by the physicists, one has, with very little error, $h=30$mm;

[41] *Mémoires de l' Àcad. du Belgique*, volume XXV of the *Mémoires couronnés et des Mémoires des savants étrangers*.

the formula above will thus give , in this case, $p = \frac{60}{d}$. If one could form a pure water bubble of one decimetre, or 100mm, of diameter, the pressure which it would exert would be consequently equal to 0.6mm, or, in other words, would make equilibrium with the pressure of a water column of 0.6mm height; the pressure which a bubble of the same liquid of one centimetre would exert, or 10mm, of diameter, would make equilibrium with that of a water column of 6mm.

For mercury, there is $\rho = 13.59$, and, according to the report of Mr. Bède, about $h =$ 10mm; the formula would thus give, for a mercury bubble, $p = \frac{271.8}{d}$; but, according to the remark which ends the preceding paragraph, this value is too low, and can be looked at only as a first approximation. It teaches us that with equality of diameter, the pressure of a mercury bubble would exceed four times and half that of a pure water bubble.

For sulphuric ether, one has $\rho = 0.715$, and one concludes from the measurements taken by Frankenheim[42] that very nearly $h = 10.2$mm; from which follows $p = \frac{14{,}6}{d}$, and one sees that at equality of diameter, the pressure of a sulphuric ether bubble would be only a quarter of that of a pure water bubble.

§ 118. Already in 1830, an American scientist, Dr. Hough, had tried to arrive at the measurement of the pressure exerted either on a bubble of air contained in an indefinite liquid, or on the air contained in a soap bubble[43]. He has a rather right idea of the cause of these pressures, however, he does not distinguish one from the other, and, to evaluate them, he starts, as I did, with the consideration of the hollow surfaces atop a column of the same liquid raised in a capillary tube; but, although a clever observer, he did not know about the theory of capillary action; also he arrives by reasoning whose error is palpable, at necessarily false values and a law.

In the verbal communication of which I spoke (§ 116), Mr. Henry described experiments by means of which he sought to measure the pressure exerted on the interior air by a soap bubble of a given diameter. Here primarily, according to the report of this communication, is how Mr. Henry operated: a soap bubble was blown at the end of the one of the branches of a U-shaped glass tube partially filled with water; the difference in level in the two branches gave the measurement of the pressure then. Unfortunately the report, which leaves something to be desired besides in clearness, does not make known the numbers obtained, and I do not think that Mr. Henry published them later on.

In a work presented at the Société philomatique in 1856 and printed in 1859 in the *Reports*, Mr. De Tessan stated [44] that if the vapor which forms clouds and fogs was made up of tiny air bubbles, the air locked up in a bubble of diameter 0.02mm would be subjected there, on behalf of it, to pressures equivalent to $\frac{1}{7}$ of an atmosphere.

Mr. De Tessan does not say, in this work, how he obtained the evaluation above, but he informs me, by a letter with which he honoured me in 1869, that he took for the measure of the pressure in question the height to which water would be supported in a capillary tube of an internal diameter equal to that of the bubble; he had thus started with the same idea as I, only it is seen that he was mistaken in the sense that he had regard only to interior surface of the liquid film. Indeed, according to the formula of the preceding paragraph, the pressure exerted on the interior air by a water bubble of 0.02mm of diameter would be equivalent to that of a water column 3 meters height, which equalizes about the $\frac{2}{7}$ atmospheric pressure; Mr. De Tessan thus found only half of the actual value.

As one will see in § 156, Sir W Thomson gave, in 1858, according to another

[42] *Die Lehre von der Cohäsion* . Breslau, 1835.
[43] *Inquiries into the principles of liquid attraction.* (Journ. of Silliman, 1st series, vol. XVII, page 86).
[44] *Reports*, volume XLVIII, p. 1045.

principle concerning the capillary height and the density, an exact expression of the pressure on the air contained in a bubble.

Lastly, in 1866, Mr. Tait calculated[45], by means of the same principle, on the one hand the pressures to which would be respectively subjected, in water, bubbles of air of very small given diameters, and, on the other hand, the pressures inside water vapor bubbles of also given diameters.

§ 119. After having obtained the general expression of the pressure exerted by a film sphere on the air which it imprisons, it remained to me to subject my formula to the test of experiment. I employed, for that, the process of Mr. Henry, i.e. the pressure was measured directly by the height of a water column with which it was in equilibrium.

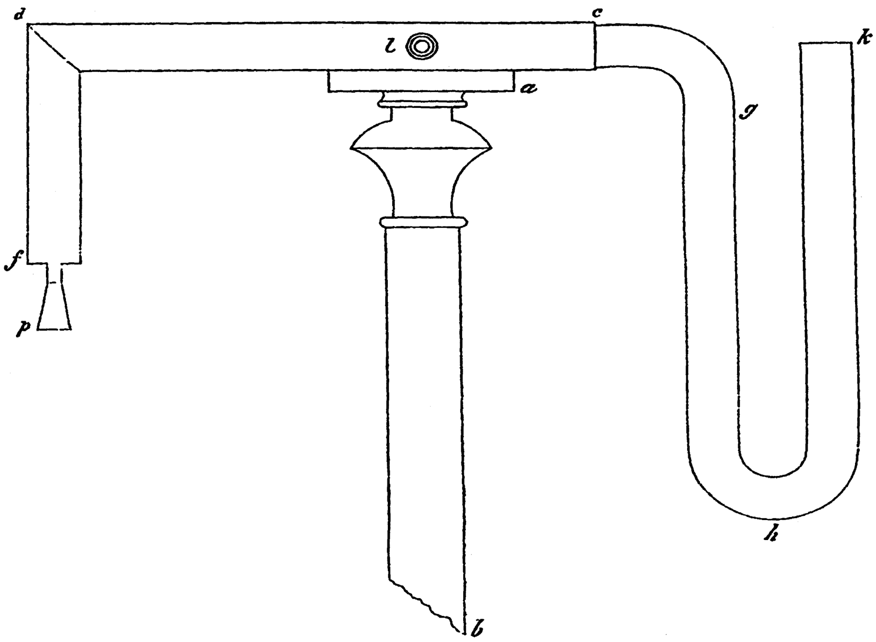


Fig. 52

From our formula, one deduces $pd = 2h\rho$; for the same liquid and at the same temperature, the product of the pressure by the diameter of the bubble thus should be constant, since h and ρ are constant. It is this constancy which I initially sought to check for bubbles of glyceric liquid of very different diameters.

The apparatus of which I made use is shown fig. 52, in vertical projection. *ab* is the upper part of a support of which the total height is 40 centimetres. On this support is fixed a copper tube cdf, at whose end c is glued a tube bent out of glass $cqhk$ intended to be used as a pressure gauge and whose internal diameter is one centimetre approximately; the length q is 20 centimetres. The tube of copper has, at l, a horizontal junction, which could not be drawn in the same figure because it is directed towards the viewer, but one sees that part in lm of fig. 53; at the end m is glued a glass tube mn which has an internal diameter of only approximately 2 millimeters. Finally, at the end f of the copper tube (fig. 52) is welded an iron attachment fp, widened in its lower part into a small funnel whose end is 5 millimetres in diameter; this funnel was slightly oxidized by weakened nitric acid (§ 110).

[45]*Note on the compression of air in an air-bubble under water.* (Proceedings of the Royal Soc. of Edinburgh, vol. V, 1865-66, p. 563).

To use this apparatus, one starts by introducing distilled water into the pressure gauge *ghk* in sufficient quantity to occupy a height of a few centimetres in the two branches; then one carries under the attachment fp a capsule containing the liquid intended to form the bubbles, one immerses there the end p of the small funnel, and one lowers the capsule; finally, applying the mouth to the opening n of the glass tube of of the junction (fig. 53), one blows with care. A bubble appears on the attachment; one gives it, with precautions which I will indicate soon, the diameter that one judges suitable, and, as soon as it has reached it, one carefully stoppers the opening with a small wax ball. Water is then a little higher in the branch *hk* of the pressure gauge (fig. 52) than in the branch *hg*, in consequence of the pressure which the bubble exerts, and it remains only to measure the difference in level and the diameter of the bubble. For the first of these measurements, a cathetometer is useful in the ordinary manner, and, for the second, one lays down the same instrument in a horizontal position, while placing it on suitable supports.

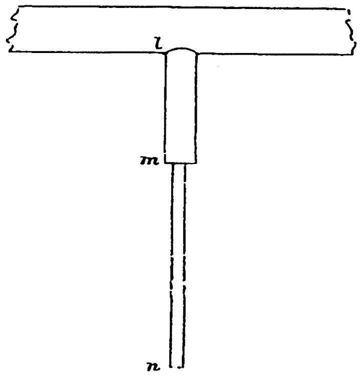


Fig. 53

§ 120. These experiments, extremely simple in theory, offer notable difficulties of execution. Initially, the air which one blows into the apparatus is hotter than ambient air, so that the bubble, after its formation, contracts a little by the gradual cooling of the air contained in its interior and the tubes of the instrument; it is thus necessary to wait some time before carrying out the measurement of the diameter.

In the second place, bubbles of large diameter exerting only a very low pressure, a small error in its measurement has a considerable influence on the product *pd*; it is necessary thus, if one wants that the results not to oscillate too much around the true value, to stop with a certain limit of diameter.

In third place, very small bubbles have also their disadvantages: to bring them to the wanted diameter, and to obviate, at the same time, the contraction by cooling, one inflates them initially much beyond the size which they must have, and one then lets them decrease spontaneously by the expulsion of part of the air which they contain; however, when this reduction arrives at a certain degree, it becomes very rapid, and one needs much care to apply the wax ball at precisely the suitable moment. Moreover, in my experiments, these small bubbles appeared to persist less longer than the larger; they frequently burst before one could complete measurements.

Lastly, although the pressure gauge of my instrument is one centimetre in interior diameter, equilibrium is established there only very slowly, and great errors would be made if one did not have regard to this circumstance.

Let us note that, when one has just formed a bubble, there is in general, at its bottom, a suspended drop, a drop whose weight lengthens a little the bubble in the vertical direction. To get rid of this small additional mass without bursting the film, one gently touches it with one of the corners of a piece of filter paper; the drop then is partially absorbed, and one repeats the same operation with other corners of the same paper, until the excess of liquid has entirely or appreciably disappeared.

Let us add a last remark. When one wants to form a bubble, if one entirely immersed the attachment funnel in the liquid, the liquid would go up, by capillary action, in the interior of the narrow tube which surmounts this funnel, and would be only partly expelled by the breath, so that after the swelling of the bubble, it could gather in a small mass in the lower part of the tube concerned, and to thus stop the communication between the bubble and the pressure gauge. To avoid this problem, one starts by wetting with glyceric liquid all the external surface of the small funnel, then one restricts oneself to immerse just its edge.

Here now is how I proceeded. For the larger diameters of the table of the following paragraph, I initially inflated the bubble up to approximately 6 centimetres, I applied the wax ball, then I waited five minutes, after which I opened the blowing tube again, I let the bubble shrink until it appeared to have the wanted diameter, and I then stoppered it by means of wax. For all the smaller diameters, I started by inflating the bubble up to about 4 centimetres, and, after having applied wax, I waited ten minutes before letting it shrink. Preliminary tests had shown that with these precautions, the diameter remained then invariable. The bubble on which I proposed to operate having thus reached the desired dimension, I removed the suspended drop from its lower part, I inclined the instrument to the right and to the left in order to wet well the two branches of the pressure gauge a little above the two levels, and I measured the diameter. I waited until ten minutes at least had been passed since the instrument was at rest, in order to give the equilibrium of the pressure gauge a certain time to be established, then the pressure was measured, and, five minutes after, it was again measured. If the results of these two measurements were not exactly the same, I took a third measurement afterwards after a new five minute interval, and so on, until I obtained two identical successive results or for which the difference was in contrary direction of the preceding ones; in the first case, the last two results were regarded as giving the value of the pressure; in the second, their difference having to be allotted to a small error of observation, one took, for value of the pressure, the average of these same two results.

§ 121. The following table contains the results of these experiments; I arranged them not in the order they were obtained, but in ascending order of the diameters, and I distributed them in similar groups of diameters. Throughout operation, the temperature varied between 18.5° and 20°.

DIAMETERS, OR VALUES OF *d*.	PRESSURES, OR VALUES OF *p*.	PRODUCTS, OR VALUES OF *pd*
7.55mm	3.00mm	22.65
10.37	2.17	22.50
10.55	2.13	22.47
23.35	0.98	22.88
26.44	0.83	21.94
27.58	0.83	22.89
46.60	0.48	22.37
47.47	0.48	22.78
47.85	0.43	20.57
48.10	0.55	26.45

The general average of the products is 22.75, and it is seen that, except for the last two, the variations from this general average are not very notable anywhere; it is seen, moreover, that they are irregularly distributed. As the ratio of the first diameter to those of the last group is about 1 to 6, these results are enough, I think, to establish clearly the constancy of the product *pd*, and, consequently, to check the law according to which the pressure is inversely proportional to the diameter. One will further see (§§ 175 and 179) another check of this same law

I must say here that, in measurements relating to the smaller bubble, that of 7.55mm of diameter, I was constrained to make a slight exception to the procedure indicated at the end of the preceding paragraph: the second measurement of the pressure exceeded the first by 0.02mm; I thus proposed to take a third measurement after a new five minute interval; but, during this time, the bubble burst. I tried several times to repeat the experiment, and always one or the other of the circumstances which I announced with regard to the very small bubbles prevented success. As the difference 0.02mm was so tiny that it could be attributed to an error of observation, and besides, in consequence of this smallness, it was extremely improbable that a new excess would have been shown in the third measurement, and finally, with a diameter of this kind, such small differences have influence only on the decimal part of the product, I believed able to regard the second measurement as giving the value of the pressure, and to preserve the result of the experiment.

As for the general average 22.75 of the results of the table, its decimal part is necessarily a little too high, because of the excessive value 26.45 of the last product. As this product and that which precedes it are the only ones which deviates notably from 22 in their whole part, one will admit, I think, that one will more approach the true value by neglecting these two products and taking the average of the others, an average which is 22.56; we will adopt this last number for value of the product pd with regard to the glyceric liquid.

§ 122. It remains to check if this value satisfies with our formula, according to which one has $pd = 2h\rho$, the quantities ρ and h being respectively, as one saw, the density of the liquid and the height that would be reach by this same liquid in a capillary tube a millimetre in diameter. For that, it was thus necessary to seek the values of these two quantities with regard to the glyceric liquid.

The density was given by means of the hydrometer of Fahrenheit, at a temperature of 17°, a temperature not very much lower than those of the preceding experiments, and I found thus

$$\rho = 1.1065.$$

To determine the capillary height, I employed the process of Gay-Lussac, i.e. measurement with the cathetometer, by taking all the precautions known to ensure the exactitude of the result. The experiment was made at a temperature of 19°. I had gotten a capillary tube whose internal diameter was only a fraction of millimetre; one will see soon why. I initially traced with a file a small feature on this tube, at three centimetres and half approximately from one of its ends, distances that I knew, by a preliminary test made on another fragment of the same tube, being a little higher than the height of the raised capillary tube; then I wet perfectly the tube inside by immersing it on several occasions to the bottom of the vessel containing the glyceric liquid, and shaking it each time that it had been withdrawn; finally, after having wiped it outside, I set up it, by inserting it in the liquid until the end of the raised column appeared to stop very near the mark, and I lowered the steel point to make it level with the external liquid. Then I brought the horizontal wire of the lenses of the cathetometer in contact with the image of the low point of the concave meniscus, and I observed every five minutes, by restoring each time the contact, until the point in question appeared stationary; I still waited, and took a measurement only after having noted for a whole half-hour the perfect immobility of the top of the column. The movements had been very small, so that the column still finished close to the mark. The reading with the cathetometer gave, for the distance from the point low of the concave meniscus at the external level, 27.35mm.

This measurement taken, I removed the tube, cut it at the mark, and carried out a measurement of the internal diameter at this point, by means of a microscope provided with a micrometer giving hundredths of millimetre directly. It was recognized that the interior section of the tube was slightly elliptic; the larger of its diameters was found to be 0.374mm and smallest 0.357mm; I took the average, being 0.3655mm, to represent the interior diameter of the tube, presumed cylindrical.

To have the true height of the capillary tube, it is necessary, as one knows, to add to the height of the low point of the meniscus a sixth of the diameter of the tube, or, in the current case, 0.06mm; the true height of our column was consequently 27mm, 41.

Now, to obtain the height h to which the same liquid would rise in a tube being exactly a millimetre in interior diameter, it is enough, under the terms of known laws, to multiply the height above by the diameter of the tube, and one finds thus, ultimately,

$$h = 10.018mm.$$

It is the place here to say for what reason I chose for the experiment a tube whose internal diameter was notably less than a millimetre. The reasoning by which I arrived (§ 116) at the formula supposes that the surface which spans the capillary tube is hemispherical; however that is never rigorously true, but in a tube as narrow as that which I used myself, the difference is necessarily completely insensible, so that by calculating then, by the law of the reciprocal ratio of the rise to the diameter, the height for a tube a millimetre in diameter, I was to have this last height such as it would be if its top surface were exactly hemispherical.

The values of ρ and h being thus determined, one draws from it

$$2h\rho = 22.17$$

a number which differs very little from 22.56, obtained in the preceding paragraph as value of the product pd. the formula $pd = 2h\rho$ can thus be regarded as checked by the experiment, and the checking will still appear more complete if it is considered that the two results are respectively deduced from completely different principles.

§ 123. Now let us approach the second of the questions announced at the beginning of § 116; this one has as its aim the search for a very small limit below which is, at least in the glyceric liquid, the value of the sensible radius of activity of molecular attraction.

The idea of an approximate evaluation of this radius was stated for the first time, I think, in 1841, by de Maistre[46]. By suspending drops of water from the lower ends of vertical glass rods, he finds that the drop has the greatest volume when the diameter of the rod is 2 1/2 lines, and the drop is hemispherical. Supposing that a water molecule in contact with the glass surface supports all the molecules located vertically below it, one might conclude that, in water, the molecular attraction extends at least 3 time the distance of 1 1/4 line. I do not need to insist on the fallibility of this deduction.

Now let us reveal the results of our own research: the exactitude of the formula $p = \frac{2h\rho}{d}$ supposes, as we will show, that the film which constitutes the bubble does not have, at any of its points, a thickness less than double the sensitive radius of activity of molecular attraction.

One saw (§ 116) that the pressure exerted by a bubble on the air which it imprisons is the sum of the actions due separately to the curvatures of its two faces. In addition, it is known that, in the case of a filled liquid mass, the capillary pressure exerted by the liquid on itself emanates from all the points of a surface layer having for thickness the radius of activity in question. Now, if the thickness of the film which constitutes a bubble is everywhere greater or equal to double this radius, each of the two faces of the film will have its unattenuated surface layer, and the pressure exerted on the air contained will have the value which our formula indicates. But if, at any of its points, the film has thickness less than double this same radius, the two surface layers do not have any more their complete thickness, and the numbers of molecules included in each one of them being thus reduced, these two layers must necessarily exert less strong forces, and consequently the sum of those, i.e. the pressure on the interior air, must be smaller than the formula indicates.

[46] *Notice sur la cause qui fait surnager une aiguille d'acier sur la surface de l'eau* (Bibi Univ., nouv. série, vol. XXXV, p 192).

It follows that if, in the experiments of §§ 120 and 121, the thickness of the films which formed the bubbles was thinned, in all their extent, below the limit in question, the results would have been too weak; but, in this case, one would have noticed progressive and continuous reductions in the pressures, which never happened, although the colors of the bubbles showed a great thinness. All the physicists admit, moreover, that the sensitible radius of activity of molecular attraction is excessively tiny.

But what precedes makes it possible to go further, and to deduce from the experiment data on the value of the sensible radius of activity, at least in the glyceric liquid.

After the film acquired a uniform thinness (§ 108), except, of course, in the lowermost part, where there is always a small accumulation of liquid, if the pressure exerted on the interior air underwent a reduction, this would be shown by the pressure gauge, and one would see it progressing in a continuous way progressively with the later attenuation of the film. In this case, the thickness which the film had when the pressure decrease started would be determined by means of the color the central space had in this moment (ibid.), and half this thickness would be the value of the sensitible radius of activity of molecular attraction. If, on the contrary, the pressure remains constant until the disappearance of the bubble, one will conclude from the color of central space the final thickness of the film, and half this thickness will constitute, at least, an upper limit of the radius.

§ 124. I made, in this direction, a great number of experiments, of which I will render account. I initially gave to the bubble a diameter of approximately four centimetres, I then let it shrink to about two centimetres, and sometimes until one centimetre, then the wax ball was applied; then, in the first experiments, I removed the drop, and I introduced the attachment with the bubble into the interior of a small bottle which I closed by simply covering the opening with a disc of paperboard pierced in its middle by the tube; finally I established contact between the horizontal wire of the lenses of the cathetometer with the top of the image of the water surface in one of the branches of the pressure gauge, and as equilibrium did not take place immediately (§ 120), I waited until it and the other contact became stationary.

Eight bubbles observed in these circumstances could be followed until their disappearance. Seven of them burst before exceeding the first second-order colors; only one appeared to reach the indigo of this same kind, but there is some uncertainty in this respect. The greatest persistence was 14 hours.

As for the contact of the wire of the lenses with the image of the surface of water, it never, with only one exception, varied in the direction of a pressure decrease; but, strangely, it sometimes varied slightly in the opposite direction. For one of the bubbles, I assured myself, by measures taken before and after these variations, that the pressure had really increased somewhat. When a similar variation occurred, it was with a certain speed, and the pressure gauge remained then stationary, either until the disappearance of the bubble, or until a new variation of the same sense.

These variations are not due to changes in the temperature, because that of the room was quite constant; they do not come either from an imperfect application of wax, because, in this case, the increase in pressure would be continuous and would be accelerated.

§ 125. These experiments could already have provided me a result; but I sought to make it so that the colors of the bubbles went further. Suspecting that a small chemical action between the iron of the attachment and the liquid deteriorated a little the constitution of this last in the vicinity of the opening, I made to adapt to it, with sealing wax, an end of a glass tube of of the same external diameter, and with rather thin walls, and I blew a bubble at the open end of this tube, a bubble that I introduced as previously into the small bottle. Then the colors went until the third order, after which they retrogressed little by little (§ 108); the bubble persisted 24 hours. To prevent (§ 109) the retrogradation of the colors, I placed at the bottom of the bottle a few pieces of caustic potash, and, by the application of small pads of lard around the joints, I made it so that after the introduction of the bubble, the bottle was hermetically sealed.

Moreover, as the little liquid which always accumulates by degrees at the bottom of the bubble must contribute by its weight to make it burst, I had this time waited ten minutes before removing the drop; the film was thus already thinner when I introduced it into the bottle, and the resultant accumulation of the later descent of the liquid was to be much less. However, under these conditions, the reduction thickness of the film continued, the bubble persisted nearly three days, and, when it burst, it had arrived at the passage from yellow to the first order white; it then presented a pale yellow central space, surrounded by a white ring. The level of water in the branch observed of the pressure gauge underwent small oscillations, sometimes in one direction, sometimes in the other, but whose last was in the direction of an increase in pressure. Although, throughout the duration of this bubble, the temperature of the room necessarily underwent small changes, the oscillations above cannot be entirely allotted to them, because, if they were, one should have seen, after each of the three nights, a movement of the pressure gauge in the direction of an increase in pressure; however I observed the opposite after the first two nights; it is only after the third that there was movement in this direction.

It follows from the progress of these same movements that if the pressure varied, it was in an irregular way, in both directions, and to end, not with a reduction, but with an at least relative increase; one can thus admit, I think, that the final thickness of the film was still higher than double the sensible radius of activity of molecular attraction[47].

§ 126. Now let us see what one deduces from this last experiment. According to the table given by Newton, the thickness of a pure water film which reflects the first order yellow is, in millionths of an English inch, 5 1/3, or 5.333, and, for the white of the same order, 3 7/8, or 3.875. One can thus take the average, which is 4.604, as nearly the value of the corresponding thickness, always in the case of pure water, in between these two colors; and, the English inch being equal to 25.4mm, this thickness is equivalent to 1/8554 of a millimetre. That said, it is known that, for, two different substances, the thicknesses of films which reflect the same color are between them in inverse proportion of the indexes of refraction of these substances. To have the real thickness of our film of glyceric liquid, it is thus enough to multiply the denominator of the preceding fraction by the ratio of the index of glyceric liquid to that of water. I roughly measured the first by means of a hollow prism of liquid, and I found it equal to 1.377. That of water being 1.336, one obtains finally, for the thickness of the glyceric film, 1/8811 of millimetre. Half of this quantity, or 1/17622 of millimetre, thus constitutes the limit provided by the experiment in question; but, to round up a bit, we will adopt 1/17000.

We thus arrive at this very probable conclusion that, in the glyceric liquid, the radius of significant activity of the molecular attraction is less than 1/17000 of a millimetre.

§ 127. All the preceding research is extracted from my Fifth Series, published in 1861. On his side, Dupre arrived, in 1866, in § 173 of his fifth Memoire *Sur la théorie mécanique de la chaleur*[48], by an absolutely different road, at the conclusion that, in

[47]I said than the bottle into which one introduced the bubble was small, and which one had hermetically sealed the joints. That could appear in contradiction with what I have advanced in § 106, that, to reach the maximum persistence, it is necessary to employ a vessel of considerable size compared to the bubble, and not to dry the interior atmosphere completely. In the current experiment, it was a question of obtaining, not greatest persistence, but the greatest attenuation of the film, which required that one prevented, as much as possible, any absorption of aqueous vapor by this film.

[48]*Ann. de Chim. et de Phys. de Paris* , 4th series, vol. VII

water, the radius of molecular attraction is much higher than 1/5000000 of a millimetre; moreover, in work on same subject, work which appeared in 1869 and about which I will speak further (§ 164), Dupre is led to admit that from thickness equal to double the radius in question until a thickness equal to this radius itself, the pressure that a bubble exerts on the interior air varies extremely little, from which he would conclude that I should have taken, for my higher limit, not half the thickness film, but this whole thickness; according to that, Dupre adopts, as the upper limit, 1/9000 of a millimetre.

But, in 1869 also, Mr. Quincke published a Note[49] where he reveals an extremely clever method by means of which he arrives, not at an upper or lower limit, but with an approximate value of the radius in question: he finds it equal to approximately 0.000050mm = 1/200000 of millimetre.

Here primarily of what his method consists: when a liquid rests against a solid vertical wall which it does not wet, it contacts this wall at an angle which, one knows, depends at the same time on the nature of the solid and that of the liquid. Accordingly, let us imagine a wedge-shaped layer of another solid substance applied to a portion of one the of the faces of a vertical plate of glass, says the author, the edge of the corner being vertical, and the thickness, excessively small at this edge, increasing by insensible degrees from there. So when a liquid rests against the plate, so that part of its edge contacts glass and another part the wedge-shaped layer, the contact angle of this second part will vary from the edge to a certain distance, because, in the vicinity of this edge, the mutual molecular action of glass and liquid will still be felt; but, beyond the distance where the thickness of the layer is equal to the radius of this mutual action, the contact angle will be constant, depending then only on the mutual action of the liquid and the substance which forms the layer.

Now Mr. Quincke succeeded in obtaining, on glass, these wedge-shaped layers of various substances, and determining, in each case, the thickness which meets the condition indicated. With a layer of metallic silver and the liquid being water, he found, indicating by l the thickness in question, $l > 0.000054$mm; with a layer of sulphide of silver and substituting mercury for water, the experiment gave him $l = 0.000048$mm; with the mercury and a layer of silver iodide, $l = 0.000059$mm; with mercury and a layer of collodion, $l < 0.000080$mm. Mr. Quincke concludes from his experiments that one can adopt, on average, $l = 0.000050$mm.

It thus appears to follow from these same experiments: first, that the sensible radius of activity of molecular attraction does not vary much with various substances, and that one can adopt, for its rough average value, 1/20000 millimetre; in the second place, that the upper limit found by me, being 1/17000 millimetre, is very near the true value; in the third place finally, that, in my experiments, as opposed to what Dupre thought, it was indeed half the thickness of the film which had to be taken.

Let us mention, in passing, that, in this Note, Mr. Quincke attributes an opinion to me that I did not express: he makes me say that, in my opinion, a liquid film cannot exist any more when its thickness becomes lower than twice of the radius of molecular attraction; one can see, by the contents of the paragraphs which precede, that an idea of this kind did not come from me. However, this idea, which actually belongs to Mr. Quincke, must be true as a general thesis; I will further return there (§ 165).

[49] *Ueber die Entfernung in weicher die Molecularkräft der Capillarität noch wirksam sind.* (Ann. de M. Poggendorff, vol. CXXXVII, p. 402)

CHAPTER IV.

Equilibrium shapes which are not of revolution. General principle concerning the creation of surfaces with zero mean curvature. Results of the geometers, and experimental checks.

§ 128. Let us come to the shapes of equilibrium which are not of revolution; and initially let us state a general principle which makes it possible to create, in a film state, any surface with zero mean curvature of which one has either the equation in finite co-ordinates, or its geometrical generation.

A surface with zero mean curvature being given, consider an arbitrary closed contour traced on it, satisfying only the conditions 1° that it circumscribes a finite portion of the surface, and 2° that this portion does not exceed the limit of stability, if the surface given has such limits; bend a wire in such a manner that it takes exactly the closed contour in question; slightly oxidize it by weakened nitric acid; immerse it entirely in the glyceric liquid, and withdraw it; you will find it occupied by a film representing the portion of surface in question.

Indeed, the film which developes in the solid contour, and which necessarily fills it, has its two faces in the open air; it must thus (§ 97) be formed so as to represent a finite portion of a surface of zero mean curvature passing through the contour in question, i.e., consequently, a portion of the given surface. One creates thus, as if by magic, surfaces which, for the most part, are extremely remarkable; we will soon see examples of them.

In the statement of the principle above, I assigned, as the first condition of a closed contour, that it must include in its interior a finite portion of the given surface; indeed, one can conceive closed contours which do not satisfy this condition: if, for example, one chose, as a closed contour traced on a catenoid, the section made by a plane perpendicular to the axis, it is clear that on both sides that this contour would not circumscribe any finite portion of the surface extending from this same contour.

As for the second condition, it is obvious that if the given surface has, compared to the kind of contour that one has adopted, a limit of stability, and if this limit is exceeded by the circumscribed portion, it is clear that its creation will be impossible, and that the film will form a portion of a different surface. In this case, there are consequently two distinct surfaces both satisfying the conditions of having zero mean curvature, of passing through the closed contour, and having a finite portion circumscribed by this contour; but for only one the finite portion is stable, and it that one which is formed. We will see also a remarkable example of the case in question.

§ 129. Meusnier announced[50] as a surface such that at each point the two principal radii of curvature are equal and of opposite signs, or, which is the same, as a surface with zero mean curvature, the skew helicoid with central axis, i.e. the helicoid generated by a straight line which slides with a uniform movement along another straight line to which it is perpendicular, while it turns uniformly around this same line.

In 1842, Mr. Catalan[51] showed that, among ruled surfaces, the plane and the helicoid above are only ones whose mean curvature is zero.

In 1859, Mr. Lamarle again took up the question under a much wider point of view; by means of geometric methods[52] he sought generally which are the ruled surfaces whose mean curvature is constant; he finds thus, in the case of zero mean curvature,

[50] *Mémoire sur la courbure des surfaces*(MEM. DE L'ACAD DES SCIENCES DE PARIS, foreign scientists, 1785, P. 477).

[51] *Sur les surfaces réglées dont l'aire est un minimum* (Journ. DE M. LIOUVILLE, vol. VII, p. 203.)

[52] *Théorie géométrique des centres et axes instantanés de rotation* (BULLET. DE L'ACAD. DE BELGIQUE 2nd series, vol. VI, p. 412).

the result of Mr. Catalan, and he shows that, in the case of a finite and constant mean curvature, there is only one ruled surface, the cylinder of revolution.

§ 130. I created, in the film state, the skew helicoid with central axis. The solid frame used in this experiment is represented in vertical projection by fig. 54: it is composed of a straight wire as the axis, around which another helical curved wire turns regularly; this last wire is bent at each one of its ends so as to lead to the axis by a straight portion perpendicular to this same axis; these straight segments, which represent the two extreme generating line positions, are welded to the axis, and thus support the helix. The iron wire has approximately a millimetre thickness, the diameter of the helix is 10 centimetres and the distance from one whorl to the following is 6 centimetres; finally there are two complete whorls. Let us add that the entire frame was oxidized by weakened nitric acid (§ 110).

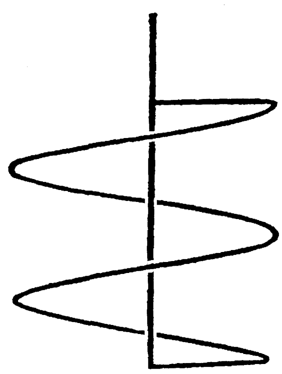


Fig. 54

When, after having immersed this frame in the glyceric liquid and having left it there for a few seconds, one withdraws it, one finds it occupied by a beautiful film extending everywhere from the axis to the whorls, and forming in a perfect way the skew helicoid in question.

Indeed, the unit formed by the wire helix, the two parts which attach it to the axis, and the portion of the axis ranging between these two lines, constitutes a closed contour which one can obviously conceive traced entirely on a skew helicoid with central axis, and which would include in its interior a finite portion of it; however a film forced, on the one hand, to adhere to all this contour, and, on the other hand, to form itself into a surface with zero mean curvature, must necessarily take the shape which satisfies these two conditions, i.e. that of the helicoid in question.

This experiment is, one sees, a first application of the general principle of § 128.

§ 131. Mr. Lamarle, generally considering the helicoids generated by a plane curve which is driven uniformly along a fixed line located as its axis, while it also turns uniformly around this same line, has searched[53] for those which could satisfy the condition of a constant mean curvature, and he found thus, in addition to the skew helicoid with central axis, four other surfaces. These five helicoids correspond respectively to five of the equilibrium shapes of revolution, which are the plane, the sphere, the unduloid, the catenoid and the nodoid. As for that which corresponds to the cylinder, it is the cylinder itself.

The differential equation of the generating curves of these helicoids is integrated by ordinary means in the cases corresponding to the plane and the catenoid; in this last, it gives an already known surface, of which we will speak again. In the other cases, the equation is integrated by elliptic functions.

Each meridian curve of the equilibrium shapes of revolution, with the exception of the plane and the cylinder, passes to the generating curve of the corresponding helicoid by simply extending in the direction of the axis following a certain law, and by preserving the distances of its various points from this axis. According to this result, the semicircle which constitutes the meridian curve of the sphere becomes a more lengthened curve, whose top is distant from the axis by a quantity equal to the radius of the semicircle in question; the meridian curve of the unduloid changes into another undulating curve, which approaches and moves away as much from the axis, but whose

[53] *Sur une classe particulière de surfaces à aire minima* (BULLET. DE L'ACAD DE BELGIQUE, 1859, 2nd series, vol. VI, p. 329).

undulations are longer; etc. The meridian curve of the plane, being a line perpendicular to the axis, cannot undergo modifications in the direction of the axis, it remains such as it is, and generates the skew helicoid with central axis; finally the meridian line of the cylinder cannot change either, and it generates, like helicoid, this same cylinder.

Let us add that the solution of Mr. Lamarle includes necessarily the shapes of revolution, those being what the helicoids concerned become when the speed of traverse of the generating curve is zero.

§ 132. The generating curve of the helicoid derived from the sphere, the curve for which Mr. Lamarle found a rather simple construction, ends at the axis at acute angles; it is thus necessary, for continuity, to conceive it being prolonged on the other side of the axis by symmetrical arcs, then passing by again to the first side, and so on, forming an infinite corrugated line, symmetrically cut by the axis all along its extent. In a particular case calculated by Mr. Lamarle, where each one of these arcs, while it slides by a quantity equal to the length of its chord, carries out very nearly seven ninths of revolution; the curve is that shown in fig. 55. If one cuts the generated helicoid by a plane perpendicular to the axis, the cross-section has the form represented in fig. 56; A is the point by where the axis passes.

I created part of this same helicoid by taking only the shape generated by one of the arcs of the curve of fig. 55. Then the transverse cross-section is only half of the curve of fig. 56, being the curve $abcdfa$. I formed, in wire, three curves equal to this last form, plus two iron plates also having the same line for contour, and intended to be used as bases for the liquid shape. These parts had approximately double dimensions of those of fig. 56; they were fixed transversely, each one by its point a, and in azimuths successively differing one from the other by 90°, in five equidistant points along a vertical straight wire; this was surrounded by cotton yarn, and was supported by a small foot. Each interval between these same parts was, as a consequence of the calculations of Mr. Lamarle, the 0.324 fraction of the chord of the one of the arcs of the generating curve (fig. 55). This system having been placed in the alcoholic mixture, one made adhere to the whole of the five curves, by employing suitable care, an oil mass in excess; then one gradually removed the the latter liquid, until the surface of the shape match exactly and in a continuous way the contours of the five curves. This point reached, the liquid shape formed perfectly one complete whorl of the helicoid which it was a question of obtaining. I did not illustrate the result here, because of the difficulty of representing it well by an engraving; but one will easily have an idea of it.

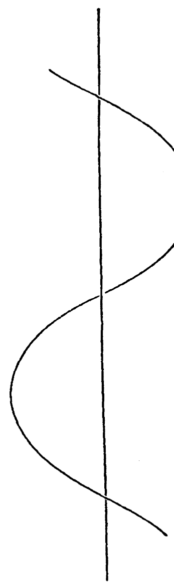


Fig. 55

The parts ba and fa (fig. 56) of the curve form, at a, a re-entrant angle, and it is impossible, with a solid system composed of naked iron wire, to form an oil shape presenting an angle of this kind (§ 31): always oil passes beyond the edge, and comes to fill in more or less great quantity the opening of the angle; it is to remove this problem that one surrounds by cotton yarn the straight wire which is used as the axis of the helicoid; oil cannot cross the obstacle which the cotton soaked with alcoholic liquid erects to it, and the re-entrant angle is maintained.

§ 133. The general differential equation of surfaces with zero mean curve, i.e. equation [3] of § 2 in which constant C is equal to zero, was, on behalf of the geometers, the subject of many works.

We will review those whose publication preceded 1870, limiting ourselves to what can offer interest for our work, and we will describe at the same time several experimental checks.

One will remember that these surfaces are indifferently called (§ 2) surfaces with zero mean curvature, surfaces of minimum area, and surfaces whose radii of curvature, at each point, are equal and of opposite signs.

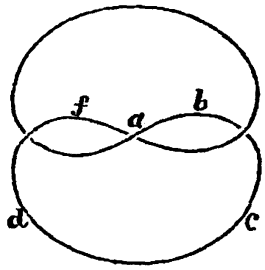


Fig. 56

Monge gave the first[54] integral of their equation; but this integral is of a complicated form, which makes its use very difficult.

However Mr. Scherk, who had already dealt with the question in his first Mémoire[55] published about 1831, began again in 1835 in a second work[56] and, partly by means of a special method, partly by treating the integral of Monge, he manages to find, in finite co-ordinates, in addition to the equations of the skew helicoid with central axis and the catenoid, already known from the researches of Meusnier[57], those of five other surfaces; here are these equations:

$$e^{\mathrm{D}z} = \frac{\cos \mathrm{D}y}{\cos \mathrm{D}x};$$

$$z = b\,l\,\frac{\sqrt{\rho^2+a^2}+\sqrt{\rho^2-b^2}}{a} - a\arctan\left[\frac{b}{a}\sqrt{\frac{\rho^2+a^2}{\rho^2-b^2}}\right] + a\theta + c,$$

where

$$x = \rho\cos\theta, \quad \text{and} \quad y = \rho\sin\theta;$$

$$\frac{1}{2}e^{2z-\frac{y}{x}\sqrt{2\rho+2t}+x\sqrt{2\rho-2t}} + \frac{1}{2}e^{-2z+\frac{y}{x}\sqrt{2\rho+2t}-x\sqrt{2\rho-2t}} = \rho + \frac{y^2}{x^2} + \frac{x^2}{4},$$

where

$$t = 1 + \frac{y^2}{x^2} - \frac{x^2}{4} = \rho\cos\theta, \quad \text{and} \quad y = \rho\sin\theta;$$

$$\frac{1}{2}e^{x+\sqrt{\frac{1}{2}\rho+\frac{1}{2}t}\csc\frac{1}{2}x} + \frac{1}{2}e^{-x-\sqrt{\frac{1}{2}\rho+\frac{1}{2}t}\csc\frac{1}{2}x} = \frac{4\sin^2\frac{1}{2}x+\rho}{y^2},$$

where

$$t = 4\sin^2 12x + y^2\cos x = \rho\cos\theta, \quad \text{and} \quad y^2\sin x = \rho\sin\theta;$$

$$\sin \mathrm{D}z = \pm\frac{e^{\mathrm{D}x}-e^{-\mathrm{D}x}}{2}\cdot\frac{e^{\mathrm{D}y}-e^{-\mathrm{D}y}}{2}.$$

Let us add that the author manages to deduce from the first of these equations this other more general one:

$$e^{Bz\sin(\beta-\alpha)} = A\frac{\cos B[x\cos\alpha+y\sin\alpha+a]}{\cos B[x\cos\beta+y\sin\beta+b]}.$$

[54] *On the integral calculus of the equations to the differences partial* (MEM. OF THE ACAD. SCIENCES OF PARIS, 1784, p. 118).

[55] *Of proprietatibus superficiei quæ hac continetur æquatione:* $(1+q^2)r-2pqs+(1+p^2)t=0$ *disquisitiones analyticæ*(ACTA SOCIET. JABLONOVIANÆ vol. IV, P. 204, Leipzig).

[56] *Bemerkungen ueber die kleinste Fläche innerhalb gegebener Grenzen* (CRELLE'S JOURNAL, vol. xIII, p. 183).

[57] See the first note of § 129.

He announces that, in a later memoire, he will subject to particular research the first of the equations above, in its relationship with the last; I am unaware if this memoire has been published; I could not find it.

§ 134. In 1843, Mr. Björling[58] sought, like Mr. Scherk, to benefit from the integral of Monge; but it compels a surface to pass through a given curve, and, to particularize this same surface, it also subjects the direction of the normal along the curve in question to a given law. His method most probably leads to finite equations representative of new surfaces; but the application which he chooses as example provides only the catenoid.

§ 135. Mr. Ossian Bonnet, while employing an auxiliary frame of reference, arrived in 1853 at a general integral[59], which does not present the problems of that of Monge; one will be able, undoubtedly, to deduce from his formulas, in ordinary co-ordinates and in finite form, the equations of a great number of new surfaces.

Mr. Bonnet announces, as an example of the results that one draws from his integral, a surface which, for certain values of the constants, is reduced, on the one hand, to the skew helicoid, and, on the other hand, to the catenoid; this surface was already known; it is that which is represented by the second of the equations of Mr. Scherk; it is also that of the helicoids of Mr. Lamarle (§ 131) which corresponds to the catenoid.

In 1855 and 1856, Mr. Bonnet, moreover, applied his method[60] to the problem already dealt with by Mr. Björling; but he does not give any equation, in finite co-ordinates, of any new surface.

He took up again these questions in 1860, in a further Memoire[61]; he expounds on a geometrical generation of surfaces mentioned above; here is this generation:

Let us consider, in the xy plane, the hyperbola represented by the equation

$$\frac{x^2}{a^2} - \frac{y^2}{b^2} = 1,$$

and take the part located on the side of positive x for the base of a cylindrical surface parallel to z. On this surface plot a curve such that the co-ordinate z of an arbitrary point M on it is in a constant ratio equal to $-\frac{2}{b}$ with the hyperbolic sector counted starting from the real semi-axis which abuts the vertex $(x = a, y = 0)$, and ending with the semi-diameter which abuts the projection of the point M in the field of the xy plane; finally let us give to this kind of hyperbolic helix a direct helical movement around the z axis, so that the helices described by its various points all have pitch $2\pi B$; the surface thus obtained will be the sought surface.

§ 136. In 1855, Mr. Serret indicated[62] a transformation of the integral of Monge which makes it possible to represent all surfaces with zero mean curvature passing through given straight lines not located in the same plane. His Memoire, however, does not contain new equations in finite co-ordinates.

§ 137. Mr. Catalan published, in 1855 again[63], two Notes in which he arrives at particular surfaces.

[58] *In integrationem æquationis derivatarum partialium superficiei, cujus in puncto unoquoque principal ambo radii curvedinis æquales sunt signoque contrario* (ARCHIVES OF GRUNERT, 1844, vol. IV).

[59] *Note sur la théorie générale des surfaces* (COMPTES RENDUS vol. XXX VII, p. 529).

[60] *Sur la détermination des fonctions arbitraires qui entrent dans l'équation intégrale des surfaces à aire minima* (COMPTES RENDUS, T xL, p. 1107). - *Nouvelles remarques sur les surfaces à aire minima* (ibid, volume XLII, page 532).

[61] *Mémoire sur l'emploi d'un nouveau système de variables dans l'étude des propriétés des surfaces courbes* JOURN. DE M LIOUVILLE, 2nd series, vol. V, page 153).

[62] *Sur la moindre surface comprise entre des lignes droites données, non situées dans le même plan* (COMPTES RENDUS, volume XL, p. 1078).

[63] *Sur une surface dont les rayons de courbure, en chaque point, sont égaux et de signes contraires* (COMPTES RENDUS, T xLI, p. 35), and *On two surfaces which has, in each point, their equal radii of curvature and contrary signs*(Ibid., p. 274).

The first of these surfaces coincides with the first of those of Mr. Scherk; but Mr. Catalan determines its form, which he describes in the following way:

The surface cuts the *xy* plane along straight lines which form angles of 45° with the *x* and *y* axes, and which divide all the plane into equal squares; two of these lines intersect at the origin of co-ordinates. The surface admits, moreover, a straight line system perpendicular to this same plane, and which divides into two equal parts the sides of the above squares. The surface is made up of an infinity of identical sheets of which each one lies wholely between four asymptotic planes forming a channel with square cross-section of infinite length; the edges of all these channels are the lines of the last system. One can represent the cross-section of the whole of these same channels by the *xy* plane as an infinite chess-board, in which the black squares would occupy the channels containing the sheets of the surface, and the white squares occupy empty channels; the black squares would contain in their centers the corners of the first squares described above. All sheets being identical, it suffices to consider one, and we will take that which surrounds the *z* axis. It is crossed by the *xz* plane following a curve located entirely above the *x* axis, which touches this axis at the origin, has the *z* axis as an axis of symmetry, and which presents two infinite branches having for asymptotes the cross-sections of the rectangular channel by the *xz* plane. The same sheet is cut by the *yz* plane following a curve identical to the preceding one, but reversed, and located entirely below the *y* axis, that it also touches at the origin. If it is imagined that the first of these curves slides over the second while remaining parallel to itself, it will generate the sheet in question. One will thus have easily an idea of it, and as it is clear that all the sheets are connected by the lines of the last system above, one will be able to picture the whole of surface. It is seen that each sheet taken separately has a great analogy with the hyperbolic paraboloid.

The second surface of Mr. Catalan is given by a system of four equations, among which it would be necessary to eliminate three quantities; but this elimination, if it is even possible, would be certainly extremely difficult. The equation of the third surface coincides with the second of those of Mr. Scherk.

Mr. Catalan presented, moreover, in the same year, to the Academy of Sciences of Paris, a Memoire[64] in which he arrives, in another manner than Mr. Bonnet, at the integral, in real form, of the general equation. Of the solutions which he draws from its calculations, one is reduced, by a suitable transformation, to the last of the equations of Mr. Scherk; three others are given by systems of equations within which it would be necessary to carry out difficult eliminations, but, for one of these last, Mr. Catalan arrives, without doing elimination, at the generation of a surface, a generation given here:

Think of a circle rolling on a straight line, thus a point S of the circle describes a cycloid; consider, moreover, the cycloid traced by the mobile radius passing through the point S, and let P be the point of contact; imagine finally, in a plane perpendicular to that of this cycloid, a parabola whose directrix is projected out of P, and which has S for its vertex; this last curve, variable in size, will generate the surface.

Lastly, in 1838, appeared the Memoire in extenso[65] from where the articles which I have just summarized were extracted.

§ 138. I created, always by means of films, a portion of one of the sheets of the first surfaces above, that which coincides with the first of Mr. Scherk. I chose a finite portion of equal extent above and below the *xy* plane.

[64] *Sur les surfaces dont les rayons de courbure, en chaque point, sont égaux et de signes contraires* (COMPTES RENDUS, vol. XLI, p. 1019).

[65] *Mémoire sur les surfaces dont les rayons de courbure, en chaque point, sont égaux et de signes contraires* (JOURN. DE L'ECOLE POLYTECHN. vol. 37, p. 129).

One understands, from the description of the surface, that it will be enough to form a solid frame from four iron wires equal in length, laid out like the four side edges of a right prism with a square base, and whose ends are joined together in a suitable way by transverse iron wires arched according to curves deduced from the equation of the surface. To express it more clearly, let us suppose the frame placed so that the four iron wires are vertical, and that, relative to the observer, two of these wires are in front of the two other ones; it will be necessary that in the lower part of the system, a transverse wire joins together the ends of two front vertical wires, and the another the ends of the two back vertical wires, and in with the upper part, a transverse wire joins together the top of the right front vertical wire to that of the right back vertical wire, while another joins together in the same way the top of the left front vertical wire to that of the left back vertical wire. The middles of the upper transverse wire will be joined together besides by a fork, so that one can hold the frame to immerse it in the liquid.

The curvature of the transverse wire necessarily varies with the relationship between the height and the width of the frame. I initially took the height equal to the width; in other words, I considered the portion of the sheet which extends above and below the xy plane by a distance equal to the half-width of this sheet. Mr. Catalan puts the equation of surface in the form:

$$z = \log \frac{\cos y}{\cos x}. \tag{4}$$

If, for more simplicity, one considers in particular the sheet which surrounds the z axis, one will easily see that, in the equation above, the half-width of this sheet is represented by $\frac{\pi}{2}$; it will thus be necessary, to have the equation of the curves which must finish the solid frame above and below, to make $z = \pm\frac{\pi}{2}$, from which one will deduce

$$\cos y = e^{\pm\frac{\pi}{2}} \cdot \cos x \tag{5}$$

I took for the solid frame a width and a height of 9 centimetres; the curves were drawn according to equation [2] and I built the frame; it is shown in perspective in fig. 57. When it is withdrawn from glyceric liquid, the film which it contains offers to the eyes the perfect realization of the sought portion of surface. Fig. 58 shows, in vertical projection according to two rectangular directions, the result so obtained.

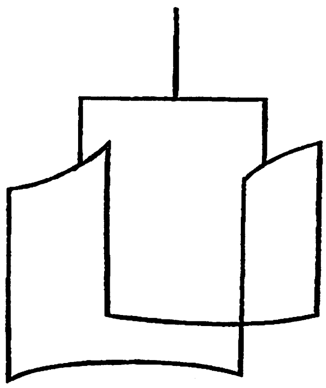


Fig. 57

Here, as in the case of the skew helicoid with central axis (§ 130), the wire frame constitutes a closed contour which can be entirely reproduced traced on the surface in question, and the film, which must pass through all this contour and have zero mean curvature, is obliged to form itself according to a surface which meets this double condition; it is yet another application of my general principle (§ 128).

One can wonder whether the surface would be formed in the same way for a much larger ratio between the height and the width of the solid frame; this formation supposes, indeed, that the sheet, taken on a more considerable

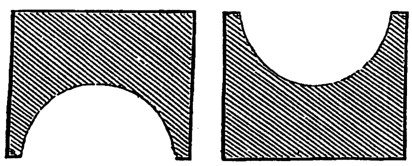


Fig. 58

part of its extent, does not become unstable. In order to test it, I built a second frame of 10 centimetres height and 2.5 width; the height is quadruple the width. By seeking the equation of the curved portions which are appropriate for this ratio, which is obtained by making $z = 2\pi$ in equation [1], I had found that these curves were reduced to appreciably straight lines, so that the new frame is composed only of rectilinear parts. However the film which was formed there did not form the surface described by Mr. Catalan: it took a shape still consisting of two hollow parts laid out at right angles with respect to each other; but their common vertex, instead of being in the middle of the system, was approximately four times further away from one of the ends than the other; moreover, by repeating the experiment several times, I saw this common vertex being placed sometimes towards the higher end, sometimes towards the lower end. I had to conclude from this that the sheet of the surface described by Mr. Catalan has a limit of stability and that I had exceeded this limit.

The experiment that I have just described thus offers us an example of the case where the second condition stated in the general principle of § 128 is not satisfied[66].

I told myself that one could probably give stability to the shape sought by benefitting from the property that the sheet contains, as one saw, two lines directed along the diagonals of the cross-section of the rectangular channel by the xy plane; I thought that by rendering one of these diagonals solid, one would oblige the common vertex of the two hollow parts to remain in the center of the system. I thus added, at the middle of the height of the new frame, a very thin wire which crosses it, as one sees in fig. 59, and, indeed, by this means, the surface of Scherk and Catalan were carried out perfectly in the frame in question.

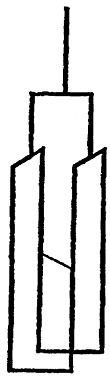


Fig. 59

§ 139. Mr. Van der Mensbrugghe, applying, on his part, my general principle, created[67], in 1866, the fifth of the surfaces of Mr. Scherk. Consideration of the equation showed him that the simplest frame to employ would be composed of two equal rectangles having their large sides quadruple the length of their small sides, and cutting each other at right angles at the midpoints of the large sides (fig. 60). Rigorously, the small sides should be curved; but, with the ratio above, their curvature would be so weak that one can leave them straight without resulting in an appreciable difference between the produced film surface and the theoretical surface; that holds when the planes of the rectangles are asymptotic to the latter.

The frame thus built does not represent just one closed contour, as it would by my principle, but actually the whole of two of these contours; and those are not the two rectangles themselves, because one assures oneself easily, according to the discussion, that none of the latter satisfies the condition, required by my principle, to circumscribe a finite portion of surface. If one separately considers the part of the frame made up of a half of the one of the rectangles and of a half of the other, one will have the first of

[66] Although I have made it a rule to close my historical remarks at the end of 1869, I will say however here that Mr. Schwarz, in a note attached to his Report published in 1871 (see No. 13 of § 508), regards this instability as due to the frame being made of wire with a notable thickness, and necessarily presenting small irregularities. The surface in question could not be formed there rigorously; without these imperfections of the frame, the surface, with its vertex in the middle, would be, according to Mr. Schwarz, in consequence of a mathematical demonstration, perfectly stable. If it is really thus, the instability that I noted would belong, not to the surface of Mr. Scherk, but on another surface which would be very near.

[67] *Discussion et réalisation expérimentale d'une surface particulière à courbure moyenne nulle* (BULLET. DE L'ACAD. DE BELGIQUE, 2nd series, vol. XXI, p. 552).

two truly closed contours, and the opposite part will be the second; and, indeed, when one withdraws the frame from the glyceric liquid, it is in these two opposite parts that the film shape is placed.

These same parts include, one sees, two of the four right plane angles formed by the rectangles, and it is clear that there is not any reason for the film shape to develop in these two angles rather than in the two others; also, when the frame is well built, and the experiment is repeated several times, it forms sometimes in one of the pair of opposed angles, sometimes the other.

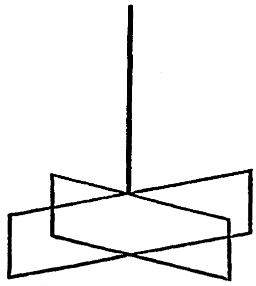


Fig. 60

The frame deviating from my principle in that it presents two closed contours instead of only one, the film system formed contains an additional film; it is flat, and it has the form of an oval whose vertices are at the two points of intersection of the rectangles, and whose plane is bisecting the the two plane angles which contain all the shape; to obtain the result which one seeks, i.e. a portion of the surface of Mr. Scherk, it is enough to burst the additional film of which I speak; the film shape then offers in its middle an empty space, and it is also what one deduces from the equation.

One could work with only one of two closed contours separately, which one would do rigorously under the conditions of my principle; then it would form only one single film, and this one would represent exactly half of the shape which forms on the whole frame after the rupture of the additional film; if Mr. Van der Mensbrugghe joined together two closed contours, it is in order to make a more complete portion of surface. It was ensured besides, by measurements with the cathetometer, that the produced film coincided, without appreciable error, with the theoretical surface.

Finally he continued, by means of the equation, the surface beyond the large sides of the rectangles, and he showed how one could extend the surface outside of the same limits, using a suitable frame provided with additional wire, without which the shape thus increased would be unstable; the result is extremely curious, but its description would require too much space.

§ 140. Mr. Mathet made known[68], in 1863, a method by means of which one can form the differential equation of all surfaces with zero mean curvature which pass through a given plane curve.

§ 140bis. In a note [69] of 1864, Mr. Enneper expounded a new method for integration of the general equation of surfaces of zero mean curvature, and he deduces from it, as an application, the following algebraic equation:

$$\left[k^2\cdot\frac{y^2-x^2}{z}+\frac{2}{3}+\frac{8}{9}k^4z^2\right]^3=6\left[k^2\cdot\frac{y^2-x^2}{2z}+\frac{2}{9}-k^4\left(x^2+y^2+\frac{8}{9}z^2\right)\right]^2.$$

The surface represented by this equation contains two straight lines located in the xy plane that bisect the angles between the x and y axes.

§ 141. In 1865, Mr. Schwarz communicated to the Academy of Berlin a Note[70] in which he summarizes part of the results that he obtained with regard to a particular minimum surface. This surface must pass through the contour formed of four edges

[68] *Étude sur un certain mode de génération des surfaces d'étendue minimum* (JOURNAL DE M. LIOUVILLE, 2nd series, vol. VIII, p. 923).

[69] *Analytisch-geometrische Untersuchungen* (zEITSCHRIFT FÜR MATHEMATIK UND PHYSIK OF SCHLÖMILCH, 9me year, p. 96).

[70] *Ueber die Minimumsfläche deren Begrenzung als ein von vier Kanten eines regulären Tetraëders gebildetes windschiefes Viereck gegeben STI.* BULLET. ACAD. DE BERLIN, meeting of April 6).

of a regular tetrahedron, chosen so as to constitute a skew quadrilateral. Mr. Schwarz created, in the film state, the portion included in this contour, by means of a gelatine solution: the film, become solid by the evaporation of water, thus gave him a permanent shape.

This shape is, as one would expect, in the form of a saddle; in addition to the sides of the skew quadrilateral, it admits two other straight lines, which would respectively join the midpoint of each of the sides to the midpoint on the opposite side. Finally Mr. Schwarz continues the surface beyond the contour indicated, and to determine the general shape of it: the total surface is made of portions identical to that above, juxtaposed in a certain way, and it is very curious[71].

§ 142. A posthumous memoire of Riemann[72], published in 1867, contains general research on minimal surfaces passing through several bounding curves. The author applies his method initially if these bounds are straight lines, of which several can intersect, for example by forming a skew quadrilateral; then where the bounds are two arbitrary circles located in an arbitrary way in parallel planes. His results are expressed by systems of equations containing the integrals indicated, and he does not give the complete equation of any surface in finite co-ordinates.

§ 143. In 1867 also, in a prize memoire for the Academy of Berlin[73], Mr. Schwarz again addresses the problem of surfaces with minimum area passing through four straight sides which form a skew quadrilateral; but, in the first part of this memoire, he treats a case more general than in his communication of 1865: the sides of the quadrilateral do not belong any more to one regular tetrahedron; only two adjacent sides are equal to each other, and the two others are in the same way equal between them. In the second part, the author finds, for the special case of the regular tetrahedron, that the final equation of the surface is expressed rationally by elliptic functions of the co-ordinates. He treats in the same way the second surface as he derives it from the first by an inflection of it; finally he joined to his work plaster models of each of these two surfaces. The Memoire in-extenso was not printed yet at the end of 1869.

The same scientist presented, in 1867 and 1868, on surfaces with minimum area, others Memoires which were not printed either at the end of 1869, and which contain extremely remarkable results; he agreed to give me first announcement, by authorizing me to mention them. Here are those which appeared me to offer the most interest:

1° When a surface with minimum area is such as one can trace a straight line on it, there is a symmetry with respect to it, in the sense that if one considers all the portion of the surface which extends on one side of the straight line, and then one makes it turn 180° around this line as an axis, one obtains the portion located on other side. One checks this easily, for example, with regard to the skew helicoid with central axis, the first of the surfaces discussed by Mr. Càtalan (§ 137), and partially created by Mr. Van der Mensbrugghe (§ 139).

An immediate consequence of this beautiful theorem is that if one makes a film shape by means of a closed solid contour which is composed only of straight lines, then

[71] Mr. Schwarz had the kindness to send plaster models to me of wide portions of the surface in question. One will understand in the following way the contour of one of these models: that one traces a regular hexagon, presumed horizontal to fix ideas; then, in the same plane, one builds, on each side as bases, an equilateral triangle having its top apart from the hexagon; then one imagines three of these triangles, nonadjacent, turning on their bases like hinges, until each one of them makes, with the plane of the hexagon and above it, an angle equal to the dihedral angle of a regular tetrahedron; then one imagines in the same way the three other triangles turning around their bases the same quantity as the first, but below the plane of the hexagon; finally one then removes the bases of all the triangles, and one will have the sought contour; I built it of wire, and I thus perfectly created, with glyceric liquid, the included portion of the surface discussed by Mr. Schwarz.

[72] *Ueber die Fläche vom kleinsten Inhait beigegebener Begrensung.* (Mém. de Göttingue, vol. XIII)

[73] *Mem. Berlin Acad. for the year 1867*, historical introduction, page IX.

one can ideally prolong the surface beyond this contour: it is thus, for example, what one finds happens outside the skew quadrilateral with the surface which is supported on its sides.

2° One can determine completely, by elliptic functions, the surface which one partially forms by raising out of the glyceric liquid, parallel to its surface, a wire square, and by stopping at a height less than that where the shape would change spontaneously[74].

One determines in the same way, using elliptic functions, the surface partially formed by Mr. Van der Mensbrugghe, if the ratio between the common length of the two solid rectangles and their height is arbitrary, large or small.

Finally, Mr. Schondorff treated, for his part, in a Memoire[75] also prize-winning in 1867, but by the Göttingen Society, the question of the surface with minimum area which rests on a skew quadrilateral; this quadrilateral satisfies the same conditions as that of Mr. Schwarz.

§ 143bis. In 1867 still, Mr. Enneper gave[76] the following equation:

$$\cos bz = k^2 \cdot \frac{e^{\frac{bx}{k}} + e^{-\frac{bx}{k}}}{2} - k'^2 \cdot \frac{e^{\frac{bx}{k'}} + e^{-\frac{bx}{k'}}}{2}.$$

This equation, if one made there $k = k' = \frac{1}{\sqrt{2}}$, can be put in the form:

$$4\cos bz = \left(e^{\frac{b(x-y)}{\sqrt{2}}} - e^{-\frac{b(x-y)}{\sqrt{2}}}\right)\left(e^{\frac{b(x+y)}{\sqrt{2}}} - e^{-\frac{b(x+y)}{\sqrt{2}}}\right),$$

and if, in the latter, one makes $\frac{x-y}{\sqrt{2}} = x'$, $\frac{x+y}{\sqrt{2}} = y'$, and $bz = \frac{\pi}{2} - bz'$, which amounts making the xz and the yz planes turn by 45°, and to move the origin, on the z axis, by a distance equal to $\frac{\pi}{2b}$, one finds the last of the equations of Mr. Scherk (§ 133); the equation of Mr. Enneper is thus a generalization of this one. I owe this remark, and the indication of the transformations above, to the kindness of Mr. Schwarz.

§ 144. It is the place here to describe two experimental checks which appear worthy of interest to me. One can infer from the research of the geometers that through an arbitrary given contour can pass an infinity of surfaces with zero mean curvature, at least when no other condition is introduced. According to that, if one builds, with slightly oxidized wire, a closed contour not plane and of such form as one wants, then one immerses it in the glyceric liquid and then one withdraws it, one must believe that a surface of this kind will be able to be always realized there in a film state, and that, consequently, one will have to find it occupied by a single film which entirely fills it.

[74] I cannot resist the desire to state, in connection with this surface, a second theorem found by Mr. Schwarz, a theorem as remarkable as the preceding, but which, I believe, was not published; here it is:

When a surface with zero mean curvature can be cut by a plane in such a way that, all along the section, it has its elements perpendicular to this plane, it presents, with respect to the plane, the symmetry which exists between an object posed on a plane mirror and the image of this object.

This theorem is applicable to the surface in question, because when one raises the wire square, all the elements of the film are vertical along the ridge of the small mass which attaches it to the liquid, and are consequently perpendicular to the horizontal plane which would pass through this ridge. Since the surface contains the four lines of the sides of the square frame, one sees that one can also apply the first theorem to it, and one can conceive it in any extent. Taken thus in its totality, it enjoys this singular property that, if one considers it as bounding a liquid mass, the space which it leaves vacant constitutes, in hollow, a shape identical to that of the filled space.

The two theorems of Mr. Schwarz thus give a marvellous ease to the prolongation of a great number of surfaces of zero mean curvature beyond the limits given.

[75] *Ueber die Minimalfläche die von einem doppelt-gleiclischenkligen räumlichen Vierech begrenzt wird.* Gottingen, 1868.

[76] *Analytisch-Geometrische Untersuchungen* (Bullet. of the Roy. Soc. of Gottingen, 1867, p. 237).

But that is what the experiment confirms: I formed from wire the most varied and oddest closed contours, and, on their emergence from the glyceric liquid, each one of them was always shown, either immediately, or after a small operation of which I will speak, to be filled entirely by only one film.

When a closed contour has a certain complication, the film which develops in it is often accompanied by additional films; but it is then enough to burst those, which is done easily with a filter paper point, leaving but one single film.

Sometimes also a portion of the film takes, when the system leaves the liquid, a bad direction, and will stick, by means of a liquid edge, to another portion of this same film; in this case, one cannot burst; but, by varying the position of solid contour while it is withdrawn, one ends up avoiding this problem. With these precautions, one constantly succeeds in forming a single film attached to the totality of a solid contour, and one produces thus, by an extremely simple means, very beautiful and very varied surfaces.

To give an idea of the singularity and complication of closed contours that I subjected to experiment, I will describe two of them here in a few words: the first consists of a knot similar to that which one would tie in the middle of a cord without tightening it, so that its various parts are notably distant from each other, and joining the two loose ends. As for the second, the wire starts with a straight and vertical segment, then bends to form a little more than two whorls of a helix whose axis is parallel to the straight portion, then is bent in another helix also of two whorls, whose axis is horizontal, and which wraps the first at a sufficient distance; it swells then in a third helix of two whorls also, with vertical axis surrounding the system of both others at a suitable distance; finally it attaches to a point of the straight wire, and thus closes the contour. It is with the latter contour that the success was most difficult: it formed extra films such that one could not burst them without making burst the whole, and it is only by varying the way in which I withdrew the contour from the liquid that I arrived at the result sought.

§ 145. In the second place, although, with a given contour, the film surface that one thus creates always appears the same in successive tests suitably carried out, experiment still makes it possible to note that there is an infinity of other surfaces with zero mean curvature which can be supported on the same contour. If, after having produced a film in a nonplanar closed contour chosen arbitrarily, one makes adhere to this film a wire ring provided with a fork by which one holds it, and wetted beforehand with glyceric liquid, then one draws this ring in a direction perpendicular to its plane, as if one wanted to draw it aside from the film, the film is not detached any, so that it extends then between the first contour and this ring; only, if one moves it away too far, equilibrium is destroyed, the portion of the film which attaches to the ring necks quickly, there is separation, and the film is restored in its former state, while a plane film will occupy the ring; but, on this side of the limit of spacing, the shape is perfectly stable. However, under this last condition, as the film continues to be based on the first contour and since its form is modified, it represents a new surface with zero mean curvature through this same contour. One can simultaneously use two rings which one makes adhere to two different portions of the film and which one draws aside at the same time, so the film extends towards one and the other at the same time; one can, moreover, substitute for the circular rings rings of any other form, and always the experiment succeeds. One produces as many different surfaces in this manner as is wanted, and who all pass through the first contour.

If it is conceived, by thought, of one of these new surfaces prolonged beyond one of the rings, one will be convinced, with a little reflection, that, since its two principal radii of curvature must be equal everywhere and of opposite signs, the prolongation in question cannot close upon itself, and must consequently extend ad infinitum. It follows that none of these surfaces could fill the first contour with a finite portion.

But the results of the preceding paragraph make it possible to state this new principle: Given an arbitrary closed contour, planar or not, among all surfaces with zero mean curvature which can be based on its totality, there is always at least one whose finite portion can entirely fill it.

CHAPTER V.

Tension of surfaces and liquid films: history. – Film systems. Laws to which they are subject; how they develop; general principle which governs their constitution. Theoretical demonstration of their laws.

§ 146. The surface layer of liquids has a singular property consisting of being in a continual state of tension, and, consequently, making a continual effort to contract. In order to to put in perspective this property, which plays a great role in the phenomena with which we occupy ourselves, we will trace its the history until the end of 1869.

The idea of a tension in the surface layer of liquids was put forward, for the first time, I think, in 1751, by Segner, who especially employs it in the determination of the shape of drops. In the Report of which I already spoke (note of § 3), an extremely remarkable memoir for the time when it was written, Segner considers a liquid drop posed on a solid surface that it does not wet, discusses the mutual forces of the molecules which make it up, and arrives at results whose truth is recognized today: he finds that the forces on which the shape of the drop depends reside in a surface layer of which the thickness is equal to the radius of molecular attraction, and that these same forces produce the normal pressures whose intensity is all the greater as they emanate from curved portions of surface; finally he shows the existence, in the layer concerned, of a tension having everywhere the same intensity, a tension which he derives from the attractions of the molecules in the tangential direction, and from the curvature. But this discussion, though very reasonable, is long, awkward, and not very understandable, in my opinion, in several points, and I doubt that it could convince anybody of the reality of the tension; it contains, moreover, errors due to the insufficiency of the concepts that one had then; also Young says on this subject: "Segner showed how the principle can be deduced from the doctrines of attraction; but its demonstration is complicated and is not perfectly satisfactory."

At all events, Segner, then applying to his deductions a clever method of calculation and experiment, arrives at this other result: that, in drops formed of the same liquid, but having different shapes and dimensions, the tension has the same value, which corresponds to saying that it is independent of the curvatures; this principle is also recognized true today.

Lastly, Segner seeks the ratio between the tension of mercury and that of water, and it finds it equal to 3.5 approximately; but as he misses the notion of mean curvature, and takes account, in his calculations, only of the meridian curvature, his numerical results are necessarily inaccurate, and the ratio above is much too small.

§ 147. In 1756, Leidenfrost[77] announced the contractile force of soap bubbles; it is based on the fact, which he was, I think, the first to describe, that if the blowing tube is left open, the bubble shrinks gradually back on itself until it vanishes, by expelling the air it contains through the tube.

But Leidenfrost does not extend this contractile force to a general property of liquid surfaces; he attributes it to the lubricating part of the soap, which, according to him, separates from the other elements of the solution, and constitutes a thin film on the outside of the bubble; moreover, still according to him, the aqueous part of the film has a force of opposed nature, being an explosive force; it is the latter which makes burst the bubble.

Monge[78], in 1787, speaks about the tension, but in a simply hypothetical way; he

[77] *De aquae communis nonnullis qualitatibus tractatus.* Duisburg.

[78] *Mémoire sur quelques effets d'attraction ou de répulsion apparente entre les molécules de matière* MÉM. DE L'ACAD. DES SC. DE PARIS).

suggests that it could be used to give an explanation of capillary phenomena.

§ 148. Young, in a celebrated work[79] published in 1805, used the principle of tension to explain a great number of capillary phenomena. He does not fall into the same error as Segner, that is to say that he considers at the same time the effects of the two perpendicular curvatures: the tension determines, at each point of the liquid surface, a pressure or a normal traction proportional to the sum of these two curvatures, and from this are born the phenomena: in a capillary tube, for example, when the liquid is raised and has thus a concave surface, normal tractions due to the tension support the weight of the column, and when the liquid is depressed, its surface being then convex, the normal pressures produced by the tension balance the hydrostatic pressure of the surrounding liquid, which tends to raise the column.

As for the legitimacy of the principle of tension, Young relies simply on capillary phenomena being able to be attributed to mutual attractions of only the surface particles; liquid surfaces "must be made up of curves of the nature of the catenary, which are supposed to be the result of a uniform tension in a surface which resists the pressure of a fluid." Finally he tries to show that one can find a cause of normal pressures and tractions only in the play of attractions and repulsions of molecules, and he leaves thus in doubt if the tension actually exists, or if, by normal forces, the things occur as under the influence from a tension.

Laplace[80] points out the research of Segner and Young, but he announces the inaccuracy of the reasoning of the first, and points out that the second did not try to derive his assumptions from molecular attraction.

§ 149. Doctor Hough, whom I have already mentioned (§ 118), in research published in 1830, appears to have arrived, on his part, without knowing the work of Segner and Young, at the idea of a contractile force or tension existing on the surface of liquids and constantly trying to shrink this surface; he seems to be led there simply by the consideration of the spherical shape of liquid drops and soap bubbles. He gives as an example of the effects of this tension the elasticity of mercury globules, which, when one increases their surface by compressing them and then leaves them to themselves, regain their spherical shape.

To explain the tension, Hough points out that the molecules of a surface are not in the presence of external molecules of the same species which can counterbalance their mutual attractions, while inside the liquid each molecule being completely surrounded by similar molecules, attractions are neutralized in all directions. He adds that one can, up to a certain point, consider the relative tensions of the various liquids by comparing the sizes of the largest drops of these liquids which preserve their spherical form appreciably when they rest on substances which they do not wet, or when they are suspended from bodies which have for them the strongest attraction.

He also makes depend on the contractile force the rise and the depression of liquids in capillary spaces, as well as attractions and apparent repulsions of floating light bodies; but, being unaware of former research, he tries to establish a theory of these phenomena, an erroneous theory, in which atmospheric pressure intervenes. Finally he admits, as a consequence of the tension, a pressure is exerted on the air which constitutes a bubble in the interior of a liquid, that is to say on that which is contained in a film sphere or a segment of a film sphere; but, as I said, he comes, with regard to the

[79] *Àn essay on the cohesion of fluids* (PHILOS. TRANSACT. 1805).

[80] *Exposition du système du monde.* Paris. – the first edition is 1796; but the remarks of Laplace being posterior to the work of Young, they appeared for the first time in a later edition; that which I consulted is from 1813.

relation between this pressure and the diameter, to a completely inaccurate law.

§ 150. Let us say some words on the memoir[81] of Mile, which appeared in 1838. The author expounds a theory of capillary phenomena, in which he also makes use of the tension; but, as he considers it, that tension would obey laws now inadmissible. He starts from the principle that, in a homogeneous liquid, the molecules seek to be arranged in a regular and identical way everywhere; however he shows that this identity is possible only when the surface of the liquid is plane, and he concludes that, in consequence of the abnormal arrangement of the molecules, curved surfaces constantly try to become plane; that effort, which results from the tensions, is all the more energetic as the curvatures are pronounced, and this same effort determines, on the mass, a pressure in the case of convex surfaces, and a traction in the case of concave surfaces; finally that from this are born capillary phenomena. According to Mile, one sees, tension would exist only in curved surfaces, and it would vary with the curvature.

§ l50bis. Mossotti, in a work[82] of the year 1843, applies, like Young, but in a more complete manner, the principle of tension to capillary phenomena. He establishes independence between the tension and the curvature, and one can conclude from his analysis that the constant coefficient which, in the general expression of the capillary pressure, multiplies the sum $\frac{1}{R}+\frac{1}{R'}$ of the two principal curvatures, is nothing other than the tension per unit length, i.e. the force exerted by the surface layer of the liquid on the two sides of a normal section of unit length. His formulas make it possible to evaluate this tension, for a given liquid, according to its density and the height to which it rises in a capillary tube of fixed diameter, and Mossotti is, I think, the first who sought similar evaluations; he finds thus, for example, the following values, which express, for various liquids, the tension in milligrams per millimeter length:

water	7.56,
mercury	44.40,
alcohol	2.59,
spirits of turpentine	2.86,
olive oil	46;

only he does not give any indication concerning the temperature.

Finally, he admits the existence of one tension on the common surface of two liquids in contact which do not mix; he inserts it in his formulas relating to liquids superimposed in the same capillary tube, and he obtains, on the basis of measurements taken in these cases of superposition, the evaluations which follow:

oil and water	0.79,
mercury and water	37.67,
mercury and alcohol	36.04.

As for the cause of the tension, Mossotti, while considering the play of gravitational and repulsive forces between molecules, arrives at a conclusion identical to that of Poisson, being that, starting from the surface until a depth about equal to the radius of activity of molecular attraction, the density is lower than that of the interior of the liquid, and he admits that, from this larger spacing of the molecules, there results, in the layer concerned, an excess of attraction in the direction parallel to the surface; this is the excess which, according to him, constitutes the tension.

§ 151. It is from the principle tension that Mr. Henry, in his verbal communication (year 1844) on the cohesion of liquids (§ 116), deduced the cause of the pressure of a

[81] *Versuch einer neuen physikalischen Theorie der Capillarität* (ANN. OF Mr. POGGENDORFF, vol. XLV, pp. 287 and 501).

[82] *Lesioni elementari di Fisica matematica.* Florence.

hollow bubble on the imprisoned air, and the law which governs this pressure (ibid). Only he regards the bubble as being able to be comparable with a filled sphere reduced to its pressing surface, i.e. he attributes the phenomenon to the tension of the outside of the film, without taking account of that of the other face.

He adds: “One easily demonstrates the contractile force of the surface of a bubble by blowing a large bubble at the end of a broad tube (about an inch in diameter); as soon as the mouth is moved away, one sees the bubble shrink quickly, and at the same time an intense draught is driven out of the tube against the face. This effect is not due to the rise of the hot air of the lungs which was used to inflate the bubble, because it occurs in the same way when cold air is employed, and also when one holds the bubble vertically above the face, so that the current is downward. ”

It is, indeed, impossible to conceive this shrinkage of the film and this expulsion of the interior air, without admitting that the film is in tension; the facts on which Mr. Henry is based can thus be viewed as an experimental proof of the reality of the tension, at least in films.

§ 152. In a work[83] from 1845, Mr. Hagen applied, as did Young and Mossotti, in a rigorous way to capillary phenomena the principle of tension, although he regards this force as hypothetical only, and he arrives at several of the results already given by Mossotti.

Taking for his starting point only the condition that there must be equilibrium between the hydrostatic forces and the tension, he mathematically shows the uniformity of this last force in two simple cases, that of a liquid raised or lowered between two solid planes, and that of a liquid raised or lowered in a cylindrical tube.

Mr. Hagen seeks, by three different processes, the value of the tension on the surface of water. In the first place, he carries out a series of measurements of the rise of the liquid between two parallel vertical planes, of which he varies the distance within wide limits. By means which he indicates, he determines each time, in a vertical section perpendicular to the two planes, the spacing of those, the height of the low point of the liquid surface between them, and that of the points where this surface comes to touch them; then, applying to these three elements a rigorous method of calculation, he deduces a value for the tension.

In the second place, he also measures the rise of water in cylindrical tubes whose internal diameter varies from 1.23mm to 3.42mm, and, by an also precise calculation, he again deduces the tension. This second process provides him results concordant with the first, and he deduces, on average, for a temperature of 10°, a value which, translated into milligrams per millimetre length, is equal to 7.53.

He notes, at the same time, that a variation of a few degrees in the temperature is without appreciable effect on the tension.

The third process is founded on the flow of liquid drop by drop: the tension is equal to the weight of a drop divided by the external perimeter of the opening. The weight of one drop is obtained by collecting a given number of these drops in a small vessel, and weighing the liquid collected. Mr. Hagen points out that this process is less exact than the preceding, because each drop, while being detached, leaves on the opening a small portion of its volume.

The measurements above lead the German physicist to this singular conclusion, that the tension of water is decreasing until a certain limit, when the liquid remains exposed with the air: the value 7.53 corresponds to a fresh surface.

Finally, Mr. Hagen reproduces the opinion, already old, that the surface layer of liquids has less mobility than the interior; according to him, and contrary to the ideas of

[83] *Ueber die Oberfläche der Flüssigheiten.* (MEM. BERLIN ACAD., 1845, and ANN. OF MR. POGGENDORFF, 1846, vol. LXVII, pp. 1 and 152).

Poisson, this layer is denser than the remainder, and he attributes the tension to closer molecules attracting each other with more energy. We will reconsider these ideas.

The next year, the same scientist gave a follow-on memoir[84], where he again measures tensions. He determines initially, by the process of rise between two parallel planes, the tension of water which remained for several hours in an open vessel, and the value which he finds, reported per millimètre[85], is only 4.69, instead of the 7.53 which corresponds to a fresh surface; thus the fact of the progressive reduction in the tension of water is confirmed. Mr. Hagen subjects to the same process pure alcohol and olive oil; the tensions obtained were respectively, per millimetre length, 2.32 and 3.42. He did not notice, with regard to these two liquids, any waning in the tension.

He describes then a new process: a horizontal flat wooden ring is suspended from a sensitive balance, and is balanced; the contact between its lower face and the surface of the liquid is established, then, by means of weights very gradually added on other side of the balance, one makes the ring rise little by little, which raises a certain quantity of liquid, and one stops when its surface leads vertically to the outside and inside edges of the ring. The force with which the liquid then draws the ring down is composed of two parts, being: 1° the weight of the portion of the raised liquid located directly under the ring, i.e. that of an annular cylinder of this liquid having for base the lower face of the ring and for height its distance of above the level; 2° the tension of the curved surfaces which lead to the two edges of the ring. If one subtracts from the weight added on other side of the balance the first of these two quantities, the remainder thus represents the total tension, and, to have the tension per unit length, it is enough to divide this remainder by the sum of the lengths of the two edges.

This process, employed with regard to pure alcohol and olive oil, provided the respective values 2.34 and 3.41, which are, it is seen, very nearly identical to the preceding ones. Mr. Hagen adds then: "the agreement of these results with those deduced from the rise between parallel planes, or the capillary phenomenon itself, does not leave anything to be desired, and thus verifies the assumption that the surface tension, which was measured directly in the last case, is the only cause of capillary phenomena."

Thus Mr. Hagen here ceases regarding the tension as a simple assumption, and sees in the above results a proof of its reality.

Mr. Hagen also determines the tension of mercury; the process of depression in cylindrical tubes gives him, per millimetre length, 36.26, and that of drops 41.14. As, in this last procedure, the surface is necessarily fresher, Mr. Hagen infers from the difference of the two results that, on mercury just as on water, the tension decreases gradually.

Solutions of gum arabic, with various degrees of viscosity, provided him tensions very close to that of water.

Supposing, in consequence of his ideas on the origin of the tension, that soap water has a tension stronger than that of pure water in order to give a persistent foam and to easily blow bubbles, he measures the tension of a weak soap solution, and finds it only 3.72; he recognizes, consequently, that he had been mistaken.

Let us say here that all the measurements reported in this second memoir were taken at temperatures between 18° and 19°.

Mr. Hagen finishes by deducing from the whole of his work the two following consequences, that he presents, however, with reserve, considering the small number of liquids subjected to observation:

[84] *Mem. Berlin Acad.*, 1846, and Ann. of Mr. Poggendorff 1849, vol. LXXVII, p. 449.

[85] In this second memoir, Mr. Hagen expresses the tensions in fractions of a gram; but, I do not know why, he takes for his unit of length the line of Paris; I thus converted all the values to the millimetre, by dividing them by 2.256, the value of the line of Paris in millimetres.

1° the degree of fluidity does not have an influence on the tension;

2° the tension is weaker as the liquid wets the other bodies better. Indeed, alcohol wets better than oil, because when alcohol is deposited on a coated oil plate the layer of this last liquid moves; oil wets obviously better than water, and water better than mercury; however the tension is growing from the first to the last of these liquids.

§ 153. In 1849, the same scientist wrote a remarkable work[86] on the experiments of Savart relating to the effects produced by the impact of the continuous parts of two animated liquid jets of directly opposed movements, experiments about which I will later speak (§§ 232 and 233) in detail. Mr. Hagen deals only with the case of equal openings and equal loads; then there is formed, around the point of meeting of the two jets, a circular plane film normal to these two jets, and from the edge of which escape a multitude of droplets.

Mr. Hagen first draws attention to these facts: that, according to the observations of Savart, the radius of the liquid disc is always much lower than the height of the load, and even, in general, does not reach a quarter this height, and second, that the drops spray off the edge of the disc only at a low speed; he concludes that the velocity at which the liquid spreads out is mainly destroyed at this edge, and he attibutes the destruction in question to the tension of the two faces of the film in the direction perpendicular to the radius: if one breaks up each of the two faces of the film into infinitely narrow concentric rings, all these rings centered where the two jets meet, increasing in diameter while going towards the edge; it follows that their tension in the direction of their circumference must be overcome, which cannot take place without loss of velocity; or even, which amounts to the same thing, the tension in the direction of the circumference determines, on behalf of each point of the ring, a capillary pressure directed along the radius and in contrary direction to the movement of the liquid; a pressure which acts as a force retarding this movement, and thus produces the limitation of the disc.

Mr. Hagen described, in support of this theory, the curious experiments given here:

If one introduces into the liquid disc a taut wire which crosses it perpendicularly a little distance from the center, continuity is broken, and a notch is formed in the disc starting from the wire; but the two edges of this notch are not simply straight lines directed radially; they are curved and turning their convexity towards the notch; the tension above draws each one of these edges in a continuous way towards what remains of the disc.

If, instead of only one wire, one employs two of them fixed a small distance apart on the two sides of the ascending vertical radius, the portion of the disc between the two wires forms a kind of jet which reaches a height not very much lower than the height of the load; here the side extension does not take place, and Mr. Hagen concludes, moreover, from the great height of this jet, that the destruction of the velocity in the disc is not due to the mutual impact of the two jets.

He then subjects his theory to calculation, and arrives at the following formula, in which R is the radius of the liquid disc, ρ that of the two jets, v the speed of the liquid in these jets, T the tension of the liquid per unit length, g gravity and γ the weight of a unit volume of the liquid:

$$R-\rho=\frac{\gamma}{2gT}\rho^2v^2.$$

As γ, g and T are constants, and as, moreover, the radius ρ of the jets is always very small relative with the radius R of the disc, this formula expresses, one sees, that the radius of the disc is appreciably proportional to the square of that of the jets and to

[86] *Ueber die Scheiben welche sich beim Zusammenstossen von zwei Wasserstrahlen bilden, und ueber die Auflösung einzelner Wasserstrahlen in Tropfen* (ANN. OF Mr. POGGENDORFF, vol. LXXVIII, p. 451).

the square of the rate of flow, or, which amounts to the same thing, to the area of the openings and the load; such are the two laws stated by Savart.

To obtain a further verification of this same formula, Mr. Hagen derives, by means of the values of R, γ and ρ deduced from the observations of Savart and his own observations, the value of T corresponding to water, and finds a result which agrees rather well with that which, in his first memoir, he had deduced from capillary phenomena in the case of a fresh surface: the value to which he came thus is 7.74, and one notice that, in the experiment of the liquid disc, the surface necessarily must be fresh, since it is always being renewed. Mr. Hagen remarks, moreover, that the measurement of the diameter of the liquid disc is difficult, because the drops are detached irregularly from the edge, and that at the same time they strongly stretch the film, so that its diameter changes continuously in each direction.

All these agreements, one understands, constitute a further proof of the existence of the tension; here, indeed, the role assigned to this force is absolutely different than in the experiments of the two preceding memoirs.

Finally Mr. Hagen obtains, to represent the thickness b of the film at an arbitrary distance r from the center, this other formula:

$$b = \frac{\rho^2(R-\rho)}{r(R-r)},$$

from which he deduces the consequence that the minimum thickness of the film is not towards its edge, but corresponds to $r = \frac{R}{2}$ i.e. is at the middle of the radius.

Although there is, in my opinion, a little vagueness in the role that Mr. Hagen makes tension play in the phenomenon concerned, however the effects produced by a wire stretched across the liquid disc, the agreement of the first of the formulas above with the laws of Savart, and the coincidence between the numerical value of the tension of water deduced from this formula and that which the author had found by an essentially different method, hardly make it possible to doubt that the theory in question is the expression of the truth. It is highly regrettable that Mr. Hagen did not seek to assure himself by experiment that the film really has a minimum thickness about the middle of its radius; it would have been a decisive test.

I must present here, with regard to the results of calculation, two significant remarks which appear to have escaped the author.

In the first place, if one goes to the edge of the film, i.e. if one makes, in the second formula, $r = R$, one finds $b = \infty$, which is impossible. But if one makes $r = R - \rho$, one obtains $b = \rho$; thus, even at a distance from the edge which is equal only to the radius of the jets, the thickness of the film would be only equal to this radius: for example, with openings of radius 1.5mm, the film of Savart reached a radius of 190mm; at a distance from the edge equal to only 1.5mm, the thickness of this film was thus, according to the formula, only 1.5mm. One sees consequently that, in spite of the increase in thickness starting from the middle of the radius, this thickness remains however extremely small until very close to the edge, and it is in the small interval remaining which the increase to infinity should be made; however, towards the edge of the film occurs, we know, a particular phenomenon that the formula could not include, being the formation of drops, which carry away the liquid which would increase in thickness as indicated by the formula.

So that the theory is absolutely complete, it is thus necessary to understand the formation of these drops; Mr. Hagen acknowledges that he does not find a satisfactory explanation, and that was to be expected, because the phenomenon depends mainly, as will be seen (§ 426), on a principle which I expounded in my 2nd Series; however Mr.

Hagen could not have known of this, which appeared very close to the same time as his Report.

In the second place, if, in the same formula, one makes $r = \rho$, one finds also $b = \rho$, from which it would follow that near the origin of the liquid disc, i.e. at a distance from the center equal to the radius of the two jets, the film would not have more thickness than close to the edge; however Savart says expressly that the film is thicker at its central part than at its edge. But at this central part also occurs a particular phenomenon whose calculation Mr. Hagen would have not easily have accounted for: the liquid of the two jets cannot abruptly change the direction of its movement into directions at right angles; the liquid streamlines of these jets, in the vicinity of the place where they meet, must inflect to pass in a continuous way from one direction to another; it follows that there is necessarily a notable increase in thickness towards the central part of the film, but, a little beyond that, its two faces can be viewed as being under the conditions of the calculation in question.

§ 154. If the surface layer of a filled liquid mass is in a state of tension, then liquid films must be comparable with stretched membranes, since tension exists in both their faces. It is seen, accordingly, that the tension of a film is double that of the surface layer of a filled mass formed with the same liquid: by adopting, for example, for the tension of water the value 7.53 milligrams found by Mr. Hagen, the tension of a water film would be 15.06 milligrams.

§ 155. In 1855, Mr. J. Thomson[87] explained by differences in tension some singular phenomena produced on the surface of some liquids:

If one gently deposits a small quantity of alcohol in the middle of the surface of the water contained in a glass, one sees this surface fleeing quickly on all sides, and if the portion of the solid wall which rises above the liquid level is wetted by water, one even sees the liquid going up along this wall and accumulating there so as to form sometimes, at a considerable height above the level, a horizontal ring whose weight then makes it fall down.

If one spreads a thin layer of water on a well-cleaned horizontal solid surface, and in the middle of this layer one deposits a little alcohol, water withdraws all around immediately, leaving a hollow space where the solid surface is exposed or rather is covered only with an excessively thin alcohol film.

This happens because, the tension of alcohol being lower than that of water, equilibrium cannot exist any more when a portion of the surface is covered with alcohol; due to their excess of tension, the aqueous portions of the total surface violently withdraw from the alcoholic portion.

Mr. Thomson attributes to the same cause the curious movements that one usually observes in the thin layer of wine which wets, above the liquid level, the interior wall of a glass containing this liquid; indeed, the thin layer in question loses its alcohol by evaporation more quickly than the remainder, and thus acquires an excess of tension.

§ 156. In a Note[88] communicated in 1858 to the Royal Society, Sir W. Thomson, on the basis of the principle of tension such as posed by Young, gives, due to this force, the following analytical expression for the pressure p exerted by a hollow bubble on the air inside:

$$p = \frac{4T}{r} + \Pi,$$

an expression in which T is the tension of the liquid forming the bubble, r the radius

[87] *On certain curious motions observable at the surfaces of wine and other alcoholic liquors* (PHILOS. MAGAZ., 4th series, vol. X, page 330).

[88] *On the thermal Effect of drawing out a Film of liquid* (PHILOS. MAGAZ., 4me series, vol. XVII, p. 61).

of this bubble, and Π the atmospheric pressure. This formula, that the author does not show, is exact, as we will see soon.

By a series of calculations, Sir W. Thomson arrives then, relative to the liquid films which develop, at a principle which we will report later.

§ l56bis. Langberg showed, in an article[89] published in 1859, that capillary forces have a notable influence on hydrometric measures, the instrument being drawn down by the weight of the liquid raised against its stem. To evaluate this weight, Langberg utilizes the tension exerted at the circumference whereby the surface of the liquid contacts the solid stem; he appears, however, to regard this force only as hypothetical. The tension varying from one liquid to another, one understands that when one uses a hydrometer for the measurement of the densities of various liquids, the results can be quite comparable only if one takes into account the influence in question, an influence which is not negligible.

§ 157. In 1863, Wilhelmy[90] tried to submit to an experimental test a proposal stated by Wertheim, whereby the weight of the liquid raised by capillarity along a solid surface that this liquid wets, varies with its nature and the curvature. The experiments of Wilhelmy seem to confirm this proposal; however if one considers the weight as supported by the tension of the liquid, and as being used to measure it (§§ 152 and 161), these same experiments tend to throw some uncertainty on the precision of the values of the tensions thus obtained.

§ 158. Today one has theoretical demonstrations of the reality of the tension, and new means to demonstrate it by experiment.

The first of these demonstrations was given, in 1864, by Mr. Lamarle, in the first part of his beautiful memoir *Sur la stabilité des systèmes liquides en lames minces*[91], a memoir which I will return to; it supposes a filled liquid mass, entirely free, subjected to only its molecular attractions, and consequently of spherical form[92].

Mr. Lamarle manages to conclude not only that the surface layer of such a mass is in a state of tension, but, moreover, that this tension is independent of the radius of the sphere; finally he points out that the same results extend without difficulty to all liquid surfaces in equilibrium, i.e., for these surfaces and for the same liquid, the tension is constant and independent of the curvatures, as had been established by Mossotti and Mr. Hagen.

I will not reproduce the demonstration as expounded in the memoir; I will modify it, according to the indications of Mr. Lamarle himself, by applying it to films; it will be thus more easily understood. In this form, it follows, in the end, that of Mr. Henry (§ 151), made more precise and more complete.

Let us suppose a film sphere, a soap bubble, for example, and conceptually cut it by a plane which divides it into two hemispheres; let us imagine this plane as solid, which will not damage equilibrium, and consider in particular one of the hemispheres. The film which constitutes it presses, we know, on the air imprisoned between it and the plane, and this volume of air reacts, by its elasticity, with an equal force; the film hemisphere and the plane are thus pushed one in one direction, the other in the opposite direction, from which it follows that there is a force exerted by the film all along the

[89] *Ueber den Einfluss der Capillarattraction auf Àräometermessungen* (ANN. OF Mr. POGGENDORFF, flight. CVI, p. 299)

[90] *Ueber die Abhängigkeit der Capillaritäts-Constanten des Alkohols von Substanz und Gestalt des benetzten festen Körpers* (ANN. OF Mr. P0GGENDORFF, 1863, vol. CXIX, p. 177), and *Ueber die Abhängigkeit der Capillaritäts-Coefficienten der Flüssigkeiten von der chemischen Beschaffenheit und Gestalt der festen Wand* (ibid 1864, vol. CXXII, p. 1).

[91] MÉM. DE L'ACAD. DE BELGIQUE, vols. XXXV and XXXVI.

[92] the exactitude of this demonstration was disputed by Dupré, and was maintained by Mr. Lamarle (see *Comptes rendus,* vol. LXIV, pp. 593, 739 and 902).

small band by which it adheres to the plane; however an equal and contrary force is obviously exerted along the same band by the other hemisphere; there is thus, over the entire length of the narrow band concerned, force in two directions opposite and perpendicular to this length; in other words, there is tension of the film. Lastly, as nothing determines the direction of our cutting plane, it follows that the same tension exists in all the extent of the film, and has the same value in all the tangential directions around each point.

The tension is regarded here as a pull; but the film resisting by an equal and contrary force, one can as well look at this last force as comprising the tension. Under this point of view, the tension is a contractile force, a continual tendency of the film to return on itself by decreasing in extent.

The mode of demonstration above leads to an expression of the tension in measurable data. Let us indicate by p the pressure per unit area that the film exerts on the imprisoned air, and consequently also the pressure of the inside outwards due to the reaction of this air. The total force which acts thus from inside outwards on one of the film hemispheres and tends to separate it from the plane, is necessarily equal to that which pushes the plane itself; it thus has as its value the product of its area by the quantity p, i.e. $\pi r^2 p$, where r is the radius of the film sphere; I neglect here the small difference between the radius of the outside of the film and that of the interior face, because of the extreme thinness of the liquid films. This expression represents at the same time, according to what I mentioned above, the total tension over the length of the narrow band along which the film is cut by the plane, and, consequently, to have the tension per unit length, the tension that I will name t, it is enough to divide this same expression by the length $2\pi r$ of the band into question, which gives[93] $t = \frac{rp}{2}$. But one saw (§ 117) that if d is the diameter of the film sphere, h the height in millimetres to which the liquid forming the film would rise in a capillary tube a millimetre in diameter, and ρ the density of this liquid, the pressure which the film exerts is equivalent, for a surface of an arbitrary extent, and, consequently, for a unit area, with the weight of a water column having for base this surface and, for height, $\frac{2h\rho}{d} = \frac{h\rho}{r}$. By taking for the unit of area the square millimetre, the quantity $\frac{h\rho}{r}$ thus expresses in milligrams the pressure which we indicated by p; substituting in the expression for t found higher, it becomes:

$$t = \frac{h\rho}{2},$$

which gives, in milligrams, the tension of the film per unit length.

This formula would also follow from those of Mossotti; it also follows from the calculations of Mr. Hagen relating to liquids in cylindrical tubes (§ 152), when one supposes the diameter of the tube equal to 1 millmeter, and when one regards the upper surface of the column as forming a concave hemisphere, which is allowed in the case of so small a diameter.

§ 159. This same formula not containing r, one sees that the tension in question is independent of the radius and consequently of the curvature of the film.

The constancy of the value of the tension, whatever the curvature of the film sphere, will be fully confirmed further by experiment (§§ 175 and 179).

Now if it is imagined that the radius of the film sphere grows to infinity, the film will become a plane, and it follows from what precedes that it will still have the same tension. However, one arrives in this manner at the tension of a plane film only by regarding this film as indefinitely wide, and thus assigning to it unrealizable conditions;

[93] If one represents with Sir W. Thomson (§ 156), by T the tension of the liquid, i.e. that of one of the faces of the film, one has $t = 2T$; by substituting this expression in the equation above, solving for p, and adding the atmospheric pressure Π, one recovers the formula of Sir W. Thomson.

one could consequently wonder whether a limited plane film, for example a film of glyceric liquid formed in a wire ring, a film which does not exert any pressure on the air, has indeed a tension; however, we will see, in the paragraphs we have just quoted, experiments which prove that a limited plane film is really in tension, and that its tension is equal to that of curved films formed from the same liquid.

§ 160. I should not neglect to mention a second point of view under which Mr. Lamarle, in the Report that I quoted, considers the tension; here, according to ideas generally accepted today, the density of the surface layer is less than that of the interior of the liquid, and consequently, in this layer, the spacing of the molecules is larger; if thus, by a change of the form of a liquid mass without change of volume, the extent of the surface layer is suddenly decreased and that thus part of the molecules of this layer go in the interior, these molecules are closer together; however, attraction, by its nature, immediately tries to bring the molecules closer; it must thus reduce the surface layer to its least extent, since, towards there, it exerts its tendency in an active way.

Thus, according to Mr. Lamarle, the tension is due to the constant tendency of attraction to bring together the molecules as much as possible, through a reduction in the surface area.

§ 160bis. Mr. Marangoni is concerned[94], in 1865, with the spreading out of a liquid drop on the surface of another liquid, like an oil drop on water. He attributes the phenomenon to the tension, and, by judicious considerations, he arrives at the following law:

There is spreading out if the tension of the subjacent liquid exceeds the sum of the respective tensions of the liquid whose drop is formed and of the surface of contact of the two liquids. When the opposite condition takes place, the drop takes and preserves a lenticular form.

The author advances that a soap water film developed in a solid ring can be pierced by a liquid je without breakingt, if the tension of the liquid of this jet is equal or higher than that of the liquid of the film; it happens, for example, with water jets through soap, pure water, oil, carbon bisulphide, and even mercury; but if the difference of the tensions is in the opposite direction, as with ether or alcohol jets, the film bursts immediately. It is even enough, so that it disappears, to simply touch it with a point wetted with one of these two liquids.

In order to show that adherence between two liquids modifies the tension on their common surface, Mr. Marangoni introduces into a capillary tube a finger formed of two different liquids in contact, for example of water and carbon bisulphide; then the surface of contact of the two liquids turns its concavity on the same side as the free face of the carbon bisulphide; however, when the tube is placed horizontally, one sees the small column moving toward the side with the free face of water. That being so, if one indicates by A the resultant force, in the direction of the axis of the tube, of the tension of the meniscus at the end of water, and by C and B the similar quantities relating to the free faces of the carbon bisulphide and the common surface, the movement observed indicates that one has $A > C + B$, from which $B < A - C$. Thus, not only the tension of the faces in contact is modified, but, in the case of the two liquids in question, it is less than the difference of the tensions.

§ 161. Dupré published in parts, from 1865 to 1868, a remarkable work[95], where he treats by new methods a continuation of questions concerning molecular forces, and where he gives also a demonstration of the real existence of the tension:

[94] *Sull' espansione delle goccie of a liquido galleggianti sulla superfice di altro liquido.* Pavie.

[95] Fifth, sixth and seventh memoir *Sur la théorie mécanique de la chaleur* (ANN. DE CHIM. ET DE PHYS. DE PARIS, 4th series, volumes VI, VII, IX, Xi and XIV).

He establishes initially that, to separate a liquid mass into two, either by perpendicular wrenching, or by slip, it is necessary to overcome a resistance, and that, reciprocally, when two liquid surfaces meet, there is a force residing in their surface layers which induces the meeting; he names it *joining force.*

On the basis of this principle, he shows that a liquid mass cannot change form with reduction in its surface area, without molecular work proportional to this reduction being produced by the joining force[96]. However this always-present force must unceasingly tend to exert the force in question, and, consequently, to minimize the surface area; the surface layer of liquids thus has a contractile force, or a tension.

Dupré indicates several extremely simple experiments by means of which one measures the tension expressed either in the free face of a filled liquid mass, or in a liquid film. In one of these experiments, for example, a weight is raised by the tension of a plane film which appears on a vertical rectangular metal plate, whose lower horizontal edge presents, in its middle, an also rectangular notch. This plate being beforehand wetted with soap water, if one applies against it across the notch, and at the level of the higher edge of the notch, a narrow and very light solid band, a little longer than the width of the notch and being wetted with soap water, then as one slides this small band from top to bottom, a liquid film is necessarily formed in the portion of the notch thus swept; however, as soon as releases the small band, it goes up abruptly in spite of its weight.

Duprés attributes the tension to the molecular forces being, on average, more intense in the tangential direction than the normal direction, in the thickness of the surface layer; it is, according to him, the excess of the first over the second which constitutes the tension.

Indicating this by F, he finds, generally, for the normal capillary pressure coming from the curves at an arbitrary point of a liquid surface, the expression $F\left(\frac{1}{R}+\frac{1}{R'}\right)$; the constant coefficient of the expression of Laplace is thus the tension in all cases, and, for the same liquid, it is always uniform, i.e. completely independent of the point of the surface considered, as well as of curvatures.

Dupré points out that the tension of liquid films is independent of their thickness, at least as long as this thickness is not less than a certain limit. Indeed, the tension existing only in the two surface layers, excessively thin layers, as we know, it is clear that the liquid between them is without influence, and that thus when, by an attenuation of the film, the thickness decreases, the tension must remain invariable.

This deduction is verified by the experiments that I described with regard to the pressure exerted by a hollow bubble on the interior air. It was seen, indeed (§ 158), that $t = \frac{rp}{2}$, from which it follows that, for a bubble of a given radius, if, in spite of the progressive thinning of the film, the pressure p does not change, it will be the same for the tension t; but I noted, one saw (§ 125), this constancy of the pressure with regard to a spherical film which was thinned spontaneously and burst only when its color attained the passage from yellow to white of the first order.

But, as I remarked (§ 123), the invariability of the pressure supposes that the film has a sufficient thickness so that there is liquid interposed between the two surface layers, i.e. a thickness higher than the double of the radius of the molecular attraction; because if the film is thinned enough so that the two surface layers arrive at contact, then are penetrated mutually, it is natural to admit that the pressure, and consequently the tension, fall in consequence of the reduction in the number of the acting molecules; one is thus led to this consequence that the limit below which the tension starts to decrease is equal to the double of the radius of the molecular attraction.

[96] In the seventh memoir, Dupré describes an instrument by the means of which he experimentally verified this proportionality between produced work and the reduction in surface area.

In his fifth memoir, where he disregards differences in constitution between the surface layer and the remainder of the mass, Dupré found that, for the same liquid at various temperatures, the tension is proportional to the square of the density; but, in his sixth memoir, where he takes account of the difference in question, he recognizes that this proportionality is not exact.

The expression $t' = \frac{h\rho}{4}$ of the tension of only one superficial layer[97] shows, moreover, that the tension varies inversely the temperature, since it is thus at the same time with h and ρ; but as, for the majority of liquids, the influence of the temperature on these two quantities is not very considerable, it follows that the tension changes little from the ordinary fluctuations of temperature: for water, for example, according to a table which Dupré gives, from $10°$ to $32°$, the tension decreases only from 7.48 to 7.15. It was seen that this little variability had already been observed by Mr. Hagen.

Dupré advances that, in consequence of the influence of the temperature on the tension, if one heats just a part of a liquid surface, equilibrium must be destroyed, and he adds: "In the case of films, the liquid forming the two surface layers of the heated part is entrained towards the cold parts by differences in contractile forces..... The film is thinned and ends up bursting." He does not describe any experiment, but, as we will see soon, one can make many on this curious subject.

He gives the means of determining, by calculation, the tension of a liquid according to the chemical equivalents of its elements.

When a liquid film bursts, it retracts by the effect of its tension; that is shown to the eyes in experiments that we must reserve for another chapter; however Dupré treats the phenomenon by his methods, and comes to express the speed there of the shrinking film by the formula:

$$v = \sqrt{\frac{4gF}{e\Delta}},$$

in which F is the tension of only one surface layer, g is gravity, e is the thickness of the film and Δ is the density of the liquid; from which it is seen that this speed is uniform, that it is directly proportional to the square root of the tension and inversely proportinal to the square roots the thickness and of the density. Dupré finds, in this manner, that the speed of withdrawal of a film of glyceric liquid of 1/9000 of millimetre thickness would be approximately 32 meters a second.

The value which the formula above assigns to the speed of retraction is probably exact enough for the majority of liquids; but, as we will further see, there are liquids with the regards to which it is different, because the phenomenon is influenced by an element which Dupré could not take account of. Let us add that the retraction is, in general, accompanied by a characteristic which complicates it; we will speak about it in § 428 and 429.

Dupré also seeks the law of another phenomenon which one can also attribute to the tension: the progressive reduction of the diameter of a bubble when one leaves open the tube which was used for inflating it; he arrives at the result that, for two bubbles formed of the same liquid, all else being equal, the squares of times during which they are emptied are in the ratio of the 7th powers of their diameters. He verifies this law by a succession of experimental verifications on bubbles of glyceric liquid.

Finally, Dupré describes several processes by means of which he measured the tension of a great number of liquids.

The first consists in the use of a hydrometer of Nicholson modified in the following way: the upper plate is replaced by a cylindrical basin in which one pours the liquid to be tested; two brass wires leaving horizontally the ends of a diameter of this basin

[97] I do not need to point out that this expression is simply half of that of the tension of a film (§ 158).

are folded up then to go down outside the vessel containing water, and to meet below the bottom by the intermediary of a plate intended to receive ballast and weights. After having established the floatation level, one vertically lowers in the basin a thin solid film, likely to be wet by the liquid, and whose lower edge is quite horizontal; as soon as this edge touches the liquid, the liquid rises by capillary force along the two faces of the film, and its tension raises the hydrometer by a certain quantity; one then adds weights to bring back the flotation level; and from these weights one deduces the tension by dividing their value by the perimeter of the edge of the film. I omit here some details of less importance which one will find in the Report, as well as a small correction to make to the result when great exactitude is wanted.

His second process is, with some small differences in the apparatus, that of which I made use (§§ 119 to 121) for the evaluation of the pressure of bubbles[98]: the tension is given then by the relation, pointed out above, $t = \frac{r\rho}{2} = \frac{pd}{4}$. According to that, for the surface stress of a filled mass formed from the same liquid, one has $t = \frac{pd}{8}$, an expression which Dupré also finds. This physicist has laid out his apparatus so as to be able to operate on very small bubbles (2.5mm to 3mm in diameter), which enabled him to subject to the experiment liquids, such as the water, which would not form larger bubbles.

Using this same apparatus, Dupré verified, as I did (§ 121), the constancy of the product *pd* of the pressure by the diameter of the bubble.

The third process is only a modification of the preceding: by means of the same apparatus still, a very small bubble of air is inflated within the liquid to test, but also as close as possible to its surface; in this case, one has to consider only the tension of one surface layer; only, to have a precise result, it is necessary to take account of the small hydrostatic pressure due to the ambient liquid.

A fourth process is based on the height of a fluid jet launched upwards from an opening of very small diameter, the height being notably decreased by the surface tension of this jet. Dupré manages to connect, by means of a formula, this tension with the other elements of the phenomenon. The formula in question contains terms which depend on quantities whose evaluation is impossible, such as friction against the edge of the opening; but Dupré, using a clever artifice, removes the difficulty, and the results of experiment agree in a very satisfactory way with those of the other processes.

Dupré draws also a process from drop-by-drop flow, and his method avoids the problem announced by Mr. Hagen (§ 152).

Lastly, in the case of a liquid drop deposited on a horizontal level that it does not wet, he obtains a relation between the tension, the weight of the drop, the density of the liquid and the diameter of the circle of contact with the plane; this last process, however, is applicable only to mercury and molten solids.

The fifth memoir gives, in a table, the tensions of eighteen bodies, the majority liquid at ordinary temperatures, others liquified by heat. For metals in a state of fusion, Dupré obtains, by the last process above, values much higher than those which belong to ordinary liquids, except for mercury: he finds, for example, for the tension of molten gold 96.2, and, that for molten tin 51.2.

§ 162. The experiments of Dupré led, in 1866, Mr. Van der Mensbrugghe[99] to imagine three new processes to note the tension of liquid films, two of which allow, moreover, evaluating this tension. Here is the first:

In a plane and horizontal wire contour, one produces a film of glyceric liquid; one

[98]When Dupré wrote his fifth memoir, he did not know the part of my research recalled above (see the note of § 196 about this fifth memoir).

[99]*Sur la tension des lames liquides* (BULLET. DE L'ACÂD. DE BELGIQUE, 2nd series, volume XXII, p. 305).

ties together the two ends of a very fine silk thread of suitable length, then, after having wet this thread with the same liquid, one deposits it with care on the film, where it forms an irregular contour. That done, one bursts the portion of the film included inside this contour; at that moment the silk thread tightens and takes an exactly circular shape. The remaining portion of the film contracts under the influence of its tension, so as to occupy the least area, which requires that the opening bounded by the thread becomes as large as possible, and consequently circular.

Mr. Van der Mensbrugghe wonders which provision the thread must adopt if one carries out the same experiment on a curved film surface, choosing, of course, a surface with zero mean curvature. He submits the question to calculation, and arrives at the following laws:

1° The thread is in equal tension in all its length.

2° The curve represented by the thread has everywhere the same radius of curvature.

3° The ratio between the tension of the thread and the radius of curvature is independent of the form of the surface and length of the thread, and is equal to the tension of the film.

The second process is a modification of the first. The solid contour which contains the film is rectangular and vertical; the silk thread, instead of constituting a closed contour, is attached by one of its ends at a point on the lower horizontal side of the solid rectangle; it leaves the film at another point of this same side, and its loose end supports a light weight. After the rupture of the portion of film thus intercepted, the thread is tightened, and takes, if the suspended weight is not too strong, the shape of a semicircle. In truth, the equilibrium is unstable, but it is maintained by the small friction of the thread against the side of the rectangle. Under these conditions, which one carries out with some care, the suspended weight gives the tension of the thread, and the radius of the half circle drawn by it is measured directly with a compass; to have the tension of the liquid film, it is thus enough, according to the third law above, to divide the first quantity by the second.

In the third process, the film is a portion of a catenoid, attached by its upper edge to a solid fixed horizontal ring, and by its lower edge to a smaller solid ring, also horizontal, that it holds suspended. This last ring itself supports a very light plate, on which one gently pours sand; the suspended solid system goes down progressively, by stretching the film, and one stops when the element of the meridian catenary which ends in the mobile ring becomes vertical or very nearly; one recognizes that the equilibrium becomes unstable then. This point reached, one has the tension of the film by dividing by the circumference of the mobile ring by the total weight of the suspended system.

Mr. Lamarle, in his report[100] on the Note that I have just analyzed, announces a fourth law relating to the form which the thread takes after the rupture of the portion of intercepted film; this law consists of what the direction of the thread must be so that everywhere the mean curvature of surface is zero. However Mr. Lamarle points out that the coincidence of this same law with the three others is generally impossible on surfaces with zero mean curvature; he shows it in particular for the catenoid, and he concludes from it that, in cases of this nature, the shape of the film must necessarily be affected; he knows, for the moment, only two surfaces, the plane and the skew helicoid with central axis, likely to satisfy the whole of the four laws.

Mr. Van der Mensbrugghe thus took again his experiments, and in a second Note[101]; he verified the conclusion of Mr. Lamarle on the nondeformation of the helicoid, and he noted, by a clever process, the deformation of the catenoid; he finds, moreover, that

[100] *Bullet. of Acad. of Belgium*, 1866, 2nd series, vol. XXII, p. 272.

[101] *Sur la tension des lames liquides,* 2nd Note (ibid, 1867, 2nd series, vol. XXIII, p. 448).

this deformation is quite notable only in the vicinity of the thread. These remarkable facts once more show the constant agreement of experiment with theory.

This second Note ends in the description of a curious experiment: a film of glyceric liquid is produced in a vertical wire ring; one deposits inside this ring, at its point low, a very light hollow glass sphere, of approximately two centimetres diameter, beforehand wetted with the same liquid; it places itself at once so as to be cut in two equal parts by the plane of the film; it remains thus in a stable state of equilibrium, and if one turns the ring on itself, it rolls inside without leaving it. Mr. Van der Mensbrugghe explains the phenomenon by the effort which the film makes constantly to occupy a minimum area; this condition requires, indeed, that the film contact the glass sphere following a great circle.

§ 162bis. To give a later example of the effects of the tension on the surface of liquid drops, Mr. Luvini has shown[102] in 1868, that these drops can themselves support weights: he brings into contact with the bottom of a drop suspended from a horizontal glass rod, a small paper system, of which he indicates the construction; when adherence is established, the drop is shortened horizontally and sags a little; then he lets go of the paper system and it remains constant; finally he adds small pieces of paper to it to increase it weight. He finds, in this manner, that a water drop of small size attached to a glass rod of 5mm diameter can support a weight of 35 to 50 centigrams. These weights appear quite heavy to me, and I am led to believe that an unperceived cause of error slipped into the experiments of the author.

§ l63. Mr. Quincke presented, in 1868 also, to the Berlin Academy a Note[103] in which he extends to solid surfaces the principle of tension. Using clever considerations, he determines this tension, per millimetre length, for a certain number of metals, and thus finds enormous values; for iron wire, for example, the tension would be equivalent to nearly 6 kilogrammes.

In the same Note, Mr. Quincke seeks the tensions of several molten metals and molten glass, at temperatures close to the point of solidification; he employs the method of drop-by-drop flow, by melting, using a very small flame, the lower end of a vertical wire of each material, and weighs the resulting droplets. He takes for the measure of the tension this weight divided by the perimeter of the section of the wire, and thus neglects the small quantity of liquid matter which, after the fall of a drop, remains adherent to the wire (§ 152); also he presents the values found only as approximate; they depart rather notably from those which Dupré had given and which, according to the method of which this last made use, were to correspond also to about the point of solidification.

In 1868 still, Mr. Quincke published another Note[104], in which he continues, by the same process, his measurements of the tension of molten bodies. He announces a second cause of error, opposed to the preceding one, and consisting in that, during the formation of the necking which precedes the fall of the drop, there is a certain surge of liquid towards this drop, which thus takes a volume, and consequently a weight, a little too large. He thinks that one can nevertheless employ this process, in the absence of another the more precise, to obtain approximate values. In the case of easily oxydizable bodies, he subjects the process a clever modification, which enables him to form the drops in an atmosphere of carbonic acid. By another modification, he determines the tension of several molten salts. The whole of his results include 29 solids in the state

[102] *Saggio di a corso di fisica elementare.* Turin, 4th edition, p. 233.

[103] *Ueber die Capillaritätsconstanten fester Korper* (COMPTES RENDUS DE L'ACAD. BERLIN, 1868, p. 132).

[104] *Ueber die capillaritätsconstanten geschmolzener Korper* (ANN. OF MR. POOGENDORFF, vol. CXXXV, p. 621).

of fusion.

He gives measurements of the tension of water deduced from the height to which this liquid rises along a previously wetted vertical wall, and thus finds, on average, at 0°, the value 8.79. He attributes the great inferiority of the values obtained by other physicists to the latter not operating rather quickly, the tension of water decreasing rapidly when the liquid remains exposed to air (§ 152).

Lastly, in his third Note[105] (year 1869), the tension of a succession of molten bodies is evaluated by a method which appears more exact to him than the preceding one: it consists in pouring on a horizontal solid plate the molten substance, in a rather large quantity so that its upper surface is appreciably plane, and measuring, using certain means, the height of this broad drop and the height where the meridian line has a vertical tangent; naming respectively these two heights K and k, Mr. Quincke finds that, if the drop has a rather large diameter, and if one represents the density of the substance by σ, the tension, that he indicates by α, is expressed by the simple formula:

$$\alpha = \frac{\sigma}{2}(K-k)^2.$$

He also applies the same method to some molten metals: he obtains, for example, for the tension of molten pure gold, the value 131.5. His results agree with those of the preceding Note for salts that he has subjected to the two processes, except for those for which the first of these processes brought a surface decomposition.

§ 164. Dupré joined together in one work the seven memoirs, of which I quoted the last three in § 161; this work, published in 1860, and entitled *Théorie mécanique de la chaleur*, contains several significant additions:

In his separate memoirs, the author treated, by utilizing the tension, a great number of capillary phenomena; in the current work, he supplements this study by an extract of a Report of his son, an extract containing the application of the same methods 1° to the facts resulting from the gradual rising of a solid plate with liquid adherent to its surface; 2° to the known phenomenon of a wire of dense material floating on a liquid, for example, floating steel needles on water.

Dupré gives then a new means of arriving, by experiment and calculation, at the value of the tension on the common surface of two liquids which do not mix: an extremely small drop of one these liquids is immersed very slightly in the other liquid, and communicates with a manometric apparatus indicating the pressure which it exerts on itself; the ambient liquid is contained in a capsule of a large diameter. Then, designating by r the radius of the droplet, by z the measured pressure, and by F_2 the tension on the common surface, Dupré finds relations from which one easily deduces:

$$F_2 = \frac{rz}{2}.$$

This formula coincides with that which expresses the tension of one liquid film according to the radius and pressure of a bubble of this liquid (§ 158), which is $t = \frac{rp}{2}$, and one easily understands that the droplet must be immersed rather little so that one can neglect the hydrostatic pressure.

Dupré also arrives at the condition necessary and sufficient so that a liquid drop is spread out into a thin film over the surface of another liquid. This condition, such as he gives it, is not expressed immediately according to the tensions; but one easily modifies it in this direction by means of the formulas of the author, and one finds then,

[105] *Ueber die capillaritätsconstanten geschmolzener chemischer Verbindungen* (ANN. OF MR. POGGENDORFF, vol. CXXXVIII, p. 141).

while indicating by F and F_1 the respective tensions of the liquid forming the drop and of that on which one deposits it:

$$F_1 > F + F_2.$$

It is the condition already found by Mr. Marangoni (§ 160bis).

§ 165. In a Note of 1869, a Note about which I spoke in § 127, Mr. Quincke attributed to me, by error, an opinion which is his, advancing, as I said, that a liquid film cannot exist any more when its thickness becomes less than double the radius of molecular attraction. It is based on supposing, in a portion of a film, the thickness becomes less than the limit above, the tension of this portion must decrease and that then the surrounding portions must retract and further thin this portion until it bursts. One cannot object that it would not be thus any more if the whole film would be uniformly thinned below the limit in question; because this film would necessarily be attached by its edge to some solid, the surface of a liquid, or another film by the intermediary of a small thicker mass which would thus preserve a stronger tension.

The principle of Mr. Quincke is undoubtedly true in general; however, an experiment that I will later describe (§ 172) appears to indicate that when there is not, between two portions of the same film, a very weak difference in tension, the film can be maintained.

§ 166. In 1869 also, Mr. Lüdtge studied[106] in a special way, from the point of view of the tensions, the phenomenon of the spreading out of a liquid in a thin film on another liquid. He arrives at the following law:

All the times that two liquids with different respective tensions satisfy the condition that their mutual adhesion exceeds the least of the two tensions, a drop of the liquid to which this least tension belongs spreads out on the surface of the other liquid, and the reverse never takes place.

This law coincides with that of Dupré (§ 164) such as this scientist expresses it in his work[107] if it is admitted that, when the relation of Dupré is satisfied, the liquid drop has necessarily less tension than the other liquid, which undoubtedly is true.

Mr. Lüdtge describes the curious experiment thusly: if, in a horizontal wire ring of two centimetres or more diameter, one produces an oil film, and one puts in contact with it a drop of soap solution, one sees this last liquid at once extending in a circular plate, which grows and seems to push back oil to the metal ring, so that at the end the soap water film completely replaces the oil film. The author adds that the same experiment succeeds with a small water film on which one deposits an oil droplet: the water film is soon then replaced by an oil film, which one can, in its turn, replace, like above, with a soap water film. Mr. Lüdtge thinks that every liquid is susceptible to being spread out over another, and can thus replace this last in the form of film; he consequently looks at these phenomena as effects of tension.

§ 167. We still have, in 1869, a remarkable Report[108] of Mr. Van der Mensbrugghe. Singular movements produced in certain circumstances on the surface of liquids had exercised for a long time the sagacity of physicists, and had given rise to a crowd of divergent hypotheses: such are the rotation and spontaneous displacements of the pieces of floating camphor on water, the kind of repulsion which tests the surface of

[106] *Ueber die Ausbreitung der Flüssigkeiten auf einander* (ANN. OF MR. POGGENDORFF. flight. CXXXV1I, p. 362).

[107] There is, in the formula of Dupré, an error of sign which one recognizes easily.

[108] *Sur la tension superficielle des liquides considérée au point de vue de certains mouvements observés leur surface* (ACAD. DE BELGIQUE, vol. XXXIV DES MÉM. COURONNÉS ET MÉM. DES SAVANTS ÉTRANGERS).

this liquid to the contact or even to the simple approach of a droplet of a volatile liquid, etc

Dutrochet had tried to explain the whole of these phenomena by hypothesizing that on the surface of all liquids there exists a certain driving force, of whose nature he is unaware, and which he names *epipolic force*. Mr. Van der Mensbrugghe shows that the epipolic force of Dutrochet is nothing other than the tension; he shows, by varied experiments and many measurements, that all the phenomena in question are due to differences in tension. As we saw, Mr. Marangoni, Mr. J. Thomson and Duprés had already explained some of them by similar differences; but Mr. Van der Mensbrugghe extends the same principle to all the series, and thus groups under only one point of view facts which seemed not to have any connection between them.

As for Mr. Lüdtge, who also utilizes (§ prev.) the differences in tension, he did his research in Berlin at the same time as Mr. Van der Mensbrugghe did his in Ghent, and the two memoirs appeared almost simultaneously.

Mr. Van der Mensbrugghe describes initially experiments which show that, according to the direction of the difference of the tensions, a droplet of an volatile liquid approaching the surface of another liquid determines there a centrifugal current or a centripetal current.

In regard of a liquid drop deposited on another liquid, and which, instead of being spread out there, takes a lenticular form, Mr. Van der Mensbrugghe seeks, by means of the respective tensions of the surface of the ambient liquid and the two faces of the lens, the analytical relation necessary for the equilibrium of form of this lens, and he deduces, in the case of spreading out, a condition which also agrees, in the end, with that of Mr. Marangoni.

In his experiments on this last phenomenon, Mr. Van der Mensbrugghe still uses a ring of fine silk thread (§ 162) that he places gently on the surface of the liquid, after having wet it; then, inside the irregular contour formed by this thread, he deposits a droplet of a liquid satisfying the condition of spreading out; at once the ring of wire tightens, and forms a circle.

The author passes then to research on the analytical condition of the equilibrium of a segment of a film sphere resting on a liquid, the respective tensions of this liquid and that of the film being able to be equal or unequal; he arrives thus at a formula that I report not now, because it will be better to include it when I have talked about a certain fact relating to cap films; one will find it in § 216. But I indicate here a verification of this formula in the case of two different liquids: it showed him that if one inflates a soap bubble of 3 to 4 centimetres in diameter, and that one deposits it on a large pure water surface, the cap to which it changes subsides considerably while extending, so as to make with the horizontal, on its border, an angle of only approximately 45°; however, experiment fully confirmed this deduction. Moreover, the film is thinned with an extreme speed, offers sharp colors, and bursts after a very short duration; the surrounding water attracts, by its excess of tension, the liquid which constitutes the film.

The author observes, like Mr. Lüdtge, the substitutions of one film for another: he advances, for example, that a cap of albumin solution resting on soap water is soon replaced by a cap of this last liquid.

He furthermore explains by a difference in tension the upward currents which appear when one slowly pours water in a vessel so that the liquid slips along its interior wall: he notes that these currents occur only when the wall is not cleaned perfectly, and that thus the surface of the water which accumulates in the vessel loses a notable part of its tension by the presence of an unperceivable lubricating layer.

In his opinion, the movements of camphor on water come from the liquid irregularly dissolving the camphor around the floating piece, and thus decreasing in tension, but by quantities which vary in different directions; the more distant portions of the surface of water then draw in all the directions the camphorated portion and, consequently, the piece, while acting with more intensity sometimes in one direction, sometimes in another. The author notes, indeed, that the tension of camphorated water is only approximately 0.6 that of pure water.

It is known that, to stop the movements of the piece, it is enough to plunge the end of a finger in the water; according to the author, an emanated oil content of the skin spreads then quickly on the liquid surface, and reduces its tension; he verifies his hypothesis by measurements, and makes sure, moreover, that a perfectly degreased finger does not produce an effect any more.

It is further known that small fragments of several other substances, such as benzoic, succinic and citric acids, some butyrates, etc, are driven on water in the manner of camphor; the author proves, by new measurements, that these bodies also lower the tension of water.

The reader will find, in the Report of Mr. Van der Mensbrugghe, the explanation, by the same principles, of still other facts.

§ 168. In a letter addressed to the newspaper *Les Mondes*[109] shortly after the publication of the Report above, Mr. Van der Mensbrugghe makes known several new experiments, especially relating to the spontaneous substitution of one film for another; here are the most curious:

1° A cap of saponin solution resting on distilled water obeys the formula of the preceding Report: it gives roughly, in the case in question, 74° for the angle formed by the film, on its border, with the horizontal surface of the water, and the aspect of the cap confirms this result, as much as the eye can judge.

2° If, at the top of the same cap, one deposits a large soap water drop, this last liquid is substituted soon, in all the film, for the saponin solution, and at once the cap subsides while spreading, so as to reproduce the results found in the preceding paragraph.

3° One makes likewise, by a process which the author states, a bubble of albumin solution of diameter notably less than 25 millimètres[110] adhere to all the interior contour of a horizontal wire ring having this last diameter; the film then takes the shape of a hollow bi-convex lens of equal curvatures; however if one deposits, at the top of its upper face, a drop of soap solution; one sees, at the same time as the substitution takes place, this face takes a greater development by thus increasing curvature, while the lower face becomes, on the contrary, less curved. The respective tensions of the two liquids being unequal, it is necessary, so that the two films exert the same pressure on the interior air, that their curvatures are also unequal.

4° Some drops of sulphuric ether sprayed on soap water foam or another liquid, make this foam disappear at once; the ether having a very weak tension, it replaces the foam films everywhere, and ether films have only an extremely short persistence.

§ 169. Finally it remains for us to mention an extract of a work[111] of Mr. Quincke, an extract published at the end of 1869, while waiting for the printing of the Report in full. Mossotti and Duprés had indicated, we saw (§§ 150bis and 164), the means of determining the tension on the common surface of two liquids in contact, but these means undoubtedly give not very precise results; Mr. Quincke employs, to solve the same question, the method which he described in the third of the Notes summarized in § 163: into a vessel partially filled with the less dense liquid, or, as he calls it,

[109] 2nd series, 7th year, volume XXI, page 302.

[110] In the article of *Les Mondes* there is *centimetres*, in error.

[111] *Ueber die capillaritätserscheinungen an der gemeinschaftlichen Oberfläche zweier Flüssigkeiten* (BULLET. OF ROY. SOC. OF GÖTTINGEN, 1869, n° 19, p. 383).

with liquid 2, he introduces a suitable quantity of the other liquid, or liquid 1, which spreads out on the horizontal bottom of the vessel in a broad drop whose upper surface is sensibly plane. Mr. Quincke measures then the quantity that, in the Note pointed out above, he indicates by $K-k$ then, σ_1 and σ_2 being respectively the densities of both liquids, the tension on the common surface, a tension which the author names α_2, is given to him by the formula

$$\alpha_{12} = \frac{\sigma_1 - \sigma_2}{2}(K-k)^2.$$

Here are some of the values obtained in this manner at the temperature of 20°.

LIQUIDS.	TENSION OF THE COMMON SURFACE.
Mercury and water	42.58
Mercury and olive oil	34.19
Mercury and spirits of terpentine	25.54
Carbon bisulphide and water	4.26
Olive oil and water	2.096
Olive oil and alcohol	0.226
Olive oil and alcohol diluted to nearly equal densities	0.693

In the case of olive oil and alcohol diluted to the same density, the process does not appear applicable; the extract that we summarize is not sufficiently explicit on the means to which the author had recourse to alleviate the problem[112].

Mr. Quincke furthermore uses the same method of evaluation for the tension of the free face of liquids which would wet a solid surface on which one would deposit them. For that, he fixes, at a certain height in the liquid, a horizontal glass plate, under which he makes pass a large bubble of air; the bubble is flattened against the lower face of the plate, and, if one then measures the distances K and k with this plate, it is enough to remove σ_1 in the formula and to disregard the sign, to have the sought result. The author obtains thus, for example, for the tension of water at 20°, the value 8.253.

He then takes up again research on the tension of the common surface of two liquids, and he indicates three other methods, but less general than the first; two of them rely, like that of Mossotti, on measuring the rising or lowering of the surface of contact of the two liquids when they are superimposed in the same capillary tube.

Mr. Quincke seeks also the condition necessary so that one liquid is spread out into a thin film over another, and is able to get a law which coincides with that of Mr. Marangoni[113].

§ 170. The series of researches whose analysis was explained in what precedes thus provide these general results:

1° Tension really exists in any liquid surface, and, consequently, in any liquid film.

2° This tension is independent of the curvatures of surface or of the film, it is the same at all points of the same surface or the same film, it is also the same, at each point, in all the tangential directions around this point.

3° It is independent the thickness of the film, at least as long as this thickness is not lower than double the sensible radius of activity of molecular attraction.

4° It varies with the nature of the liquids.

5° For the same liquid, it varies in the opposite direction of the temperature; but, at ordinary temperatures, it shows few changes.

[112]See the full Report indicated under n° 2 of § 508.

[113]For research after 1869, see the articles listed in § 508 as Nos. 3, 4, 6, 8, 9, 10, 16, 18, 20, 23, 27, 28, 29, 31, 33, 34, 36, 39, 40, 41, 44 and 45.

6° One has a great number of processes for the experimental measurement of the tension, and each one of these processes leads to an expression of the tension according to the data of the experiment. Most convenient is indisputably the first of Dupré, i.e. that of the hydrometer.

7° As for the cause of the tension, four hypotheses were proposed: in the first place, that of Segner, that I did not understand well, and whereby the tension would come from the mutual attraction of the molecules of the surface layer in the tangential direction and of the curvature of this layer; in the second place, that which is common to Mossotti, Dr. Hough and Dupré, and which makes the tension depend on the asymmetry of the molecular forces in the thickness of the surface layer, with slight differences in the manner of considering this asymmetry; in the third place, that of Mr. Hagen, who attributes the tension to a larger density of the surface layer; finally, that of Mr. Lamarle, who regards the tension as due to, by a contraction of the surface layer, a portion of the molecules of this layer passing into the interior and thus decreasing the spacing, the general tendency of attraction to bring together the molecules being partly satisfied.

§ 171. In my opinion, the true cause of the tension, if it is not stated in a completely explicit way, at least is sufficiently indicated by Henry and Lamar1e in their demonstrations of the existence of tension in film spheres (§§ 151 and 158). It is undeniable, since the work of Laplace, that, if one considers only the effect of the curvatures, a convex surface layer exerts on the liquid a normal pressure at each point, and a concave surface layer exerts, on the contrary, a normal traction also at each point; but, in the state of equilibrium, this pressure or traction fights against a resistance coming in general from hydrostatic forces; however it is seen that a surface layer curving thus, pressing or pulling, and which meets an opposed resistance, must be in tension, as is an inflated bladder which presses on its interior air, or, in other words, the molecules of this same layer must be in a state of forced spacing along the tangential direction. If one wants, there is the reciprocal theory of Young: this supposes the tension, and shows that tangential tractions that it determines around the same point give for the resultant force a normal pressure if the surface is convex, and a normal traction if the face is concave; however, as shown by Laplace, this pressure or this traction exists only by the effect of the molecular attractions; one can thus break up it, around each point, into tangential tractions, which constitute a tension.

Poisson appears to have had a similar opinion; in the preamble to his *Nouvelle théorie de l'action capillaire,* the illustrious geometer says, while speaking about Young:

"He himself relied on the identity of the surface of a liquid with that of a membrane in tension at all its points, an identity which can be only the consequence and not the principle of solution of the problem."

The tension is thus a necessary result of curvature, however one understands that it is independent of the values of these curvatures. Indeed, let us consider, to simplify, at a point of a normal section of a convex liquid surface, the two tangential components of tension and the elementary part of the pressure to which they give birth. If the curvature decreases, for example, then this part of the pressure decreases at the same time; but, in addition, the angle between the two components of tension increases, so that these components, or elementary tensions, can keep the same values as for the first curvature. If the curvature decreases until vanishing, then the part of the pressure under consideration also vanishes, and the two components of tension are in the prolongation of each other; there is thus then indetermination; it is the case of plane liquid surfaces, and one has the right to wonder how the tension is generated in similar surfaces. But it should be noticed that those are always bordered by portions of strong transverse curvatures: it is what occurs, for example, at the edge of the surface of a liquid contained in a vessel, and it is likewise with the intermediary of small portions of this kind that a liquid film adheres by its edge to the surface of a liquid, a solid wire, or other films. Although, in these last two cases, the portions in question are so narrow that they escape the sight so to speak, one understands, a priori, the need for their existence; indeed, the faces of a film cannot lead abruptly to the layer which wets the solid wire; they must obviously bend close to this layer, to approach it tangentially, and the same thing must occur along the line whereby a film adheres to other films; besides we will describe (§ 219) experiments where this is shown to the eyes. Now, the tensions that have, in consequence of their curvatures, these portions of connection, must, due to continuity, be propagated to all the extent of the surfaces which they border. This explanation was suggested to me by Mr. Lamarle.

172 I announced (§ 161) curious experiments relating to the effect of heat on the tension of liquid films; here is one which is due to my son Felix; I will describe another of them in § 208:

A bubble of glyceric liquid one decimetre in diameter being deposited on a ring, it tends to take, at least at its top (§ 108), a color other than the red or the green of the last; then ones brings the end of a finger with care as close as possible to this top, to see the color of this same top changing in a space from three to four centimetres in diameter, so as to show a thinning: if, for example, the top is yellow, it passes to the green; when the finger is rather hot, there is sometimes production of two successive colors; finally as soon as the finger is removed, the original color reappears. It is to this experiment that I referred at the end of § 165.

§ 173. We now will occupy ourselves with a set of facts closely related to the tension; I want to speak about the constitution of systems of films.

Let us start with a very simple case: it is known that when a bubble of air rising in a liquid arrives at its surface, it does not break this surface immediately, but raises there a film in the shape of segment of a sphere; and if the liquid is glyceric liquid, the film cap thus generated persists long time. However, let us imagine that a second bubble of air rises from the bottom of the vessel, and that at the moment when it is close to reaching the surface, it is partly under the first bubble; it will determine also the formation of a film, which will rise necessarily to the side the first, so that the two quantities of air respectively imprisoned by these two films will be separated by a portion of the second, as by a liquid partition. But this partition will not keep the curvature of the remainder of the second film, as I will show.

Due to their liquid nature, films obviously meet at angles along linear edges: it is necessary, for continuity (§ 171), that there is formed, all along the line of meeting, a small mass with surfaces strongly concave in the direction perpendicular to this same line; in the case concerned, this mass is too tiny to be distinguished, but, as I already announced, we will verify its existence later; however, the two caps and the partition being thus linked by a small mass which has its own curvature, it is clear that this small mass establishes an independence between the respective curvatures of the three films.

That being so, let us notice that the partition must also constitute a portion of a sphere, because it is under the same conditions as the two other films; it has, like those, for a border the small mass of junction and the liquid of the vessel. As for its curvature, it depends obviously on the difference of the forces exerted on its two faces by the two imprisoned portions of air. If these two portions of air are equal, the two films will belong to equal spheres, which will press the two volumes of air with the same intensity, and consequently the partition, subjected on its two faces to equal forces, will not have any curvature, or, in other words, will be a plane; but if the two quantities of air are unequal, in which case the two films will belong to spheres of different diameters, and, consequently, will unequally press these two quantities of air, then the partition, pressed on its two faces by unequal forces, will bulge towards the side where the elasticity of the air is the least, until the force which it exerts, due to its curvature, on the side of its concave face, balances against the excess pressure of the air which bathes this face.

Let ρ, ρ' and R be the radii of the spheres to which belong respectively the largest film, the smallest film, and the partition, and let p, p' and q the respective pressures that they exert, due to their curvatures, on the air which bathes their concave faces. These pressures being (§§ 116 and 121) inversely proportional to their diameters, and consequently to their radii, one will have:

$$\frac{p}{q} = \frac{r}{\rho} \quad \text{and} \quad \frac{p'}{q} = \frac{r}{\rho'};$$

but, according to what we saw above, it is necessary, for equilibrium, that one has:

$$q = p' - p;$$

from which it follows that

$$1 = \frac{p'}{q} - \frac{p}{q}.$$

Substituting in this last equation the values above of $\frac{p'}{q}$ and $\frac{p}{q}$, and solving for R, it becomes:

$$R = \frac{\rho\rho'}{\rho - \rho'},$$

a formula which will give the radius of the partition, when one knows those of the two films.

If, for example, these two films belong to equal spheres, then $\rho = \rho'$, and the formula gives $R = \infty$; i.e. then the partition is a plane, as already found. If the radius of smallest of the two films is half of that of largest, in other words, if $\rho' = \frac{1}{2}\rho$, then the formula gives $R = \rho$; in this case, the curvature of the partition will be consequently equal to that of the largest film.

To supplement the study of our film system, it remains only to seek the angles at which the two caps and the partition join; however, these three films having of identical tensions (§ 170), and thus drawing their common edge with equal forces, it is clear that they must make between them, at this edge, equal angles, angles which are consequently 120°. I make here, of course, the idealization of a small mass of junction, because of its extreme thinness.

We will see soon all these results verified by the experiment.

§ 174. If the bubble of air which gives birth to the second film ends up at the surface of the liquid far enough from the first film so that the two segments of spheres are completely isolated, or, which is the same, if one deposits on the liquid two bubbles sufficiently distant from one another, the bubbles are transformed at once into segments of spheres, and, driven by the capillary forces in the manner of light floating bodies, will approach by degrees until they contact, at least if they were not originally separated by too great an interval. To render comprehensible what must happen then, let us point out a fact shown, within my alcoholic mixture, by filled oil spheres. When two such spheres suddenly touch, their union is not in a state of equilibrium; once contact is established, the two spheres merge to form only one sphere, and it is easy to grasp the reason for it: one could not produce this contact at a single point; it necessarily takes place on a small face, so that the two masses form only one, actually; however since this one is finite and entirely free, and it is of revolution, the only shape of equilibrium which it can affect is (§§ 37 and 78) that of a single sphere. Now it is seen that the same thing must tend to occur with regard to our two segments of film spheres, as soon as the two small annular masses with concave surfaces in the meridian direction which run along their bases and attach them to the liquid, are linked at the place of their contact, i.e. the two caps will tend to form only one of them; but so that this tendency could have its full effect, it would be necessary that the two films opened at the place of the contact, and as cohesion is opposed to it, it is understood that the opening will be replaced by a partition, and that thus the system will arrange itself like that of the preceding paragraph.

§ 175. The existence of the partitions is a well-known fact of all those who have had fun making soap bubbles; but I had to submit to the control of experiment to establish the preceding results, and for the first time those which relate to the curvature of the partition and the angles under which this partition and the two films are cut.

To this end, I traced, on three paper sheets, three shapes representing the bases of three formed systems, each of two portions of film spheres and a partition. I understand by the base of such a system the whole of the arcs of circles along which it is based on the surface of the liquid, neglecting the small annular masses. Here is one method by which I plotted the drawings in question:

Let us suppose two films originally form two complete spheres, spheres which partly penetrate in a manner to create a partition, and imagine all this system cut by a plane passing through the centers of the two spheres; it is clear that the center of the sphere to which the partition belongs will find itself on the line which contains the two centers above. That said, the angles at which the two films and the partition meet

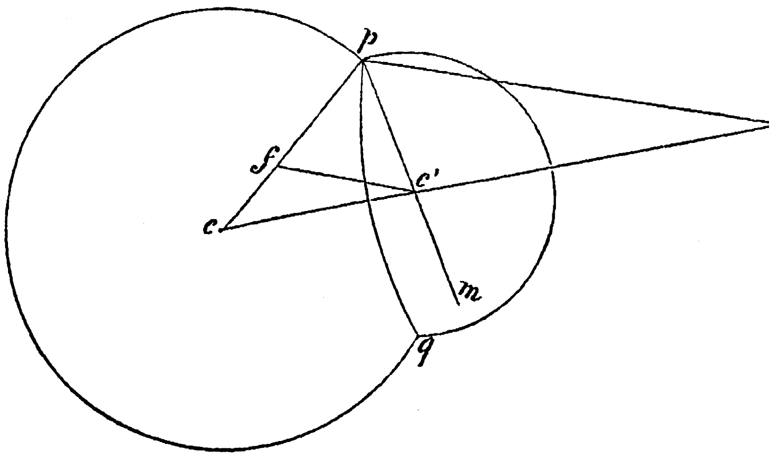


Fig. 61

having to be 120°, it is seen that the radii of the two films led to a point of the line of intersection will form between them an angle of 60° and one will as easily see that the radius of the partition led to the same point will also form an angle of 60° with the other which it is nearest. Thus let (fig. 61) p be one of the two points where the three arcs join two films and the partition are cut by the plane in question, a plane which we will take for that of the diagram, and let $pc = \rho$ be the radius of the largest bubble. Let us extend the indefinite lines pm and pn in such a way that the angles cpm and mpn are each 60°. On pm let us take pc' equal to ρ', i.e. to the radius of the smallest bubble; let us join cc', and prolong the line until its meeting with pn at d. The three points c, c' and d will be obviously the three centers, and pd will be the radius r of the partition, so that if, of these three centers and with these radii, one traces three portions of circles leading on the one hand to the point p and other to its symmetrical point q, one will have the diagram showing the cross-section of the system of the two films and the partition. Only this construction, considered as representing the base of a film system, supposes that the two caps, before their mutual penetration, are hemispheres, a condition which never is rigorously fulfilled, at least when the caps rest on a thick layer of liquid (§§ 214 to 216).

In the three drawings about which I spoke, and which are represented at a third of their size in fig. 62, 63 and 64, the arcs were drawn in broad features of approximately a millimetre; one will see soon why. In the first drawing, the diameters of the two bubbles were equal; in the second, they were as 2 to 1; and, in the third, as 3 to 1. To employ one of these drawings, one places it on a table, and one poises above it a thin glass plate, of which one wets the upper face with glyceric liquid. That done, one inflates a bubble of the same liquid, that one deposits on the plate above the portion of circumference which represents the base of the smallest bubble; this bubble spreads out at once so as to constitute a cap; it arranges itself so that the base has a diameter a little less than the portion of circumference in question. One then inflates a second bubble, which one deposits in the same way above the portion of circle representing the base of the other bubble, and after arranging itself by spreading out over the plate, it has a diameter a little too small. If, while depositing this second bubble, one was to put it in contact with the first, the two caps penetrate each other partially and become united with a partition. Things being thus prepared, one soaks the opening of the pipe in the glyceric liquid, as if for blowing a bubble, then applies the pipe opening to the lower part of the smallest film, one blows a little, then one does the same operation for the other bubble, one returns to the first, then at the second, and so forth[114], while sliding at the same time, by small quantities, the glass plate on the drawing, and, with the suitable care, one manages to give to the two bubbles the diameters of the portions of traced circles, and then the base of the film system

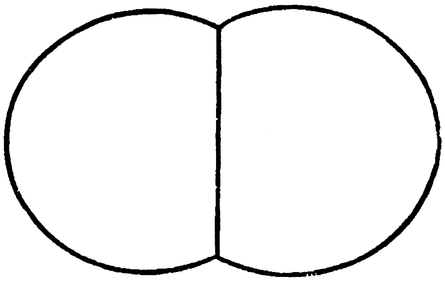


Fig. 62

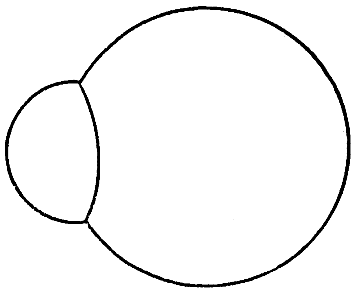


Fig. 63

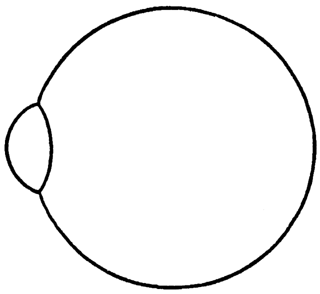


Fig. 64

[114]This operation would be carried out much more easily if one employed instead a glass tube tapered at one of its ends; after having wet it with glyceric liquid, one would introduce it into the bubble, and one would blow gently on the other end. At the time when my experiments were made, this means, which Mr. Lamarle made use of later in another circumstance (§ 204), had not occurred to me

obtained, bases made of those of the two bubbles and with that of the partition, coverthe drawing exactly. I said that the drawings were in broad features; these features must be seen through the small annular masses; if they were fine, the refractions produced by the small masses concerned would prevent one from distinguishing them. Let us add that, on the glass plate, the caps, when without their mutual penetration, are exactly hemispherical (§ 216), so that the verification above does not leave anything to be desired.

§ 175bis. But there remains a point to be cleared up: we determined (§ 173) the radius r of the partition on the basis of the relative values of the pressures respectively exerted by the three portions of segments of a sphere on the two imprisoned quantities of air. On th other hand, we come to build this radius on the basis that the partition and the two films must meet at angles of 120°. However, one does not see a priori a clear relation between the principles which are used respectively as bases with these two determinations, and one can wonder whether the two results coincide; that is what we will examine.

Let us take (fig. 61) $pf = pc'$, and join $c'f$; the angle cpc' being 60°, the triangle fpc' will be equilateral, and there will be consequently $fc' = pc' = \rho'$; by the same reason, the angle $fc'p$ will be 60° like the angle $c'pd$, from which it follows that the lines fc' and pd will be parallel; one will be able to thus pose:

$$\frac{pd}{fc'} = \frac{pc}{fc};$$

finally, substituting in this formula pd, fc' and pc by their respective values r, ρ' and ρ, and observing that fc is equal to $\rho - \rho'$, one will conclude:

$$r = \frac{\rho\rho'}{\rho - \rho'}.$$

It is identically the value which the first method gave; thus the two principles, seemingly independent, lead to the same result.

§ 176. To also verify the results of § 174, one deposits successively, on the surface of a thick layer of glyceric liquid contained in a large porcelain dish, two bubbles of this same liquid, so that the two segments of spheres that they form are separated by a certain interval. When this is more than one centimetre, the bubbles moved, indeed, one towards the other, and were linked with a partition; only if the two bubbles had large diameters (10 centimetres or more), the partition occurred in general only when the meeting took place a few moments after the formation of these films; when those were initially a little too distant from each other, so that time necessary for their spontaneous union was rather long, they were linked without partition by transforming into only one large cap, undoubtedly because they had become too thin, so that the incipient partition broke before one could note the existence of it.

§ 177. If a third film segment of a sphere joins with two others already united, the system will have obviously three partitions, one coming from the meeting of the first two films, and two from the meeting of each one of these same films with the third. These three partitions will necessarily end at the same arc of junction, and, by supposing that they still have spherical curvature, it will be necessary that at the lines of junction of each of them with two of the films, the angles are still 120°; it is necessary, moreover, by the reason given at the end of § 173, that along the arc of junction of these three partitions, the angles between them are also 120°. That said, let us see by what means we will be able to trace the base of a system of this kind, just as we traced (fig. 61) that of a system of two bubbles. After having described (fig. 65) the bases of

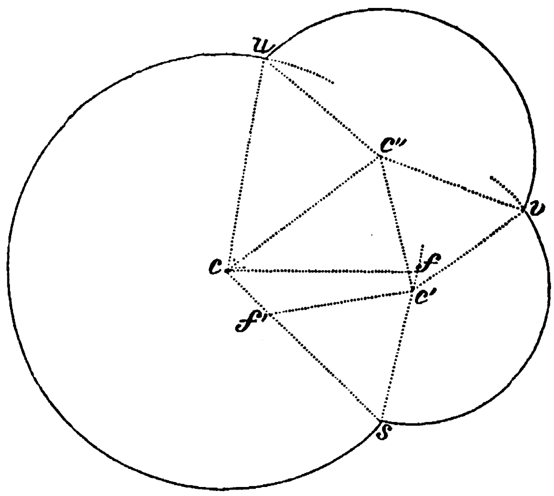


Fig. 65

the first two films, bases having for centers c and c' and for radii the lengths given that we will still indicate by ρ and ρ′, let us carry, starting from the point s where these two bases meet, and on the radii sc and sc', two lengths sf' and sf both equal to the given radius ρ″ of the third base, then, with the points c and c' as centers and with the lengths cf and $c'f'$ as radii, let us trace two arcs of circles; their point of intersection c'' will be the center of the base of the third film, bases that one will describe then with the radius ρ″.

Indeed, suppose the problem solved and this base traced. If one draws, from the point where it abuts one of the first bases, the lines uc and uc'', which will be respectively equal to ρ and ρ″, these lines will form between them an angle of 60°, like the lines sc and sc'; from which it follows that the triangle $c'uc''$ will be congruent to the triangle csf, in which sc and sf are also respectively equal to ρ and ρ″, and thus cc'' will be equal to cf; by the same reasoning, the triangle $c'vc''$ will be congruent to the triangle $c'sf$, and consequently $c''c'$ will be equal to cf'.

Now let us trace the bases of the three partitions. Those of the three films being described (fig. 66) by the preceding layout, one will determine, as in fig. 61, the center d of the partition belonging to the first two films and on the basis of s, by carrying out sd forming with sc' an angle of 60°, until its meeting, at d, with the line cc' prolonged; one will determine in the same way the center f of the partition belonging to the first and to the third bubble by carrying out uf forming an angle of 60° with $c''u$, until its meeting at f with cc'' prolonged; finally one will determine by the same process the center g of the third partition. It remains only to describe, with the points d, f and g as centers, and with the radii ds, fu and gv, three arcs of circles starting respectively at the points s, u and v, and moving about the middle of the shape; these arcs will be the bases of the three partitions, always on the assumption that these partitions would be portions of spheres.

If one built the shape carefully, one will recognize:

1° That the arcs of which I to speak end all three at the same point, o; 2° that the three centers f, d and g are laid out in straight line; 3° that if one joins the point o to these three centers, the angles fod and god are equal and 60°.

§ 178. But as one could believe that these results of a graphic construction are simply very approximate and not completely exact, I will establish them in a rigorous

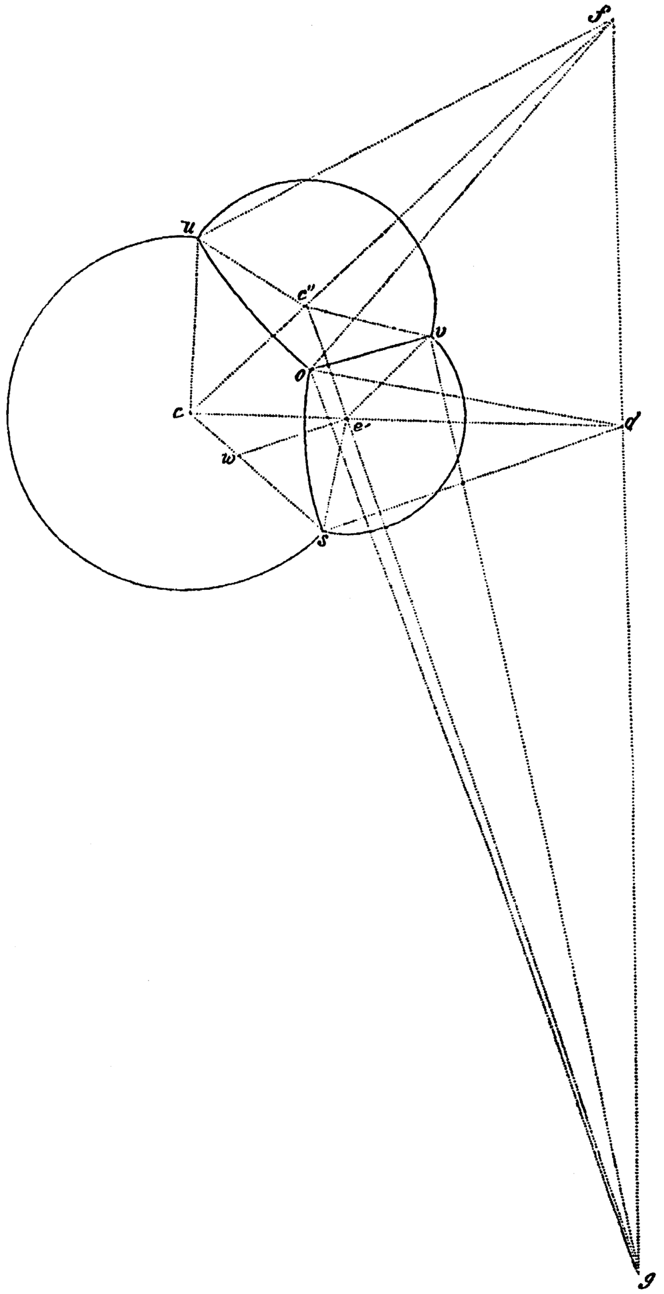


Fig. 66

way; it is to Mr. Van der Mensbrugghe that I owe this demonstration.

We shall show initially that the three centers f, d and g are in a straight line. For that, repeating what we did in fig. 61, let us relate to sc(fig. 66) a length $sw = sc' = \rho'$, and let us join $c'w$; we know that this last line will be parallel to sd, and consequently we will be able to say

$$\frac{dc}{dc'} = \frac{sc}{sw} = \frac{\rho}{\rho'}.$$

For the same reason, by considering the two portions of circles which have centers c and c'' and which intersect at u, one will have, by reversing only the two ratios,

$$\frac{fc''}{fc} = \frac{\rho''}{\rho};$$

and finally, the two portions of circles with centers at c' and c'' and intersecting at v, will likewise give

$$\frac{gc'}{gc''} = \frac{\rho'}{\rho''}.$$

Multiplying these three equalities term by term, it becomes:

$$\frac{dc \cdot fc'' \cdot gc'}{dc' \cdot fc \cdot gc''} = 1.$$

Let us notice now: 1° that the three centers d, f and g are on the prolongations of the sides of the triangle $cc'c''$; 2° that the six quantities dc, fc'', gc', dc', fc and gc'' are the distances, counted on these same prolongations, from the points d, f and g to the three corners c, c' and c''; 3° that, in the last formula above, the three factors of the numerator represent lines, no two of which have a common endpoint, and it is the same for the three factors of the denominator. However it is known, by a theorem of transversals, that when the condition expressed by this formula is met with regard to an arbitrary triangle, the three points in question, taken on the prolongations on the three sides, are necessarily in a straight line. Our three centers d, f and g thus enjoy this property.

This first point established, let us show the others. Let us regard the point o as being simply the intersection of the two arcs uo and vo having for centers f and g; let us join od, of and og, and, without seeking initially if od is really the radius of the arc having for center d and ending at the point s, show that the angles fod and god are each 60°, or, which amounts to the same thing, that the angle fog is 120°, and that the line od bisects it.

Now let us seek to determine the lengths fd and gd, and, for this purpose, consider them as respectively belonging to the triangles fcd and $gc'd$ in which we will be able to evaluate the sides fc, dc, gc' and cd, as well as the angles that they include. To arrive at these last values, let us calculate the sides of the triangle $cc'c''$. By means of the triangle csc', where the sides cs and $c's$ are respectively ρ and ρ' and have between them an angle of 60°, one easily finds

$$cc' = \sqrt{\rho^2 + \rho'^2 - \rho\rho'};$$

the triangles cuc'' and $c'vc''$ give in the same way:

$$cc'' = \sqrt{\rho^2 + \rho''^2 - \rho\rho''}, \quad \text{and} \quad c'c'' = \sqrt{\rho'^2 + \rho''^2 - \rho'\rho''}.$$

One deduces from there, by known formulas,

$$\cos c'cc'' = \cos dcf = \frac{\rho^2 + (\rho - \rho')(\rho - \rho'')}{2\sqrt{\rho^2 + \rho'^2 - \rho\rho'}\sqrt{\rho^2 + \rho''^2 - \rho\rho''}};$$

one will find same manner

$$\cos cc'c'' = \cos gc'd = \frac{\rho'^2 + (\rho - \rho')(\rho'' - \rho')}{2\sqrt{\rho^2 + \rho'^2 - \rho\rho'}\sqrt{\rho'^2 + \rho''^2 - \rho'\rho''}};$$

On the other hand, one has:

$$fd = \sqrt{\overline{fc}^2 + \overline{dc}^2 - 2\overline{dc} \cdot \overline{fc} \cdot \cos dcf},$$

$$gd = \sqrt{\overline{dc'}^2 + \overline{gc'}^2 - 2\overline{dc'} \cdot \overline{gc'} \cdot \cos dc'g},$$

formulas where the lines cd, fc, dc', gc' still remain to be determined; but, in the triangle csd, where wc' are, we know, parallel to sd, one has:

$$\frac{dc}{cs} = \frac{dc'}{sw} = \frac{cc'}{cw};$$

from which, by replacing cs, sn, and cw by their values ρ, ρ' and $\rho - \rho'$, like cc' by his value found higher, one obtains

$$cd = \frac{\rho}{\rho - \rho'}\sqrt{\rho^2 + \rho'^2 - \rho\rho'}, \quad \text{and} \quad dc' = \frac{\rho'}{\rho - \rho'}\sqrt{\rho^2 + \rho'^2 - \rho\rho'};$$

the triangles cuf and $c''vg$ will give on their side:

$$fc = \frac{\rho}{\rho - \rho''}\sqrt{\rho^2 + \rho''^2 - \rho\rho''}, \quad \text{and} \quad gc' = \frac{\rho'}{\rho'' - \rho'}\sqrt{\rho''^2 + \rho'^2 - \rho''\rho'}.$$

It thus comes, after making substitutions and reductions to shorten it,

$$\sqrt{\rho^2\rho'^2 + \rho^2\rho''^2 + \rho'^2\rho''^2 - \rho^2\rho'\rho'' - \rho\rho'^2\rho'' - \rho\rho'\rho''^2} = P,$$

$$fd = \frac{\rho}{(\rho - \rho')(\rho - \rho'')} \cdot P,$$

$$gd = \frac{\rho'}{(\rho - \rho')(\rho'' - \rho')} \cdot P,$$

and consequently

$$fg = \frac{\rho''}{(\rho - \rho'')(\rho'' - \rho')} \cdot P,$$

One draws from there:

$$\frac{fd}{gd} = \frac{\rho(\rho'' - \rho')}{\rho'(\rho - \rho'')}.$$

In addition, according to the result of § 173, one has, by observing that fo and go are respectively equal to the radii fu and go of the two partitions which we consider,

$$fo = \frac{\rho\rho''}{\rho - \rho''}, \quad go = \frac{\rho''\rho'}{\rho'' - \rho'}, \quad \text{d' where} \quad \frac{fo}{go} = \frac{\rho(\rho'' - \rho')}{\rho'(\rho - \rho'')};$$

the two reports/ratios $\frac{fd}{gd}$ and $\frac{fo}{go}$ are thus equal, and, consequently, the line c is the bisector of the angle fog.

Knowing, from the preceding, the three sides of the triangle fog, one deduces from it, with all simplifications done,

$$\cos fog = -\frac{1}{2},$$

from which it follows that the angle fog is 120°, and, consequently, that the angles fod and god are each 60°.

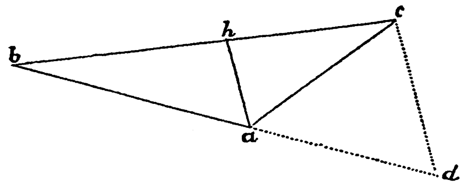


Fig. 67

Let us seek finally the length of the bisector do. For that, notice that, in any triangle of which one of the angles is 120°, there is an extremely simple relation between the bisector of this angle and the two sides which include it. Indeed, let abc be a triangle where the angle at a is of 120° (fig. 67),; let us prolong the side ba by a distance ad equal to ac, and join cd; this line will be parallel to the bisector ah, because the angle dac will be 60°, and since ad is equal to ac, the triangle dac will be equilateral, and the angle dca will be 60° as will the angle cah; there will be thus

$$\frac{ah}{dc} = \frac{ba}{ba+ad},$$

or, because of $cd = ad = ac$,

$$\frac{ah}{ac} = \frac{ba}{ba+ac},$$

and finally

$$ah = \frac{ac}{ba+ac}.$$

This result applied to the triangle fog (fig. 66), thus gives

$$c = \frac{fo \cdot go}{fo+go},$$

and, after substitutions,

$$c = \frac{\rho\rho'}{\rho-\rho'};$$

but it is there precisely (§ 173) the value of the radius d of the third partition. It results thus, as we ourselves proposed to show, that the center of three partition are in a straight line, that the base of these three partitions intersect at a point, and finally that the radii of these bases make between them angles of 60°, and that thus these same bases join at an angle of 120°.

Now, so that we can regard the three partitions as comprising really some portions of spheres, it remains still to establish that these three portions intersect along a single arc, but it follows obviously that the centers f, d, g of these three spheres are in a straight line, and that these same spheres have a common point at o.

Thus all the theoretical conditions are satisfied by three partitions of spherical curvature laid out, with the three caps, so as to form a system having for base that which one traces by the construction of the preceding paragraph; one must thus look at as extremely probable that the system will take this form in reality.

§ 179. All this, indeed, I verified by experiment, by employing the means described above (§ 175), i.e. I traced in broad features on a paper, by the method indicated, the base of a system of three hemispheres with their partitions, then I has placed over it a plate of glass moistened with glyceric liquid, and finally I deposited on top of the three portions circles, three bubbles initially a little too small, that I successively increased by the small operation mentioned, while at the same time sliding, when that was necessary, the glass plate on the paper. However, the base of the film system thus formed was still superimposed perfectly on the drawing.

The graphic constructions that I gave in what precedes for the bases of the systems of segments of spheres suppose implicitly the law of inverse proportion of the pressure to the diameter; the exact coincidence of the bases of the systems carried out with the traced bases thus provides a new verification of this law, to add to the direct verification obtained in § 121. It is to the current experiments to which I made allusion in the paragraph that I have just quoted. These same experiments confirm also, at least with regard to films of spherical curvature, independence between the tension and the curvature (§ 159).

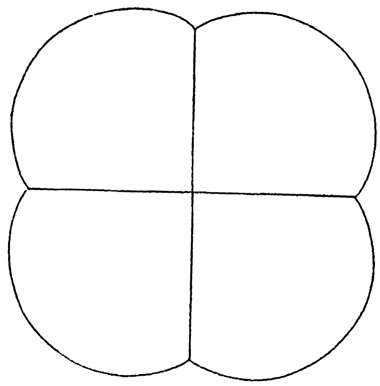


Fig. 68

§ 180. If it is imagined that a fourth segment of a sphere comes to join itself with the system of the three preceding ones, one will be able to conceive two different arrangements of the union, apart, of course, from where the fourth cap would be placed so as to be linked only with only one of the others. One of these arrangements would contain four partitions joined along only one edge, and the other would contain five of them joining along two edges. To simplify the graphic question and constructions, I will suppose the four caps equal in diameter, in which case all the partitions will be obviously plane.

Then, one can admit, initially, that the four caps are linked so that their centers are placed like the four corners of a square, which will give the system whose base is represented in fig. 68, where there are four partitions leading to the same edge at right angles; this system is evidently a system of equilibrium, since all is symmetrical.

One can admit, in the second place, that, three caps being initially linked, the fourth is linked to two of them; in this arrangement, the four centers will be at the corners of a rhombus, and one will have the system whose base is represented in fig. 69, where there are five partitions.

This system is obviously also, because of its symmetry, an equilibrium system; but here, only three partitions meet at one liquid edge, forming between them angles of 120°.

Now if one tries to form on the glass plate the first of these two systems, one does not reach that point, or, if it occurs, it is only during one inappreciable moment, and passes quickly to the second. As for this last, one obtains it directly without difficulty, and it persists.

One must conclude that, in the first system, the equilibrium is unstable, and thus it

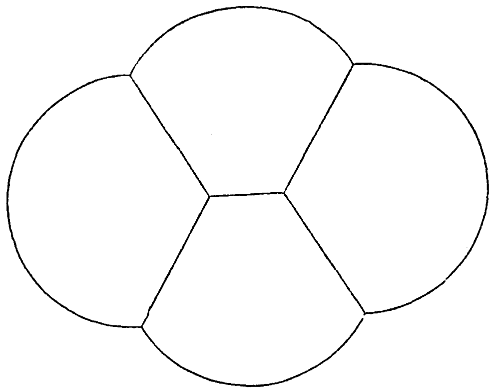


Fig. 69

becomes probable that four partitions leading to the same edge cannot coexist.

Let us note besides that, in the film assembly of fig. 61, the semicircular liquid edge which links the two segments of a sphere is common to only three films, the two caps and the partition, and, we know, these three films form between them equal angles; in the same way in the assembly of fig. 66, each liquid edge which links pairs of caps, and that which links the three partitions, is in common with only three films forming between them equal angles; finally it is also what obviously happens in the assembly of fig. 69.

§ 181. Let us notice, in addition, that, in the systems of figs. 66 and 69, there are four edges leading to the same point, being the three which link the caps pairwise, and that which links the three partitions; however, from the equality of the angles between the three films linked along the same edge, one easily deduces that the four edges which meet at the same point also make between them, at this point, equal angles. Indeed, by initially supposing films to be planes and, consequently, the edges rectilinear, the system constitutes obviously the whole of four trihedral angles in each one of which the three plane angles are equal; however, in virtue of a known theorem, this equality involves, for any of these trihedral angles, that of the plane angles; but, each one of the latter being common to two of the trihedral angles, it follows that, in the system, all plane angles, i.e. the angles which between them the four edges form, are equal. In our systems of caps and partitions, the films, or at least a part of them, are curved, and consequently it is the same for the edges; but one obviously can, at the point common to those, to replace the curved films by their tangent planes, and the curved edges by their tangents, which will be the intersections of these planes.

Reciprocally, if the four edges make equal angles between them at the point which is in common to them, then it follows from symmetry that the films must also make equal angles between them at their common edges. Thus, in all cases, the equality of the angles at which four edges end at a common point and the equality of the angles at which three films link with the same edge are, at least in the vicinity immediate of the point in question, necessary consequences of each other. Now let us seek the common value of the angles between the edges. According to what we have just said, it is enough to have four lines leading to the same point under equal angles. That being said, to follow the simplest way, let us consider a regular tetrahedron *abcd* (fig. 70). Let *o* be the center of this tetrahedron. Let us form the lines *oa*, *ob*, *oc*, *od* to the four corners; these lines will obviously form between them equal angles, whose value will be, consequently, that which it is a question of obtaining. That said, let us prolong the line *ao* until *p*, where it reaches the base, and join *pd*; the triangle *opd* will be right-angled at *p*. Now notice that the point *o* is the centre of gravity of the tetrahedron, and that the point *p* is the centre of gravity of the base *bcd*; but it is known that if, in an arbitrary pyramid, one joins the top in the centre of gravity of the base, the centre of gravity of the pyramid is located on this line, at three quarters along its length starting from the top; *op* is thus a third of *oa*, and as the point *o* is at equal distances from the four corners, *op* is also a third of *od*. In the right-angled triangle *opd*, one has consequently $\cos dop = 1/3$; from which finally follows

$$\cos doa = -\frac{1}{3}.$$

Thus when four lines lead to the same point at equal angles, each one of these angles is that which has as a cosine $-1/3$; one finds, according to that, that this angle is $109^\circ\ 28'16''$, i.e. very nearly 109 degrees and half.

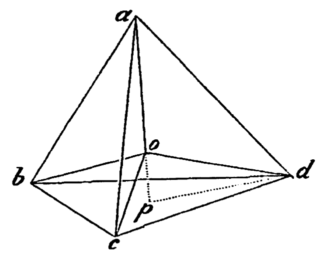


Fig. 70

Such is thus the value of the angles at which, in the film assemblies that we studied up to now, the liquid edges end at the same point.

§ 182. These systems were simply those which result from the joining of segments of spheres together; but one can produce much more curious systems. We saw, in chapters III and IV, the ease with which one develops films of glyceric liquid based on wire contours; if thus we take any of the polyhedric frames which we used for the creation of polyhedrons of oil (§ 29), that are slightly oxidized, and we immerse it in the glyceric liquid, leave it there a few seconds to wet well, and withdraw it, then we will have to find it, and we will find it, indeed, occupied by a system of films; however the system, which one would expect to see made up irregularly according to the chance movements that one made with the frame while lifting it from the liquid, always presents, on the contrary, a perfectly regular and symmetrical arrangement. This arrangement varies with the shape of the frame; but, in general, it constantly reproduces the same system in a frame of the same polyhedron.

For example, the cubic frame gives invariably the system which I represent here (fig. 71): it is composed, one sees, of twelve films leaving respectively the twelve solid edges and leading all to a single quadrangular flat film located at the middle of the system. The edges of this flat film are slightly curved like all the other liquid edges, and consequently all the films, except for the central flat film, have slight curvatures. The curvatures of the liquid edges which leave the corners of the frame are not marked enough so that one could indicate them in the drawing. When the experiment is carried out, it is necessary to have care not to immerse in the liquid the totality of the fork which supports the frame, because, if that were done, it would develop also a film in this fork, a film which would modify the system of the others. I will point out that my cubic frame is 7 centimetres on a side.

§ 183. The film systems thus developed in the frames have excited the admiration of all the people to whom I have showed them; they are, as I said, of a perfect regularity, their liquid edges have an extreme smoothness, and their films show out after some time the richest colors.

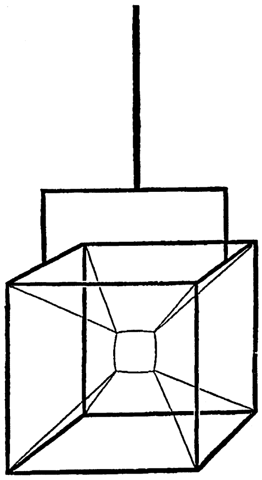


Fig. 71

To observe any of them conveniently, and without agitating them, one sets the frame which contains it on a small wire frame similar to that which is represented in perspective in fig. 72; the frames which I use myself have four centimetres width, twelve length and two and half height.

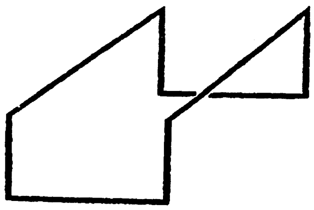


Fig. 72

After each series of experiments, one washes the frames by passing them in rainwater, then one deposits them on filter paper, and one lets them dry. Let us say here that, for the creation of these film systems, one can, equivalently, replace the glyceric liquid by a simple soap solution. The shapes will be far from durable, but one can always re-immerse the frame as often as wanted, and to renew the observation thus.

§ 184. Now, whatever the polyhedron whose edges form the frame that one employs, if the produced film system is attentively examined, one will recognize that always along the same liquid edge three films end, and that the liquid edges leading to the same liquid point are always four; thus these numbers, which we had already noted in the systems of caps, are found in all the film systems, so that their invariability constitutes two laws. Moreover, here, as with regard to the caps, it follows from the equality of the tensions that the three films linked by a liquid edge necessarily make between them, at this edge, the equal angles, and that, consequently, the four edges meeting at the same liquid point also make between them, at this point, equal angles, which constitutes two other general laws.

§ 185. Among the frames which I subjected to experiment, three, being those of the regular tetrahedron, the equilateral right triangular prism, and the regular octahedron, provide systems made up of plane films; however those make it possible to note the equality of the angles between the liquid edges, because one can apply measurements to them; they consequently make it possible also to verify the equality of the angles between the films.

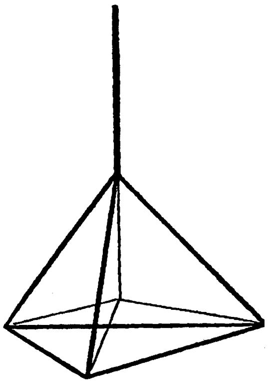


Fig. 73

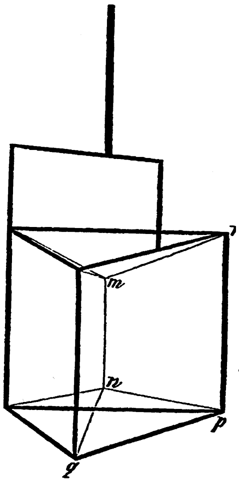


Fig. 74

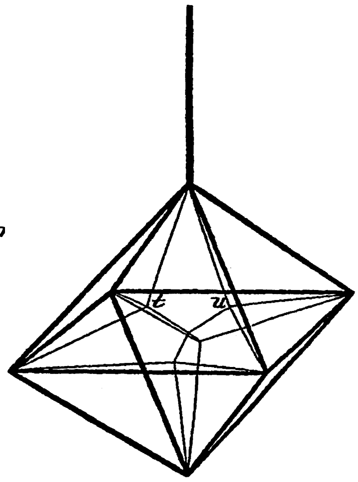


Fig. 75

I reproduce here (figs. 73, 74 and 75) drawings of these three systems; one should not forget, for the understanding of these drawings and the majority of those which will follow, that from each solid edge leaves a film moving towards the interior of the frame, and attached to the other films by liquid edges, which in the drawings appear as fine features. The system of the tetrahedron, one sees, is simply made of six films which join along four liquid edges leading all to the center of the shape; that of the triangular prism presents two pyramids being based on the bases of the frame and whose tops and are at the ends of a liquid edge parallel to the sides of the prism; finally that of the octahedron, one of the most beautiful that one can obtain, contains, in addition to the twelve films based on the solid edges, six quadrilaterals lengthened in the direction of one their angles, having each one the vertex of this angle at the one of the corners of the frame, and the oppposite vertex at the center of the shape; these quadrilaterals are laid out so that, for each pair leading to two opposite corners of the frame, their planes are at right angles to each other. Let us say here that, in the octahedral frame, other systems can be formed; we will speak about it later.

In the system of the tetrahedron (fig. 73), the equality of the angles is obvious, all being perfectly symmetrical in all directions; besides one sees that the four liquid edges occupy precisely the positions of the straight lines *oa*, *ob*, *oc*, *od* of fig. 70. In that of the triangular prism (fig. 74), the angle between the vertical edge *mn* and the oblique edge *np*, for example, having $-1/3$ for cosine, it follows that the vertical height of the point *n* above the plane of the base is a third of *np*; from which it is easily deduced that by indicating by *a* the length of the edge *pq* of the base, this vertical height is equal to $\frac{a}{2\sqrt{6}}$. According to that, if *b* indicates the length of the side edge *pr* of the frame, one has

$$mn = b - \frac{a}{\sqrt{6}}.$$

In the frame that I employed, the edge *b* was 70.70mm, and the edge *a* 69.23mm[115];

[115] It is necessary to explain here how these evaluations were obtained. Since a wire frame cannot be geometrically perfect, I measured each side edge separately, and I took the average of the results, then I did the same with regard to the edges of the bases; but these measurements were not taken directly on the edges, because the terminations of those were more or less masked by the weldings. Here is how I proceeded: the frame being placed vertically, and one of its side faces compared to the cathetometer, I determined, while aiming as close to the vertical edge of right-hand side as the weldings allowed, the distance between the two horizontal edges, then I did the same thing close to the left vertical edge; then I turned the prism on its axis in order to successively present to the cathetometer the two other side faces, and I repeated on each one of

in substituting these numbers in the formula above, one finds mn = 42.44mm; however measurement with the cathetometer gave mn = 42.37mm, a value which differs from the preceding one only by 0.07mm, i.e. by less than two thousandth part.

As for the octahedral system (fig. 75), it presents, in each quadrilateral of which I spoke, a dimension easy to measure with the cathetometer: it is the distance between the vertices t and u of the two opposite obtuse angles. On the other hand, on the basis of the principle that these two angles, as well as that whose vertex is in the center of the shape, must have $-1/3$ for cosine, one easily manages to show that this distance tu must be exactly one third of the length of the edges of the octahedron. To show it, let $stou$ (fig. 76) be one of the quadrilaterals; let us make the two diagonals tu and os, which will intersect at right angles at m; let us indicate by d the distance tu, by α the common value of the three obtuse angles, and by a the length of the edge of the octahedron. In the triangle ots, the angle at t being α, and the angle at o being $\frac{1}{2}\alpha$, the angle at s will be $180^\circ - \frac{3}{2}\alpha$; it follows that the two right-angled triangles omt and smt will give

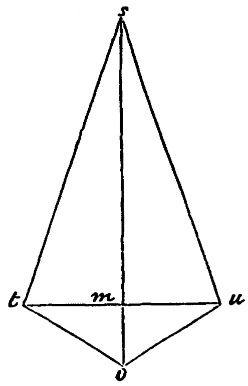


Fig. 76

$$om = \frac{\frac{1}{2}d}{\tan\frac{1}{2}\alpha}, \quad \text{and} \quad ms = \frac{\frac{1}{2}d}{\tan\left(180^\circ - \frac{3}{2}\alpha\right)}.$$

Note now that the diagonal os, the sum the two lengths om and ms, is half the height of the octahedron, and consequently, as one will easily see, is equal to $\frac{a}{\sqrt{2}}$. One will thus have

$$\frac{1}{2}d\left\{\frac{1}{\tan\frac{1}{2}\alpha} + \frac{1}{\tan\left(180^\circ - \frac{3}{2}\alpha\right)}\right\} = \frac{a}{\sqrt{2}}.$$

But, knowing that, the cosine of the angle α is $-1/3$, one will find

$$\tan\frac{1}{2}\alpha = \sqrt{2} \quad \text{and} \quad \tan\left(180^\circ - \frac{3}{2}\alpha\right) = \frac{1}{5}\sqrt{2}.$$

These values being substituted in the formula above, one obtains, as I have claimed,

$$d = \frac{a}{3}.$$

them the same operations; I measured thus, for the length of the side edges, six not very different values, whose average was 69mm, 83.

That done, I laid out the frame horizontally on suitable supports in such a way that, one of its side faces still facing the cathetometer, two edges of the bases which bounded it on the right and on the left were vertical, and I operated with regard to those as with regard to the preceding ones, then, turning the prism on its axis, I passed in the same way to both other faces, so that I had also, for the edges an average of 68.36mm.

But these two averages were to undergo a small correction; indeed, the liquid films do not end on the solid wire along the lengthwise lines of which I measured the distances, but along other lengthwise lines located more towards the inside of the frame, from which it follows that the two numbers above are a little too small. To arrive at the correction in a simple way, let us notice that under the influence of the tension, the plane of each one of our films must pass through the axis of the cylindrical solid wire on which the film abuts, and that, consequently, the prolonged liquid edges will lead to the points where these axes intersect; we thus can, without modifying in any manner the film system and, consequently, the length of the liquid edge mn, substitute, in thought, for our frame, the whole of the axes of the solid wire which make it up, which obviously amounts adding to each one of our two numbers the diameter of this wire; however this diameter, found also by means of the cathetometer, was found, as the average of ten measurements taken on various wires, equal to 0.87mm; it is by the addition of this quantity to our two numbers that one obtains the values of b finally given in the text.

In my frame, measurements have given[116] for the value 69.49mm, of which the third is 23.16mm; and for the distance d, the value 23.14mm. The difference 0.02mm between the computed value and the measured value is, one again sees, as unimportant as in the preceding case.

§ 186. The film systems of the other frames, i.e. those which contain curved films and consequently curved liquid edges, further verify, and in a quite curious way, though a little less precisely, the equality of the angles at which these edges end at a liquid point; they thus also verify, at least in its immediate vicinity (§ 181), the equality of the angles between the three films which join along each one of these same edges.

Let us take as the first example the film system of a cubic frame, a system for which fig. 71 reproduces the drawing. Each angle of the central quadrangular flat film having to be of 109 degrees and half, and consequently greater than a right angle, it follows that the sides of this flat film cannot be rectilinear and must constitute slightly convex arcs towards the outside; however, it does, indeed, appear so in the system that is formed.

I represent in fig. 74 the film system of the equilateral triangular prism, and I said that by indicating by a the length of the edges of the bases, the height of each film pyramid which is based on these bases is equal to $\frac{a}{2\sqrt{6}}$; but it is understood that if the height of the prism is less than double of this quantity, or, in other words, less than 0.4 of the sides of the bases, the system in question could not occur. In this case, the analogy with other systems about which I will speak hereafter had led me to predict that the system would be composed of an equilateral triangular film parallel with the bases, placed at equal distances from these last, and attached by other films to all the solid edges; but as the angles of this film triangle were to be 109 degrees and half, but the angles of an equilateral triangle with rectilinear sides are only 60°, it was necessary that the sides of our film triangle were convex towards the outside, like those of the flat film of the system of the cube, but that their curvatures were much more marked; however, that is what experiment fully verified; only the curvatures weren't very strong except in the vicinity of the corners. The height of my frame was approximately one third the length of a side of the bases; fig. 77 represents the system seen from above; this system thus constitutes a second example in support of the proposal in question.

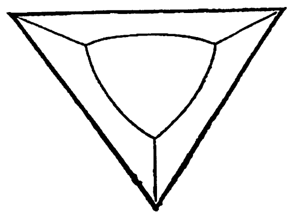


Fig. 77

In the third place, if one takes for the frame that of a regular pentagonal prism of which the height is not too large relative to the bases, the film system presents, in the middle of its height, a pentagonal film parallel with the bases, and to which come to be attached, as with the triangular film of the preceding system, all the films based on the solid edges; however, the angle between two contiguous sides of a regular pentagon being 108°, it is very close to our angle of l09°, so it follows that the sides of the pen-

[116] A regular octahedral frame being able to be regarded as formed of the whole of three squares made out of iron wire whose planes intersect along the diagonals, the frame was placed so that these squares faced the cathetometer one after the other with two of their sides directed vertically, and, in the three positions of the frame, I measured, also as close as possible to each one of these vertical wires, the distance between two horizontal wires. I thus obtained twelve values, whose average was 68mm, 59; I took, moreover, on various wires, eight measurements of their diameter, measurements which gave an average of 0.90 mm. According to the considerations explained in the preceding note, I added this diameter to the number above, and thus I found the value indicated in the text.

As for the value of d, as the small irregularities of the frame introduce slight differences between the six quadrilaterals, I measured this distance in each one of them, and the value of d in the text is the average of these six measurements.

tagonal film must be appreciably straight, and it is again what experiment confirms: in the frame that I employed, the length of the edges of the bases is 5 centimetres, and the height of the prism is 6 centimetres; the sides of the pentagonal film are approximately 2 centimetres, and the the eye cannot distinguish any curvature there. In this system, the films which leave the solid edges of the bases and will be attached to the sides of the central pentagon, are plane, and that must be, since they are bordered on the one hand on the rectilinear edges of the bases and on the other hand by the appreciably straight sides of the central film; it follows that the oblique liquid edges which go from the corners of the bases to those of the central pentagon, seem straight. As for the triangular films which leave the solid vertical edges, they are strictly plane in consequence of the symmetry of their position. The system in question is represented by fig. 78.

In fourth place finally, if the frame is that of a regular hexagonal prism, the film system is similar to the preceding in its general arrangement, the central film being, of course, hexagonal; but as the angle on two contiguous sides of a regular hexagon is 120°, i.e. notably greater than our angle of 109°, the sides of the film in question must be appreciably curved towards the interior, and that is what the system formed shows. The height of the frame that I employed is in the ratio 7 to 6 to the distance between two opposite sides of the base, or, in other words, to the diameter of the circle which would inscribe this base.

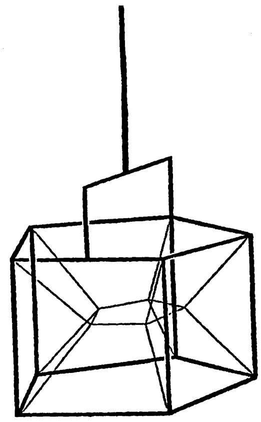


Fig. 78

§ 186bis. Moreover, one can form a system with curved liquid edges, in which the angle between these edges is suitable for direct and precise verification: in a large wire ring provided with three feet, one forms a plane film of glyceric liquid, then one deposits on this film, close to one another, two small bubbles of the same liquid sensibly equal in diameter. Each one of them tends to be transformed into a biconvex film lens, encased by its edge in the plane film; but, because of their contact, these two lenses join themselves with a partition. Except by unusual chance, they will not be exactly of the same diameter, but one can eliminate the difference by aspiring a small portion from the air contained in largest, by means of a tapered tube whose point was wetted beforehand with glyceric liquid (note of § 175); then the partition is plane, and the system, seen from above, presents the aspect of fig. 78bis; *ab* is the projection of the partition.

The ring of which I made use has a diameter of 20 centimetres; it is made of a wire of 3 millimetres thickness, and the feet are 8 centimetres high. To carry out the experimental verifications announced, I traced on a paper, on the basis of the theoretical value of the angle at *a*, the projection of the system, which is extremely simple: the angle of the two tangents at *a* must have, we know, for cosine $-1/3$, and that of the two radii which would join the point *a* to the centers of the two lenses, is the supplement; this last angle was constructed, and I extended each of its sides a length of 4 centimetres from *a*; then, taking as centers the two points thus determined, I traced two portions of circumferences with endpoints *a* and *b*; finally I drew the line *ab*.

One conceives that if, after having formed in the ring a system a little smaller than this drawing, one sets this same drawing between the feet of the ring, and then, closing an eye, one holds the other at a suitable height above the system, one will be able to ensure oneself if it is possible to move the assembly of the lenses so it is projected exactly on the diagram. However, to do the experiment in this manner would offer difficulty, because the system of lenses, obeying small agitations of the ambient air, changes place continually. To remove this problem, I stretched beforehand, across a diameter of the ring, a thin wire, which, after the creation of the plane film, was necessarily embedded in it, and I deposited the two bubbles in such way that the wire was embedded also in the partition; the system loses its mobility thus.

To carry out the experiment, I covered the drawing with a thin glass plate in order to protect it, then, after having developed the plane film, I set the ring by its feet on this plate, and I made it so that, to the eye placed well above, the wire projected on the line *ab* of the drawing, a straight line which I had prolonged besides on the two sides; the two bubbles then were deposited, and I equalized with great care the two lenses. Then the eye suitably placed saw effectively the contour of the assembly of the real lenses to be projected exactly on the drawn contour.

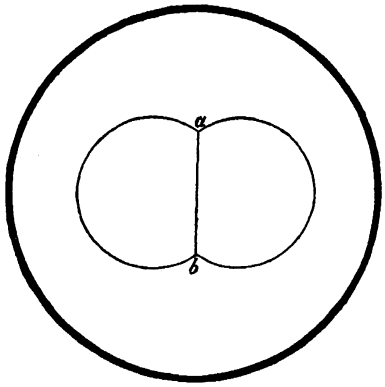


Fig. 78bis

§ 187. The fact about which I spoke (§ 180) to show that a system in which more than three films meet at equal angles at one liquid edge is in a state of unstable equilibrium, refers also to a system which would offer at the same time more than four edges leading to the same liquid point. Instability should thus be also attributed to this last circumstance, and it should be decided if it belongs exclusively to one or the other, or only to their meeting.

For that, let us return to the experiment described in § 139, i.e. take as the solid system a set of two rectangles which intersect at right angles in the middle of two of their opposite sides; only, to make the effect apparent and more convenient to handle, let us give to the two rectangles less width and more height (fig. 79).

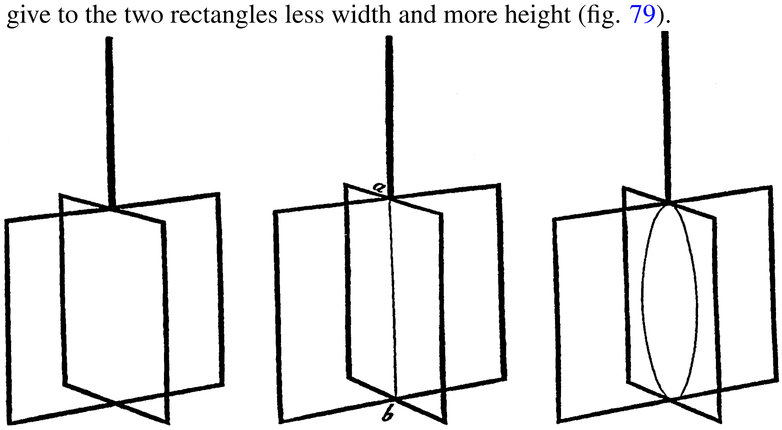


Fig. 79 Fig. 80 Fig. 81

The simplest film system that one can conceive in this frame would be composed of four plane films respectively occupying the four halves of the rectangles and meeting along a single rectilinear edge *ab* (fig. 80), which would join the two points of intersection of these same rectangles. This system, because of its symmetry, would be obviously an equilibrium system, and it would not present any liquid point common to several edges; but the edge *ab* would be common to four films. However, when one withdraws this frame from the glyceric liquid one never finds it occupied by the system that I have just indicated: when it is formed, instead of the edge *ab*, there is always, as with the frame of § 139, a plane film (fig. 81) bordered by two curved edges to which are attached the curved films based on the solid edges. Here, it is seen, each one of the two liquid edges is common only to three films, and it should be concluded that instability is really a property of the film systems in which this condition would not be met.

Let us add that Brewster, who was pleased to reproduce and vary my film systems, has imagined[117] an extremely curious modification to the experiment above: he made the two rectangles mobile around their points of intersection, so as to vary the plane angles, and then, by gradually increasing those angles bisected by the oval film, he saw this film narrowing progressively, in such a way that, when the angles concerned became 135°, the oval film was reduced to a simple straight liquid edge joining the two points of intersection; but, immediately, the system changed, and a new oval film appeared, bisecting the two other plane angles. Brewster thus succeeded in creating, but only for one inappreciable moment, a system in which four films are linked by the same liquid edge, and this pretty experiment completes the verification of the instability of such a system.

As for the second circumstance, I will initially point out that if, in the cubic frame, one conceives a system formed of twelve triangular plane films based respectively on the twelve edges solid and leading to the center of the frame (fig. 82), this system, because of its perfect symmetry, will be necessarily an equilibrium system, and one easily sees that at each liquid edge only three films will meet and, moreover, will form between them equal angles; but there will be eight liquid edges leading to the central point. However we know that with the glyceric liquid, this system does not occur, and that one always obtains that of the fig. 71, in which only four liquid edges lead to each corner of the central quadrangular flat film. It is allowed to conclude from this fact that instability also belongs to any system in which one liquid point is common to more than four edges.

One can obtain in a permanent way the system of fig. 82; but it is, as in the case of instability of § 138, by introducing into this system a solid part; it is enough, indeed, to stretch, from a corner of the frame to the opposite corner, a very fine wire. However, when one withdraws the frame thus laid out from the glyceric liquid, the system which occupies it is not immediately that in question; it still contains a quadrangular flat film; only it is much smaller than that of fig. 71, and is not placed symmetrically with respect to the frame: it is based by one of its corners on the middle of the solid diagonal; but one sees it soon decreasing spontaneously in extent, until vanishing completely, so that the system then becomes that of fig. 82. Changes stop there, and the system remains perfectly stable in this state, when the waning of the flat film happened with enough slowness; but often this waning is faster, and then occurs another singular phenomenon: at the moment when the flat film vanishes, one sees it reform another much smaller still, located on the opposite side of the solid diagonal, having its plane perpendicular to that of the first, and resting, either by a corner, or by the middle of one of its sides, on the middle of the solid diagonal[118] then this second flat film decreases and vanishes like

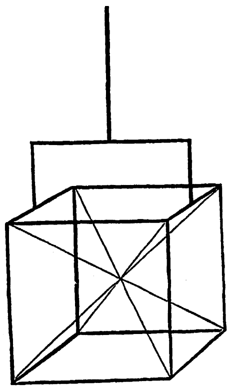


Fig. 82

[117] *On the figures of equilibrium in liquid films* (TRANSACT. OF THE ROYAL SOCIETY OF EDINBURGH, vol. XXIV, 1867).

[118] My frame having been accidentally deformed, then repaired, the second plate, when it occurred, was not placed completely any more as indicated above; the difference undoubtedly came from a small irregularity

the preceding one; in this case thus, the system reaches its final form only by a kind of oscillation.

§ 188. One must, I think, now regard as well established for all film assemblies, the laws which I have just discussed. However these laws lead us to an extremely remarkable consequence: the foam which is formed on some liquid, for example on Champagne wine, beer, or soap water which one agitates, is obviously a film assembly, composed of a crowd of films or partitions which meet and which imprison between them small gas portions; consequently, although all seems governed by chance, it must be subject to these same laws; thus its innumerable partitions join necessarily everywhere three at a time at equal angles, and all its edges are distributed in a way that there are always four of them meeting at one point, forming equal angles there.

I verified these facts by the following experiment: I immersed at the bottom of a vessel containing glyceric liquid the head of a slightly obliquely held pipe and I blew in a continuous way through the pipe, in order to produce a series of many rather large bubbles of air which rose through the liquid. I caused thus, as children with soap water do, the formation of an assembly of partitions rising between the edges of the vessel, an assembly obviously of the same constitution as the foam, but of which the various parts have dimensions much larger; however, also as far the eye could penetrate in this system without being obstructed, it was recognized that everywhere one edge was common only to three partitions, and that there were never but four edges leading to one point. As for the equality of the angles between these edges, there were certain places where three of those which led to a point seemed to be a little nearer to the same plane, but, by looking with attention, I noted that these edges were strongly inflected approaching their point of meeting.

One explains easily the generation of such an assembly, and consequently that of foam: the first gas bubbles which arrive at the surface of the liquid give rise to segments of spheres which are joined as those with which we occupied ourselves previously, and soon all the surface of the liquid is covered with them; then the films which produce the subsequent gas bubbles rise necessarily into this first assembly, by making the formation of lower partitions, in such a manner that there are, in a short time, two systems of films superimposed, then, the gas bubbles continually arriving, this unit is raised in its turn, and so on, the whole being laid out with more or less symmetry, following the differences in volume of the successive gas bubbles and the distribution of the points where they reach the surface of the liquid, and the light assembly made up of partitions imprisoning in spaces which they separate all the volumes of gases which constituted respectively the bubbles, acquires more and more height. If the bubbles are very tiny, the partitioned assembly will be composed of parts too small for the eye to distinguish in general, and one will have thus a foam.

§ 189. Let us return to film systems on frames, and complete their study. Let us examine initially the way in which the films which constitute one of these systems must be arranged while one withdraws the frame from the liquid, and immediately after one withdraws it.

Let us start with the case of a prismatic frame withdrawn so that its bases are horizontal. When the upper base leaves the liquid, each solid edge of which it is composed will be necessarily followed by a film. That being so, if the angle between two adjacent side faces of the prism is equal or greater than 120°, i.e. than one of the equal angles which three films form between them when meeting at a liquid edge, the films, which will leave, as I have just said, all of the edges of the base, will, one will see, remain attached to the vertical solid edges, and that as long as the lower base will not have left

existing in the frame, either before, or after its repair.

the liquid.

Let us take as example the frame of a regular hexagonal prism, a prism for which the angle of two adjacent side faces is 120°, and consider when it is yet only partly out of the liquid. Let us suppose, for the moment, that the films from the edges of the upper base slant towards the interior of the frame, in which case other films will leave necessarily from the vertical edges to lead to the liquid edges which will link the first. All these films will be attached to the liquid of the vessel by small masses with concave transverse curvature, raised along the films' lower edges; however it is clear that under the influence of their tension, these same films will end at the tops of those small masses along vertical directions; the films which leave the edges of the base will have consequently to inflect while going down towards the liquid, and thus they will be, in the direction of their height, convex towards the interior of the shape. But as they will be in contact by their two faces with the open atmosphere, it must be (§ 97) that their mean curvature is zero, or, in other words, that at each one of their points, their curvatures, in two perpendicular directions, are equal and opposite (§ 2); therefore, since the films in question are convex in the direction their height, they will be concave in the direction of their width; however, by the double reason of this concavity and their direction inclining towards the interior of the frame, our films will necessarily make between them pairwise angles greater than those of the faces of the prism, and consequently greater than 120°, which we know is impossible; thus these same films will have to remain, as I have claimed, adherent with the side solid edges as long as all the frame is not out of the liquid.

This deduction is fully confirmed by experiment when the frame of a regular hexagonal prism is withdrawn from glyceric liquid, in the position indicated; one obtains simply, until the lower base leaves, plane films occupying all the side faces.

The thickness of a solid wire, a thickness which, in my frames, approaches a millimetre, would seem, in truth, to be enough to establish independence between these films; but here it has nothing to do with the phenomenon: I built a hexagonal prismatic frame in which the side edges were simply hairs, and things happened in absolutely the same manner. In this frame, of which the arrangement would be difficult to represent by a small engraving, the upper base is raised by springs, so that the hairs, which lengthen in the liquid, are always taut.

§ 190. But, it is understood, it cannot any more be thus in the case of a frame whose bases have less than six sides, because then the side faces form between them angles less than 120°, the films which start from two adjacent edges of the upper base and which, along the corresponding vertical solid edge, would be in communication by the intermediary of the liquid which wets this edge, must tend to be detached from this same edge and to move both towards the interior of the frame, in order to restore between them the angle of 120°; then also, of course, they will develop a third film based on the vertical solid edge in question, in such a way that these three films are linked by an oblique liquid edge based at the junction point of the three solid edges.

Experiment also verifies that when one withdraws a prismatic frame with a square base or a cubic frame from the liquid, one sees, as soon as the upper base leaves, the films take an inward direction, and the effect is more marked still with the frame of the equilateral triangular prism; one notes, at the same time, that these films behave as I have claimed in the preceding paragraph, i.e. they inflect to lead to the surface of the bulk liquid in vertical directions, and they are concave in the direction of their width.

As for the regular pentagonal prism, for which the angle of two adjacent side faces is 108°, and consequently rather not much lower than 120°, one conceives that the tendency of the films to slant inward must be weak, and that thus the thickness of the metal wire of which the solid edges of my ordinary frames are formed, is sufficient, in this case, to establish independence between the films; with the pentagonal frame of fig. 78, one does obtain only, while withdrawing it, plane films in the side faces; but I made a frame in which the side edges were very fine wire, and then the films slanted inward; only, as must be, they slanted much less than for both preceding frames. This frame with fine side edges is represented in fig. 83; the bases are held by means of two handles *a* and *b*, by which one holds the frame to immerse it.

§ 191. Now let us see how these various systems are completed, when one continues to raise the frames. Let us take again initially the case of the hexagonal prism, and suppose that the frame has the ratio of dimensions indicated at the end of § 186. When the lower base emerges, there is formed a film extending from this base to the surface of the liquid, a film which is narrowing from top to bottom. If the frame is raised further, one soon reaches a point where the equilibrium of this last film is not possible any more; because it then spontaneously collapses with speed, and is closed while separating from the liquid of the vessel, and comes to constitute a plane film across the lower base of the prism. But this plane film, making right angles with those which occupy the side faces, will not be able, according to what was known as in the preceding paragraph, to persist thus; the films will have to be arranged so that the base film makes angles of 120° with the side films. However, that is what happens in the simplest way: the base film in question climbs in the interior of the frame while decreasing in area, and thus draws with it the side films; each one of these last thus divides into two the liquid edge which links it with the first, while other films, based on each vertical solid wire, are attached also to the preceding ones along the liquid edges which link those pairwise, and equilibrium is established when the central film reaches half the height of the frame, because then all is symmetrical; there is thus the system of which I spoke at the end about § 186.

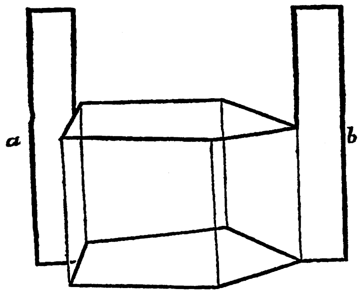


Fig. 83

One understands that, in this system, the oblique films which move towards the central film can make pairwise angles of 120° only in the condition of being convex, in the direction of their width, towards the interior of the shape, which applies also to the curves on the sides of the central film; but, in consequence of the need of a zero mean curvature, this convexity requires that the films in question be concave in the direction their height; in this manner all the laws are satisfied, and, in the system formed, one notes indeed these two opposite curvatures of the oblique films. As for the central film and the films based on the side solid edges, they are necessarily plane, because of their symmetrical position compared to the others.

A thing to notice is that, in this same system, the oblique liquid edges, which go to the corners of the central film, do not leave exactly the corners of the two bases, but points located at a small distance from these last corners, on the solid side edges; one easily grasps the reason for it: in consequence of the rectilinear shape on the sides of the bases, the transverse convexity of the oblique films could not exist close to them; it is thus necessary, so that it can be established, that these films do not leave the solid side edges to take their oblique directions, but start at a certain distance from the bases; it is thus at this distance only that can be born the oblique liquid edges.

§ 192. Let us pass to the cases of the pentagonal prism with fine side edges, quadrangular prism or cube, and triangular prism, cases in which the films based on the edges of the upper base take, as we saw, inward slanting directions as soon as this base leaves the liquid. When these films start to be shown, the small masses raised on their lower border will necessarily draw on the surface of the liquid a polygon of the same number of sides as the solid base, i.e., according to the frame, a pentagon, a quadrilateral or a triangle. Only, as the films in question are vertical at their lower part and they join at angles of 120°, it will be necessary that the sides of the polygons above also form between them angles of 120°, which requires obviously that they be convex towards outside; these sides must share besides the horizontal curvature of the films, curvature which we know to be concave towards the interior of the shape and consequently convex towards the outside. This convexity on the sides of our polygons will, one understands it, be very slight for the pentagon, more marked for the quadrilateral, and more still for the triangle. All that is also verified by experiment.

These first facts established, let us follow the development of the film system in each of the three frames separately, as it is raised more.

With the pentagonal frame, the curvilinear pentagon drawn on the surface of the liquid is initially narrowed a little, until a rather considerable part of the height of the frame is out of the liquid; then it widens again, and, when the lower base is level with the liquid, the slanting films come to attach, by their lower edges, to the sides of this base. However these films then do not occupy the side faces of the prism : they belly slightly, in the direction of their height, towards the interior of the frame, so that they are joined pairwise by liquid edges of which each one constitutes an arc of low curvature resting at its two ends on the ends of a solid side edge. Lastly, shortly after the exit of the lower base, the phenomenon is completed as for the frame of the preceding paragraph; i.e. the film extending from the base in question to the liquid comes, in plane form, to occupy this same base, then goes up quickly while drawing inward the other films, to give, ultimately, the system of fig. 78.

With the frame of the quadrangular prism, the curvilinear quadrilateral drawn on the liquid decreases until it vanishes, then is replaced by a small horizontal liquid edge, the ends of which each connect to two downward liquid edges, which deviate from each other; these three edges border a vertical film plane parallel to two of the faces of the prism and being attached by other films to the solid edges. Things do not change in nature when one continues to raise the frame: the plane film in question simply increases in height and becomes increasingly broad in its lower part, until the lower base of the prism starts to emerge from the surface of the liquid; then the two downward liquid edges are based at their ends at the middles of two opposite sides of this base; then, after the exit is complete, the film which comes to occupy the base is transformed quickly into four oblique films, two trapezoids and two triangles, which complete the system. In the particular case of the cube, there is thus the system of fig. 71. If the height of the frame is larger than the length of the sides of the bases, the oblique films which are based on these last are identically the same ones as for the cube, and the films based on the side edges as well as the central film planes simply have more height.

Lastly, with the frame of the triangular prism, the curvilinear triangle on the surface of the liquid narrows more quickly, and vanishes when the frame has risen only a small distance, so that the triangular pyramid which must be based on the upper base in the final system, is completed; then, continuing to rise, one sees a vertical straight liquid edge extending from the apex of this pyramid down to the surface of the liquid, an edge which is common to the three films based on the side solid edges. Things remain the same while one raises further, the three films above and the vertical liquid edge only increasing in height, until after the exit of the lower base; then the film which goes across this base converts instantaneously into the second triangular pyramid, which completes thus the system of fig. 74; I suppose, of course, that the frame has a sufficient height not to give the system of fig. 77.

§ 193. Now let us take a symmetrical frame around an axis passing through a corner, such as that of a regular pyramid, that of a regular octahedron, etc, and withdraw it by this corner. It is obvious that, in this case, it cannot form films occupying the faces which meet in the corner in question, because the space that they would leave between them and the liquid would be air space; it is thus necessary that the films based on each solid edge respectively move towards the interior of the frame.

If there are only three solid edges joining the corner in question and symmetrically laid out, as in the tetrahedron, or the cube which one would withdraw by a corner, it is clear that the films based on these three solid edges will be linked by a single liquid edge going down vertically from the solid top corner of the surface down to the liquid of the vessel, and that is what happens indeed. With the regular tetrahedron, things go thus until after the exit of the base, then the system is completed in the same manner as the triangular prism, and gives the result of the fig. 73.

If there are more than three solid edges leading to the corner which one withdraws, it will necessarily, by the fact of the instability about which I spoke in §§ 180 and 187, form additional films. Let us take as an example the regular octahedron. One understands that the films based on the four solid edges will be linked, not according to a single liquid edge, but according to two liquid edges based on the top corner and bordering a vertical auxiliary film, in a manner so that along each one of these last edges three films meet forming between them equal angles. The auxiliary film is destined to form, in the complete film system, the upper quadrilateral (fig. 75). Until the square, the common base of the two pyramids which constitute the octahedron, leaves the liquid, the whole of the films keeps the same arrangement; then, while one continues to withdraw the frame, one sees modifications occurring that it would take a long time to describe, and consequently the system tightens towards the form drawn in fig. 84, where the two faces *abc* and $a'b'c$ are each occupied by a plane film. This form is completely reached at the time when the lower corner of the frame leaves the liquid; but at once a change takes place, and the system takes the form of fig. 75. Although this change is very fast, one however can, with a sufficient attention, and by starting the experiment again several times, observe how it occurs: the two films which occupied (fig. 84), as I said, the faces *abc* and $a'b'c$, are raised towards the interior of the frame while turning around the solid edges *ab* and $a'b'$, and at the same time there develops, starting from the lower corner, an initially very small quadrilateral, which grows until its upper corner reaches the center of the frame, and which then constitutes the lower quadrilateral of the final system; at the same time also the corners *f* and *g* of the curvilinear quadrilateral *sfgc* go up a certain distance; this quadrilateral is shortened, its edges become straight, and it forms finally the higher quadrilateral of the same final system. Fig. 85 represents the phenomenon in the process of formation, frozen at the moment when the quadrilateral which increases has acquired half its final height. One will easily understand, according to this drawing, how are generated the four other quadrilaterals of fig. 75.

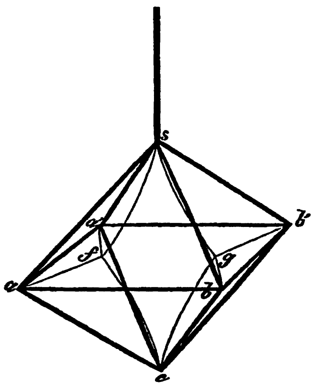


Fig. 84

So that all these phenomena reliably occur, it is necessary to withdraw the frame quite vertically; it is necessary, moreover, that this frame is well built, that the iron wire which make it up have the least possible thickness, and especially that at the corners of the octahedron, they are linked in a quite accurate way, at least on the side which faces the interior of the frame.

As I already said, this same frame can also give systems very different from that of the fig. 75. These systems are made of curved films; they develop especially when one slightly inclines the frame while withdrawing it from the liquid: one of them, for example, which appeared rather often at home, contains, in its middle, a hexagonal film placed parallel to two of the faces of the octahedron, and having the sides slightly curved inward (§ 186); these sides are attached to the tops of the two faces above by triangular films, and to the edges of these same faces by trapezoidal films; moreover, the corners of the film concerned are attached by triangular films to the other solid edges.

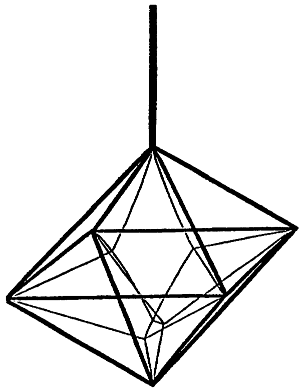


Fig. 85

The various examples that I gave with details will be enough to render comprehensible how film systems are generated, and to show that theory can give an account of all the characteristics that this generation presents.

§ 194. If care is taken that there are no air bubbles on the surface of the liquid of the vessel before immersing a frame there, the film system obtained will not present any space closed on all the sides by films, and thus all the films will be in contact by their two faces with the open air. Indeed, while one withdraws the frame, if the system, before it reaches the fast modification which gives it its final arrangement, contained a space closed on all the sides by films, this space should have been born and grown in size with the rising of the frame; however that is impossible, since the air which should fill it would have had no entrance to penetrate there; for the same reason, the system, during this time of its generation, could not present any space closed partly by films and partly by the surface of the liquid; finally, when the fast modification occurs, the film or the films which go up then in the system not finding space of the second kind to complete enclosing of it with film, the complete system necessarily satisfies the condition indicated.

We insist here on another law, being that, in the film system of an arbitrary frame, all the films constitute surfaces with zero mean curvature; that is obvious since, according to what precedes, all have their two faces in the open air.

§ 195. Let us return to the systems of prismatic frames. In addition to the facts that I have explained, these systems presented other curious facts to me that I will report here.

The system which one obtains with the pentagonal frame of fig. 78 is, we saw (§ 186), composed of appreciably plane films; however if one considers oblique films which leave two corresponding sides of the two bases to link with one of the sides of the central pentagonal film, and if attention is paid that these two oblique films must form between them an angle of 120°, one will see obviously that, for bases of given size, an increase in the height of the prism involves a reduction in the extent of the central pentagonal film, and that there is a limiting height beyond which the existence of this film is impossible. By supposing that the oblique films are rigorously plane, one easily finds that the limit in question corresponds to the ratio between the height of the prism and the diameter of the circle inscribing the base being equal to $\sqrt{3}$, i.e. to 1.732; but, because of the small curvature of the films above, that is only an approximate value; we will find the exact value below (§ 204).

One must naturally wonder what becomes of the film system when this limit is exceeded. To find out, I built a frame in which the height was approximately $2\frac{1}{2}$ time the diameter of the inscribed circle, and it gave me a singular result: when it was withdrawn from glyceric liquid, as the side edges are of ordinary wire, all the side faces are initially occupied by plane films, and, after the complete exit, a plane film is also formed across the lower base, then climbs between the others by forming a pentagon which is decreasing, all that as with the frame of fig. 78; but the pentagonal film decreases much more quickly, then vanishes, and, at that moment, the system makes an abrupt change, by making an odd arrangement which it would be difficult to represent in a clear way by a perspective drawing, but of which I however will try to give an idea. On the two bases rest respectively two inward slanting identical assemblies, composed of five curved films, and of which one is represented in projection in the field of the base in fig. 86; it is seen that there is in each one of them a pentagonal film, two quadrangular films and two triangular; these two assemblies are connected between them by films which leave the five side edges of the prism, and by two other intermediate films also directed along the length of the frame and based on the liquid edges *ab* and *bc* of one of these same assemblies to lead to the corresponding liquid edges of the other.

I do not need to point out that the same thing would still occur if, instead of exceeding the limit indicated, one reached it exactly, i.e. if one gave to the prism the height which would correspond precisely to the vanishing of the pentagonal film; indeed, there would be then ten liquid edges meeting, in the center of the system, at one liquid point, and consequently the equilibrium would be unstable.

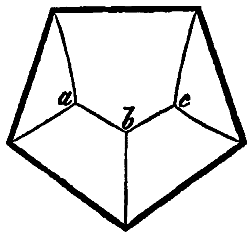


Fig. 86

§ 196. Although, in film systems of the prisms with a greater number of sides, the oblique films must be notably curved, it appear to me probable that there was to be also, for each one of these prisms, a limit height beyond which the system could not contain a central polygonal film, and that this limit should differ little from that relating to the pentagonal prism. To verify this, I tried the hexagonal prism initially, with a frame of which the height was also about 2 times the diameter of the circle which would inscribe the base. However, to my great surprise, it still formed a central hexagonal film, though much smaller than with the frame of § 186; but the system had undergone a modification which maintained the possibility of existence of this film: points of the side solid edges from which the oblique liquid edges left (§ 191) were located much further from the corners of the bases, in the kind of way things would be arranged if, actually, the frame had been shortened. In this arrangement, the films based on the sides of the two bases thus remain, until a rather long distance from those, adherent with the solid side edges, from which it follows that one must regard the unit as an imperfect film system; I say imperfect, because the films which start from all the sides of the same base are, in the parts which remain attached to the solid side edges, separated from each other and made independent by these edges.

§ 197. For prisms with over six side faces, and in which, consequently, the angle of two adjacent faces is higher than 120°, the films remain, to a much greater extent, attached to the side solid edges. For example, with an octagonal frame in which the ratio between the height and the diameter of the circle inscribing the base is about the same one as in the frames above, the central octagonal film, instead of being small, is on the contrary very large, and the two oblique liquid edges based on any of its corners will contact the corresponding solid side edge at two points whose distance is only approximately a sixth length of this edge, and consequently a little less than half of the diameter of the circle inscribed at the base; in this case thus, the films which start from two corresponding sides of the bases, leave the side solid edges to move towards the central octagonal film only when approaching the middle height of the frame, and, up to that point, they occupy, in an appreciably plane form, the side faces of the prism.

In the hexagonal frame of the preceding paragraph, the distance between the points on a side solid edge where the two liquid edges based on one of the corners of the central film make contact, is approximately the double of the diameter of the inscribed circle; in the octagonal frame, it is, as we have just seen, a little smaller than half of this diameter; in a heptagonal frame, it is, as one would expect, intermediary between these two values, and equal about with the three quarters of this same diameter. I also tested a decagonal frame, and, in this case, the distance in question is only a sixth of the diameter.

The thickness of the metal wire has little influence on these facts; only, with frames in which the side edges are made of very fine wire, the distance between the contact points of the oblique liquid edges increases a little.

But a small difficulty arises: the direction of the inward slanting films is obviously regulated by the need for these films to make between them pairwise angles of 120°, and which, in addition, they are bordered by the concave sides of the central polygonal film. However, in the frames which we have just considered, it seems that these conditions would also be met if the central polygonal film were smaller, and if, at the same time, the films started to slant starting closer to the bases; one can thus wonder what, in the frames of the prisms of a number on sides higher than six, determines the great extent of the central film and the little separation of the points of contact of the oblique liquid edges. Let us say, as of now, that the cause lies in a principle of minimum area that we will explain below (§ 209) and to which all the film systems are subject.

§ 198. I then built an octagonal frame of which the height was only one third of the diameter of the circumcribed circle. Then, according to the value given above for the spacing of the points of contact in question, all these points should have been at the corners of the prism; but it was not, as follows: these same points were still at a certain distance from the tops, and their spacing was only a sixth of the diameter of the inscribed circle; also the octagonal film had increased. The same effect occurred with a heptagonal prism of which the height was half of the diameter of the inscribed circle, i.e. less also than the spacing of the points of contact evaluated previously with regard to the prisms of this number of sides.

Moreover, the phenomenon takes place even in the hexagonal prism, since (§ 191) with a frame of this kind of which the height was only $1\frac{1}{6}$ time the diameter of the inscribed circle, the points of contact of the oblique liquid edges were still at a small distance from the corners.

§ 199. The explanation of these last facts follows from what we said at the end of § 191: let us consider an octagonal or heptagonal prismatic frame tall enough so that the films which leave from the sides of the bases occupy in an appreciably plane form notable portions of the side faces. At the places where these films leave the faces in question to move towards the sides of the central polygonal film, they are, we know, concave towards the outside in the direction of their width; if thus one designs the frame crossed by two planes perpendicular to its axis and passing through the two series of points where are born, on the side solid edges, the oblique liquid edges, these two planes cut the films following arcs concave towards outside, and if these arcs are imagined solidified, the equilibrium of the system will not be disturbed. According to that, if one built a frame having for height the spacing of the points of contact of the oblique liquid edges on one solid side edge, and if one gave to the iron wire which forms the sides of the bases above the curve of the arcs, it is clear that the film system formed in this frame would have its oblique liquid edges leaving exactly the corners;

but with a frame of this height or a lesser height and whose sides of the bases are straight, the condition relating to the transverse curvature of the oblique films, and consequently to the equilibrium shape of these films, obviously cannot be satisfied unless the points of contact of the oblique liquid edges are placed at a certain distance from the corners on the solid side edges.

The facts that we have just announced verify, one sees, that which we claimed at the end of § 197 on the possibility of equilibrium, with regard to a prism of a given number of sides with various dimensions of the central film, and various spacings of the contact points of the oblique liquid edges.

§ 200. If, in the various systems which we have just studied, one compares the central polygonal films, one notes that the curvature on their sides is increasing from the hexagonal film to the decagonal film, which constitutes new facts to add to those of § 186 in confirmation of the law relating to the angles at which liquid edges end in the same liquid point.

The transverse curvature of the oblique films which move towards the sides of the central polygonal film being related to the curvature on these same sides, it must be less in the heptagonal prism than in the octagonal, and less still in the hexagonal one; it is because of the weakness of the curvature in question in the latter prism, that it gives, when it does not have too much height, an almost perfect film system, with its hexagonal film.

§ 201. While thinking of the generation of film systems, I wondered whether, at least in the hexagonal prism, and with the frame of § 196, if one could create a system free from a central polygonal film by withdrawing the frame from the glyceric liquid so that the axis of the prism was horizontal, instead of being vertical as in the preceding experiments. I thus gave to the fork a provision which enabled me to act thus, and I, indeed, succeeded completely. Furthermore, I obtained two different systems, according to whether the frame was withdrawn so that two side edges left the liquid at the same time, or that one made one leave initially, then simultaneously its two neighbors; these two systems are like that which is formed in a sufficiently tall pentagonal frame (§ 195), i.e. they are composed of two assemblies of oblique curved films connected to each other by other films directed along to the length of the prism. The projection of one of these assemblies in the field of the base is represented, in the first mode, by fig. 87 and, in the second, by fig. 88. But that is not all; to produce the first of these two systems, it is necessary, when one withdraws half of the frame from the liquid, to complete the operation very slowly; when one acts without this precaution, it forms a third system of still another kind, a system of which, in spite of its simplicity, it is rather difficult to give a clear idea by a description or a drawing: it contains two curved hexagonal films resting respectively, by one of their sides, on one of those of the bases, and moving obliquely towards the interior of the frame; the other sides of these hexagons have, as always, a concave curvature; the curved sides of each one of these same hexagons are connected to the corresponding sides of the close base and the corresponding sides with the other hexagon by curved films; finally, the liquid edges which link these last films pairwise end at other films based on the side edges of the frame. The sides of the bases on which the two hexagonal films are based belong to the face of the prism which first left the the liquid.

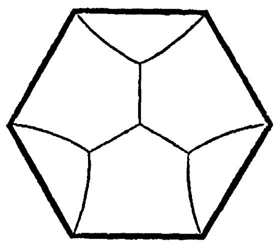


Fig. 87

The heptagonal prism gives similar results, with a frame having its dimensions in the same ratio. Only, initially, the three systems are imperfect, in the sense that, in the first two, the films which leave on the sides of the bases remain, for a certain distance from the corners, adherent with the side solid edges, and which, in the third, the films which go on the curved sides of one of the heptagonal films to the corresponding sides with the other, are attached, on most of their length, with the solid side edges, while affecting, in this extent, an appreciably plane form; moreover, new strangeness, the system provided by the second

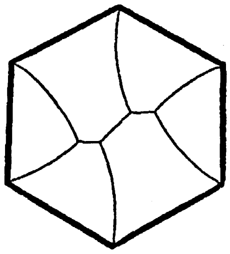


Fig. 88

mode is unstable; barely formed, it starts to change spontaneously: the two assemblies located close to the bases lengthen, initially slowly, then more and more quickly, reach each other, and at once appears the imperfect system with the heptagonal film in the middle. Projection, in the field of the base, of one of the assemblies of the system due to the first mode is represented in fig. 89; projection relating to the second mode could not be drawn, because the spontaneous modifications that the system suffers prevented me from observing this well.

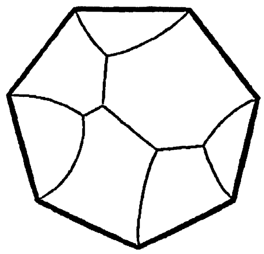


Fig. 89

The facts that I have just described, joined to those which I reported at the end of § 193, show that with certain frames, the results differ according to the way in which one withdraws these frames from the glyceric liquid.

Supposing that the instability of the second system of the heptagonal frame was because this frame did not have enough length, I built another in which the length was triple the diameter of the inscribed circle; but I did not gain anything; moreover, the first two systems occurred with difficulty, and the third was almost always obtained, i.e. that which contains two oblique heptagonal films leaving corresponding sides of the bases. Lastly, with a frame still more lengthened, there is never but this last system.

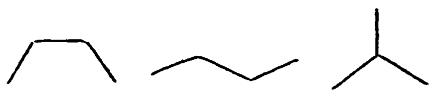
As for the octagonal prism, of which I modified the ratio between its length and the diameter of the inscribed circle, it obstinately refused to give anything but the imperfect systems with the octagonal film in the middle or the two oblique octagonal films, and this last is also the only one which is formed when the ratio is sufficiently large.

§ 202. Mr. Van Rees, who did me the honor of repeating my experiments in Holland, found, with regard to the film systems of prismatic frames, an extremely remarkable principle which he agreed to communicate to me, and which I will describe here; but this principle will be understood without difficulty only by people who have seen the systems to which it applies.

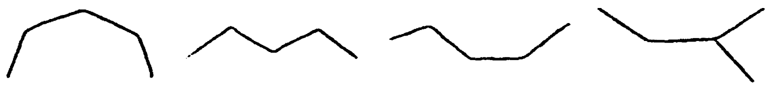
The systems in question are like those of which fig. 86 to 89 show projections. In the systems of this species, we know, about each base of the frame rests a kind of inward slanting pyramid which has an apex of small liquid edges meeting between them, either end to end by forming angles, as in the fig. 86 and 88, or in a way that there are three of them meeting at the same point, as in fig. 87 and 89; each one of these small edges borders, we also know, a film parallel with the axis of the prism and contacting, at its other end, the corresponding small edge on the other pyramid. Now, here is the statement of the principle of Mr. Van Rees:

If n indicates the number of the side faces of the prism, 1° the number of the small edges forming the apex of each of the two basal pyramids, and consequently also the number of the longitudinal films going from the small edges of one of the pyramids to those of the other, is $n-3$; 2° the number of the systems of this kind realizable in the same frame is equal to that of the different open shapes that one can form with $n-3$ small edges, under the condition that there are never more than three meeting at the same point.

Thus, for the triangular prism, one has $n-3=0$, and we know, indeed, that, in the film system of this frame, the apex of each inward slanting pyramid is a simple point. For the quadrangular prism, $n-3$ is equal to 1, there is only one small edge at the apex of each pyramid, and consequently only one possible system of the kind which we consider. For the pentagonal prism, $n-3$ is equal to 2, there are two small edges, and as one can form with those only one open shape, which is an angle (fig. 86), it has also one possible system of the kind in question. For the hexagonal prism, the principle indicates three small edges, and, consequently, the shapes:



There are thus three systems; I had produced two (fig. 87 and 88), and Mr. Van Rees produced all three. For the heptagonal prism, the principle led to these shapes:



from which come four systems; my experiments had provided me only two of them, and all four were obtained by Mr. Van Rees. For the octagonal prism, there are at least thirteen shapes, giving thirteen systems; I have not succeeded in developing any of them; Mr. Van Rees has made several, and believes that one can make them all, but he thinks that the majority are unstable.

To cause the generation of the systems in question here, Mr. Van Rees initially creates the system which contains, at the middle its height, a polygonal film parallel with the bases; then he re-immerses a small part of the base of the frame in the liquid, and then withdraws it; the film plane which then will occupy this base, and which thus imprison air in the space bounded by it, by the oblique films and the polygonal film, goes up while driving out the latter in front of it, and determines, in the middle of the shape, a kind of closed polyhedron film, whose side faces, in same number as those of the prism represented by the frame, are convex towards the outside; that made, he bursts one of these side faces, and the resulting system is then that in which the small edges of which I spoke constitute a broken line of which all the angles are of the same direction. He passes then from this system to one of the others which are appropriate for the frame, by blowing on one or another of the liquid edges parallel to the axis, along the plane of one of the interior longitudinal films to which this edge belongs. He adds that one arrives sometimes more easily at the first system of this kind without making the interior film polyhedron initially, by simply blowing on one of the edges of the polygonal film.

The principle of Mr. Van Rees undoubtedly applies to all prismatic frames, whatever the number of their side faces; but I do not think that one can push the experimental verification beyond the octagonal prism, for which it is already difficult.

§ 203. It was not only with the prismatic frames that Mr. Van Rees employed in the preceding partial re-immersement; when, after having obtained the ordinary system on an arbitrary frame, he re-immersed it by a few millimetres on one of its faces, then it was withdrawn, the film across the base which then goes up between the others, always leads to the formation of an interior film polyhedron. This polyhedron generally has the same number of faces as that which is represented by the frame, and these faces are always more or less convex.

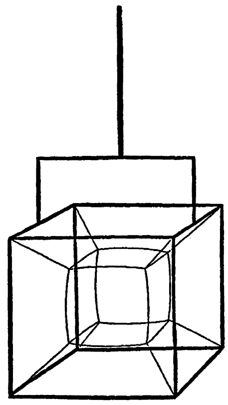


Fig. 90

One often obtains, in this manner, extremely pretty results: for example, with the cubic frame, the new system (fig. 90) contains, in its middle, a film cube with edges and faces slightly convex, attached by its edges to the films based on the solid edges; in the same way, with the frame of the tetrahedron, the new system presents, in its middle, a film tetrahedron with convex edges and faces, etc. One easily explains these convexities by the laws relating to the angles of the films and the liquid edges.

The films which constitute the interior polyhedrons in question, are obviously not of mean curvature zero; they are in contact, on one their faces, with an enclosed mass of air, and not with the open air. I will return hereafter to these same polyhedrons, of which Mr. Lamarle made a special study.

Mr. Van Rees, who, as saw we, manages to modify certain systems by a suitable force of his breath, made an interesting application of this process to the ordinary system of the cubic frame (fig. 71): in this system, the central quadrangular flat film is, we know, parallel with two opposite faces of the cube; but, because of the symmetry of the frame, it is obviously indifferent, for equilibrium, whether this parallelism takes place compared to one pair of faces or another; the flat film can thus also occupy three positions, and it is understood that it takes only a very slight cause to determine its choice. Also, when one withdraws the frame from the glyceric liquid, one finds the flat film in question sometimes parallel with the faces front and back, sometimes parallel with the faces of the right-hand side and the left, and it occurs some times that it is placed horizontally. However, one can make it change at will, and several times in a row, from one of these three positions to another; it is enough, for that, to blow very slightly on one of its edges through the face of the frame on the side of which this edge is: one sees the flat film then narrowing in the direction of the breath, to be reduced to a simple line, then to reform in its new position.

By a means that I will indicate, one determines, in the systems of certain frames, a modification of another kind, and rather curious: if, after having formed the ordinary system of the cube, one bursts the central quadrangular film, the system immediately makes an arrangement very different, and also regular, although it presents a hole in its middle; one brings about a similar result, but with two holes, by bursting, in the octahedral system (fig. 75), initially the upper quadrilateral, then the film which replaces the lower quadrilateral.

§ 204. The second part of the Report of Mr. Lamarle, the memoir about which I already spoke in § 158, contains, with regard to my film systems, a succession of results of which I will point out the principal ones; these results relate to especially the interior film polyhedrons:

1° A second process for the creation of these film polyhedrons consists in initially producing the ordinary system of the frame, then inflating a bubble of suitable size and introducing it into the system; it attaches to the system's films, and, when one withdraws the tube which was used for blowing it, it forms an interior film polyhedron at once. One can increase or decrease at will the dimensions of this polyhedron: for that, one introduces there the tapered end of a tube, after having wet it with glyceric liquid, and one blows or aspires through this tube.

2° In the tetrahedral frame, the faces of the film tetrahedron formed inside are of spherical curvature, and consequently the edges of this same tetrahedron are of circular curvature; the center of the sphere to which any of the faces belongs is located at the opposite corner; finally, the center of the circumference to which any of the edges belongs is located at the midpoint of the chord of the opposite edge.

3° In the frame of the triangular prism, when the ratio between the height of the prism and the side of the base lies between certain limits, one can, at will, by suitable operations, obtain, in the middle of the shape resulting from only one immersion, a triangular film parallel with the bases or a liquid edge parallel with the side edges. Between some limits, one can develop an interior triangular prism, but its faces are never of spherical curvature.

4° In the cubic frame, the faces of the interior film hexahedron are of spherical curvature, and the radius of the spheres to which they belong is equal to one and half times the length of the line which would join two opposite corners of one of them.

5° In the frame of the pentagonal prism, so that the ratio between the height and the diameter of the circle which would inscribe the base corresponds precisely to the vanishing of the central polygonal flat film, it is necessary that this ratio is equal to $\frac{\sqrt{2}}{\sqrt{3-\sqrt{5}}} = 1.618$.

With this same frame, when the ratio is not much lower than the limit above, one can obtain, as the result of only one immersion, a very small pentagonal film in the middle of the shape, or the other system, i.e. that which presents a kind of inward slanting pyramid on each base. As for the interior film polyhedron, it has faces of spherical curvature only under certain conditions of volume.

6° In the frame of the regular dodecahedron, the interior film dodecahedron has its faces of spherical curvature, but of a very large radius.

7° In all the systems above with an interior film polyhedron, when its faces have spherical curvature, all the films which extend from its edges to those of the frame are plane, and consequently all the liquid edges which join its corners to those of the frame, are straight.

8° In the regard to the octahedral frame, an exercise in reasoning leads a priori to five different systems, systems which are most probably the only possible ones, and I had observed only two well (§§ 185 and 193); the three others respectively contain, in their middle, a pentagonal film, a trapezoidal quadrangular film, and an equilateral quadrangular film. All these systems are formed at will, and one can make them pass, also at will, from one to the other. In those which have only plane films (fig. 75), the dimensions of the various parts have extremely simple numerical ratios between them and the dimensions of the frame; I announced one of them in § 185.

Suitably treated theory indicates, in this same system with plane films, the possibility of fifteen different interior polyhedrons; it derives them from each other, and the whole is verified by experiment.

9° When one creates one of these interior polyhedrons, one sees being formed six small triangular films which end in it; by bursting two or four of these films, one obtains systems of a particular kind, and which one can call incomplete. For three of those, which result from the disappearance of two opposite films, the interior polyhedrons, originally octahedral, became hexahedrons, and have very elegant forms; the disappearance of two other opposite films transforms one of these hexahedrons into a tetrahedron of a curious aspect in regard to the contours of its faces.

§ 205. I have designated as imperfect (§ 196) systems in which films remain, on part of their extent, adherent at the same time to two solid edges. The systems which do not present this circumstance, I call them perfect because all the films completely depend on one another; except in some very rare exceptional cases, one obtains these perfect systems in the frames of the polyhedrons of which all the plane angles are lower than 120°: for example, as we saw, in those of the tetrahedron, the cube, the octahedron, and the prisms for which the number of the side faces is less than six; the frame of the regular polyhedral dodecahedron whose plane angles are only 116° and a fraction, also gives a perfect system.

When one withdraws from the glyceric liquid the frame of a polyhedron of which all the plane angles are, on the contrary, greater than 120°, one simply always finds each face, less one, filled by a plane film; the exceptional face remains empty; it is needed as an opening for the entry of the air. This is not truly a film system, since all the films are made independent from each other by the intermediary of solid wires; we can thus name them null systems: they occur, for example, with the frame of the regular icosahedron, where the plane angles are 138° and a fraction, with that of the whole of two hexagonal pyramids or of a greater number on sides, joined together by their bases, and such, that at the edges of the common base the plane angles exceed 120°, etc.

Thus when all the plane angles exceed 120° a little, one obtains, in some cases, a true film system: it happens, for example, with the frame of the polyhedron formed by cutting down the corners of a cube by equilateral sections which join, in a manner that there are only triangular faces and square faces. But, in this polyhedron, all the plane angles are only approximately 125°; moreover the true and symmetrical film system occurs with difficulty, and only when one withdraws the frame by a triangular face; when one withdraws it by a square face, it always gives a null system.

§ 206. In the Report that I already quoted (§ 187), Brewster indicates a curious process for the production of film systems for several frames: the frame which one wants to use being wetted beforehand with glyceric liquid, but not containing any film, one inflates, in its interior, a large bubble, which sticks to the whole of the solid edges, so that each face is occupied by a film, then one bursts one of these films, and the ordinary system appears at once. In addition to the singular spectacle of this instantaneous transformation, the process of Brewster thus offers the advantage of forming the film systems by requiring only a very small quantity of liquid. I must say that I vainly tested this process with my frames; it is probable that those of Brewster had much smaller dimensions; but here is another process which also requires only a little liquid, and which gave me every success; I employed it with the frames of the cube and the hexagonal and pentagonal prisms, and I do not doubt that it is also appropriate for those of all the polyhedrons for which no plane angle exceeds 120°: one pours the glyceric liquid in a small plate, where it can form a layer of only a few millimetres thickness; then, if one wants, for example, to form the system of the cube, one successively immerses in the liquid the four side faces of the frame, so that each one of them is occupied by a plane film; finally one immerses and one also withdraws the base, and the system completes itself at once. If it is a prism frame, one likewise fills one face after another, all the side faces and then the base. I will point out here that the edges of my cubic frame are 7 centimetres long.

§ 207. The film systems varying with the shape of the frames, one can imagine polyhedrons selected so that all their plane angles are less than 120°, build some of the frames, and test them, for the simple aim of curiosity, to observe the systems which occur there. One will often obtain, in this manner, extremely pretty results.

§ 208. After having mentioned (§ 172) the experiment of my son relating to the effect of heat on the tension of the liquid films, I said that I would make known another; here it is:

One forms the system of the cubic frame, then one introduces the strongly heated end of a glass rod into the hollow pyramid formed by the films which leave the four edges of one of the faces of the cube, and one chooses one of the pyramids whose bottom is an edge of the central flat film; at once one sees this flat film decreasing in extent. It is that, in consequence of the position of the rod, the flat film warms up less than part of the surrounding films, and thus preserves a relative excess of tension. One cannot attribute the effect to dilation of the most heated films, because, if all the tensions remained equal, the form of the system obviously could not change; the films which would tend to dilate would send only one portion of their liquid to the central film.

I will add here a foreign experiment on the action of heat. I had said myself that if one could form a system of which part of the films were formed of a liquid and the other of another liquid with different tension, the system could not satisfy any more my laws for the legality of the angles between the films and the liquid edges. Such a creation is undoubtedly impossible in a complete way, but one can approach somewhat by the following means: one again produces the system of the cubic frame, and one poses it so that the central film is horizontal. That done, if one introduces in the middle of this film the end of a small brush soaked with glyceric liquid, the film does not make any change; but if the brush is impregnated with a liquid with stronger tension, one sees the central film contract very notably. I obtained the best result with an albumin solution prepared simply by beating an egg white, then letting the foam convert itself partially into liquid: the central film which, in my frame, has approximately 13mm dimension in the two directions, is tightened then until not being more than 8mm when the brush is removed, it takes again 9mm quickly, then seems to remain in this state for a few seconds, after which it returns slowly to its originating dimensions.

In this experiment, the liquid of the brush is spread more or less on the film, so that the portion which is covered with it has the tension belonging to this same liquid, tension which cannot be balanced any more, without modification of the system, by the tensions of the other films. It will be seen (§§ 258 and 299) that the albumin solution has indeed a tension much higher than that of glyceric liquid.

A liquid of less tension than that which constitutes the system must determine, on the contrary, an enlarging of the central film, and I also have verified it: the film system was formed with albumin solution, in a cubic frame of side 5 centimetres; I deposited on the central film a droplet of soap solution, a liquid whose tension is appreciably equal to that of glyceric liquid; at once the droplet spread so as to replace, in the central film, the albumin solution, and, at the same time, this plate underwent a notable increase.

§ 209. Now let us consider film systems from a more general point of view.

The tension constituting a ceaseless effort to decrease the area of liquid surfaces, it follows that, in any film system, the sum of the surfaces of the films must be a minimum.

Thus there is a principle which governs the constitution of all these systems.

§ 210. When I posed this principle, in 1861, at the end of my 6th Series, I understood that there is a dependence necessary between this same principle and the laws which I had found (§ 184) relative with the numbers of the films linked by the same liquid edge and liquid edges leading to the same liquid point, as with the angles of the films between them and of the edges between them; but I could not grasp this dependence, and it appeared nearly impossible to me to discover it.

But, in the first part of the Report of which I above summarized (§ 204) the second, Mr. Lamarle included the question, and solved all the difficulties, with a marvellous sagacity and a rare felicity.

He starts by establishing more clearly than I had done the principle of the minimum of the sum of the areas of the surfaces; then, on that basis, he deals with the films meeting at the same liquid edge. He imagines an arbitrary number of plane films based on solid edges and joining all together along a common liquid edge, and he cuts the whole by a plane perpendicular to it. The cross-section being composed of straight sides leaving respective fixed points and leading all to the same point, he initially shows, by considerations of elementary geometry, that if the lines are three, their sum will be a minimum when they form between them equal angles. If the lines are more numerous, he shows, always by simple considerations, that, to have a minimum sum in an absolute way, it is necessary to substitute for the single junction point, several junction points connected to each other by additional lines, in such way that at each one of these points there are only three lines forming between them equal angles. Lastly, the reduction in the number of straight sides happening right from the start of these modifications, i.e., in the case of more than three lines, for example, as soon as the point of junction is duplicated to give rise to the straight sides and to the additional points, it follows that the demonstration also applies to curved lines, because one can always replace those by their tangents in the immediate vicinity of the junction point. Mr. Lamarle shows whereas all these results extend to the films themselves, plane or curved, whose whole is cut by the plane in question; i.e. the minimum of the sum of the surfaces requires that these films join three by three, at equal angles, at each liquid edge.

Thus is completely shown and deduced from the principle of minimum area two of the laws pointed out above.

Mr. Lamarle passes then to the question of the liquid edges joining at the same liquid point. To treat it, he imagines that plane liquid films all end at the same point of the interior of the system, and he seeks the conditions which these films must fulfill so that they can join three by three at equal angles, in accordance with the preceding laws. He considers the point which is common to them as the center of a sphere, that they thus come to cross along arcs of large circles; there is in this manner a certain number of hollow pyramids having for apices the same point, and, for bases, spherical polygons of which all the angles are 120°. Mr. Lamarle initially points out that these polygons can be only triangles, quadrilaterals and pentagons, which provides him an analytical relation between the respective numbers of these various polygons and the total number of the films; he finds another by the condition that the sum of surfaces of these same polygons must represent the total surface of the sphere; finally all the polygons in question must be simply juxtaposed, without encroachment of one on another in certain places or holes between them in other places. By means of these three conditions, Mr. Lamarle finds that there are only seven possible assemblies of films based at the same point and joining three by three at equal angles.

If, in each one of these assemblies, one replaces the sides of the spherical polygons by their chords, one has the whole of the edges of a polyhedron, and the seven polyhedrons thus formed are: the regular tetrahedron; the triangular right prism with equilateral base, with a given ratio between the height and the side of the base; the cube; the right pentagonal prism with regular base, with a given ratio between the height and the side of the base; two particular polyhedrons composed of quadrilaterals and pentagons; and finally the regular dodecahedron. In the interior of these polyhedrons, the numbers of liquid edges are respectively 4, 6, 8, 10, 12, 16 and 20.

Now Mr. Lamarle shows that, for each one of these systems, except for that of the regular tetrahedron, one can always conceive a mode of deformation from which results, starting from its origin until a certain limit, a reduction in the sum of the surface areas of the films; the arrangement which takes place in the system of the regular tetrahedron, i.e. four liquid edges leading to the same liquid point at equal angles, is thus the only one which can be maintained; those of the six other systems, if they could be formed, obviously would immediately change to reach the condition of minimum area. Thus, when the films are plane, the liquid edges which join at the same liquid point are necessarily four, and form between them equal angles. Finally Mr. Lamarle shows that the same conclusion applies to curved films, and, consequently, to curved edges; indeed, nothing limits the smallness of the sphere mentioned above, and consequently one is able to suppose this sphere rather tiny so that the portions of films included in its interior can be regarded as plane.

The two laws concerning the edges are thus shown by Mr. Lamarle as completely as those which relate to the films, and also deduced from the principle of minimum area.

Let us say that the modes of deformation supposed by Mr. Lamarle, and that he comes, by means of a clever design, to make refer all to the same principle, are precisely those which lead to the actual results, i.e. to the permanent systems formed in frames of iron wire

§ 210bis. At a conference held in Prague in 1868, and published in 1872 (see No. 26 of § 508), Mr. Mach described the following fact: one imprisons in a thin rubber membrane the frame of a regular tetrahedron, and one substitutes for the wire of suspension based at one of the corners, a narrow tube which communicates with interior space, then one extracts the air by this tube; one sees the membranous faces then growing hollow more and more, the two parts of the membrane which are pressed on the same solid wire press one against the other, and one obtains finally the film system with its four edges leading to the center.

To explain the tendency of liquid surfaces towards a minimum area, Mr. Mach puts forward an extremely simple idea: the molecules of the surface of a liquid are attracted towards the interior by those which are located more deeply; from there a tendency of these surface molecules to penetrate into the interior of the mass or the film; but, as a part of them leave the surface thus, this is reduced, and the phenomenon necessarily stops when it became a minimum. This theory, one sees, has much analogy with that which Mr. Lamarle presented (§ 160) to explain the tension.

CHAPTER VI.

Theory of the generation of liquid films; different means to produce these films; characteristics which they present according to the process employed to develop them.

§ 211. In the three preceding chapters, we studied liquid films from the point of view especially of the forms which they take under various conditions where one places them; we now will tackle another question, which seems to me quite worthy of interest, the generation of the films in question; we will try to show that this generation is a necessary result of the cohesion and viscosity of the liquid. We will thus examine in this report the various modes of production of liquid films, and we will announce at the same time the characteristics that films present when obtained by these various means.

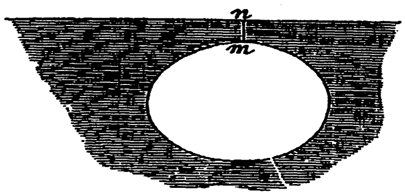


Fig. 91

Let us initially take an extremely simple case, that of the film in the shape of a segment of a sphere developed at the surface of a liquid by a bubble of air which has risen through this liquid. Let us consider the bubble of air at the time when it is no more than a few millimetres from the surface (fig. 91)[119]. So that its top crosses the distance *mn* which separates it from the outside, it is necessary that the molecules of liquid located all around this small line are driven out in all directions at the same time, so that these molecules will try to make relative displacements. Let us imagine, to simplify, that the upward movement of the air bubble is uniform, so that, in intervals of equal times, the bubble drives out, from between it and the upper surface of the liquid, equal quantities of this liquid. Let us imagine, moreover, that the liquid does not have any viscosity. Then, as the distance *mn* decreases, the portions of liquid driven out during the intervals of time above will take increasingly large respective speeds, since they will have to carry out their movements in increasingly narrow spaces; thus relative displacements of the molecules of liquid are faster as the top of the bubble of air is closer to reaching the surface. I supposed uniform the upward movement of this bubble; but, actually, it is accelerated, and its acceleration will increase further the speed of the relative displacements in question.

Now, one knows that viscosity opposes the relative displacements of molecules of liquid with a resistance which grows considerably with the speed of these displacements. If thus, to pass to the real case, we restore to our liquid its viscosity, resistance to the sideways transport of the liquid molecules around *mn* will be increasing as this line decreases in length, and will become very large when this same line becomes very small. It necessarily follows that when the top of the bubble of air arrives close to surface, the thinning of the portion of liquid which still separates it cannot be carried out any more with a speed equal to that of the upward movement of this top; and consequently, so that the air which constitutes the bubble continues to go up and passes above the level of the liquid, it is necessary obviously that this liquid tears, or else it is raised. However one cannot doubt any more, since the excellent research of Donny[120], Henry[121] and Dupré[122], that the cohesion of liquids is of the same order as that of

[119] In this drawing, the bubble of air is represented flattened a little in the vertical direction; that is what takes place indeed, in consequence of the resistance of the liquid.

[120] *Mémoires de l'Académie de Belgique* volume XVII of the 7 *Mémoires couronnés et des savants étrangers*. The work of Mr. Donny was presented at the Academy in December 1843.

[121] *Philos. Magaz.*, 1845, vol. XXVI, page 541.

[122] *Théorie mécanique de la chaleur.* Paris, 1869, chap. VIII

solids; it is thus understood that when the distance *mn* is rather reduced so that its later reduction cannot be done any more with a speed about equal to that of the rise of the top of the bubble, the liquid will still present around *mn* far too much resistance to the disunion of its molecules for it to tear, and that thus it will be raised by the bubble in the shape of a film; and as this film, during its generation, is moved upwards by the bubble of air and is attached around its contour with the liquid of the vessel, it will have to be convex towards the outside.

As soon as the film starts to be born, it must develop more: because, on the one hand, pushed without delay by the bubble of air, it must continue to rise, and, on the other hand, the liquid to which its contour adheres cannot follow it in mass because of its weight; thus this liquid will have to remain behind; but, due to the cohesion and the viscosity, there cannot be rupture between the incipient film and the surrounding liquid, and the film must simply increase, until the action exerted upwards on the lower part of bubble of air has had all its effect.

I have drawn attention (§ 11) to the fact that when a mass of oil a little less dense than the alcoholic liquid in which it is immersed rises to the surface of the latter, it flattens initially more or less against this surface, as if it encountered a resistance, it breaks the surface, and then presents a portion of plane surface more or less wide, on the level of that of the alcoholic liquid. This phenomenon is now explained in a natural manner by the considerations which precede: the oil sphere is like a bubble of air, it can surface outside only by dividing the molecules of the layer of the ambient liquid, but this is not able to be thinned very quickly because of its viscosity, and it resists rupture due to its cohesion. Only it is clear that, in this case, the film could not be raised above the level. Finally the same considerations apply to the kind of resistance that resists the union of two oil masses which one brings together in the alcoholic liquid (§ 6).

§ 212. Let us return to our convex film developed by the rise of a bubble of air. When it reaches its full development, and thus remains stationary, we know it will have to take one of the equilibrium shapes which would be appropriate for the surface of a liquid mass without gravity; however, this shape, which was formed by an equal action in all the directions around the vertical axis of the bubble of air, must obviously be of revolution, and, as it is closed on the axis, it can (§ 37) constitute only one: a portion of sphere.

Now let us see what the theory will teach us on the extent of this portion compared to the complete sphere. Due to its tension, our film makes an effort to occupy the least possible area. Consequently, if one neglects certain characteristics about which I will speak soon and which, moreover, do not have an appreciable effect when the volume of air is somewhat large, the question is brought back to seek which is, for a given volume, the segment of sphere whose surface is smallest. This problem is treated easily by calculation, and one finds the segment in question is a hemisphere; but one arrives more simply still at the same result by the following reasoning, of which I owe the idea to Mr. Lamarle.

Let us conceive two arbitrary spherical segments of equal volume and applied one against the other by their bases. So that the convex surface of each one of them is the least possible for the volume contained between it and the common base, it is enough obviously that the total convex surface of the whole of these two segments is the least possible for the total volume; however, according to a known principle, this last condition will be met if the unit constitutes a single sphere, in which case each of the two segments will be a hemisphere.

Our liquid film, if it contains a sufficient volume of air, must thus take a hemispherical form, and that is what observation verifies, as everyone knows.

§ 213. We now address the small characteristics to which I referred above.

Initially, the liquid of the vessel rises a little, by capillary action, on the outside and the inside face of the film shape, as it does at the base of any film which comes to be attached to its surface (§§ 189 and 192); it thus forms the small annular mass with concave meridian surfaces on the crest of which the cap rests.

In the second place, one understands, accordingly, that if the imprisoned volume of air is rather small so that the space circumscribed by the edge of the film has little diameter, the surface of the liquid in this same space, will not have any plane part, but will present, even in its middle, a more or less marked concave curvature, as inside a narrow tube. This result agrees with experiment, and I assured myself, by a means that I will indicate soon, that the central portion of surface in question ceases appearing plane when the diameter of the film, at the crest of the small annular mass, is below approximately 2 centimetres.

Finally, in the third place, even with a rather large volume of air so that, in the space circumscribed by the film, the surface of the liquid appears absolutely plane in almost the totality of its extent, this surface must be lowered below the external level by the pressure that the film, due to its curvature, exerts on the imprisoned air, and that is what one notes by the following process:

In a large porcelain dish set on a table opposite a window, one pours a layer of glyceric liquid approximately 2 centimetres thick; then, after having inflated a bubble of the same liquid, one deposits it in the middle of the surface of this layer, where it forms a spherical segment at once. One then places oneself in order to see the sky by reflection on the surface in question, and one holds a black wire stretched horizontally a small distance from the film in such manner that a portion of its reflected image is in the space circumscribed by this film. The total image of the wire is thus formed of three parts, two external and one interior to the film shape; the first two bend in the vicinity of the film, in consequence of the capillary rising about which I spoke; as for the third, if the circumscribed surface has, in its middle, a plane portion, one will find, while moving the wire back and forth, a position of it for which the middle of the image will be rectilinear. That is what takes place with films whose diameter exceeds 2 centimetres, but below this limit, all the part of the image interior to the film appears curved.

When the film has a large diameter, this part of the image of the wire is rectilinear in almost all its length; it bends only towards its ends, again because of the capillary rising; but its straight portion is not in the prolongation of the straight portions external to the film, it appears a little low. This lowering, which shows that the circumscribed plane surface is, as I have claimed, below the external level, is much less marked when the diameter of the film is more considerable, which must take place, because of the reduction in the curvature and, consequently, the pressure of the film, but it is still very sensible for a film one decimetre in diameter.

§ 214. The reasoning of § 212 supposes that the film is based by its edge even with the plane surface of the liquid of the vessel, and that the portion of this surface circumscribed by the film preserves its plane form and its level; however these conditions never all being entirely filled, as we have just seen, it follows that the reasoning in question cannot be regarded as sufficiently rigorous when there is a notable difference from the idealized conditions on which it rests. Let us try to specify more precisely.

If one fills with glyceric liquid, until a little above the edge, a broad saucer of porcelain levelled beforehand and poised on a table facing a window, so that after depositing a bubble, one places oneself so as to see the film projected against a dark background, and, closing one eye, one holds the other at the level of the small annular mass, one distinguishes perfectly the two meridian lines of this small mass on the outside of the shape, as well as the meridian lines, starting from the top of the crest, that face the inside. One thus sees very well this crest, and one can consequently roughly estimate its vertical height above the external liquid level. One recognizes thus that, for large caps, this height hardly exceeds 2mm, and that it is less still for small ones. In addition, when the film is large-sized, when, for example, its diameter is one decimetre, the portion of the surface of the liquid circumscribed in its interior can be regarded as exactly planes in almost all its extent. Lastly, according to the experiments of the preceding paragraph, with a similar film, the lowering of this surface, though quite significant still, is however very tiny. It follows from results of § 121 that a presumedly hemispherical film one decimetre in diameter would exert on the interior air and, consequently, on the circumscribed portion of the surface of the liquid, a pressure equivalent to that of a water column of 0.226mm height; to evaluate this same pressure in a column of glyceric liquid, it is enough to divide the preceding quantity by density 1.1065 of this liquid, which gives 0.204mm. Such is consequently the value of the small lowering which the surface of the liquid in the space circumscribed by the film would undergo. With a similar volume of air and a hemispherical film, the things would be thus appreciably under the conditions of the reasoning in question, and one will conclude from it that then the film will have to take this form indeed or that, at least, the variation will be inappreciable.

But it is easy to show that with a sufficiently small volume of air, the film will be far from constituting a hemisphere. Let us imagine, for example, a bubble of air being only one millimetre in diameter, and going up on the surface of the liquid; let us suppose, for one moment, that it forms a hemispherical cap there. Under this hypothesis, the portion of the surface of the liquid circumscribed by the film and counted starting from its edge or, if one wants, starting from the crest of the small annular mass, would necessarily constitute, because of its tiny dimensions, a concave hemisphere, so that the bubble of air would continue to form a whole sphere a millimetre in diameter. That said, we point out that the pressure exerted by a spherical film due to its curvature, is (§ 116) the sum of the forces due separately to the curvatures of each one of its two faces, or, since these two actions are equal, the double of one of them; however the force of the interior face of our small hemispherical film would be, as for its effort to reduce the bubble of air, counterbalanced by the opposite force of the concave hemisphere which would limit the bubble in a lower position, as I said, and it would remain, on the one hand the force due to the outside of the film, a force which would push the bubble of air from top to bottom, and, on the other hand, a small hydrostatic pressure which would push this bubble upwards if the lower point of this one were below the level of the liquid. But, in the case of the glyceric liquid, it again follows from the results of § 121, while taking, according to the remark made above, half of the value which they give, and while dividing by the density of the liquid, which the first of the two actions above would be equivalent to a difference in level of 10.19mm; while by supposing even the absence of the small annular mass, the second would come obviously only from one difference in level equal to the radius of the bubble of air, i.e. to 0.5mm, With our small hemispherical film and volume of air, equilibrium is thus impossible; for it to exist, it is necessary that the bubble of air remains almost wholly below the level of the liquid, and thus gives birth only to one hardly raised film and of a very low curvature; then, indeed, the small hydrostatic pressure which will tend to raise the bubble of air will be equivalent to the tiny weight of a volume of liquid in not very less than that of this bubble, and the light pressure exerted by the film due to its low curvature will be enough to counterbalance the small opposite forces.

Experiment again fully verifies this deduction of the theory. I poured, until a certain height, glyceric liquid in the vessel with plane glass walls being used for the experiments on the oil masses; the liquid was agitated a little so that small bubbles of air were introduced there; I chose one of them of approximately 1mm diameter sufficiently close to the one of the walls, and I observed it through this wall, while placing my eye successively a little below the level the liquid, then above. In this manner I recognized that the small oil droplet appeared spherical, and that it was almost entirely immersed, so that the projection above the level was very weak.

§ 215. It is clear, accordingly, that if one forms successive films on the surface of soap water or glyceric liquid with smaller and smaller diameters, starting from one decimetre, one will arrive at a limit below which the films will start to appear appreciably reduced, i.e. to appear to constitute less than one hemisphere. To roughly determine this limit with regard to the glyceric liquid, I deposited, as I indicated in the preceding paragraph, bubbles on the surface of the liquid contained in a saucer a little more than full, and I assured myself that they appear hemispherical only for diameters greater than approximately 3 centimetres; below this value, the bubbles form appreciably lesser segments compared to the whole sphere, and this reduction is all the more marked as the diameter of their base is smaller.

§ 216. What I have just explained about film caps is extracted from my 6th Series, which appeared in 1861; however, as I said in § 167, Mr. Van der Mensbrugghe gave, in 1869, a formula relating to the equilibrium of a film cap resting on the same liquid or a different liquid. Here is the passage of his Report which refers to this subject: "If one diagrams the cap cut by a meridian plane, the section will include two close parallel arcs of circle down to the level of the ambient liquid; in the liquid's vicinity, the arcs will be deviating from each other to join, the first, the external surface while passing through a point of inflection, and the second, the interior surface limited by the film, while remaining always concave. That being said, let us consider one of the portions *abcd* (fig. 92)[123] of the annular mass starting from the crest *bc*, limited by two curves *ba*, *cd* such as I have labelled them; if, in the plane of level, we lead the tangent to the curve *ab*, this line will cut the curve *cd* at a point *d*. Now see which are the forces which tend to move in the horizontal direction the liquid contained in the section which has as limits the crest *bc* of the annular mass, the curves *ab*, *cd*, and the rectilinear part *ad*; these forces are: 1° the tension T of the external liquid applied at a; 2° the horizontal components of the two tensions t acting at b and c; and 3° the horizontal component of the tension t' applied at d (all these tensions are directed towards the outside of the small liquid mass); if thus α and β represents the angles which form between the horizontal and the tangents at b or c and at d, we will have:

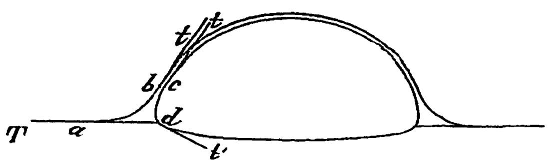


Fig. 92

$$T = 2t\cos\alpha + t'\cos\beta."$$

Let us note with regard to the tension t' that, when the bubble was deposited, its lower portion necessarily covers the surface with the liquid inside the cap, and undoubtedly mixes more or less with this liquid; the tension t' which belongs to this portion of surface is thus most probably intermediate between the tensions T and t.

I indicated (§§ 167 and 168) the verifications to which the author subjected his formula if the two liquids differ; but this same formula also applies to the case of

[123]In this figure, for demonstration, I exaggerated the thickness of the film and the dimensions of the annular mass.

only one liquid, and, consequently, to the facts of the three preceding paragraphs; it is expressed as follows:

".....if the three tensions T, t and t' are equal to each other, in the case of a soap water film, for example, formed on the surface of the same liquid; the equation becomes then: $2\cos\alpha + \cos\beta = 1$; however, when the film is very large, the pressure exerted on the air which it contains is very low, and consequently β differs very little from 0; it follows that then α is brought very close to 90°, i.e. the film can be regarded as appreciably hemispherical. If we now decrease the dimensions of the cap, the pressure of the interior air increases and with it β; then α becomes increasingly small and the film constitutes a less and less large portion of the whole sphere. This consequence shows in a complete way what was expounded in this respect by Mr. Plateau."

One sees, moreover, that if $\cos\alpha$ is very close to 1, one must have $\cos\beta$ very close to -1; that is the case of an extremely small bubble of air, which produces only an unperceivable projection in the liquid surface (§ 214).

Further, let us announce an extremely curious consequence about the same formula, again under the condition of only one liquid: if one deposits the bubble, not on a thick layer of this liquid, but on a glass plate which is simply wet by it, there cannot any more be a depression in the space circumscribed by the cap; there is thus necessarily $\cos\beta = 1$, and, consequently, $\cos\alpha = 0$; consequently the cap, even with a very small diameter, must be exactly hemispherical, as I claimed at the end of § 175, and that is what experiment verifies fully: I deposited, on a glass plate wet beforehand with glyceric liquid, small bubbles of the same liquid, giving caps of 5mm to 7mm diameter, and they appeared completely hemispherical, while caps of a similar diameter floating on a thick layer of the liquid appeared strongly depressed.

Finally, the formula of Mr. Van der Mensbrugghe applies not only to the films in the shape of caps, but obviously to any film which comes to be attached to the surface of the liquid, such as those which leave the sides of the upper base of a prismatic frame of less than six sides, when one withdraws this frame from the liquid (§ 192): when the liquid surface is plane and on the same level on the two sides of the small mass raised at the base of the film, there is $\cos\beta = 1$ and $\cos\alpha = 0$, from which it follows that the film ends vertically at the crest of the small mass; it follows, as I said in § 189, as an immediate consequence of the tension.

§ 217. Let us return to the caps. Although such a film developed at the surface of a liquid is an equilibrium shape, absolute rest does not exist there however: the force of gravity, know we it, obliges the molecules without delay to slide from the top to the bottom of the film, so that it is under continual movements. This phenomenon, moreover, is not so simple as it first appears; we will make a detailed examination of it in the two following chapters.

§ 218. We began (§ 211) our study of the generation of films by seeking how a film segment of a sphere is formed by the action of a gas bubble which rises from the interior of the liquid; now let us pass to films obtained using another process. Let us take again the experiment of § 20, an experiment which consists, one saw, of producing, in a solid cylindrical band, within the alcoholic liquid, a biconcave oil lens. Let us increase the curvature of this lens until the two concave surfaces are close to touching at their middles[124]. One could believe a priori that if liquid were withdrawn further, the mass would be divided at the point where this contact happened, and the oil would withdraw in every direction towards the metal band. However, that is not what happens: one observes then, in the center of the shape, the formation of a small clearly delineated circular space, through which objects do not appear decreased any more, and one easily

[124] To carry out this operation, it is understood that it is necessary to apply the nozzle of the syringe, not in the middle of the shape, but close to the metal band, where the thickness of the liquid is greater.

recognizes that this small space is occupied by an oil film with plane faces. If one continues to remove liquid gradually, this flat film increases more and more in diameter, and one can thus extend it until a rather small distance from the solid surface. There is thus a liquid film developed within another liquid. In my experiment, the metal band had a diameter of seven centimetres, and I could increase the flat film until the maximum distance of its circumference from the solid face was only approximately five millimetres; but, at this moment, it broke, and the liquid which constituted it withdrew quickly towards that which remained still adherent to the metal band.

The generation of such a film receives the same explanation as that of the segment of a sphere: indeed, the centers of the two concave surfaces cannot go towards each other without liquid being driven out between them towards the thicker parts of the mass, and, by supposing that removal of liquid takes place at a uniform speed, the movements of the driven out molecules are necessarily all the faster as the space in which they are carried out narrows more; there must thus come a moment when, in consequence of viscosity, the speed of these movements cannot correspond any more to that of removal, and then the liquid shape must break, or take a heterogeneous form, because it becomes obviously impossible that its surfaces continues to satisfy in all its extent the same equilibrium condition, i.e. to have everywhere the same mean curvature; but cohesion is opposed to the rupture; so consequently there is the inception of a plane film to which is attached the remaining full mass, whose faces have negative mean curvature.

Let us say here, with regard to the segment of a sphere, that once the film develops, equilibrium does not exist any more but in the general form of the system: in consequence of the concavity of the curvatures of the full part, it constantly sucks the liquid from the film, from which it follows that this film always goes on thinning, until it bursts.

§ 219. Let us take again also our full polyhedrons of oil formed (§§ 29 and 30) in wire frames within the alcoholic liquid. If, after having formed one of these polyhedrons, one applies the nozzle of the syringe about the middle of one of its faces and oil is gradually withdrawn, one sees remarkable phenomena occurring. Let us choose as an example the cube. As soon as the syringe starts to act, all the faces are indrawn simultaneously and by same quantities, so that the square solid contours are used as bases for six identical hollow shapes. One realizes this happens for the maintenance of equality between the pressures.

If more portions of liquid are removed, the faces grow more and more hollow; but, for well appreciating what occurs when this operation is continued, it is necessary to state a preliminary proposal here. Suppose that one introduces into the vessel a square iron plate, whose sides have the same length as the edges of the metal frame, then one puts in contact with one of the faces of this plate an oil mass equal in volume to that which is lost by one of the faces of the cube; the liquid, after having extended on the plate, will present in relief the same shape which the face of the modified cube presents in hollow. Then, indeed, while passing from the hollow surface to the surface in relief, the radii of curvature corresponding to each point will only change sign, without changing absolute values; and, consequently (§ 2), since the equilibrium condition is satisfied with regard to the first ofthese surfaces, it will be it also with regard to the second.

Now, let us imagine a plane passing through one of the sides of the plate, and tangent to the surface of the liquid which adheres to it. As long as this liquid is a small quantity, one conceives, and experiment verifies it, that the plane in question will be strongly tilted towards the plate; but if the quantity of the liquid is gradually increased, the angle between the plane and the plate will grow, and will be able, as acute as it was, to become obtuse. However, as long as this angle is lower than 45°, the convex surface of the liquid adhering to the plate will remain identical with concave surfaces of the mass attached to the metal frame and suitably reduced; but, beyond this limit, the coexistence, in the frame of six identical hollow surfaces with surface in relief becomes obviously impossible: because these surfaces would be cut mutually. Thus, when one continues to remove liquid from the mass which formed the cube, it arrives at a point where the equilibrium shape ceases being realizable according to the ordinary law; however one is then obviously under conditions similar to those of the experiment of the preceding paragraph; thus films start to be formed. These films are plane; they start from each wire of the frame, and bind to the latter the remainder of the mass, which continues to have six concave surfaces.

One realizes, indeed, that, by this modification of the liquid shape, the existence of the whole system in the metal frame becomes again possible, because nothing prevents concave surfaces any more from taking a form which agrees with the law of mean curvature.

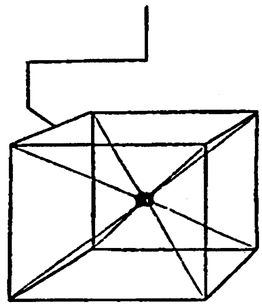


Fig. 93

If one removes still more portions of liquid, the films are increasing, while the full mass, which occupies the middle of the shape, decreases in volume, and one can thus reduce this mass to very tiny dimensions; fig. 93 represents the entire system in this last state. It is even possible to make the small central mass entirely disappear, and to thus obtain a complete film system; but, for that, it is necessary to employ some care which I will indicate. When the central mass became rather small, it is initially necessary to wipe the nozzle of the syringe perfectly, without which oil adheres outside it for a certain distance, and this attraction maintains around it a certain quantity of oil which the instrument cannot absorb in its interior. In the second place, it is necessary to bring the nozzle of the syringe rather low so that it is close to reaching the lower surface of the small mass. That being done, one sees, during suction, this surface rise until it touches the opening of the instrument, and this last then absorbs as much alcoholic mixture as oil; but one should not worry about this circumstance, and one sees the small mass decreasing by degrees, to disappear finally completely. The system is then that of fig. 82; but it is formed only during the action of the syringe; if, when this action is complete, the nozzle of the instrument is slowly withdrawn, one sees developing the quadrangular central plate of the system of fig. 71.

Thus, by the gradual exhaustion of its mass, our cube of oil is converted into a film system similar to that which one immediately obtains by withdrawing the frame from the glyceric liquid; the only difference is that, in the latter, the liquid edges are, as I have said, of an excessive thinness, while, in the film oil system, they have a visible width, which makes it possible to note what I have claimed (§ 173), which is that, in a film system, the liquid edges constitute small masses, and that they have strong concave curvatures in the transverse direction.

Let us indicate a precaution necessary for the complete creation of this system: from the moment where the films occur, it is necessary to extract the liquid as quickly as possible, until the central mass reached a certain degree of smallness. Indeed, as soon as the films start to appear, the liquid edges necessarily start to appear; however these edges, because of the concavity of their surfaces, exert (§ 33) a suction on the liquid of the films, and thus determine a gradual thinning of these last; moreover, a similar suction, but less energetic, is produced by the central mass, which also has concave surfaces; if thus the exhaustion of this mass by the syringe were carried out with too much slowness, the system could break before it was finished. When the central mass is sufficiently reduced, and the experimenter soon learns what point is suitable, it is necessary to slow down extraction more and more, and finally to employ the other precautions which I mentioned. The film oil system thus formed in a cubic frame of 7 side centimetres, can persist half an hour.

§ 220. When one arrives, during the action of the syringe, at the system of fig. 82, if, instead of withdrawing the instruments slowly, one abruptly detaches it with a small jolt, the additional film does not develop; but one sees the small mass of fig. 93 very quickly reforming. This fact confirms in a remarkable way the explanation which we gave at the end of the preceding paragraph. Indeed, at the moment when the nozzle of the instrument separates from the system, it can be regarded as composed of hollow pyramids; however, the apices of these pyramids must constitute not simple points, but small concave surfaces. Now these small surfaces having very strong curvatures in all directions, so they will give rise to a suction more energetic than that which is due to the liquid edges, because, in these last, the curvature is zero along one direction; the oil of the films must thus be transported much more abundantly towards the center of the shape than towards the other parts of the junctions of these films; moreover, there being twelve films leading to this same center, oil flows there by a great number of sources at the same time. These two causes thus contribute to determine, in accordance with the experiment, the fast reappearance of the small central mass.

Let us note, in passing, that the impossibility of holding the simple system of the pyramids without the action of the syringe, i.e. without its nozzle constituting a solid point in the center of the shape, gives us a superabundant experimental proof of the instability of an equilibrium film system where more than four edges lead to the same liquid point.

§ 221. All the other liquid polyhedrons change, like the cube, by the gradual extraction of their mass, into film systems identical to those which one obtains with the glyceric liquid. However, I could not bring them all to the complete state; some still contained small masses which I did not manage to make disappear, and which deteriorated the form of the system; but that was because, I think, the end of the nozzle of my syringe was not narrow enough; with a more tapered nozzle, I could undoubtedly have removed these small masses. Considerations similar to those which I employed with regard to the cube would show, in each case, which films take birth as soon as hollow surfaces with constant mean curvature cease being able to coexist in the solid frame.

Formation of the system coming from the octahedral frame offers remarkable characteristics: at the beginning of the operation, when the films are born, they are all plane and are directed towards the center of the frame, so that the system narrows towards the form represented fig. 94; but when extraction reaches a certain limit, an abrupt change occurs: the films are curved, and, if extraction is continued, the system makes a singular arrangement, of which it is difficult to give a precise idea by diagrams. Fig. 95 shows projections of them on two perpendicular vertical planes, and it is seen that the aspects of the system observed on two adjacent sides are opposite.

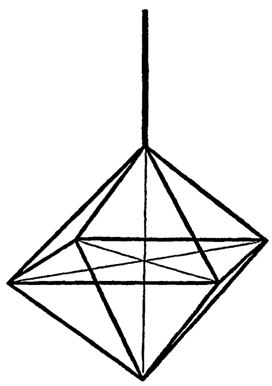


Fig. 94

By examining this same system, one easily recognizes that it is different from fig. 75, in that the films are curved. This is easily explained: I said that the form of some of my film oil systems was deteriorated by the presence of small masses which I had not succeeded in extracting; however, in the octahedral frame, when, while re-

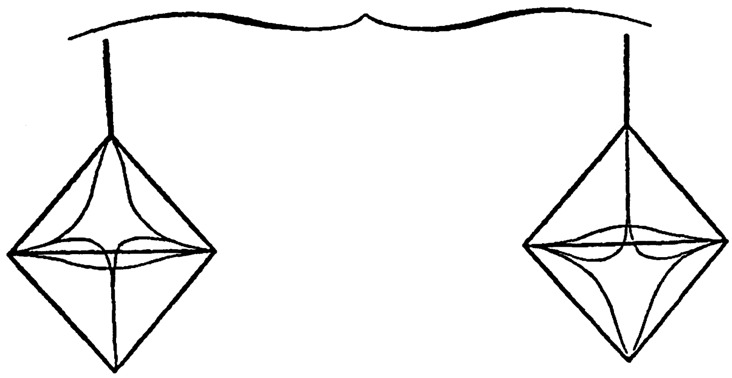


Fig. 95

moving oil gradually, I arrived at the point where the system changes spontaneously, the masses of junction having still a rather great thickness, and the oil which compose them accumulating, in the final system, all around the points where four edges should meet so as to form masses much larger there than the liquid edges in question, finally several of these same edges being rather short so that the masses which occupy their ends are in communication along the curve between them, one understands that there must result an influence on the shape of the edges and the films, and it is not doubtful that if one could only, without rupturing the system, sufficiently reduce the thickness of the masses in question, all the films would become plane.

The abrupt modification which occurs during the extraction of oil in the frame with which we occupy ourselves, brings also a curious experimental proof about the instability of an equilibrium film system which does not satisfy the laws of § 184; indeed, if the system reached the arrangement of fig. 94, towards which it tends until the masses in which the films end are sufficiently reduced, there would be everywhere four films with the same liquid edge, and there would be six liquid edges contributing to the center of the shape.

§ 222. Since oil is spread so easily, in the alcoholic liquid, into rather persistent thin films, one understands that one must be able to create, with these same films, all the equilibrium shapes; and those will be then rigorously exact, because one will have completely eliminated the force of gravity.

One forms the catenoid in the following way: the two rings of § 40, wet beforehand with oil, being laid out one above the other in the alcoholic liquid, one attaches to the lower ring, at about equidistant points, three small oil masses each having approximately a centimetre diameter[125], then one lowers the upper ring until it is only one or two millimetres above the other, and one makes it turn on itself alternately in two opposite directions, so as to as extend as uniformly as possible the oil masses over the entire length of the whole of the two rings. Once this point is reached, one raises the upper ring, with the film catenoid between them.

If one continues to raise the upper ring, one reaches the point where equilibrium ceases[126], and one sees the catenoid being necked more and more and converting, like the film catenoid of glyceric liquid (§ 111), into two plane films occupying the two

[125]One brings these small masses towards the metal ring by means of a wire (§ 6).

[126]It can happen, with certain qualities of oil, that the film obtained in this manner always tears before the point in question; in such a case, one can increase a little the quantity of oil; one will be able to take the diameter of the small masses to 12mm or even to l4mm.

rings respectively.

§ 223. To obtain, under the same conditions, a film sphere, or hollow oil bubble, one realizes that it will be necessary initially to make a small mass of this liquid adhere to the lower end of an iron tube immersed vertically in a certain quantity of alcoholic liquid, then to pour slowly, by the other end of this tube, a portion of the same alcoholic liquid, which must inflate the bubble.

But this experiment, if simple in theory, requires a rather great number of precautions which I will indicate.

To facilitate the introduction of the alcoholic liquid, the tube must be widened in a funnel in its upper part, and, so that it has a quite stable position, one needs to adapt to the base of the widening an iron disc from 7 to 8 centimetres in diameter, with the tube through its center, and which one will put on the neck of the opening of the lid of the vessel (§ 4). Moreover, the lower opening of the tube must be provided with a thin edge of approximately 1.5mm width; the purpose of this addition is to prevent the small oil mass intended to form the bubble from rising partly along the external wall of the tube: oil stops at the contour of the small edge, according to the facts described in § 14, and is laid out in a perfectly symmetrical way. Let us add that the diameter of the tube is significant; that which gave me the best results is 16mm. Fig. 96 represents the cross-section of the system.

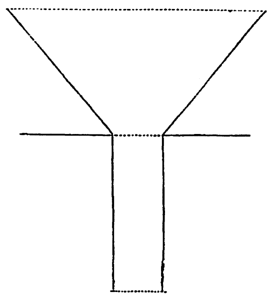


Fig. 96

It is obvious that the alcoholic liquid which one wants to fill the bubble must have exactly the same density as the external alcoholic liquid. One easily satisfies this condition by removing beforehand, by the tap of the vessel, a portion of the same liquid contained in this last, and using this portion to inflate the bubble.

This liquid will necessarily have to arrive in the bubble in a slow and gradual way, especially at the beginning: it will have to run initially drop by drop, then in a thin stream, and, moreover, to fall into the funnel close to its higher edge, so that, slipping along the inclined wall before going down in the tube, it thus takes less speed. But if, to carry out this operation, one holds the bottle which contains the liquid in one's hand, one may never come, whatever care that one takes, to give the bubble all the diameter that it can acquire, and that for two reasons: initially, it is impossible to graduate in a rather regular way the rate of flow, and sometimes the liquid arrives in too great abundance, producing in the interior of the bubble of considerable movements which make it burst; in the second place, the heat of the hand somewhat increases the temperature of the liquid of the bottle, and thus decreases the density, which results in a tendency for the bubble to rise, which makes it go to one side or another, and which, deteriorating the symmetry of action, also causes rupture. To remove these two causes of failure, I built a brass bottle provided with a tap and feet, so that, when it was set on the glass plate which is used as a lid for the vessel, the opening of the tap came a little higher than the edge of the funnel; I introduce into this bottle the liquid necessary to form the bubble, and I then let it run out the funnel by the tap, with a speed that I then graduate at will, without having to fear the influence of the heat of the hand[127].

§ 224. Using the system of apparatuses which I have just described, one easily obtains the results just laid out. By giving to the small oil mass which was attached to the opening of the tube a diameter of approximately three centimetres, I often formed large bubbles 12 centimetres in diameter, and I would have gone further, without any doubt, if the vessel had had more capacity[128]. When one arrives at the great dimension that I have just indicated, if one removes the funnel by a movement with suitable speed, the bubble remains behind, and the film of which it is formed prolongs while remaining adherent with the opening of the tube, so as to constitute a kind of trail; then this is necked quickly and separates into two parts, and the lower part will close and complete the bubble. This phenomenon is easily explained if, to simplify, we suppose that the movement of the tube is carried out in a direction exactly perpendicular to the plane of the opening. The total film shape obviously will not be able to cease being of revolution, and since it meets the axis and that it is not simply a portion of a sphere, it cannot constitute any more an equilibrium shape (§ 37); it must thus change spontaneously, and it is clear that this modification will consist of completing the sphere; the latter is thus entirely isolated in the middle of the liquid which fills the vessel. It persists in this state for a more or less long time, which can go sometimes beyond an hour, after which it bursts spontaneously. The experimenter soon learns how to know the speed with which it is necessary to withdraw the funnel: if this speed is too high, the bubble bursts; if it is too small, the bubble rises with the tube, and still bursts when the tube opening leaves the surface of the alcoholic liquid.

Calculation gives, for the average thickness of the film which forms the bubble in the case above, 0.3mm, i.e. less than one third of a millimetre; I say the average thickness, because the film does not have a uniform thickness, and it must be, in certain places, much thinner than 0.3mm.

One can inquire why, when a similar bubble is isolated from the tube, it does not persist indefinitely; one sees, indeed, in the capillary actions, no reason which must bring its rupture. It is necessary, I think, to seek the cause of this rupture in a residual chemical action exerted by the alcoholic liquid on the oil.

As in the experiments which I have just reported, I had not been able to push the swelling of the bubbles until its limit, I reduced the small initial mass to 2 centimetres in diameter. Then the bubbles usually broke between diameters of 7 and 11 centimetres. Sometimes, however, I succeeded in raising the diameter up to 12 centimetres, which assigns to the film an average thickness of 0.09mm, i.e. of less than one tenth of millimetre; but I never could isolate these so thin bubbles: they burst spontaneously before the funnel was withdrawn, the others while it was withdrawn.

As for the generation of the spherical films with which we occupy ourselves, it is clear that the liquid gradually introduced into the small oil mass, exerts, from inside outwards, a normal pressure at each point of the interior surface of the oil film which wraps it, and thus this layer, which cohesion prevents from breaking, must spread while thinning. But, moreover, this thinning tends to being uniform, because if it decided to thin more in a place, viscosity, by the reasons which we already gave, would make

[127] I will announce a rather curious fact here. I had initially used a tinplate bottle provided with an iron tap; but when the alcoholic liquid contained in this bottle contained by chance small oil spherules, those, while leaving the tap, sometimes absorbed iron oxide, and, becoming thus very heavy, went down rather quickly to the bottom of the oil bubble; however, when that happened, if the ferruginous spherule was tiny, one saw, after a few seconds, the oil film thinning suddenly at the place where this spherule rested, the thinning being propagated by a shrinking of oil, for a small distance around the point of contact, then the bubble bursting almost at once in this same place.

[128] The vessel which I used myself had only 15 centimetres interior width; but these dimensions are too small; this is why, in § 4, I prescribed 20 centimetres.

more difficult its later progress in this same place, and the remainder would attenuate then more quickly. If the film does not reach a strictly uniform thinness, that is because, undoubtedly, the irregularity of the small movements with which the liquid flows, that is to say, oil is not perfectly homogeneous, and contains portions more viscous than others.

One understands that while attaching to two rings or two facing discs an oil film sphere thus developed and of a suitable diameter, one will be able to transform it into a cylinder, an unduloid, or nodoid, as we transformed into these shapes (§§ 112 to 114) the bubbles of glyceric liquid.

§ 225. Now let us apply our theory to the generation of films bordered on the solid wire of which our frames are composed, when those emerge from soap water or glyceric liquid. Let us suppose that one immerses in one of these liquids a simple horizontal wire ring carried by a fork, then one raises it out of the liquid at a suitable speed, while maintaining it always parallel to the surface of this liquid. As long as the distance from the ring to this plane is very small, the liquid will rise a little, by capillary action, while presenting, outside and inside this same ring, two small surfaces with concave meridian curves. However it is easy to see that as the ring continues to go up, these two small surfaces will be growing hollow gradually in the meridian direction. It is known, indeed, that when one slowly raises a solid disc put beforehand in contact by its lower face with the surface of a liquid likely to wet it, the portion of this liquid raised by the disc above the level presents soon, in the meridian direction, a necking which increases as the disc goes up; the same must happen for our small surfaces which face outwards, and it is clear that the other small surface, i.e. that which faces inside, must, for the capillary equilibrium of the small mass raised by it, must undergo similar modifications.

Our two small surfaces will thus be approaching mutually as the ring continues its upward progress, until they are close to touching. But they can approach thus only by driving out a portion of the liquid between them; however, if the rise of the ring is not too slow, viscosity and the cohesion of the liquid will act here as in the preceding cases, and there will be formed, for the same reasons, a film, which will extend between the small portion of remaining liquid suspended along the ring and the small annular mass raised on the surface of the liquid of the vessel.

It is obvious that these considerations would apply also if the ring, while it is withdrawn, would be oblique or vertical, instead of horizontal, and that they would apply in the same way when the wire, instead of being curved circularly, would be folded in an arbitrary polygon. Always there would be formed, by the same causes, a film between it and the surface of the liquid; if thus we immerse in soap water or glyceric liquid one of our frames, itsiron wires will be, as they leave the liquid, attached to it by films, as indeed experiment shows.

§ 226. Let us take again our horizontal ring, withdraw it from the liquid while keeping it quite parallel to the liquid surface, and let us direct our attention to the inward slanting film which it actuates. The ring being circular, this film will constitute a shape of revolution, and since it is in contact on its two faces with the open air, its shape will be necessarily a portion of catenoid. Let us try to determine how this portion is located in the complete catenoid.

The film can obviously be considered, in thought, as pertaining to a film catenoid between our ring and another equal ring placed a suitable distance below. That said, we know (§§ 58 and 80) that between two equal rings whose spacing is less than the limiting spacing, there are always two possible catenoids, unequally necked; but we know also (§§ 60 and 111) that when one performs the experiment, either in the alcoholic liquid with a filled oil mass or an oil film, or in the air with a film of glyceric liquid, it is always the less necked catenoid which occurs, the other being most probably unstable. One must admit consequently that, in the current experiment, the film which extends between the ring and the liquid from the vessel, will have to always belong to the less necked catenoid, and that is what takes place, indeed, as one can see from the small obliqueness of the film.

It follows that by using a ring of a rather large diameter, 70mm for example, the space circumscribed by the small annular mass which attaches the film to the liquid of the vessel will be sufficiently wide so that the surface of this liquid can be regarded there as plane and on the same level as the outside; consequently the film, because of its tension, will end at the crest of the small annular mass in a vertical direction, and that is again what the aspect of the shape formed verifies. The meridian catenary of our portion of catenoid can thus be considered without appreciable error as having its top at the crest of the small annular mass; from which we will conclude finally that our film constitutes half of the catenoid which would lie between two rings equal to that from which it leaves and distant one from the other by double the vertical height of this film.

§ 227. But, between two equal rings, there is, we also know, a possible catenoid only until a limit of spacing very close to two thirds of the diameter of these rings, and, when one reaches this limit, the catenoid formed, if it is a film, pinches off spontaneously, and splits into two plane films occupying the two rings respectively; our current film must thus be transformed spontaneously into only one plane film in the ring, when this ring, while going up gradually, reaches, above the crest of the small annular mass, a height equal to half of the limit which I have just pointed out, i.e. when the distance from the plane of the ring to the crest in question is very close to a third of the diameter of this ring.

To subject the theory to the control of experiment, I attached the ring, by the tail of its fork, to the end of the lens of a cathetometer (§ 110). I placed, at a certain distance below this ring, a cup completely full of glyceric liquid, then, lowering the system of the lenses until it had only a very small interval between the surface of the liquid and the ring, I made it exactly parallel to this surface by curving the tail of the fork a little, using pliers. That done, the system was lowered further, so that the ring immersed in the liquid, and I then raised it with care by means of the screw of gradual movement, until the film started to be formed, then it again was lowered, but only by the distance necessary to cancel the film, so that the lower circumference of the ring was appreciably even with the crest of the small annular mass. I then made the first reading on the scale of the instrument, after which I raised the system, in one large movement, by a distance a little less than one third the diameter of the ring, and I continued, with much care, using the screw of gradual movement. However I was not long in seeing the film narrowing spontaneously, and with enough speed, at its base, to close itself in this place while separating from the liquid, and to make itself very whole in the ring to occupy it in the plane form. Lastly, after the termination of the phenomenon, I made a second reading on the scale. The internal diameter of the ring used was, according to a measure taken simply with a divided rule, 69.6mm; however the difference of heights read on the scale, i.e. the distance from the lower part of the ring to the peak of the small annular mass, was 22.57mm, a quantity very close to a third of the preceding one.

This result does not constitute a precise verification, but it is enough, for the present, to show the agreement of experiment with theory.

§ 228. It is easy for us to now explain the development of complete film spheres by blowing through a widened tube.

When one immerses in soap water or glyceric liquid the widened opening of a tube open at the other end, that of the head of a clay pipe, for example, and it is then withdrawn, there is necessarily formed a film based on the edge of this opening; however if the rising of the tube is operated so that the edge in question remains horizontal, this same edge will obviously play the role of our ring, and the film will be a portion of catenoid. If thus one continues to raise the tube, the film will be closed soon at its base, will separate from the liquid, and will go, in the plane form, to fill the opening. When one wants to inflate a bubble, one admittedly does not take care to maintain the opening horizontal during the rise of the tube, and then the film cannot belong any more to the catenoid; but it is understood that it takes a more or less similar form, and that, taking part of the properties of this shape, it will separate in the same way from the liquid, also giving as the result a plane film in the opening; that is besides what one easily verifies by means of the ring of the preceding paragraph, by holding it with the hand by the tail of its fork and by withdrawing it from the liquid in an oblique position.

When one moves away from the liquid after having immersed and having withdrawn the widened opening of the tube, this opening thus carries always with it a plane film. That being so, if one blows then on the narrow end, the film in question being subjected on one of its faces to an excess of pressure on behalf of the air, it will have to break or bulge towards the outside; however, unless it is not of an excessive thinness, its cohesion will be more than sufficient to prevent it from breaking; it will consequently start to bulge while extending; and as besides water films of soap or glyceric liquid are thinned only slowly by the descent of the liquid towards their lowest part, our film will continue to bulge and to extend; finally, since it is based on a circular periphery and it is continuous in all its extent starting from this periphery, it will constitute a portion of sphere.

In the process of blowing, the portion of sphere thus formed must be always increasing in diameter; but this increase ends up causing the rupture of the film; indeed, it is thinned progressively, initially by its extension, in the second place by the gradual descent of the molecules towards its lower part, and finally, at least when it is a simple soap solution, by the evaporation of water. There must thus arrive a moment when the film will be attenuated so much, that it will burst for the lightest cause. Now if, before approaching the point where this last phenomenon would occur, one ceases blowing and one gives to the tube a rather fast movement upwards, the bubble, in consequence of its inertia and the resistance of the ambient air, will remain more or less behind; but, because of its cohesion and its adherence at the solid edge, the film, in general, will not break, and the bubble will remain connected, for one moment, to this solid edge by a film trail, like the oil bubble of § 224; then the trail will be pinched off in the same way, to separate in two portions, whose upper will go up towards the opening and will occupy it in the state of a plane film, while the lower will close the bubble, so that the latter will be isolated in the air and in the state of a whole sphere; only here the phenomenon is carried out with too much speed for the eye to grasp the process. As for the film plane which was again placed on the solid edge, it could, if it is not too thin, be used to inflate a second bubble, and as children know, indeed, to thus form perfectly several successive soap bubbles without re-immersing the opening in the liquid.

Everyone knows that one can also inflate bubbles at the end of a narrow tube not widened; for example at the end of the stem of a pipe from which one removed the head. In this case, when one immerses end of the tube in the liquid the and withdraws it, capillarity maintains a small column of this liquid inside, and when one blows then on the other end, the column above will form on the opening a small mass in which the air is introduced to extend it and work it into a bubble, absolutely like the alcoholic liquid does with regard to the small oil mass in the experiment of § 224.

§ 229. Let us come to the generation of films in which the role of cohesion and viscosity is not completely the same as with regard to the preceding ones.

Let us point out another process known, but singular, for the creation of plane liquid films, or nearly such: one takes between the two hands, by the bottom and the neck, a bottle containing a small quantity of liquid, carefully stoppered; it is held horizontally, and one gives a movement to it which obliges the liquid to sweep all around the interior concave surface of it; as soon as one stops, one in general sees one or more plane films laid out across the bottle; one can then upright the bottle and place it on a table with its films, which are then horizontal; I suppose, of course, that one employs a liquid giving a sufficient persistence, such as soap water or glyceric liquid.

One can explain in the following way the generation of these films: at the time when one ceases the movement of the bottle, the liquid, which forms a layer on all the interior concave surface, slows down its rotation, and the portion which then occupies the higher half of this surface, falls down by its weight; however, because of the irregularities inherent in the operation, this fall of the liquid takes place in certain places preferably, where the layer has the most thickness; there are thus formed, across the bottle, curtains of liquid of a considerable thickness; these curtains are thinned quickly by the later descent of the liquid which constitutes them, but the portions which go down thus decreasing more and more in mass, their movement is blocked more and more by viscosity; finally when they are sufficiently reduced, they cannot go any more than very slowly, and the curtains in question become genuine films.

The films produced by this process with glyceric liquid present remarkable properties: they have an astonishing duration, and their colors reach the black. To obtain the best results, it is necessary to choose a bottle as exactly cylindrical as possible; it is necessary, moreover, for straightening it, to mount it on a shelf with adjustment screws, in order to make quite horizontal the film which one wants to observe; one judges this horizontality by the arrangement of the colors.

In spring of 1862, the temperature of the room being 21°, I formed, in a bottle 7 centimetres in diameter, such a film, with a glyceric liquid which was not the best; I had not had regard to a small defect of cylindricity of the bottle, and the film was bent a little, turning its convexity upwards. It was divided soon into irregular Newton's rings, then a black spot appeared, and I adjusted the screws with care so as to bring and to maintain this spot about in the center; the black then extended gradually, with periods of stopping and slight reduction, and it was only after 17 days that the film was black in its totality; the following day, i.e. 18 days after its formation, it burst, but I had, by carelessness, jostled its shelf; without this circumstance, it would have perhaps persisted several days longer.

During the summer of 1865, I again made, with a glyceric liquid recently prepared and whose bubbles of one decimetre persisted only 5 and 6 hours, a film of this species in a bottle 10 centimetres in diameter, then I placed it, using a suitable support, in a tilted position of approximately 30° to the horizontal, so that the plane of the film made this same angle with the vertical. This film became entirely black after one hour or two, I think, and it persisted in this state for close to 10 days, in spite of its large diameter and its slope.

I said, while speaking about the first of these films, that it was slightly bent and turned its convexity upwards. This form came from the bottle being somewhat narrower in the lower part than the upper part; we know, indeed (§ 216), that due to its tension, a film which contacts a solid face, must be directed normally to this face; however if the interior surface of the bottle is exactly cylindrical, a transverse plane film will meet this condition of perpendicularity; but if the bottle is more or less conical, it is not the same, and the film is curved then so that its outer parts are normal to the solid surface. If the bottle were a little broader in bottom than in top, the film would be curved obviously in the contrary direction; but, in this case, the liquid would gather, by the action of gravity, about the middle of the film, and consequently it could never become black in all its extent; it is thus necessary, if one cannot get a completely cylindrical bottle, to take one for which the largest diameter is at the top.

The majority of liquids, if not all, are likely to thus give transverse films in a bottle. I obtained some, for example, with distilled water, in a bottle 14 centimetres in diameter; only they do not have any persistence.

§ 230. Certain films are developed by the spreading out of a moving liquid. Savart first called attention to these films in two beautiful memoirs, of which we will summarize here the parts of interest to our subject.

In the first[129], the famous physicist studies in particular the phenomena which occur when a continuous liquid jet, launched vertically downward from a circular opening, comes to strike normally the middle of a small solid disc. Under this condition, the liquid is spread out in a sheet or film which, all things being equal, takes different forms according to the rate of the flow. Let us quote the passage here where Savart describes the phenomena generally; the vessel is a broad vertical tube closed in a lower position by a plate in the center of which an opening is bored; the load is originally 2 meters, and the vessel is emptied freely; the liquid is water.

"To fix ideas, we will suppose that the disc has 27mm diameter, that the distance from this disc to the opening is 20mm and the diameter of the opening is 12mm. At the moment when the flow is established, if the liquid is perfectly calm in the tube, the jet, after having struck the disc, is spread in all directions, and forms a continuous circular sheet, whose diameter is approximately 60 centimetres. The central part of this sheet is thin, flat and transparent, but its circumference, which has a greater thickness, is turbid and presents itself in the aspect of an annular zone covered with a great number of radial grooves cut by other grooves, but circular, which project far a multitude of droplets.....

"The level of the liquid in the tube continuously dropping, the diameter of the sheet increases little by little; at the same time the aureole changes aspect, it becomes more transparent, its width decreases, it gets very bumpy, and finally it entirely disappears when the pressure at the opening is only approximately 60 or 62 centimetres. Then the sheet reaches its maximum diameter, which is approximately 80 centimetres, and it appears as a broad perfectly continuous cup, whose concavity is turned downward, and whose free contour, slightly notched, throws a great number of droplets which leave the protruding angles of the serrations.

"The pressure at the opening always continuing to decrease, the flat sheet that we have just described decreases gradually in diameter, but at the same time it bends on itself with its lower part, going towards the stem which supports the disc, and, when the pressure drops to 32 to 33 centimetres, it is closed entirely by taking the form of a solid of revolution approximately 40 centimetres in diameter and 45 height, whose surface is perfectly connected and whose generator much resembles a half-lemniscate"

Let us say, in passing, that Mr. Tyndall, in a given lesson, in 1854, with Londres[130], varied the shapes of these films by receiving the jet in small hollow cones of various openings; then, when the load is moderated, the films are shaped so as to appear like vases.

§ 231. It is understood that the liquid, at the time when it comes to strike the disc, is deviated laterally in all directions, and there must result a tendency to tearing following the prolongations of the radius of the disc; but cohesion is opposed to this tearing,

[129]*Mémoire sur le choc d'une veine liquide lancée contre un plan circulaire* (ANNALES DE CHIMIE ET DE PHYSIQUE DE PARIS, vol. LIV, 1833, p. 55).

[130]*Some phenomena connected with the motion of liquids.* (Philos. Magaz., 4th series, vol. VIII, p. 74).

viscosity puts, on its side, an obstacle to the liquid thinning too much in certain places, and there is consequently the simple formation of a continuous film. Here thus, as in the cases previously studied, it is again the cohesion and the viscosity which govern the development of films.

As for the aureole, which appears only for strong jets, one can conclude from the observations contained in the Report of Savart that it is due to a vibratory movement of the liquid, probably resulting from the shock against the disc.

One also understands that, under strong loads, gravity cannot produce any significant inflection of the film, the time employed by the liquid molecules in going from the edge of the disc to that of the film being too short; but that, under weaker loads, it is not the same any more, and thus the film must then present the shape of a reversed cup.

Savart attributes to molecular attraction the effects which occur when the load continues to decrease, the lower part contracting, then the closing of the film, but it is limited to this simple outline. As I will have to point out facts of the same category, I will specify more.

When a liquid film is curved and its two principal curvatures are in the same direction, each one of its elements exerts, we know, a directed normal pressure on the side of concavity. According to that, as soon as the film above inflects appreciably under the action of gravity, a new force, the capillary pressure of which I speak, occurs, increases the inflection, and the effect grows as the speed of horizontal traverse of the molecules decrease, until finally the capillary pressure prevails, and the film completely closes.

Thus, initially, all things being equal, the film is closed under a stronger load as the opening is smaller: for example, with an opening of 18mm of diameter, the load corresponding to the closing of the film was only 21 to 22 centimetres, while it was more than 5 meters with an opening of 1mm, 5. And, indeed, it is clear that the larger the opening is, the more liquid it brings in a given time, and consequently the more the film must have thickness; however the capillary pressure being due to the curvatures emanating from only the two surface layers of the film, it varies only with the curvatures and is independent the thickness; but the mass to be driven being proportional to this thickness, the capillary pressure in question produces necessarily all the more marked effect as the thickness is less, from which it follows that, to prevent the film from closing itself, one needs also an all the more considerable load.

In the second place, with large openings, the vertical axis of the closed films exceeds the diameter of their equator, and the opposite takes place with small openings; however that must be according to what precedes, because the thinner the closed film is, the more the capillary pressure fights with advantage against gravity which tends to lower the lowest point of this film.

In the third place, a rise in the temperature of the liquid, by decreasing cohesion, must also decrease the capillary pressure, so that the film is closed under a smaller load, and that is whatexperiment again verifies: for example, with an opening of 3mm and a disc 13m of diameter, the loads corresponding to the closing of the film at the temperatures of 4° and 90°, were respectively in the ratio 1 to 0.29.

Lastly, as one could envisage it, the nature of the liquid exerts a considerable influence: in a succession of comparative experiments made, at a temperature of 8°, on water, alcohol, sulphuric ether and mercury, the loads corresponding to closing, evaluated all in water columns, were roughly as the numbers 1, 0.9, 0.3 and 3.

§ 232. The second memoir[131] to which we have already referred (§ 153) concerns the effect resulting from the mutual shock of the continuous parts of two liquid jets

[131] *Mémoire sur te choc de deux veines liquides animées de mouvements directement opposés* (ANNALES DE CHIMIE ET DE PHYSIQUE DE PARIS, vol. LV, 1833, p. 257).

launched from circular openings in directions which, at the point of meeting, are exactly opposite. The openings were 3 centimetres away from one another; the loads could be raised until nearly 5 meters; at the point of meeting, the directions of the two jets were horizontal; the liquid was water. Again let us let Savart himself describe the general phenomena, in the case of equal loads and equal openings of 3mm diameter.

"There was formed, at all pressures, at the point of meeting of the two jets, i.e. in the middle of the interval which separated the openings, a circular plane sheet, thicker in its central part than at its contour, whose plane was normal to the tangent of the two jets, and which ended in an annular zone, which was disturbed, agitated and noisy, when the pressure exceeded 120 centimetres, but which became perfectly smooth and flat in all its extent when the pressure dropped below this point. For the strongest pressure, which was 488 centimetres, the diameter of this sheet was initially 24 to 25 centimetres; then it increased little by little as the pressure became lower, and when it was only from 115 to 120 centimetres, it was then approximately 38 centimetres; then it decreased again.... "

And further:

"With openings of 4mm and then of 6mm having been substituted for those of 3mm, similar phenomena were offered to the observation, with this difference however, that the diameter of the sheets was all the larger, with equal pressure, as that of the openings was itself larger.

"As long as the pressure is strong and the sheets are surrounded by an aureole, they appear appreciably circular, and the point of meeting of the jets occupies the center; but as the pressure decreases, their upper vertical radius decreases in length, while on the contrary the lower is increasing.... For pressures lower than those where the aureole disappears wholly, the sheets are constantly surrounded by a small round rim from which escape a multitude of droplets which, in general, follow curves included in the same vertical plane which contains the sheet."

Savart is led by his observations to the two following laws:

For the same openings, the diameter of the sheet without aureole is appreciably proportional to the simple load, and, for the same load, when it is rather weak, it is sensibly proportional to the surface of the openings.

With unequal openings, whose diameters are as 1 to 2 or to 3, and for equal loads sufficiently large, "There is formed a conical sheet whose apex is located at the point of meeting of the two jets, in the middle of the interval which separates the openings, and whose concavity is turned on the side of the jet of the least diameter. When the pressure is sufficiently large in regard to the diameter of the openings, this sheet ends in a haloed part. When the pressure decreases, this aureole is erased little by little, the diameter of the base of the sheet becomes larger, and, after having reached a certain limit, it starts to decrease. The pressure always continuing to decrease, the sheet is closed by taking a form which approaches in general that of a lengthened ellipsoid whose large axis is horizontal."

§ 233. The films in question in this second memoir obviously form from the same causes as those of the first, and the aureoles can also be attributed to vibratory movements of the liquid, coming from the shock; one easiliy explains by the action of gravity the difference between the upper and lower vertical radii for loads rather not very considerable; one gives the same reason for the conical shape of the films in the case of unequal openings and strong loads; finally, the surface of these conical films being curved in a direction, this curvature must also give birth to a capillary pressure, which closes the film when the speed is sufficiently reduced.

We saw (§ 153) that Mr. Hagen indicated the very probable cause of the limitation of the films in this same memoir when they are without aureole, and his explanation is obviously also appropriate for the limitation of the open films of the first memoir; to complete as much as possible the theory of the phenomena, it is still necessary to explain the formation of the drops which escape from the edge of all the films in question; we will do that in § 426.

The aureoles which appear in these same films under strong loads, and which, as I pointed out, appear due to vibratory movements, would also make, undoubtedly, an extremely interesting study; but the observations of Savart on this subject are not sufficiently complete, and it would be necessary to submit the phenomenon to new and more detailed experiments; the best procedure to employ would be, I think, that which I made known first[132], and which consists of observing through an opaque disc bored with narrow radial slits and rotating with a speed which one can graduate at will.

§ 234. Magnus, in the first part of his *Hydraulic Research* [133] published in 1855, described a succession of new and curious experiments by means of which he also obliges a liquid to be spread out into films by the effect of movement. The learned physicist had the idea to seek what occurs when the continuous parts of two jets meet, either in opposite directions, or by forming an angle between them. Let us summarize here the most salient facts among those which he observed.

Initially, with two horizontal water jets whose axes intersect and form between them an angle of approximately 40°, with equal openings and a moderate common load, there develops, starting from the point of meeting, a first vertical film lengthened in the direction of the general movement, and whose upper and lower edges meet at a certain angle at the most distant end; from this end leaves a second similar film, but having its plane perpendicular to that of the first, then comes a third film, which is again vertical, and, beyond that, the liquid scatters. These films are thickened towards their edges, and there often results definitely drawn beads; the experiment offers thus a new example of beads at the free edges of the liquid films. Magnus speaks not about drops launched by these edges, and if one consults the figures which accompany the Report, one does not see, indeed, any projection of drops represented close to the end of the third film.

Magnus attributes the limitation of these films and their pointed lengthened form to the cohesion of the liquid, which pulls in a continuous way the two edges one towards the other, and finally obliges them to meet. The explanation will be more precise if for cohesion one substitutes the tension of the two faces, and if one adds to it the capillary pressure due to the transverse curvature of the edges. Magnus explains the succession of the films by noticing that since the meeting of their thick edges takes place at a certain angle, it must give birth to a phenomenon similar to that which comes from the meeting of the two jets themselves, i.e. the development of a film forming a right angle to the plane containing the axes of these edges.

In the second place, openings being always equal, and speeds equal and moderate, if the two jets, instead of meeting so that their two axes intersect, only overlap a little, and if, moreover, the angle that they form is approximately only 35°, they continue their race beyond the point of meeting, with a film developed between them, and inflect in order to meet again further on; then the same thing reproduces, and, under favorable conditions, it can even happen that it repeats a third time before the whole is reduced to drops; let us add this characteristic, that the jet which at the first meeting was the upper, becomes the lower at the second meeting, and so on.

Magnus again simply attributes to cohesion the kind of traction exerted by the in-

[132] *Sur un nouveau moyen de déterminer la vitesse et les particularités d'un mouvement périodique très rapide, tel que celui d'une corde sonore en vibration, etc.* (BULLET. DE L'ACAD. DE BELGIQUE, 1836, vol. III, p. 364).

[133] *Hydraulische Untersuchungen* (ANN. OF Mr. POGGENDORFF, vol. XCV, page 1).

terposed films and which brings back the jets towards each other; we will say, as in the preceding case, that the effect is produced by the tension of the two faces of these films joined to the capillary pressure due to the transverse curvature of the external parts of the two jets.

§ 235. It was seen that the films of Savart, when they take curved forms and one reduces sufficiently the speed of the liquid, are closed by the effect of the capillary pressures exerted by all the points of their two faces due to their curvature. It is to the same kind of phenomenon that the singular generation of bubbles observed by my son Felix belongs[134].

The experiment consists in launching obliquely in the air soap water contained in a cup, so as to spread out the liquid in sheets or films; this film tears, in general, in several portions, of which each one is closed at once to constitute a complete hollow bubble, which floats down with more or less slowness. Sometimes one obtains only one bubble, which can then reach 8 or 9 centimetres diameter; but usually it forms several, and they are then smaller.

The theory of the phenomenon is extremely simple, and follows from what I explained in the preceding paragraphs. Let us consider the case where the film does not tear, and produces consequently a single bubble; what we will say with regard to this film will apply equally to the partial films resulting from tearing. By the same causes as in the films of Savart, causes which we will examine later (§§ 426 and 427), at the edge of the current film is formed necessarily a rim, which is resolved into small isolated masses; my son noted, indeed, that this edge is notched and lets many drops escape. As soon as it is developed, the film starts to fall due to its weight; but, in consequence of the resistance of the air, its central part goes down much less quickly than its edge, around the length of which runs the mass of the rim, and it thus takes a strongly convex shape, turning its convexity outward; it is thus appreciably under the same conditions as those of the first memoir of Savart, the resistance of the air against its central part playing here the role of the small solid disc; and as the capillary pressures born from the curvatures at all the points of the two faces do not have to fight against the translatory movement based at the center, these pressures quickly close the film at the bottom, and form it into a complete spherical bubble.

My son had not succeeded in obtaining in this manner pure water bubbles, because he operated from too low a height; but Mr. Van der Mensbrugghe[135], by launching liquid from a window of an upper story, saw pure water giving perfectly complete bubbles, and he had the same result with alcohol, spirits of turpentine, petroleum oil, olive oil and several salt solutions; he concludes that the majority of liquids, if not all, are likely to swell, by this process, in hollow bubbles.

As for my assumption relating to the formation of vapor bubbles, an assumption recalled in the Note of my son, and consisting of claiming that the gas vapor passes to the liquid state in the form of isolated plates, and that these plates, generally curved, are closed like the films which have just been in question, I give it up today after a more attentive examination; indeed, to give rise to bubbles as tiny as those of the vapor of clouds, it would be necessary that the plates were themselves extremely small, and consequently they would fall only with great slowness; one could not thus suppose a fast fall for their free edges and an energetic influence of the resistance of the air on the remainder only. What would happen on the contrary to such plates, it is that the capillary pressures due to the curvatures of their faces would erase these curvatures,

[134] *Sur un mode particulier de production de bulles de savon* (BULLET. DE L'ACAD. DE BELGIQUE, 1882, 2nd series, vol. XIII, p.288).

[135]) *Sur quelques effets curieux des forces moléculaires des liquides* (BULLET. DE L'ACAD. DE BELGIQUE, 1864, 2nd series, vol. XVIII, p. 161).

and that at the same time the free edges retracting on themselves with an excessive speed because of their thinness, would transform instantaneously the plates into small filled globules.

§ 236. The experiments above suggested to me the idea of a mode of creation of bubbles a little different, but also founded on the resistance of the air. A wire ring 7 centimetres in diameter, for example, is fixed, by a point of its contour, at the end of a straight stem directed according to the prolongation of a diameter. Holding this ring by its stem, one develops a plane film in it by immersing it in a good household soap solution, and withdrawing; then one gives it at once in the air, with a suitable speed, a translatory movement perpendicular to its plane. The resistance of the air then hollows the film behind, and transforms it into a kind of lengthened bag, strongly bulging in its posterior part, and presenting a narrowing in the vicinity of the ring; finally, by a small deceleration in speed, the narrowing is closed, and a bubble, whose diameter can be approximately double that of the ring, is isolated in the air. Usually the portion of the narrowing which is based on the ring bursts as soon as this narrowing is closed; but sometimes it will form a new plane film in the ring, and one can then obtain a second bubble immediately after the first. To succeed in this experiment, one needs some practice, because success depends on more or less speed which one gives to the ring.

The phenomenon is explained by the same principles as the generation of complete bubbles by means of a pipe (§ 228); indeed, moving air is directed against an adherent film with a motionless solid contour. Although this contour with its film is transported normally to it in motionless air, the results must obviously be similar.

§ 237. Further, let us mention the odd production of bubbles announced by Minary and Sire[136].

The experiment, such as these physicists describe it, "consists in pouring in a certain quantity of olive oil from one and half to two times its volume of concentrated sulphuric acid, and violently agitating the mixture using a glass rod.

"Agitation being practised in a stemmed glass of a suitable capacity, one is not long in seeing rise a crowd of small hollow bubbles which fly in all directions. The largest (which sometimes reach 1 to 2 centimetres in diameter) generally fall down in the mixture after a weak rise; but smallest spring easily into the ambient air by taking advantage of its agitations. It occurs in these circumstances for bubbles of great tenuity, and all the more as agitation is more violent and is carried out in a certain direction which appears to support their formation.... The mixture of oil and acid does not allow one to easily blow bubbles at the end of a widened tube; it is rare that one can produce some from 1 to 2 centimetres in diameter, which besides do not persist."

I look at as impossible that the gas portions which come from the interior of the mixture above can form on its surface film segments of a sphere, and are able to continue to raise the liquid film so as to complete spherical film bubbles isolated in the air; indeed, the imprisoned gas has obviously not nearly enough mass so that the inertia of its upward movement overcomes the capillary pressure exerted by the convex film which it determines on the surface of the mixture. I did not repeat the experiment of Minary and Lord, and consequently I will not venture any explanation. This experiment does not appear to me without danger of the acid droplets being able to be projected in the eyes of the observer; it is necessary, I think, to look through a screen of glass.

§ 238. It remains for me to speak about a last kind of film; they are formed, as those which we reviewed in §§ 230 to 235, by a moving liquid; but cohesion and viscosity play in their generation only a secondary role, because the molecular flow lines which

[136]*Sur un mode particulier de formation de bulles liquides* (COMPTES RENDUS 1862, vol. LV, p. 515).

one can look at them as made up of do not have a tendency to deviate from each other. Such is from top to bottom the vertically launched jet from an opening in the shape of a straight slit or lengthened rectangle.

These films also present remarkable phenomena; the jet above for example, appears, one knows, formed, for a certain distance from the opening, of a succession of portions whose planes have alternating directions at right angles to each other, as in the first of the experiments quoted in § 234, and the fact is obviously explained in the same manner: as soon as the jet leaves from the opening, the two edges of the film which constitute it are driven one towards the other by capillary pressures due to their strong transverse curvatures; there are thus formed rims, which, having suddenly met angularly, oblige the liquid to open out in a perpendicular plane, then the same effect happens in this second film, and so on. Magnus, who devoted part of his hydraulic research[137] to the jets launched by noncircular openings, explains the phenomenon concerned here by a theory which reverts to the preceding one.

§ 239. I made known a long time ago an experiment[138], which seems to me curious in that it shows a large vertical liquid film of which a free edge, provided with a rim, is straight and tilted to the horizontal. It consists in making water run out through a vertical rectilinear slit in the side wall of a vessel, from a point close to the bottom to above the level of the liquid. It is the upper free edge of the film which presents the form indicated; only it starts to appear exactly straight only starting at a small distance from its origin, i.e. the point of the slit corresponding to the level of water in the vessel, but it preserves its straight aspect until the spot where the rim which borders it is met by the lower free edge; the angle which it forms with the horizontal is a little more than 45°; the rim, not very thick at its upper end, grows bigger from there until the other end; such is at least the general appearance which it offers to the first observation; we will see further that its real constitution is not so simple. The lower edge of the film is also furnished with a rim, which is also growing bigger starting from its origin, but which is much less bulky than the first; finally this lower rim is not directed exactly according to the parabola that would trace an isolated flow line from the lower end from the slit: it is less downward, and likewise, close to its origin, it starts by having a rising direction, so that it always will meet the upper rim higher than the parabola in question would.

The theory explains of all these characteristics. If, by thought, one substitutes for the slit a continuum of openings infinitely small, infinitely close, and arranged along the vertical, if, moreover, one supposes that each one of the flow lines launched by these openings describes exactly its parabola without being influenced either by resistance of the air nor by other flow lines, and if one seeks the envelope of all these parabolas, one easily finds that it is a line forming with the horizontal an angle of 45°. Passing from these fictitious conditions to the real conditions, and neglecting, for one moment, the effect of the capillary pressures at the free edge considered, one understands that nothing prevents each flow line from describing the portion of its parabola between the point from where it escapes and where it will touch the enveloping line; but to continue to follow this same parabola, the flow line should go down in the thickness of the film through the lower flow lines, which is impossible; it is constrained to skirt all those, i.e. to carry out its later progress in the direction of the free edge. This free edge must thus appreciably affect the rectilinear form found for the fictitious conditions; but the flow lines which go there and thus accumulate there in all the more considerable quantity as one more moves away from the point located at the level of the liquid, must,

[137] See the note of § 234.

[138] *Note sur la figure de la nappe liquide qui s'écoule par une fente étroite, rectiligne et verticale, partant du fond d'un réservoir et s'élevant jusqu'au-dessus du niveau du liquide* (BULLET. DE L'ACAD. DE BELGIQUE, 1836, vol. III, p. 145).

independently of the action of the capillary pressures, to form along this same edge a rim which is growing bigger starting from its upper end.

On their side, capillary pressures must produce a significant effect for a small distance from the upper end, because, close the top of the film, the free edge has received yet only a few flow lines, and the speed of projection of the liquid is not very considerable; in the space in question, these pressures must thus drive back the free edge and give it a concave curvature in the longitudinal direction; but, it is realized, the effect must be much less marked as the slit is less narrow, and that is what experiment verifies, as soon will be seen; with a slit of 2mm of width, the concave curvature extends at most a centimetre and half of distance[139]. As for the rim of the lower free edge, one understands that it is due almost entirely to capillary pressure, and one easily explains, accordingly, its rising close to the slit, at its least volume. Finally, if, in the experiment, the angle which the upper free edge forms with the horizontal exceeds 45° a little, one can attribute it, I think, to the tension of the two faces of the film.

My apparatus consists of a cylindrical tank 50 centimetres in diameter and 54 height; the slit, of which the height is 49 centimetres, consists of an interval between the edges of two thick iron rules, joined together by a cross-piece at each of their ends; these edges are cut in bevel on the side which faces the interior of the vessel.

I repeated the experiment with slits of various widths, and I recognized that the most suitable width was 2mm approximately; when the slit is notably narrower, the capillary pressure at the top of it is so energetic that it prevents the exit of the liquid until a certain distance below the level: with a slit of about 1/3 of millimetre, the liquid is retained thus on a stretch of approximately six centimetres and it is only bottom of this stretch which emits the upper free edge of the film; moreover, with a too narrow slit, the lower free edge is much raised, so that the rectilinear portion of the upper rim is shortened considerably. With a slit of 2mm of width, resistance to the exit of the liquid appeared more, the lower free edge was raised only slightly close to its origin, and one could note the rectilinear form of the profile of the upper rim over a length of approximately 75 centimetres.

I must insist on the nature of both rims. That of the lower free edge is transparent, almost uniform, and resembles a curved crystal stem; but that of the higher edge presents an extremely singular constitution: starting from about the middle of its length (always with a slit of 2mm), its two side parts each convert into a sheaf of droplets, and the intermediate part shows disorder, as if itself were made of moving drops; if one looks with attention at the portion of this same rim lying between its origin and the place where its resolution into drops takes place, one notes that it is striated longitudinally, though in a little confused way. We will reconsider (§ 446) this constitution.

As one can see in an addition in my Note[140], Le François generalized the question by analysis, on the assumption of the exactly parabolic and independent flow lines, by supposing the wall of the vesselto be inclined, and the slit bored according to the direction of the greatest slope. He arrived thus at curious results, among which I will restrict myself to quote the following: 1° whatever the slope of the wall, the free edge of the film which leaves from the liquid level is always rectilinear; 2° for a given slope of the wall, if it is imagined that the liquid is alternatively placed side then other of this wall, the straight free edges respectively corresponding to these two cases are always perpendicular between them

§ 240. No liquid not having cohesion and viscosity, it follows from our theory that all must be likely to be converted into thin films. And, indeed, we saw, in the

[139] At the time when I published my Note, I had studied capillary forces less than today; also I gave, in this same Note, an erroneous explanation of the concave curvature in question.

[140] BULLET. DE L'ACAD DE BELGIQUE, 1836, vol. III, p. 222.

experiments of Savart, water, alcohol, the ether and mercury spread in such films; we saw in the same way water, alcohol, spirits of turpentine, petroleum oil and olive oil, in the experiment of Mr. Van der Mensbrugghe, to swell in complete hollow bubbles; finally Mr. Gladstone, in a Note in question soon, affirms that all liquids give, by agitation, film caps on their surface.

Let us add that that for the majority of liquids, if not all, the films can, by the use of adapted means, acquire considerable dimensions; that is what is shown, for example, by films of Savart, that I have just pointed out.

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EXPERIMENTAL AND THEORETICAL
STATICS OF LIQUIDS
SUBJECT TO
MOLECULAR FORCES ONLY,

BY

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STATICS OF LIQUIDS

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TO ONLY MOLECULAR FORCES.

CHAPTER VII.

Investigation into the principal causes on which the easy development and persistence of liquid films depend: Surface viscosity; influence of the ratio between the viscosity and the tension.

§ 241. If all liquids can develop into thin films, they present nevertheless, for the ease of this development and the persistence of the generated films, considerable differences: one inflates easily, for example, large bubbles at the opening of a pipe with soap water, but nobody would think to try it with pure water; the transverse films in a bottle persist an enormous time if the liquid employed is glyceric liquid, and they burst almost immediately if it is water (§ 229).

While the easy extension of soap water and some other liquids into thin films of great extent is generally attribute to viscosity, I will show that viscosity, at least such as one understands it, plays only a tiny role in this ease of extension, and I will try to arrive at the true cause of the phenomenon.

§ 242. Mr. Gladstone is, I think, the only one who dealt a little seriously with the question. This scientist published, in 1857, a Note[141] on the foam which is formed, by agitation or otherwise, on the surface of some liquid; I will translate the passages which refer to our subject:

"All liquids," said Mr. Gladstone, "when one shakes them with air, form bubbles; but, for some, these bubbles burst and disappear as soon as agitation ceases, while others show a more or less permanent foam. This difference between liquids appears to hold with a specific character, and one can, up to now, make it depend on no other quality.

"In general, aqueous solutions of organic compounds are most likely to give foam.

"The solutions of acetates are particularly disposed to the production of a persistent foam; they have this property to such a degree, that I could sometimes, among various mixtures of salt solutions, recognize by this means those which contained an acetate. Iron acetate is the best; but acetates of copper, lead and other metals present the same property in a very marked way. However, acetic acid itself does not show any provision for the formation of foam. The bubbles developed by the agitation of alcohol or ether disappear instantaneously.... Iron citrate is similar to acetate.

"This ability to foam is completely independent of density: a dense solution of sulfindigotic acid foamed by agitation, but an ammonium chloride solution of great density does not produce any durable foam, while, on the other hand, a weak soap solution, which differs little from distilled water, gives rise, as everyone knows, to a very persistent foam.

This Note, one sees, is especially about foam; but, we know, this is only one assembly of films, and it appears natural to admit that a liquid which forms, by agitation, an abundant and persistent foam, must inflate easily in bubbles on the opening of a pipe or a tube. That is, indeed, the general case, and soap water offers us a familiar example. I however met, in this respect, curious exceptions; I will make known them later.

[141] *Note on froth* (PHILOS. MAGAZ., 4me series, vol. XIV, p. 314).

Mr. Gladstone announcing iron acetate as remarkable from the point of view of foam, I got a concentrated solution, as neutral as possible, of iron peroxide acetate; it foamed very well, and one could indeed easily inflate, with the opening of a pipe, some bubbles of five and even sometimes six centimetres in diameter.

What is especially important for us to notice, is that Mr. Gladstone states that the ability to foam does not depend on any known property of liquids, and that he consequently looks at differences of viscosity and cohesion as insufficient to explain the diversity which liquids have in this respect.

§ 243. Let us take up the question where he left it, and try to continue it.

While cohesion is opposed to the rupture of films, tension constitutes, on the contrary, a force which acts unceasingly to cause this rupture. But the tension is necessarily lower than the cohesion of the surface layers, without which it is obvious that the creation of films would be completely impossible.

In the second place, since the tension is independent of the thickness (§ 161), it follows that a film does not have, by itself, more tendency to break when it is thin than when it is thick.

This deduction seems, at first sight, to agree badly with observation; indeed, one usually sees films decreasing much in thickness before bursting: when a soap bubble is inflated, it often reaches a large size, and bursts consequently only when the film becomes very thin; if one deposits on the surface of soap water a bubble of this liquid, a bubble which is transformed at once into a segment of a sphere, the color of its top can go, one knows, to intense black, which corresponds to a thickness of approximately 0.00001mm, etc.

However, examine the matter more closely. Soap bubbles and caps of the same liquid also frequently burst before the films which constitute them are much attenuated; when a large bubble formed of good glyceric liquid is deposited on a ring, the film initially thins, then takes again (§ 108) an increasing thickness, and it is only when it approaches its originating value again, that the bubble breaks; one can produce pure water films in various manners: for example in segments of a sphere on the surface of the liquid by the rise of bubbles of air, in the plane form across a bottle, etc; however, except for rare exceptions, these water films remain perfectly colorless until their disappearance, from which it follows that they break when they still have, for films, a considerable thickness. We will see besides that many other liquids are in the same case.

Thus if films appear more likely to burst when they are thinner, it is probably that they resist less external causes such as small shocks, etc; we saw, indeed (§ 229), that a large film of glyceric liquid protected, as much as possible, from these external causes become black in all its extent, and persisted, with this excessive thinness, for a great number of days.

§ 244. As I will have to compare the films of a great number of liquids, I will describe the processes which I employed for their production and their observation.

Films formed of the same liquid and in the same circumstances persist, in general, more or less for the same time; however, for the very great majority of liquids, films of any considerable dimensions burst almost at the moment of their development; it was thus necessary to be limited to small films. I chose the caps produced on the surface of liquids by the rise of bubbles of air, and I paid attention only to those whose base was between 10mm and 12mm diameter. Here is the process with which I was most successful:

One sets at the bottom of a glass bottle a small porcelain or glass vessel whose edge is approximately four centimetres in diameter, one fills it a little above the edge with the liquid to be tested, then one introduces into it the lower end of a glass tube shaped as I shall describe: that which I used for my experiments has internal diameter 5mm; its lower end, bent into a right angle over a very small length, narrows, and its opening is only 2.5mm; a short rubber tube connects the other end to a second tube of glass, which can thus move in all directions, and to which one applies the mouth. This provision makes it possible for the experimenter to be placed conveniently: he holds the rubber tube pressed on the edge of the neck of the bottle, and he gives to the external glass tube a direction obliquely downward; this last tube is bent besides, towards its free end, at an obtuse angle, in order to lead horizontally to the mouth; there it is closed by a filter paper plug which tightens enough to let air enter only in small quantities and make blowing as moderate as is wanted; let us add that the most suitable depth of the lower opening below the surface of the liquid depends on its nature. The experimenter easily finds out about these small modifications, and soon acquires the skill to produce at will caps of the necessary diameter.

When a liquid provides caps of a sufficient persistence, those tend to stick to the tube; for preventing that, one needs, as soon as one of them is formed, to gently raise the tube out of the liquid; as the cap cannot run up the edge of the small vessel because of the slight convexity of the liquid, it remains about in the middle of the surface, and consequently under the most favorable conditions; indeed, it is then entirely free, and the wall of the bottle protects it against small agitations of the ambient air, and against the breath of the experimenter when he observes closely.

Finally, the majority of liquids require other precautions still, if one wants to remove their caps from any foreign influence. One of these influences is evaporation, when the liquids are more or less volatile. To prevent it, one starts by pouring in the bottle a small layer of the liquid to be tested, or simply water if the liquid to be tested provides only water vapor; then one applies against the interior wall, from the bottom to the top, on the right and on the left of the experimenter's line of sight, a broad band of filter paper impregnated with the same liquid; or, if this liquid is caustic, one spreads it all around the interior wall so that it is wet by it; one then lowers the small empty vessel to the bottom of the bottle, and one closes that with a very tight rubber plate in the neck opening with two holes; through one of these holes, with friction, the tube being used for blowing passes; through the other, one introduces the stem of a small funnel, a stem which must be enough long to reach the opening of the small vessel, and one closes this funnel from the outside, with a small cork stopper. That done, one leaves the apparatus alone for a time that one judges sufficient so that the interior atmosphere is saturated with vapor. After this time, which, in my experiments, was at least two hours, one uncorks the funnel, and, by its channel, one fills the small vessel, then the stopper is replaced, and one carries out the tests immediately.

With very volatile liquids, such as alcohol, sulphuric ether, etc, these precautions are insufficient, because, undoubtedly, of the difficulty in completely saturating the atmosphere within the bottle. In this case, one produces the caps by simply shaking the liquid in a bottle let rest beforehand for several hours after being strongly agitated. But if this last process makes it possible to operate in an atmosphere as saturated as possible, it presents disadvantages which must make one avoid it as much as one can: the liquid is moving during the appearance of the caps, which makes the observation difficult, and, if the caps do not have a very short persistence, they often will stick to the wall, where they become more or less deformed.

Some nonvolatile liquids, such as glycerin, sulphuric acid, etc, absorb moisture from the air, which constitutes another foreign influence. To prevent this, one introduces at the bottom of the bottle a substance which itself absorbs moisture, such as calcium chloride or sulphuric acid, all the rest being laid out as previously. After a time regarded as sufficient so that the atmosphere of the bottle is dried, one fills the small vessel, and one operates at once.

§ 245. My experiments, carried out with all the precautions which I have just described, led me to divide liquids, from the point of view of their films, into three principal categories.

The liquids which make up the first category present the following characteristics: when strongly agitated in a bottle, they never produce very abundant foam, some even do not give any at all; they are not inflatable in bubbles with the opening of a pipe, or if bubbles are sometimes obtained, they hardly exceed the opening in diameter; their caps have only a rather short duration, lasting very variably for each liquid, and very different, as for its maximum, from one liquid to another, but never exceeding a small number of minutes; for several of these liquids, all the caps remain colorless until their rupture; for others, the majority also remain white, but a relatively small number show, after a more or less long interval, a weak beginning of coloring. This usually consists in the appearance, at the top of the cap, of a system of narrow red and green rings, which does not exceed 1.5mm in diameter; this system develops in a very short time, then keeps the same tiny dimension until the cap bursts; sometimes, moreover, the most durable caps end up being covered, on all the rest of their surface, with a pale rippled pink and green effect; for some liquids, only this rippled effect appears, i.e. without there being beforehand formation of the small rings. Lastly, a quite singular thing, the aqueous liquids for which one observes, in an atmosphere saturated with water vapor, these phenomena of incipient colors, do not show any trace when they are placed in a desiccated atmosphere, and thus their evaporation, instead of being removed, is activated.

Thus, in short, the general characteristics of this first category are little or no foam, impossibility of inflating bubbles, short duration of the films, absence of colors on the caps or slow coloring, weakly only, and not offering, on any of the film, red and green of the last orders.

Among the many liquids which line up in this category, I will cite water, glycerin, sulphuric and nitric acids, ammonia, and solutions saturated with tartaric acid, potassium nitrate, sodium carbonate and calcium chloride.

The liquids of the second category, like the preceding, develop little foam or develop none, and do not let themselves inflate in bubbles on the opening of a pipe; their caps have, in general, durations much shorter still; but, for the same liquid, all the caps, or at least a part of them, are covered, at the moment of their formation or very shortly after, with marked colors of the various orders on all their surface; these colors can be laid out in horizontal rings, and then, for some liquids and under certain conditions, they indicate that the thickness of the film is growing from the base to the top of the cap. Let us add that, in consequence of their slight persistence, it is often necessary to do multiple repetitions of such experiments for judging colors well and their arrangement.

Thus, generally, the liquids in question are distinguished from those of the preceding category by a prompt coloring of the films, pronounced, and showing the colors of all the kinds.

The liquids of this second category are fatty oils, lactic acid, crystallizable acetic acid, spirits of turpentine, alcohol, benzine, dichloroethane, chloroform, sulphuric ether, carbon bisulphide, and undoubtedly a great number of others.

The liquids which belong to the third category form, by agitation, a voluminous and very persistent foam; one easily inflates them in bubbles with the opening of a pipe; their caps are maintained much longer than those of the two preceding categories, usually several hours, sometimes even several days; they have initially, in general, a very notable colorless phase, of which the duration differs much from one liquid to another, then are tinted gradually, but in a manner which varies a little with the liquids.

These liquids are very few; they are reduced, I think, to solutions of various soaps, saponin solution and that of albumin; one can join iron peroxide acetate solution to it. I do not speak of the sodium oleate solution, because it must be placed with those of the soaps, nor of the glyceric liquid, whose property to extend easily in large bubbles results from the soap or sodium oleate which it contains, nor of the wood decoction of Panama, which contains saponin, nor of still some other liquids into the composition of which enters an albuminoid substance.

Certain substances solid at ordinary temperature, but which heat melts, also have, in this last state, the property to easily give bubbles of large diameter; such are glass, and a mixture of rosin and linseed oil, as indicated by Mr. Böttger[142].

Lastly, one understands, the three categories above are not so distinct that there are not some liquids forming so to speak passage from one of them to another: I will cite as examples a solution of one part of gum-arabic in ten parts of water, which takes part at the same time of the first and the third category, and a suitable rosin solution in olive oil, which takes part of the second and the third.

§ 246. Before going further, I will explain in some detail the particular facts relating to the caps of each liquid which I subjected to experiment[143]

The substances employed were, with a few exceptions, such as one finds them in the trade; it was needless for the object of my work to seek to have them chemically pure. Let us start with the first category.

Distilled water. 1° In an atmosphere saturated with its vapor. Of a hundred successive caps, eighty-three, of which the durations varied from a split second to 7″, remained colorless until their rupture; sixteen showed, after respective colorless phases of 1″ to 6″, the system of small red and green rings; among these last, two of which had the longest durations, being 11″ and 13″, ended up with the red and green pale rippled effect; in that of 11″, the red and the green of the small rings made way for other colors little by little; finally a cap, of which the duration was of 10″, presented, while approaching its rupture, the pale rippled effect without small rings.

2° In an atmosphere desiccated by means of sulphuric acid. A hundred caps, of which the durations varied from a split second to 12″, remained completely white.

Glycerin of Price, in a desiccated atmosphere. A hundred caps produced in the ordinary manner, all remaining colorless, but persisting for a maximum of only 2″. One obtains some more durable caps by quickly forming some small ones under each other, which merge to only one, which easily gives the necessary diameter; forty caps were generated by this process, and, on one of them which persisted 80″, I distinguished, after 46″, the small system of rings.

Sulphuric acid, in a desiccated atmosphere. A hundred caps of a split second to 28″, including six, of 3″ to 8″, offer, after intervals of 2″ to 5″, the small system of rings; in one of those, the small rings ended up becoming crimson and blue.

Nitric acid, in an atmosphere saturated with its vapor. A hundred caps of a split second to 1″, all remaining colorless.

Ammonia, in an atmosphere saturated with its vapor. A hundred caps of a split second to 4″, all remaining colorless.

Saturated tartaric acid solution , in an atmosphere saturated with water vapor. Ninety caps of a split second to 142″, all remaining colorless.

Solutions saturated with sodium nitrate[144], in an atmosphere saturated with water

[142] *Beiträge zur Physik und Chemie.* Frankfurt A. M., 1838. p. 13.

[143] As I have since learned, Fusinieri had made, mainly, these same experiments (§ 324), but with a very different aim, and drew some very different conclusions than mine.

[144] Salt had been purified by a second crystallization

vapor. A hundred caps of a split second to 6″, all remaining colorless.

Solution saturated with sodium carbonate[145]. 1° In an atmosphere saturated with water vapor. A hundred caps of a split second to 26″, including five with the small system of rings after intervals of 3″ at 9″.

2° In an atmosphere desiccated by calcium chloride. A hundred caps of a split second to 30″, one of 58″, all remaining colorless.

Solution saturated with calcium chloride. As this liquid probably tended neither to emit, nor to absorb aqueous vapor, I tested it by leaving the bottle open. Seventy caps of 1″ to 229″, including five of 116″ to 229″, showed, after intervals of 100″ to 150″, the pink pale rippled effect and without the small system of rings;. the durations of those which remained colorless were from 1″ to 148″.

I must say here that, for some of these liquids, for sulphuric acid, for example, the much smaller caps, like 3mm to 4mm, often persisted much longer, ending up with coloring.

§ 247. Let us act in the same way with regard to the second category.

Olive oil, in the open bottle. The caps persist, to the maximum, 0.7″; all, after an interval so short that its existence is doubtful, express colors: one sees going down until the base, and very quickly, the red and green rings, followed by blue and crimson rings, then by an orange ring, then yellow, which leaves in its interior a white space; this space darkens, become of a bluish gray, and invades almost all the cap; finally the top darkens further, and the cap bursts. The colors are thus arranged so as to indicate a decreasing thickness from the base to the top of the caps, an arrangement which we will name *direct*.

Oil of sweet almond, in the open bottle. Maximum duration 0.2″; phenomena similar, but more difficult to observe, because of short persistence; one can, however, tell that the colors are also direct.

Lactic acid. This liquid absorbing moisture from the air, I tested it in an atmosphere desiccated by calcium chloride. Duration of 1″ to 18″; after a very short but appreciable interval, phenomena similar to those of the olive oil; only the time of the descent of the rings is 1″ to 2″, after which the whole cap is white, then passes to slightly bluish gray starting at the top, etc; the colors thus have still the direct arrangement.

The same liquid tested in the open bottle gave only caps of 0.6″ at the maximum, and all showed colors in the inverse arrangement[146], from which it follows that in these caps, the thickness was growing from base to the top.

Crystallizable acid acetic. 1° In an atmosphere saturated with its vapor, process of shaking in a bottle. Maximum duration 0.8″; all the caps, after a very short white phase, are shown colored, mostly in horizontal rings, and offering the direct colors.

2° In the open bottle. Duration of a fraction of a second to 2′; extremely short colorless phase, then suddenly colored markedly opposite, with the first order white at the base, the red and the green of the last orders at the top; these colors persist without changing, and going neither down nor up; but the higher rings quiver a bit.

Spirits of turpentine. 1° Process of shaking in a bottle. Duration of a fraction of a second to 6″; all the caps are colored as of their formation, and, in almost all those where the arrangement is regular, the colors are direct and go down very quickly; in some I observed the bluish gray on all the surface, sometimes alone, sometimes after the descent of other colors; out of a great number, only one presented the opposite arrangement.

[145] Same remark.

[146] This liquid, such as it had been provided to me, was not very viscous, and consequently contained water; I concentrated it by heating it in a bain-marie for several hours

2° In the open bottle. Duration of 12″ to 4′; as of the moment of formation, colors opposite, as for lactic and acetic acids in the same circumstances; but, after a time which varies from 4″ to 30″, one sees occurring a singular phenomenon: all the system of rings slides quickly down one side, in order to constitute only vertical half-rings, having their common center at the level of the liquid; at the same time the Newton's rings most distant from this center are tightened so that their system occupies less than half of the cap, of which all the remainder is then white of the first order, and things remain in this state until rupture.

Alcohol. 1° Process of shaking in a bottle. Maximum duration 1.3″; all the caps are colored, after a very short white phase; in those with horizontal rings, the colors are direct.

2° In the open bottle. Duration of a fraction of a second to 10″; after an extremely short colorless phase, sudden opposite colors, and not changing, as for the three preceding liquids under the same condition.

Benzine[147] *and dichloroethane*. 1° Process of shaking in a bottle. For each one of these two liquids, maximum duration 1″; after a longer white phase, and sometimes much, the colored phase, the majority of the caps offer rings, and the colors of the latter are almost always direct.

2° In the open bottle. All the caps are colorless; for benzine, they burst at the moment or almost at the moment of their formation, and, for dichloroethane, they have a maximum duration of 0.6″.

Chloroform and sulphuric ether. 1° Process of shaking in a bottle. Maximum duration 1″; almost all colorless, extremely rare ones regularly colored, and sometimes offering the direct arrangement, sometimes the opposite arrangement; white phase longer than the colored phase.

2° In the open bottle. All colorless; for chloroform, bursting at the moment of their formation; for ether, persistent to a maximum of 0.4″.

Carbon bisulphide. One never obtains colors, at least at ordinary temperatures. In the bottle, the maximum duration is 0.8″. in the open bottle, all the caps burst at the moment of their formation.

One will understand more definitely why, in spite of the absence of coloring, I put this liquid in the second category. It is seen, moreover, that it belongs to this one by its short persistence; in the opened bottle, it behaves like chloroform, and one will easily admit that, in the bottle, the absence of the colors is due to the short duration of the caps, which burst before the end of the small white phase.

§ 248. Let us pass to the third category.

Solution of soap of Marseille[148], in an atmosphere saturated with water vapor. The caps remain initially colorless during an interval of 6″ to 20″, then become covered with an extremely pale red and green rippled effect, which one sometimes sees being born at the top; this rippled effect darkens, and then one recognizes that it is made of three broad zones, in each one of which one of two colors float upward in the shape of tadpoles; those have an upward movement, and change color while passing from one zone to another; the dominant colors of the zones, always due to the tadpoles which go up, vary then, and indicate the direct arrangement. After a time of 3′ to 20′ starting from the formation of the cap, one sees appearing, at the top, a small black spot which is surrounded by white, grows very slowly and finishes, after an interval of half an hour to two hours, by invading all the cap; this persists then in this state, and its total duration

[147] It was very nearly pure benzine, prepared by Mr. Donny.

[148] This liquid was prepared by dissolving, at a moderated heat, one part of soap in 40 parts of distilled water, filtering the solution after cooling, and putting it through the filter until it became limpid; it should be employed the very same day of its preparation; as of the following day, it is already more or less degraded.

is several hours; caps persisted beyond twenty-four hours, with a characteristic about which we will speak further.

In the always-closed bottle, but without water at the bottom nor on the walls, the caps last only $4'$ to $5'$, and the black spot only reaches, at the maximum, 5mm diameter.

Solution of soft household soap[149], in an atmosphere saturated with water vapor. Colourless phase of $5''$ to $14''$; the later phenomena were followed only for one cap; it initially behaved appreciably like those of household soap, until it had become entirely black; but, by observing it one hour and half later, I noted with surprise that it was again colorless, with some yellow points, and offered only one very small black spot. It burst shortly afterwards, and had persisted beyond three hours.

Solution of rosin soap containing potash[150], in an atmosphere saturated with water vapor. The caps present an exceptional characteristic in this category: they do not have a colorless phase; as to their appearance, they are covered with red and green rings which, shortly afterwards, are transformed into a general rippled effect of the same colors; a little later, this rippled effect takes on other nuances, or gave way to a uniform yellowish green color dottedwith blue. On several caps, $10'$ to $30'$ after their formation, a black spot appears at the top, grows rather quickly, and invades the totality or almost the totality of the cap. Maximum duration one hour.

Solution of saponine[151], idem. colorless phase of $25'$ to $40'$, then appearance of a red and green general rippled effect, in which one sees sometimes, later, a little crimson and blue. Maximum duration twelve hours.

Albumine solution[152], idem. Colourless phase of several hours, then appearance of a red and green general rippled effect. The cap persists then in the same state, and its total duration can be several days.

Solution of iron peroxide acetate, idem. The majority of the caps express an odd phenomenon: after a colorless phase of $15''$ to $30''$, one sees being born, at the base of the cap, red and green rings which early are converted into a rippled effect of the same colors; this rippled effect extends gradually to more or less height, fades, and disappears to give place to a second colorless phase; this is followed, approximately

[149]It was prepared like that of household soap, with the difference that it contained one part of soap for 30 parts of water; I believed it necessary to make this proportion a little stronger, because of the great moisture of the soft soap.

This soap, of a much lower quality, being undoubtedly rather impure, the solution, although made perfectly limpid by filtration, is not long in tarnishing on its surface, where probably comes to gather some foreign substance; one thus needs, before making use of it, to remove the layers by means of a spoon, or to collect from them, with a siphon, the subjacent liquid.

[150]This soap not being, I think, in the trade, I prepared it by dissolving, hot, a fine rosin powder in a potash lye made of one part of solid potash and 20 parts of distilled water. To have a neutral liquid, I continued to add rosin until there remained a notable deposit at the bottom; by cooling, it precipitated a considerable quantity of nondissolved soap; I then added to the unit half of its volume of distilled water, I heated it again, then let the mixture sit until the following day, after which I elutriated it. This liquid gave, with the pipe, bubbles whose maximum diameter was 18 centimetres.

I must say that a new preparation, carried out with another rosin sample, gave me different results; the solution, instead of depositing, by cooling, nondissolved soap, became gelled, and the addition of water determined an abundant precipitate there; I made it disappear completely by dissolving in the liquid some small fragments of potash; but, with the solution thus obtained, the maximum diameter of the bubbles was only 12 centimetres. I was thus satisfied with the tests of caps carried out by employing the first solution

[151]Approximately one part of saponin in 100 parts of distilled water. I say approximately, because a circumstance prevented me from knowing the exact proportion. This solution gave, with the pipe, bubbles 12 centimetres in maximum diameter.

With other saponin samples, I had, to obtain these large bubbles, to employ a little less proportion of water. It is necessary to have care to bring the liquid, by filtrations, to a state of perfect limpidity; a cloudiness, even slight, considerably reduces the bubbles

[152]To prepare this solution, I simply beat fresh egg whites in snow, then waited until this snow had melted to liquid of sufficient quantity; finally I added to it a tenth of its volume of distilled water. This mixture gives, with the pipe, bubbles 13 centimetres in maximum diameter

half an hour after the formation of the cap, by a new red and green rippled effect which is shown everywhere at the same time, and which, when the cap persists long enough, takes then other colors. These caps can be maintained about twenty-four hours. For some others, there is only one colorless phase, but which can last one hour.

§ 249. There remain the two intermediate liquids mentioned at the end of § 245.

Solution of one part of gum arabic in 10 parts of water. This solution does not give bubbles with the pipe. I made, in an atmosphere saturated with water vapor, eleven caps, among which seven had durations from 20″ to 60″, and remained colorless until their rupture; on two others, which lasted 1′ approximately, there were, after a colorless phase of 20″, the appearance, at the top, of a slight red and green rippled effect remaining in the same state; but two persisted respectively sixteen hours and twenty-one hours, and were covered entirely, after long colorless phases, with a red and green rippled effect which, later, passed to the yellow, crimson and blue. Finally this liquid provides a rather abundant and extremely durable foam.

Among the caps, one sees, the majority behave like those of the first category, but some like those of the third; the abundance and the persistence of foam belong to the third category, and the non-formation of the bubbles to the first.

The so1ution in question constitutes one of the exceptional liquids to which I referred in § 242, as providing a voluminous and persistent foam, but not letting themselves inflate in bubbles.

Solution of rosin in olive oil[153], in the open bottle. After a very short colorless phase, all the caps express similar phenomena of coloring to those of pure olive oil (§ 247), only they are slower; the duration of the caps is very variable, and can reach 2.5′. These facts are of second category; but, with the opening of the pipe, one obtains bubbles of 3.5 centimetres at the maximum, which is a tendency towards the third.

With several other liquids, such as saturated solutions or suitably concentrated neutral borates of sodium and potassium, perchloride of iron, gold chloride, etc, one obtains small bubbles of 3 to 4 centimetres, and, if one examined these liquids from the point of view of their caps, one would find, undoubtedly, that they also belong to intermediate categories, or, at least, that they are extreme cases of one of the three principal categories.

§ 250. Although the experiments whose results are reported in the four preceding paragraphs were carried out on small-sized films, they revealed remarkable facts, such as some of the characteristics which distinguish our three categories, the great influence of the atmospheres in which the films are produced, etc, and several of these facts throw, one will see, a great light on the questions that I try to solve in this chapter.

Consider a cap at the time when it has just been developed, and let us seek what must occur there. We know that the liquid, pulled by gravity, goes down on all the sides around the top, from which results a progressive thinning of the film; but we will examine more closely how this thinning takes place.

To simplify, let us initially give our attention to one of the two faces of the film, the convex face, for example, and imagine it divided into horizontal molecular rings, from the top to the base. All these rings go down, and consequently each one of them is always increasing in diameter, which requires that its molecules deviate more and that other molecules, pertaining to the subjacent layer, come to place themselves in the interstices to restore a uniform arrangement. The same thing must apply to the concave face, and it is clear, moreover, that similar molecular movements occur even in the thickness of the film. It is obviously at the top and in its vicinity that the phenomena

[153] I did not measure the proportion of rosin; only it was noted that it was not to exceed a certain limit, undoubtedly because then the liquid is too viscous. The solution was prepared hot, then, after cooling, filtered through a sufficiently permeable paper

in question are most marked; it is there that the variations of the molecules are especially considerable and that, consequently, the attraction of the interior liquid is most abundant.

If thus the cause that I have just announced acted alone, thinning would always be fastest at the top and its surroundings, and the film would always have a decreasing thickness starting from the base. But, as I already pointed out (§ 109), these inequalities in thickness give birth to a second cause, which tends to erase them, or at least to decrease them; indeed, the thicker portions, being heavier, more easily overcome the resistance of friction opposed to their descent, and thus must consequently be most accelerated at the base; however, in consequence of this acceleration, the molecules are deviating more and more in the meridian direction, starting from the top, from which an attraction results from increasingly abundant interior liquid near the base, and the increase in this last attraction must compensate, in all or partly, the waning of that which comes from the first cause.

Finally, a last cause is added to the second: it is that, the more one approaches the base, the steeper the slope on which the liquid slides. If the first of the three causes prevails, the film will necessarily have a decreasing thickness from the base to the top; if it happens that this first cause is counterbalanced by both others, the thickness of the film will become uniform, and will also then continue to be reduced everywhere; finally if the last two causes predominate, the thickness is increasing from the base to the top; however, we saw these three cases being carried out.

Indeed, when one operated under suitable conditions, the first was shown, immediately or shortly after the development of the film, in caps of all the liquids of the second category, except, of course, those of carbon bisulphide; it was undoubtedly shown also in those of rosin soap, though one could not clearly deduce it from the colors of the rings; finally it was again shown, after the long white phase, in the household soap caps. The phenomenon probably occurs, in the other caps of the third category, during the white phase, and in all those of the first; but the absence of colors does not make it possible to be certain of it.

The second case followed the first in sweet almond oil, olive oil caps, and those of lactic acid formed within a desiccated atmosphere, since, while approaching rupture, the totality or almost the totality of their surface had first order white or the bluish gray which precedes the black. Some caps of spirits of turpentine offered a similar result; in those of rosin soap, a fine red and green rippled effect which followed the rings can be regarded as about equivalent with a uniform color, because the average thicknesses of the small portions of reds and green juxtaposed is undoubtedly the same on all the extent of the cap; and this must apply, with stronger reason, to the similar rippled effect which appears after the white phase in other caps of the third category and in some of those of the first, ripples which usually are born everywhere at the same time.

Finally, the third case is that of the caps of the second category on which the colors made, under certain conditions, the opposite arrangement.

§ 251. Let us dwell one moment on this third case. If one thinks about it a little, one will understand that the acceleration speed due to excess thickness of the lower portions of the film is at most able to erase this inequality, and that a waning thickness from the top to the base, waning which gives the inversion of the colors, can be attributed to the variations of the slope; that is supported, indeed, by a simple experiment:

In a glass tube of 1.5 centimetre diameter and 15 length, closed at one end, I introduced a small quantity of spirits of turpentine; then, holding this inclined tube at approximately 45°, I produced there, by suitable shakes, a transverse film, a film also consequently tilted, but which, being plane, presented the same slope top to bottom. The film thus placed having its lower face turned towards the liquid and its higher face towards the opening of the tube, was, with regard to evaporation, appreciably under the same conditions as the caps of the same liquid formed in the open bottle; however, while these last had clearly taken on opposite colors (§ 247), the plane film of the tube, in all the successive tests, showed direct colors: at first, these colors were, on the basis of the lower band, blue, indigo, purple, orange and yellow, and it occupied more half the height of the film, then one saw being born immediately, at the top, the white which extended quickly by driving back the other colors, and invaded almost the totality of the film.

§ 252. The caps of the second category are colored, I saw, immediately or after very little time, on all their surface, and their colors reach in one moment, either at the top, or at the base, the yellow or the white of the first order, and even sometimes a gray close to black; from which it should be concluded that the films of this second category are thinned with an extreme speed. For several liquids admittedly it is only in part of the caps which the attenuation is so thorough; but as, with regard to these liquids, the maximum duration hardly exceeds $1''$, one can admit that the caps which remain white are made of films accidentally thicker, and would soon be covered with all the colors, if they persisted a little longer time. That is, indeed, what we observed in the caps of benzine, dichloroethane, chloroform and ether, where the colorless phase approached $1''$ sometimes. I will reconsider this point later.

In the caps of the first category, there never was, we also saw, immediate coloring or almost such; the great majority remain white until their rupture, although, for some liquids, they can last beyond $2'$; for the rare caps where phenomena of coloring are observed, these phenomena are reduced, in general, to a tiny system of rings occupying the top and preserving its small dimensions; finally, in the very restricted number of cases where there is total coloring, it is shown only after several seconds, sometimes after two minutes. It results obviously from all the films of the first category thinning, on the contrary, very slowly.

In the caps of the third category, there is also, again we know, a generally long white phase, and the coloring which appears then never varies rapidly. It follows that, in the third category, as in the first, the thinning of the films is extremely slow. In truth, as an exception, the caps of the rosin soap solution do not have a colorless phase, and are initially covered with red and green rings; but they can persist one hour, while changing aspect gradually.

Is it necessary to attribute to viscosity, as one hears, this great difference in the speed of thinning of the films between the second category and the two others? By no means, because fatty oils and lactic acid, which belong to the second category, are liquids much more viscous than the majority of those of the first and third; finally, spirits of turpentine, also of the second category, are more viscous than the water, which is of the first.

Now, what characterizes a film is the considerable extent of surface area relative to volume; that forces us thus to recognize here an influence of the faces of the film, and to seek the cause of the great difference in question in a kind of viscosity belonging to the surface layers, independent, or nearly so, of the interior viscosity, and which, very weak in the liquids of the second category, is, on the contrary, very strong in those of the first and the third.

As I already said, an opinion of the same kind had been advanced a long time ago, and it has been held since by several physicists; I will later return to it (§§ 261 to 290), and one will see then what distinguishes my principle.

§ 253. Let us apply this claimed principle to the phenomena. Again let us take a cap at the time of its generation, again imagine its two faces divided into horizontal molecular rings which go down while widening, and consider in particular one of them at its departure from the top. It is clear that, for a small way, the distances between the molecules of this ring increase considerably: for example, from the position where its diameter is 0.01mm, to that where it is 0.1mm, these distances become multiplied by ten. One will easily admit, moreover, that the movements in question do not happen with a mathematical regularity, and that thus, in the same ring, the molecular intervals do not remain absolutely equal between them. That said, let us imagine that some cause puts an obstacle in the way of the free arrival of the subjacent molecules in the interstices; one or the other of those will become soon rather large so that the attraction of the molecules which it separates cannot counterbalance the tension any more; then these molecules will easily involve their more interior neighbors, which also spread, separation will deepen gradually, and the film will tear at this point. However, in the caps of the first category, the surface layers have, according to my principle, a very strong viscosity, the molecular movements are difficult there, and it is understood that since, very close to the surface of one or the other of the faces, an increased molecular interval can not have time to be filled before the tension, if it is rather energetic, there determines tearing. Such is, in my opinion, the explanation of the rupture of almost all the caps of the first category before one distinguishes on them any coloring.

But, it is also understood, this tearing cannot be complete; it can only cause a local attenuation, around which the molecules will take a regular arrangement; there will be then a system of small rings at the top. This system must thus appear abruptly, which is in conformity with experiment, and it must preserve its dimensions then appreciably, the cause which gave birth to it having completed its effect, which is also in conformity with experiment. But obviously nothing prevents the remainder of the film from continuing to be thinned by degrees, and, indeed, two of our distilled water caps which had the small system in question, showed, shortly before their rupture, the general rippled effect.

As for the final rupture in the presence of this small system, it comes from the thin portion of film which occupies the center of it thinning further and thus trying a tearing which, this time, is complete in consequence of the thinness; i.e. the film in question attenuates to such a point that it cannot resist the disturbances that come from the outside any more.

If these ideas are exact, any cause which will tend to impress irregular movements to the molecules of the faces of the film will have to support tearing; however, that is confirmed by a curious experiment: if one produces distilled water caps in an atmosphere saturated with alcohol vapor, vapor whose absorption by the external surface of the films necessarily causes disordered movements, all burst at the moment of their formation.

But why do the water caps never show the small system of rings when one develops them in a desiccated atmosphere? It is that, undoubtedly, when a tearing tends to occur with relatively little force only to give way to this small system, the external molecules whose spacing would have brought partial tearing, are removed by evaporation before the phenomenon could progress; thus in this case, tearings energetic enough to break the film are the only ones which will be effective.

Finally, how is it done that the water caps, which persisted as long a time in a desiccated atmosphere as in a wet atmosphere, never have, in the first, the general rippled effect, although evaporation apparently activated thinning? Let us try to explain this singularity. Each of the two surface layers having one of its faces open to the air, the molecules which occupy this face feel much less resistance in their movements than those more deeply located in the same layer; these molecules must consequently go down with less slowness, and communicate part of their small excess speed to the subjacent molecules; consequently evaporation, by removing without delay the molecules of the outside of the cap, prevents this speed communication, and thus delays the descent. If thus evaporation tends to accelerate thinning by withdrawing matter from the film, it tends at the same time to slow it down by slowing down the descent, and it is understood that the second effect can override the first. We will soon see this conjecture supported.

One sees now why it is impossible to inflate bubbles with liquids of the first category: it is that the film cannot extend under the action from the breath without the molecules of its two faces deviating continuously from attraction into their interstices of the more interior molecules, which gives rise to multiplied chances of tearing.

Often even the plane film that one draws with the opening of the pipe burst before I had time to start to blow. That was because this film was attached to the circumference of the opening by the intermediary of a small mass with extremely strong concave transverse curvature, and that it, under the terms of these strong curvatures, strongly attracted to it the liquid of the film (§ 219); however, there resulted, especially in the vicinity of the circumference, great molecular movements which, again because of the relative freedom of the molecules of the two extreme faces, cause, in these last, considerable variations in the attraction of the interior liquid.

Finally, it is for the same reason that the liquids in question never give abundant and persistent foam; each of the small films which together compose the foam being also attached to the surrounding small films via small masses with very strong concave curvature.

§ 254. In the films of the second category, tearings by the causes which I announced must be infinitely rarer; here, indeed, according to my principle, the molecular mobility of the surface layers is very large, and consequently there is little obstacle to the arrival of interior molecules into the enlarged interstices of the external ones. Also, we saw films of this category quickly reaching an extreme thinness, either in all the extent of the same cap, or especially at the top or the base. If the films break then, it is undoubtedly under the influence of the small vibrations propagated by the ground, and it is understood that the films of the various liquids must unequally resist this accidental cause of rupture; thus, while the sweet almond oil caps persist to a maximum of only 0.2″, those of lactic acid can last 18″, and those of spirits of turpentine 6″ in the bottle, and until 4′ in the open bottle.

This rapid attenuation teaches us why one does not manage either to inflate bubbles with the liquids in question: when one drew a plane film in the opening of the pipe, the suction operated by the small mass which runs along the circumference, and the descent of the liquid due to the opening not being held perfectly horizontal, makes this film almost instantaneously so thin that it often bursts by the small inevitable movements of the hand before one could carry the tube to the mouth; and when that does not happen, incipient extension of the film by blowing, and the descent of the liquid towards the low point, soon bring the same effect. These considerations also apply to foam.

However, one conceives that there can be liquids with very mobile surface layers, but such that their films, even extremely attenuated, still have enough cohesion to resist more or less the above causes of rupture; these liquids will be inflatable in bubbles a few centimetres in diameter, and it is that which rosin solution in the olive oil offered an example (§ 249) to us.

§ 255. The phenomenon of the inversion of the colors is also related to the slight viscosity of the surface layers, since it appears only with the liquids of the second category; and, indeed, so that thinning can take place more quickly in the bottom of a cap than in the top, it is needed that the lower portion at least of the two surface layers does not involve the higher portion; one needs that there is a kind of independence between these two portions, independence which obviously requires a great molecular mobility in the layers in question. It is understood, moreover, that the causes which can bring disorder in the surface molecules will support this independence by disturbing the connection of the various points of the same layer; thus the absorption of moisture in the caps of lactic acid, and evaporation in those of acetic acid, of spirits of turpentine and alcohol, determine the inversion of the colors.

I will point out here that these causes of disorder still cause the phenomenon of inversion at the time same as they are not very intense. For example, I produced caps of acetic acid and spirits of turpentine in the closed bottle while employing all the precautions indicated in § 244; for the spirits of turpentine, I had even introduced into the bottom of the tube wadding soaked with the same liquid, in order to saturate the air brought by my breath; in these circumstances, the interior atmosphere was very close to being saturated, so that evaporation was necessarily quite weak; however, the colors were clearly opposite. Under these same conditions, the least durable caps of acid acetic had only red and green at the base, which shows well that the development of the opposite colors comes from the bottom of the film starting by being thinned more quickly than the top.

But thinning initially so fast at the bottom should not be long in slowing down, due to the reduction in thickness of this portion of the film, and soon the loss which this same portion undergoes by the descent of its liquid and evaporation must be exactly compensated by the liquid which arrives from higher portions; from this moment, the lower rings must thus appear stationary in their colors and in their positions, which we actually noted, one saw, in the caps which persist long enough.

I tried (§ 253) to render comprehensible that evaporation could slow down thinning; moreover, that is what our experiments confirm on the caps with opposite colors; I said above, that the acetic acid and the spirits of turpentine still gave similar caps in an atmosphere to very close to saturated and when, consequently, evaporation was considerably reduced; however the respective maximum durations were then $0.4''$ and $2'$, while, in the opened bottle, i.e. with a free evaporation, the respective maximum durations rose with $2'$ and $4'$.

As for the side fall of the system of the rings in the caps of spirits of turpentine with opposite colors, one can, I think, explain it by assuming that there is exceeding thickness in the higher portion of the cap, with a second cap of less base poised on the first; this second cap is, indeed, in an unstable state of equilibrium, and small outside causes must make it slip down the side. Only it is singular that alcohol caps produced in the opened bottle, caps which, we know, have also the opposite colors and persist long enough, do not present the same phenomenon.

§ 256. Let me say here that Mr. Van der Mensbrugghe proposes to me an explanation of the inversion of the colors very different from that which I advanced in the preceding paragraphs. The experiment of § 172 shows that an excessively tiny variation in the temperature, and consequently in the tension of part of a liquid film, is enough to bring very notable changes in the distribution of the colors; however Mr. Van der Mensbrugghe supposes that, in the caps of the second category formed of volatile liquids within an unsaturated atmosphere, evaporation is, by an unspecified cause, a little more abundant at the top than at the base; thus, at the top of the cap, cooling is a little larger, and, consequently, a small increase in tension, from which an attraction results for the liquid of the lower part, and an increasing thickness upwards. One would thus clearly explain the maintenance of the colors in their respective positions, of the larger duration of the cap when the colors are opposite than when they are direct, of the non-inversion of the colors in an atmosphere saturated with the vapor of the liquid, and finally of the ascending currents from the base which were observed by Fusinieri (§ 324). Lactic acid, which is not volatile, admittedly also gave caps of opposite colors;

but this liquid absorbs moisture from the air, and if it is admitted that this absorption is somewhat more active at the top of the cap, one will understand that it must also result an increase from it from tension.

This theory is clever, but it should be supported by new experiments: it would be necessary to show the origin of the difference of evaporation or absorption at the top and the base; moreover, if one thinks that the inversion of the colors still appears in an atmosphere almost completely saturated, one will have difficulty conceiving that the insensible excess of tension which could be born in this case is enough for surmounting the action of gravity on the liquid of the cap; finally, after the side fall of rings in the spirits caps of turpentine, the excess of evaporation at the top should bring them back there; Fusinieri observed, in truth, similar returns, but they should have also occurred in my experiments.

In addition, according to my theory, liquid goes down continuously from the higher part of the cap to repair the loss that the lower part would undergo; if thus Fusinieri were not mistaken, if the ascending currents really take place, they constitute a serious objection against my explanation; finally this does not show how, in a cap which persists sometimes several minutes, evaporation does not end by thinning the film appreciably, and thus leaving the invariable colors opposite until the rupture. Later research will undoubtedly clear up these difficulties.

§ 257. A fact more obscure than the inversion of the colors is the spontaneous rupture, before the end of the white phase, of the great majority of the caps of chloroform and ether, and all those of carbon bisulphide. This phenomenon appears to depend not on evaporation itself, since it occurs in an atmosphere as saturated as possible within the bottle, but rather on the main tendency of the liquids to evaporate. Indeed, if one arranges all our volatile liquids of the second category according to the ascending order of their respective volatilities, one has the following series: 1° acetic acid and the spirits of turpentine; 2° alcohol; 3° benzine and dichloroethane; 4° chloroform; 5° ether; 6° carbon bisulphide[154]; however we saw that, in the bottle: 1° all caps of acetic acid, spirits of turpentine and alcohol were colored either without a white phase, or after a very short white phase; 2° all those of benzine and dichloroethane were colored in the same way, but after a white phase which approached 1″ sometimes and left then with the colored phase only a fleeting duration; 3° almost all those of chloroform and ether burst without colors; 4° all those of carbon bisulphide burst in this manner; from which one can infer that the propensity to be burst during the white phase grows with the propensity to evaporate.

The alcohol employed in these experiments was commercial alcohol; I wanted to know what pure alcohol would give, which is placed, as for its volatility, between the precedent and the pair benzine and dichloroethane; however in the bottle, much of its caps burst colorless; it is thus, under this point of view, between the pair above and the pair chloroform and ether, and thus constitutes a slight anomaly; but I do not think that this one is enough to prevent me from claiming the influence of volatility generally.

Now how can the simple tendency to volitilize cause the rupture? Isn't it allowed to believe only if, in a free atmosphere, the liquids in question easily lose such an amount of their surface molecules by evaporation, it is because these molecules have very little coherence between them? On this assumption, one understands that one needs very little to bring about a tearing in spite of the mobility of the surface layers; then also a cause of disorder in the external molecules, evaporation, for example, will support this tearing, and we saw, indeed, that, in the opened bottle, the caps of benzine,

[154] To compare from this point of view the liquids in question, I filled exactly a series of identical watch glasses placed apart from each other on the ledge of a window facing North, at a temperature of 18°, and I observed them from time to time in order to note their respective reductions.

dichloroethane, chloroform, ether and carbon bisulphide burst at the moment of their formation, or hardly persist beyond a half-second.

Finally, I said to myself that if volatility really exerted an influence so marked on the caps of these liquids, the phenomena which they present should change if one decreased volatility by a great fall in the temperature. However, that is what the experiment confirmed: bottles respectively containing chloroform, ether and carbon bisulphide were exposed during two hours outside, at a temperature of -4°, then I made there, always outside, the test of the caps. Under these conditions, the durations increased only a little, but the colored ether and chloroform caps were much more frequent, and carbon bisulphide gave a rather great number of highly tinted caps; finally the white phases preceding the colors were much shortened. This last fact appears to indicate that, even in the bottle where one took all the precautions to saturate the interior atmosphere, saturation is not absolutely complete, so that the caps always undergo a tiny evaporation; consequently, indeed, it is understood that a very low temperature, still reducing this small remainder of evaporation, accelerates a little thinning, and, consequently, shortens the white phase.

One now sees that carbon bisulphide, which, at ordinary temperatures, never expresses coloring, should, however, be placed, as I did, in the second category. Let us add that if it gives with such an amount of difficulty the colored films, that must hold, partly, with the high value of its index of refraction; indeed, according to the known law, it follows from this high value that the appearance of the colors requires, in the carbon bisulphide films, more thinness than in those of all the others liquidate.

§ 258. Let us come finally to the third category, i.e. the most significant, that of liquids which can be inflated in bubbles. Here, as in the first category, the surface layers have little molecular mobility, and thinning is carried out with slowness; but tearings are rare, since, in spite of the descent of the liquid and the action of the breath, the films persist and can reach a great extension. If one admits the ideas presented in § 253, one will conclude that, in liquids of the current category, the tension is insufficient to produce tearings, and that is what comes to support the comparison of the respective tensions of the water and our household soap solution: the tension of a water film at rdinary temperature, is, according to Dupre, 14.6, and according to the same scientist, who agreed to determine it for me, that of a film of soap solution is only 5.64, i.e. between half and one the third of the preceding one.

However, so that a liquid can extend in bubbles, it is not essential that its tension is weak in an absolute way; it is enough that it is weak relative to the viscosity of the surface layers, or, in other words, that the ratio between surface viscosity and the tension is rather large. For example, the respective tensions of the films of the solution saturated with calcium chloride and of the albumin solution, tensions measured by Mr. Van der Mensbrugghe[155], being 11.06 and 11.42, i.e. about equal and both rather

[155]The majority of the tensions of which we will have to make use were evaluated by Mr. Van der Mensbrugghe, by means of two different processes: the first was with that of the hydrometer of Dupre (§ 161); the second, which is due to Mr. Van der Mensbrugghe, has this advantage which it makes it possible to operate on an extremely small quantity of liquid; here of what it primarily consists of:

A fine cotton thread is stretched horizontally between two distant fixed points of approximately 12 centimetres apart. In addition, a glass tube of one decimetre length and about 1mm of external diameter, is furnished, close to each of its ends, with a small ring of thin wire, and supports, by a cotton yarn attached in its middle, a small paper plate. To measure a tension, one first wets the horizontal wire with the liquid to be tested, then one transports the tube under it, so as to touch it by the two small rings; between this tube and the horizontal wire runs thus a narrow space, which one fills with the same liquid with a brush; after which one lets go of the tube, which remains suspended by the tension of the two faces of the small liquid mass. One then gently pours fine sand on the small plate, until the tube is detached. Finally one weighs the whole of the tube, the plate and sand, and one divides the weight, expressed in milligrams, by the length between the two small rings; the quotient is the value, in milligrams, of the tension, per millimetre, of a film of the strong, and yet the first of these liquids does not give bubbles, and, with the second, one obtains some that reach 13 centimetres in diameter; but like, in the calcium chloride caps which were rippled (§ 246), the colorless phase was only, at the maximum, 150″, and which, in those of albumin (§ 248), it was several hours, it is seen that the surface viscosity of this last liquid must be looked like much the higher than that of the first, and that thus the ratio between this viscosity and the tension are as much larger with regard to the second liquid as with regard to the first.

If one compares in the same way, from the point of view of their tensions and viscosities of their surface layers, the soap solution and that of albumin, the values above show that the tension of the films of the second is double that of the films of the first; but, in the soap caps, the colorless phase is only, at the maximum, 20″, while, in those of albumin, it is, as I have pointed out, several hours; thus, while passing from the first liquid to the second, the tension and the viscosity of the surface layers both increase considerably, so that their ratio remains sufficiently large.

Tearings require relative movements of the molecules, and the intrinsic viscosity of the surface layers, viscosity which makes these movements difficult, also obstructs those which lead to tearings as well as those which bring interior molecules in the increased interstices of the external ones. Thus, while passing from soap to albumin, the tension, i.e. the force which tends to tear the films, becomes double, but the resistance to this tearing increases at the same time by the increase in the viscosity of the surface layers, and the albumin films extend in bubbles like those of soap, only to a lesser degree.

§ 259. Such is the theory which I propose: so that a liquid can develop films at the same time large and persistent, and consequently can inflate in bubbles, it is first necessary that the intrinsic viscosity of the surface layers of its films is strong, so that thinning takes place with slowness; but it is necessary, moreover, that its tension is relatively weak, so that it cannot overcome the resistance opposed to tearing by viscosity when, in the surface movements, the molecules spread in area. The liquids which have at the same time a strong surface viscosity and a relatively strong tension, do not give bubbles, because, for them, the tension is always able to overcome the resistance in question. Finally, liquids which only have a low surface viscosity do not give bubbles, because their films reach in very little time an extreme thinness, and then they break due to the small shocks come from outside, or by other foreign causes.

Only, I have to present a remark here. Consider two liquids of which one has a surface viscosity less energetic than the other. If one stuck simply to the principles above, one should claim that the sufficient tension to operate a tearing is necessarily weaker according to the same proportion in the first of these liquids as in the second, or, in other words, than with equal chance of tearing, the ratio of the two elements, surface viscosity and tension, is the same in the two liquids; but it should be paid attention that, when a surface interval is increased too much, the subjacent molecules come to fill it with less difficulty in first liquid than in the second, so that tearing requires relatively more tension to be achieved. We arrive thus at this consequence: that with equality of chances of tearing, or, which amounts to same thing, with equality of maximum diameter of the bubbles when the liquids are of the third category, the ratio of the two elements is less with regard to the liquid whose surface viscosity is less strong. And from there arises obviously a second consequence, that if the ratio of the two elements is the same for the two liquids, the chances of tearing become less for that which has least surface viscosity, so that it must give bubbles larger than the other, or than, while

liquid.

Mr. Van der Mensbrugghe measured several tensions by both processes successively, and always the results were appreciably in agreement.

it gives some, the other does not give any. We will further see (§ 297) the results from the experiments agreeing with these deductions.

Finally, a last consequence, that we already know and that we know to be verified by the facts, is that, in the second category, where surface viscosity is extremely low, the chances of tearing are, in general, so to speak null whatever the tension, so that the films arrive freely at an excessive thinness; also many of liquids of this category would form large bubbles, if the speed of thinning were not an obstacle. Only, for the most volatile, a property intervenes which brings back the chances of tearing, and which appears to consist (§ 257) of a defect of cohesion in the surface layers.

Our theory explains, one could be convinced, in a satisfactory way all the phenomena observed in the experiments previously described; up to now however it is still too hypothetical, but we will see new facts grouping around to give it, I hope, solid support.

§ 260. Before explaining these new facts, I must, to complete what relates to our small caps, mention an extremely curious phenomenon which those of household soap solution presented to me. As I said (§ 248), these caps become entirely black after two hours at the maximum, and persist then, in this state, sometimes beyond twenty-four hours; however, in these so-persistent caps, I noted with surprised a progressive and continuous reduction in the diameter, so that they end up vanishing completely; during this gradual reduction, the film remains always black. It should be concluded that the descent of the liquid is unceasingly compensated by the tightening of the cap; it is what explains its long duration.

One of the principal arguments by which one sought to prove the impossibility of the vesicular state in visible water vapor, consists of what the air imprisoned in the interior of so tiny a vesicle would be subjected, on behalf of the film, to a considerable pressure, (§ 118), and, consequently, would pass gradually through this film, so that the vesicle would be reduced soon to a filled droplet; however, it is seen, my household soap black caps carry out this gradual passage of the interior air through the liquid envelope. In truth, if visible water vapor were in the vesicular state, the envelopes would obviously not be black, and would have consequently a thickness much larger than the films which constitute the caps in question; but, in addition, the pressure on the interior air of the water vesicles would be more than a thousand times as strong as in our recently formed caps.

§ 261. I now pass to the above newly announced facts. I initially sought to establish, by direct experiments, the existence of an intrinsic viscosity of the surface layers, and the differences which it presents from one liquid to another. Here is the mode of experimentation that I adopted, and which succeeded perfectly.

In the center of a cylindrical glass cup approximately 11 centimetres in interior diameter and 6 of depth is fixed a pivot 2 centimetres high, carrying a magnetized needle; this, in the form of a very lengthened rhombus, as usual, is 10 centimetres long, 7mm wide in its middle, and about 0.3mnm thick; the duration of each one of its small oscillations under only the influence of the magnetism of the ground is roughly $1.7''$. The cup is provided with adjustment screws so that one can make the pivot vertical, and all the system is placed on a table in front of a window exposed to the North. A small brass wire bracket, being used as a locater, grips the edge of the cup at the point located in the magnetic meridian line, on the South side; another similar bracket is on the East side 90° from the first, and there is a third between them 5° from the first, so that of this third bracket to the East bracket, the angular distance is 85°. Finally, a graduated paper band is stuck on the external wall of the cup, starting from the South bracket and going towards the West.

All being in order, when one wants to carry out an experiment, one pours in the cup the liquid to test, until it just reaches the level of the lower face of the needle;

one assures oneself besides, by looking through the wall of the cup, that the face in question is, as exactly as possible, in the prolongation of the surface of the liquid, and that small bubbles of air are not adhering there. That done, one brings, by means of a bar magnet, the point of the needle which was directed towards the South, exactly opposite the East bracket, and one maintains it there by settng the bar on an external support, at the height of the needle and close to the cup; one waits a few moments so that the surface of the liquid becomes again motionless, then one removes the bar abruptly, by withdrawing it in the lengthwise direction of the needle, and one counts the time which the latter employs to reach the following bracket, i.e. to traverse an angle of 85°; finally one notes the angle which it describes, continuing its motion, beyond the magnetic meridian line, an angle that one measured using divisions of the paper band. One counts time only until 85° from the starting point, and not until the magnetic meridian line, ' because with some viscous liquids, the needle slows down so much while approaching this last point, that the moment when it reaches it cannot be specified.

One then adds more of the same liquid up to approximately two centimetres above the needle, then, seizing the latter with a brass grip, one turns it over in the interior of the liquid, one makes leave the cover the bubble of air which is committed there, by absorbing it with a pipette, one replaces the needle on the pivot, and one carries out the determinations of duration and angle like above.

In general, when the needle, either on the surface, or in the interior of the liquid, passes the magnetic meridian line, it is limited to returning there slowly and then to stop there.

In these experiments, my son carried out the operation of the bar, and observed the needle; it pronounced a first *tick* at the moment when it removed the bar, and a second *tick* at the moment when the point of the needle passed in front of the following reference mark. For my part, holding close to my ear a watch which beat the fifths of second, I could estimate the time of the course within less than one tenth of a second. Each observation was in general repeated ten times, and I took the average of the results, which were always very concordant. Let us add that when they were volatile liquids or absorbents, one covered the cup with a bell-jar, through which one observed, and which, in the case of the aqueous solutions, one applied, inside this bell, the pieces of filter paper soaked with water and placed so as not to prevent the sight of the course of the point considered of the needle; in this case also, the cup was set on a plate in which one poured a little water.

§ 262. Let us start with the results relating to distilled water; they were obtained at a temperature of 18° to 19°.

On the surface of this liquid, the duration of the course of the angle of 85° was found, las the average of ten observations, equal to 4″, 59; the smallest and largest of the partial values were respectively 4″, 5 and 4″, 7.

In the interior of the liquid, ten observations also gave, on average, for the duration of the same course, 2″, 37; the extreme partial values were 2″, 3 and 2″, 5.

Thus, although, on the surface, only one of the faces of the needle rubs against the water, while inside the two faces rub simultaneously and that, consequently, the needle seems to have to meet a double resistance, however it goes close to twice less quickly on surface than inside. One must thus conclude that the surface of the water opposes a particular resistance, that it is necessary well to attribute to an intrinsic viscosity to the surface layer of this liquid.

In truth, in the interior, the whole of the needle and its cover loses a small part of its weight, and consequently presses a little less on the point of the pivot; but, in addition, the cover rubs by all its surface against the water, and, moreover, the section of the needle, which has, as I said, 0.3mm height, pushes the liquid directly, from which are born resistances more than sufficient to e compensate for the slight reduction in friction on the point.

And that is not all. On teh surface, the needle, continuing its motion, described, on average, beyond the magnetic meridian line, an angle of almost 8°, while inside, in spite of its higher speed, it exceeded the magnetic meridian line only 3° 1/2. These seemingly contradictory facts initially much astonished me; but I quickly had the explanation of it, and it provided me with a new proof of the strong surface viscosity of water: I started again the experiment on the surface, but after having powdered it with a light cloud of lycopodium; then it was recognized that thet whole surface turned at the same time as the needle[156], only with lesser speed; it is thus the surface layer which, while thus turning, so propels the needle far beyond the magnetic meridian line, and consequently it is very clear that inside the liquid, where this action does not exist, the needle reaches only a much smaller distance.

I said that the surface turns less quickly than the needle; it has to overcome, on all its extent, friction against the subjacent liquid. By observing the lycopodium, one notes, along the leading edge of each half of the needle, a current from the pivot to the point; and, indeed, the needle not being able to slip on the surface layer because of its resistance, and having enough force to move in spite of this obstacle, it is necessary that the parts of the layer in question on which it acts immediately are deviated and form the currents about which I spoke.

Finally, I reflected that by blocking the rotation of the surface layer, one would increase resistance to the movement of the needle, and that, consequently, one would slow down this last. I thus built two small rectangular glass partitions, suitable to be installed in the cup following of the directions going of the wall of this one towards the axis. Both these partitions have 40mm length, 12mm height and 2mm thickness; each one of them is fixed by a brass wire bent in the shape of a bracket, which grips the edge of the cup, but which rises above this edge, so that one can easily more or less lower the partition in the liquid. The two partitions were placed opposite one other, the first in the Southern part of the cup, at 42° approximately to the West of the magnetic meridian line, and the second, consequently, in the Northern part, at 42° to the East of this same meridian line; finally, so that they could not exert any capillary action on the needle, one inserted them until their upper surfaces were level with that of water. Under these conditions, the average duration of the course of 85°, on the surface, amounted to 6″, 44; then also the needle did not exceed any more the magnetic meridian line.

These experiments leave, one sees, no doubt about the existence, in the surface layer of water, of intrinsic viscosity, much higher than viscosity of the interior of same liquid, and if one considers that the thickness of the surface layer of a liquid is equal to the sensible radius of activity of the molecular attraction and consequently of an excessive smallness, one will have to conclude from the facts above that the intrinsic viscosity of the surface layer of water is extremely large. Let us notice here that this so thin layer must involve in its rotation the liquid under unclaimed until a certain depth, so that the total mass which turns much exceeds that of the layer in question; that is what explains how this mass can acquire enough speed to carry the needle beyond the magnetic meridian line. Let us add that, in consequence of its excessive thinness, the surface layer could probably only oppose, by itself, a low resistance to the movement of the needle, and that most of the resistance observed must be due to the drive of the mass in question.

§ 263. I tested glycerin of Price then. Here, because of the strong interior viscosity,

[156]One will see (§ 286) that the movement of all the surface of water is produced by that of a body placed on this surface, had already been noted

the friction either of one of the faces, or of the two faces of the needle against the liquid, was to produce considerable resistance, and the durations were consequently much larger than with regard to water. I must add that the experiments were made in January, at a temperature of 15°, and that at this not very high temperature, the glycerin is much more viscous than in summer. I made only two observations on the surface, and two inside.

In the first case[157], the values of the duration were 36′ and 35′ 30″; in the second, they were both 19′ 30″.

Thus, for glycerin, as for water, the speed of the course of the angle of 85° is much larger in the interior than on the surface, which shows in the same way the existence, in the surface layer, of an energetic intrinsic viscosity.

As for the angle beyond the magnetic meridian line, it is null, on surface as well as inside, in consequence of the low speeds of the needle; it simply reached the meridian line, approaching which its progress became extremely slow.

To assure myelf that the surface layer turned with the needle, I did not employ lycopodium, of which it had been difficult to disencumber the glycerin then; I initially brought back the needle to its starting point, then, while it was kept there, I deposited on the surface of the liquid, in the magnetic meridian line and approximately 14mm from the wall of the cup, a small fragment of gold sheet; then, after having covered the apparatus with the bell, I freed the needle, and observed the gold sheett. Hardly had the needle traversed 1° or 2°, when I saw the gold sheet being put moving, as if it were pushed back; after the 85° course of the needle, this same gold sheet had described, towards the West, an arc of approximately 30°. The surface layer of glycerin, thus turns, like that of water, at the same time as the needle, and also with less speed.

§ 264. With the saturated solution of sodium carbonate, at a temperature of 17°, the durations, each obtained by the average of eight very concordant observations, were: on the surface 8.04″, and inside 4.59″. On the surface, the needle passed the magnetic meridian line by approximately 6°, and, inside, it simply reached it. For the test of the rotation of the surface layer, there was recourse, like above, to the gold sheet; this started to be driven at the same time as the needle, and described an arc of approximately 30°. The conclusion is thus again the same.

I will point out that the duration 4.59″ of the course of the 85° inside this solution, is precisely equal to that which we above found for the same course on the surface of distilled water; however, as I said, inside our solution the needle stops at the magnetic meridian line; it is thus a new proof that, on the surface of water, the needle goes beyond this meridian line only because it is pulled by the movement of the surface layer.

§ 265. With the solution saturated with potassium nitrate, I obtained, by the average of ten observations, at a temperature of 19°: on the surface, 4.41″ duration, angle beyond the magnetic meridian line 5° 1/2, lasted inside 2.38″, angle 3°; thus the same conclusion; I considered it useless to carry out the test of the gold sheet.

§ 266. By submitting to the same testing the solution saturated with calcium chloride, I saw the duration, on surface, gradually increase: it rose, in six observations, from 15″ to 21″. Suspecting that this result could come from a weak chemical action exerted on the needle, action giving rise to an iron compound which, swept on the surface by the needle, increases surface viscosity, I coated the needle with a varnish of shellac, as well as the pivot until close to the point, then I started again. Then, indeed, the increase was shown more, and I had, on average, at a temperature of approximately 19° on surface, duration 14.85″, angle 2.5°, lasted inside 8.52″, angle 0°; thus, same conclusion again.

[157]To avoid as much as possible the absorption of the moisture of the air, I had coated the interior of the bell with glycerin, except the portion through which one was to observe

I believed to be able to limit myself, with regard to the first category, to the five preceding liquids; moreover sulphuric, nitric and tartaric acids, as well as ammonia, would have strongly acted on the needle or the layer of varnish with which I would have covered it. I now pass to the second category.

§ 267. Let us look, initially, at that which relates to alcohol. The experiments[158] with this liquid, for the average duration of the course of the 85°, on surface, gave 1.48″, and, for that of the same course inside, 3.30″. Here thus, contrary to the preceding liquids, it is on surface that the duration is smaller. The angle described beyond the magnetic meridian line was, on average, on the surface, 21°1/2, and, inside, 3°1/2. The partitions did produced absolutely no effect; finally, the small floating body [159] is remained motionless until the needle came to run up against it.

It follows obviously from these results that, in alcohol, the viscosity of the surface layer does not at all exceed that of the interior of the liquid, and we will have to decide whether it is lower or not. It also follows from same results that if the needle, on the surface, is transported beyond the magnetic meridian line, it is due to its own acquired speed. The experiments above were carried out at a temperature of 17° to 18°.

The day when the observations of § 262 were made on distilled water, I carried out, immediately after, a new determination of the duration and angle inside alcohol, in order to be able, to compare these elements with those of water in identical circumstances; I found the duration thus equal to 2.66″, and the angle equal to 2°1/2; the duration, one sees, is a little larger and the angle a little smaller than with regard to water under the same conditions; the interior resistance of alcohol to the movement of the needle thus appears to be a little higher than that of water; it is known besides that alcohol runs out less quickly than water through a narrow tube; but that undoubtedly holds because alcohol adheres to solids more strongly than water. As for the differences between the values above relating to the first of these liquids and those previously obtained, I will return there later.

§ 268. Here are results with spirits of turpentine, obtained the same day as with alcohol: average duration on surface, 1.40″; inside, 3.43″; average angle beyond the magnetic meridian line, on surface, 22°1/2; inside, 1°. As with alcohol, the tuft waits, without leaving its place, until the needle comes to run up against it; according to this last result, I considered it needless to make use of the partitions. The viscosity of the surface layer of spirits of turpentine thus does not exceed that of the interior.

The comparison between the values above and those which relate to alcohol under the same conditions, lead us to a significant consequence: the 3.43″ duration and the angle 1° inside the spirits, are one a little larger and the other much smaller than the 3.30″ duration, and the angle 3°1/2 inside alcohol; it thus appears that interior viscosity is more energetic in the spirits. Now we recall that, on the surface, the needle must overcome, by its face in contact with the liquid, its interior viscosity; consequently if the viscosity of the surface layer were, in each of the two liquids, simply equal to interior viscosity, the needle should show a larger resistance on the surface of the spirits as with that of alcohol; however, it was seen, it is not thus on the surface, the duration was a little less and the angle a little larger for the spirits than for alcohol; it consequently seems necessary to admit, in the surface layer of the spirits, a particular mobility which decreases resistance, and we arrive thus at this probable deduction that,

[158] If one restricts oneself to placing the bell on the cup the level of the liquid drops appreciably, in spite of this precaution, during the tests, because of the volatility of alcohol, and that influences the angles especially. In order to remove this problem, I covered the interior of the bell with filter paper soaked with alcohol, while leaving uncovered the portion necessary to allow observations; the whole was set on a plate in which I poured a little alcohol.

[159] It was not a gold sheet: these sheets deposited on alcohol and some others liquids such as spirits of turpentine, ether, etc, go down invariably to the bottom; I substituted a fragment of seed tuft there.

among the liquids of the second category, the spirits of turpentine at least has, in its surface layer, a viscosity lower than in its interior. To shorten the language, I will express the fact by saying that the surface layer of this liquid has a *negative excess* of viscosity. It is, however, a point which I will soon reconsider.

§ 269. In olive oil, one could believe, with the first outline, that one finds a weak positive excess. Indeed, the results with this liquid, at a temperature of 15°, were, on average: duration on surface, 30.30″, and, inside, 79.54″; in both cases, the needle reaches simply the magnetic meridian line; but, in the presence of the partitions, the duration on the surface was 31.42″, i.e. somewhat higher than that obtained without their employment; finally the gold sheet was put moving, but only after one course of the needle of more than 30°, and it had moved only 4° away from the magnetic meridian line at the end of the course of the 85°.

However, the small positive excess that these experiments seem to reveal is not real: I added a little oil in the cup so that the needle was immersed in the liquid, but only to 1mm above surface, and I redid, under these conditions, the test of the gold sheet. I saw it then moving as soon as the needle started to be driven; only it went with much more slowness; it stopped at the same time as the needle when the latter reached the magnetic meridian line, and it had traversed only 20°. The height of oil above the needle having been successively increased, the effect decreased, but, even for a two centimetres height, it was still very notable: the gold sheet started when the needle had described approximately 30°, and it moved 9°.

It follows from these facts that, in the case of a very viscous liquid such as oil, the needle involves with it a considerable mass which pushes the liquid in front of it, and this action is felt immediately at a long distance in front of the needle. If the effect is less marked when the needle is simply on the surface, it is that then only one of its faces acts to involve and push the liquid. It is thus seen, the intrinsic viscosity of the surface layer of oil has nothing to do with the movement of the gold sheet and the small delay brought by the partitions, and the positive excess of this liquid is only one appearance due to the effects of interior viscosity[160]. Also, the speed of the thinning of the caps (§ 247) must make one suppose that oil has, on the contrary, a negative excess.

In the not very viscous liquids, such as water, alcohol, etc, the needle must also communicate a certain movement to the adjacent portions, and it was of interest to know what the same experiments would give, with these liquids. Distilled water was initially tested; however, when the needle was immersed 1mm, it impressed movement on the gold sheet only at the moment when it passed below; but while it returned slowly to the magnetic meridian line after having gone approximately 2° beyond that, the gold sheet continued to go, and traversed 30°. Any effect ceases when there is above the needle a height of water of 7mm. In alcohol, the results were similar; only, at 1mm below surface, the needle went far beyond the magnetic meridian line, the small floating body accompanied it until the end of its movement, then went still a little further; moreover, so that it did not have an action any more, one needed a height of alcohol of 9mm. Thus, with the not very viscous liquids, either they have, like water, a positive excess, or, like alcohol, they do not have any, the mass pulled by the needle does not exert any significant impulse ahead, so that it influences by no means the deductions drawn from the tests of the gold sheet and the partitions.

These last facts suggested me the idea of an experiment suitable to completely put beyond doubt the absence of positive excess in the surface layer of oil: I told myself that if one covered water in the cup with a thin oil film on the upper surface

[160]The same drive and similar push must have taken place with regard to the glycerin (§ 263); but, with this last liquid, positive excess was clearly shown by the circumstance that the duration on the surface was much larger than in the interior, and by the considerable movement of the gold sheet.

in which the needle would carry out its movement, the effects described above of the interior viscosity of the oil could not occur, and, consequently, if oil does not have positive excess, the gold sheet would remain motionless. However, the experiment fully confirmed this forecast; only, to my great surprise, I found that if the oil film is very thin, the positive excess of water is felt: the gold sheet then starts at the same time as the needle, and describes a large angle. The thickness of oil for which the gold sheet does not move any more, is approximately 1mm.

§ 270. For the sulfuric ether[161], I found, at a temperature of 16°: on surface, 1.12″ duration, angle beyond the magnetic meridian line 47°; duration inside 1.49″, angle 12°. The experiment with the tuft presented difficulties, because this small body, before one releases the needle, wanders constantly on the surface of the liquid; however, by releasing the needle as soon as possible after having placed the tuft, one could note by several tests that this one was simply run up against. The surface layer of ether thus does not have either any positive excess.

As for the seemingly spontaneous movements of the tuft, they come, without any doubt, from the evaporation of the liquid, although one places, for each test, the bell on the apparatus.

§ 271. Carbon bisulphide[162] provided, at a temperature of 16°: on the surface, 1.20″ duration, angle beyond the magnetic meridian line 36°; duration inside 2.0″, angle 8°. With the tuft, the same difficulties and same result as for ether; same conclusion consequently, an absence of positive excess.

In the Report summarized in § 167, Mr. Van der Mensbrugghe attributes the seemingly spontaneous movements of floating pieces to the evaporation of the liquid is not carried out perfectly uniformly in all directions around the piece, which results from small inequalities of temperature, and, consequently, of tension. In support of this explanation, he quotes, inter alia, the following fact: if one covers with a glass plate half of a cup containing carbon bisulphide on which float solid particles, one sees those of the sheltered portion to wander towards the discovered portion, where evaporation is free.

§ 272. Before reporting the results of the same tests on the liquids of the third category, let us return to the question of negative excess. The facts which led me to claim this property may appear insufficient; but the idea came to me from a simple means, suitable to highlight it with regard to alcohol, if it existed in this liquid: alcohol, indeed, mixes in all proportions with water, which has, it was seen (§ 262), a great positive excess; if thus one carefully mixes a suitable quantity of alcohol with water, and if indeed alcohol presents a negative excess, it will have to destroy the positive excess of water. However, that is what experiment verifies fully: one prepares a mixture with equal volumes of water and alcohol, and one carries out, on this mixture, the test of the gold sheet; one notes that it is simply run up against by the needle.

The proportion of alcohol which is enough to produce the simple neutralization of the positive excess of water, is lower than that which I have just indicated; but, with the latter, the experiment is easier, while with smaller proportions, it presents difficulties resulting from the alcohol loss by evaporation at the surface of the mixture.

To carry out the experiment above, I left the cup uncovered; in this manner, if the mixture contained too much alcohol, the layer losing it by evaporation would arrive

[161] Here, still more than with alcohol, the volatility of the liquid tends to produce a lowering of the level. The means used with regard to alcohol (see the first note of § 267) would have exposed the observer to breathing too much ether vapor; I thus proceeded in the following way: I put a little too much liquid in the cup, and I simply covered it with the bell, then, during the series of tests, I removed the bell from time to time to observe the positioning of the needle, and, among the partial results obtained, I preserved only those which corresponded to a regular position.

[162] Same procedure as for ether.

gradually at the neutral point; then, evaporation continuing, the positive excess of water would start to reappear. However, that is what happened; I measured the angle initially described by the needle beyond the magnetic meridian line; it was 14°, and the gold sheet tested immediately afterwards, it was simply run up. A few minutes later, the angle was only 12°, later still 10°, and the gold sheet was always simply run up against it. For the angle of 9° 1/2, the gold sheet was pushed ahead when the needle was with approximately a degree; finally when the angle was reduced to 5°, the distance from the needle to the gold sheet at the time when this one started to move, was four degrees. Throughout these test, the evaporation of alcohol caused the level of the liquid to drop little by little; but it was restored from time to time by introducing, by means of a pipette, at a certain depth below surface, a suitable quantity of the same mixture. The temperature was 18°.

One should not conclude from this experiment that the angle which corresponds to the neutral point is approximately 10°: the needle, while traversing its way, more or less mixes the top layer with the subjacent layers, and this results in a disturbing cause of which one cannot evaluate the influence.

It thus should necessarily be recognized that alcohol and, with stronger reason, spirits of turpentine, have a negative excess, i.e., in each one of these liquids, the viscosity of the surface layer is less than interior viscosity. It is seen, moreover, that negative excesses in question are considerable.

§ 273. Finally, a very different means enabled me not only to again note the existence of negative excesses, but to even determine roughly the relative values of these excesses for several liquids. It is known that the oscillations of the magnetized needle are controlled by the same law as those of the pendulum; the formulas concerning the movement of this last in a resistant medium, thus apply also to the movement of our needle on or in a liquid. If it is admitted that the resistance of the medium is proportional to the square the speed of the pendulum, the differential equation of the movement of it can, one knows, to be integrated one time, and this integral is:

$$\left(\frac{d\theta}{dt}\right)^2 = amCe^{am\theta} + \frac{2g\cos\theta}{a(1+a^2m^2)} + \frac{4gm\sin\theta}{1+a^2m^2}\cdots \tag{6}$$

in which θ is the variable angle that the pendulum makes with the vertical, a the length of the corresponding simple pendulum, m the resistance for the unit speed, g gravity, and C the arbitrary constant.

To apply it to our needle, let us take for the origin of the angles not the home position, i.e. the magnetic meridian line, but the starting point of the needle, i.e. the position with 90° of this meridian line, and indicate by ω the variable angle; there is thus $\theta = 90° - \omega$; let us replace moreover am by the only letter k; this one will then represent a quantity proportional to resistance; let us determine the arbitrary constant C by this condition that, for $\omega = 0$, speed is null; finally let us consider ω as representing the total angle described by the needle up to the point which it reaches beyond the magnetic meridian line, for which the speed is also null. With these conventions, the integral above becomes simply:

$$\sin\omega + k\cos\omega - ke^{-k\omega} = 0 \tag{7}$$

When the experiments are made known, with regard to a liquid, the angle described by the needle beyond the magnetic meridian line on surface or in the interior, it is enough to add 90° to this angle to have ω, or the total angle traversed since the starting point; then substituting this value of ω in the equation [2], one will deduce by numerical methods the corresponding value of k. In order to avoid confusion, we will preserve k for resistance on surface, and we will name inside resistance k'.

Before going further, I must point out that our formula cannot determine k for liquids with positive excess; with those, indeed, the angle described on surface beyond the magnetic meridian line is due, in all or partly (§ 262), to the needle being carried by the surface layer. The complete application of the formula [2] is thus restricted to the liquids which do not have positive excess, i.e. with those on which the gold sheet or the tuft simply awaits the needle.

The resistance due to interior viscosity must, as I already remarked, be about twice as large when the needle is driven in the liquid than when it is driven on the surface, since, in the first case, it rubs by its two faces, while, in the second, it rubs by only one; if thus, for a certain liquid, the surface excess were equal to zero, or, in other words, if the surface layer had same viscosity as the interior, one should appreciably have, with regard to this liquid, $k = \frac{1}{2}k'$; I say appreciably, because (§ 262) of small causes, such as the action of the section of the needle inside the liquid, the loss of weight under this same condition, etc, undoubtedly deteriorate somewhat this equality. For a liquid with negative excess, there will be consequently

$$k < \frac{1}{2}k', \quad \text{or} \quad k - \frac{1}{2}k' < 0$$

but the formula [2] allows the calculation of this difference for all liquids without positive excess and on which the needle exceeds the magnetic meridian line; one can thus, as I said, note the existence of negative excesses, and obtain, at the same time, their approximate relative values.

§ 274. That is what I did for the four liquids alcohol, spirits of turpentine, sulphuric ether and carbon bisulphide; by substituting in formula [2] the deduced values of ω, for these liquids, from the experiments of § 267 with 271, I obtained the results shown in following table

LIQUIDES.	VALEURS of k.	VALUES of k'.	VALUES of $k - \frac{1}{2}k'$.
Spirits of turpentine	2,34	57,28	-26,30
Alcohol	2,48	16,34	-5,69
Carbon bisulphide	1,24	7,11	-2,31
Sulfuric ether	1,05	4,71	-1,20

Thus the results of the formula confirm our preceding deductions fully: they announce negative excesses in spirits of turpentine and alcohol, and show that that of the spirits is larger than that of alcohol; but they teach us, moreover, that carbon bisulphide and the ether also have negative excesses. If one compares these results with the fast thinning of the caps of fatty oils, as well as the analogy of the phenomena, on the one hand between the caps of the acids lactic and acetic and those of spirits of turpentine and alcohol, and, on the other hand, between the caps of benzine, liquor of the Dutchmen and chloroform, and those of ether and carbon bisulphide (§ 247), one will have to look at as quite probable that the property to present a negative excess belongs to all the liquids of the second category.

In the table above, I arranged the liquids in descending order of their negative excesses; however, this order is also the descending order of their interior viscosities, as that results from the respective values of the angles described by the needle beyond the magnetic meridian line in the interior; if thus it is allowed to draw some conclusion from results relating to a restricted number of liquids, we will say that negative excess appears to be all the more large as it belongs to a more viscous liquid. If it is thus, fatty oils and the lactic acid must still have more considerable negative excesses than that of the spirits of turpentine.

The smallness of negative excesses, or, what amounts to the same, the lesser mobility of the surface layers in the carbon bisulphide and ether, is, undoubtedly, the principal cause relative to the length of the white phases in the caps of these two liquids. It is true that the cold, which must still reduce this mobility, shortens however, we saw, the phases in question; but, it should not be forgotten, two opposite causes then appear to be present: on the one hand the low temperature of the films themselves, which must, actually, tend to lengthen the white phases, and, on the other hand, the reduction in the small remainder of evaporation, which tends, on the contrary, to shorten them (§ 253), and it may be that this last influence overrides the first.

In our table, the values of negative excesses are expressed according to a unit which is not well defined, one thus should look at only relative values there; and still should one look at them only as rough approximations. Indeed, the formula from which they are deduced is founded on a law of resistances which, one knows, is not rigorous[163]; in the second place, my process of measurement of the angles leaves something to be desired, so that I was not justified to push the precision, even in the averages, beyond the half-degree; moreover there is an influence which the formula could not account for, and which must increase all the angles more or less: it is that due to its acquired speed, the portion of liquid pulled by the needle (§ 269) carries necessarily the latter a little beyond the point which, without that, would constitute the limit of the angle.

§ 275. We can now report the results of the tests with the needle on the liquids of the third category. Here again we will have to note quite remarkable facts.

Let us see initially what relates to the household soap solution at 1/40. With a solution which I had just prepared, I found, ata temperature of l8° on the surface, 4.82″ duration, angle beyond the meridian line magnetic 10°; duration inside 2.58″, angle 5°.

These results agree, one sees, with those of the caps to clearly show a strong surface viscosity. It was necessary, for a reason which one will further understand, to compare them with those furnished by distilled water under identical conditions; I thus operated the same day on distilled water, and I obtained: on the surface, 4.93″ duration, angle 10°; duration inside 2.58″, angle 5°.

As the observations of this kind inevitably comprise small errors of which there must, in general, remain something in the averages, I started again, at another time, by operating the same day on the two liquids; the temperature was approximately 21°. The results were: with the soap solution, on the surface, duration 4.14″, 1 angle 6°1/2, duration inside 2.23″, angle 4°1/2; with the distilled water, on the surface, 4.07″ duration, angle 8°; duration inside 2.08″, angle 4°.

In the first of these two couples of series, the results relating to the two liquids hardly differed between them; in the second, they moved apart a little, which is undoubtedly due to the inevitable errors of the observations, especially with regard to the duration inside; for this, indeed, the speed of the needle being much higher, it was extremely difficult to announce with precision the moment of the passage of the point past the reference mark. At all events, one can conclude from the whole of these same series that the presence of 1/40 of soap changes only slightly the surface viscosity of the liquid; we will further see it appears to decrease it a little (§ 292).

Alcohol already showed us (§ 267) that, for the same liquid, values of the durations and angles obtained by of the series of observations carried out at different times can

[163] Coulomb showed (*Mém. de l'Acad. des Sc. de Paris*, year IX of the Republic), by many a succession of experiments, that, when a solid plane is driven very slowly in the direction of its surface inside a liquid, resistance is proportional to the simple speed. But the speeds which he employs are approximately five times less than the slowest of those of my needle in the liquids which provided the results of the table. To have exact results with speeds of my experiments, it would probably be necessary to regard resistance as made up of two terms, one proportional to the simple speed, the other with the square this speed; but then the formula would undoubtedly not be suitable for integration.

very notably differ from each other; distilled water as well as the soap solution offer new examples of them: for water, the series of § 262 and those which we have just reported gave, on the surface, the respective values 4.59″, 4.93″, and 4.07″, and, inside, 2.37″, 2.58″, and 2.08″; the soap solution shows, one could see, similar differences. Let us say here, to return there, all these differences come from the variations of the magnetism of the needle: during the long time that the whole of my experiments required, this magnetism has more than once decreased, and when the reduction appeared too large, I subjected the needle a new magnetization. Also, when it was a question of comparing one liquid with another, there always was care to operate the same day on both.

As for the angles, it seems that they should be, for the same liquid, all the larger as the durations are smaller, since then the needle reaches the magnetic meridian line with more speed acquired either in itself, or in the layers which it put moving; however, it is precisely the opposite which takes place in general, as our measurements show. One can, I believe, explainf this singularity by observing that when the needle is more strongly magnetized, its direct force tends to cancel its speed at a less distance beyond the magnetic meridian line. Some exceptions, however, lead me to think that sometimes another influence, whose nature escapes to me, acts more or less on the angles.

Although one easily obtains enormous bubbles with an aqueous household soap solution, one does not manage to form some, even the small ones, with an alcoholic solution of the same substance. That is easy to envisage according to our results, knowing that alcohol has a considerable negative excess.

§ 276. The solution of soft household soap at 1/30 (see the second note of § 248), gave, at a temperature of 19°: on the surface, 4.40″ duration, angle 6°1/2, duratino inside 2.38″, angle 5°; thus the same conclusion as to surface viscosity.

§ 277. With the rosin soap solution (see the third note of § 248), I found, at a temperature of 18°: on the surface, 7.30″ duration, angle beyond the magnetic meridian line 5°; duration inside 4.48″, angle 0°; arc described by the gold sheet 26°. The rosin soap solution thus also has a surface viscosity with positive excess.

§ 278. Let us come to the most extraordinary liquid of all those which I examined; I want to speak about the saponin solution. That which I initially tested contained 1/80 of saponin, and did not appear more viscous than pure water; however, on its surface, the needle, set, like always, at 90° of the magnetic meridian line, then released, did not leave its position, in spite of blows struck to the table, absolutely as if the liquid had been overlain with a film of a solid nature. However, the surface had the perfect polish of a liquid, and, moreover, by slightly agitating it with the end of a spatula or a wire, one could not recognize the least trace of a film. Solutions with 1/100 and even with 1/160 presented the same results.

I carried out the test of the needle inside the liquid with the solution of a second saponin sample, the first having been exhausted by other experiments. This second sample was not completely excellent: to obtain the best results, it should have been dissolved in a less quantity of water; the solution that I employed was at 1/60; it gave bubbles of 12 to 13 centimetres, and, on its surface, the needle placed at 90° from the magnetic meridian line remained in the same way perfectly motionless. In the interior, the duration of the course of the 85° was 2.72″, and the angle beyond the magnetic meridian line 2°; the temperature was 16°. I also repeated, the same day, the test inside distilled water, and I obtained: 2.66″ duration, angle 2°. These results differ little from each other, and one must conclude from it that with the solution with 1/100 of the first sample, they would have been brought still closer; one can thus claim that the interior viscosity of a good saponin solution is appreciably equal to that of pure water.

The observations reported above hardly make it possible to consider resistance to the movement of the needle on surface resulting from the formation of a film; one is thus led to admit, in the saponin solution, an extremely strong viscosity, and that is confirmed by the following experiments:

If the surface were overlain with a film, this should come either from the evaporation of water, or of an action of oxygen in air on saponin, action that, moroeover, is not known to chemistry; however, I left alone for three days a solution with 1/100 of the first sample in a cup on which a paper was poised to simply shelter the liquid from dust, and, after this long time, I noticed no change in the surface.

In the second place, when a bubble which one inflates with the pipe has suddenly broken, it does not disappear as would a soap bubble: one sees falling from the opening of the pipe a mass lengthened in the vertical direction, tightened in the horizontal direction, and consisted a kind of crumpled membrane of a white material. If one receives this mass on the liquid, it forms at once a whole agglomeration of irregular caps there, and, if those are quickly examined, it is recognized that their aspect holds a second crowd of very lengthened small masses of air which seem imprisoned in the films; but soon these small masses disappear, the caps are regularized more or less and appear completely transparent; finally if these same caps are burst, no trace of film remains on the surface of the liquid. In this experiment, one understands, the bubble is detached from the opening, and then, due to the pressure which it exerts, drives out, by the opening thus formed, the air that it contained; but, in consequence of the rigidity of its surface layers, it can collapse on itself only while folding and thus imprisoning small masses of air in a great quantity of cylindrical channels; only one does not see well why this crumpling takes place in order to tighten the bubble only in the horizontal direction. When this species of folded membrane falls on the liquid, the small channels above break one after the other, and finally, when the small caps are burst, all takes again its liquid aspect perfectly.

One thus sees, these appearances of membranes are simply due to an enormous viscosity of the surface layers, and not to the generation of a true solid film. Here are some more singular facts depending on the same causes:

One inflates, with the opening of the pipe, a bubble approximately 6 centimetres in diameter, then one aspires by the pipe; the bubble then, instead of returning on itself in all the directions, decreases only in the side direction, and, if one stops in time the aspiration, is transformed into a cone having the opening for base. The surface of this cone is initially wrinkled, then becomes perfectly plain, and the film persists then in the same state with its conical form.

One deposits on the surface of the liquid a bubble approximately 4 centimetres in diameter, and, maintaining the opening of the pipe in contact with the cap to which this bubble changed, one blows to increase its dimensions, until it breaks. At once the film subsides on the liquid in several great portions, of which each one remains separated from the surface of the liquid by a film of air, and is reduced little by little as if it returned to the mass by the portion of its edge remained adherent, taking several seconds to carry out this withdrawal. When all disappears, the surface shows liquid as perfectly as before.

The solution of saponin is certainly the liquid which provides the most abundant foam, and perhaps most persistent: it is enough to dissolve in water 1/4000 of good saponin so that the liquid, agitated in a bottle, still gives a foam of 35mm height, which requires several days for its complete vanishing. In addition, alcohol does not exert any chemical action on saponin; it does not even dissolve it, cold, in notable quantity with the help of water. However, if one adds to a saponin solution an equal volume of alcohol, agitation develops nothing any more on the mixture but a hardly sensible foam, which disappears almost instantaneously. The negative excess of alcohol completely neutralizes the positive excess of saponin.

§ 279. I said above that a 1/4000 solution of good saponin still develops, by agitation, an abundant and persistent foam; but this liquid refuses to inflate in bubbles with the opening of a pipe; it is thus a second example to be added to that presented by (§ 249) gum arabic solution at 1/10, of a liquid providing a rather voluminous and very durable foam, and refusing to form itself into bubbles.

While speaking (§ 161) about the formula at which Dupre arrives to express the speed of withdrawal of a liquid film which breaks, I have claimed that this formula disregards a significant element to which Dupre could not have regard, and which must, for some liquides, make the results very inaccurate; the element in question is the surface viscosity; we saw above, indeed, that the surface viscosity of a saponin solution exerts such an influence on the phenomenon, than a film of this solution can require several seconds for its withdrawal.

§ 280. The interior viscosity of a solution of good saponin is, we saw, with very close to that of pure water, although this solution gives, on the opening of a pipe, bubbles 12 centimetres in diameter; one can conclude also from the values of the duration inside in the comparative series of § 275, despite the small divergence which one meets there, that the interior viscosity of the 1/40 household soap solution is very near that of pure water; and, however, with this solution, one inflates, with the opening of a pipe, bubbles of more than 25 centimetres in diameter. Let us add that one still forms bubbles of 10 centimetres with a solution with 1/500 of the same soap, a liquid whose interior viscosity obviously cannot differ in an appreciable way from that of water; if, moreover, we remember that very viscous liquids, such as olive oil, glycerin and gum solution, to which one can add molasses and syrup of pure or diluted glucose, are completely unsuitable to the generation of bubbles, we will not be able to preserve any doubt about the error of the accredited opinion which attributes to ordinary viscosity the property of liquids which are easily developped in large bubbles.

However, the influence of interior viscosity is not completely null, especially with regard to the films of the first and the third category. In those of the second, the two surface layers having more molecular mobility than the interposed layer, the descent of the liquid is carried out mainly by the first, and more or less the viscosity of the interposed layer must have little effect; thus the oil films attenuate with an extreme speed (§ 247), in spite of the strong interior viscosity of the liquid. But in the films of the first and the third category, where molecular mobility is less in the surface layers than in the interposed layer, the interposed layer necessarily takes more part in the descent, and its viscosity must intervene at a certain point; we will further see some (§ 294 and 298) from the examples.

§ 281. the solution of albumin, prepared as I indicated (fifth note of § 248), presents, although with a degree less pronounced, properties similar to those of the saponin solution: on the surface, the needle, released at 90° from the magnetic meridian line, employed approximately three-quarters of an hour to describe an angle of 35°, and did noit go further; inside, the duration of the course of the 85° was only 9.77″.

The surface viscosity of this liquid, though less enormous than that of the saponin solution, is thus still extremely energetic; also when the bubbles reach 11 to 12 centimetres, they give membranes similar to those of the saponin bubbles.

If one adds to our albumin solution 10 times it volume of distilled water, the mixture still provides an abundant foam and very persistent, but one does not manage any more to form it into bubbles, which constitutes a third example similar to those that I already announced; I will further test (§ 304) explaining this singular phenomenon.

I did not operate with the needle on the iron acetate solution; I had at my disposal only too small a quantity.

§ 282. Thus the results obtained with the magnetized needle with regard to the fifteen liquids which I subjected to this kind of test, fully confirm the deductions drawn (§ 252) from the experiments on film caps; one thus can, I think, regard as established the following principle:

The surface layer of liquids has an intrinsic viscosity, independent of the viscosity of the interior of the mass; in some liquids, this surface viscosity is stronger than the interior viscosity, and often by much, as in water and especially in saponin solution; in others liquids it is, on the contrary, weaker than the interior viscosity, and often also by much, as in spirits of turpentine, alcohol, etc.

§ 283. Descartes appears to be the first who has advanced the idea of an intrinsic surface viscosity; he expressed it thus:[164] "the surface of water is much more indisposed to divide than is the interior, as one sees by an experiment in which all small enough bodies, although made of extremely heavy matter, as are small steel needles, can float and stay above, when it is not divided yet, but when it is, they go down right to the bottom without stopping."

Descartes explains that is how, when the sea water evaporates, the salt crystals float on its surface.

§ 284. Later, according to research of Petit[165] (year 1731), he attributed the phenomenon of the floating needles to the presence of an adherent layer of air on their surface; but, in 1806, Rumford communicated, to the Academy of Science of Paris, a Mémoire[166] where he seeks to prove that the layer of air does not have any influence, and that on the surface of water exists a resistant film which supports the needle. As for the origin of this film, he says simply: "If the water molecules strongly adhere one to the other, a continuation necessary of this adhesion must be, it seems to me, the formation of a species of skin on the surface of this liquid."

The experiments of Rumford especially consist in covering water with a layer of another suitable liquid; such as spirits of turpentine, and to drop through this layer small heavy bodies; those stop on the surface of water, and remain floating there.

§ 285. In 1806 also, and later in 1814, Link[167] explains the phenomenon in the same manner; but, while Rumford had spoken only about water, he extends the same design to all liquids. He regards the appearance of the film as due to the molecules of the surface being attracted on only one side; this inequality of action generates an obstacle to displacements.

Link deduced from that a curious theory: he thinks that if a liquid were modified so as to present a very great number of surfaces instead of only one, its mobility would be decreased considerably; and noticing that solids are composed of fibres or plates, or, more generally, from very small distinct parts, he concludes that it is only to this circumstance that should be attributed the solid state; according to him, if one could establish an intimate contact between all the small parts which constitute a solid, one would transform it into a liquid, but then, in consequence of the uniform arrangement of molecules, each one of them would be also attracted in all directions, and would thus preserve a complete mobility, at least in the interior of the mass. It will be seen (§ 321) that a similar opinion on the constitution of solids had already been given by

[164] *the Meteors* . Leyde, 1638; page 187 of the vol. V of the Works of Descartes published by Victor Cousin.

[165] *De l'adhérence des parties de l'air entre elles, et de leur adhérence au corps, qu'elles touchent* (Mém. de l'Acad. des Sc. de Paris, 1731, p. 50).

[166] *Expériences and observations on the adhesion of the molecules of water between them* (Mém. of Acad. of the Sc of Paris, 2nd half of 1807, p. 97).

[167] *Ueber Naturphilosophie*. Leipzig, 1806; also see: *Ueber Festigkeit und Flüssigkeit* (Ann. of Gilbert, 1807, vol. XXV, p. 133), and *Theorie der Flüssigkeit und Festigkeit*, etc. (ibid, 1814, vol. XLVII, p. 1).

Leidenfrost.

§ 286. In a Mémoire[168] published in 1808, Prechtl adopts the ideas of Link on the surface viscosity of liquids, and, to prove the existence of the film on the surface of water, he describes some new experiments, of which here is the most conclusive:

One deposits on the surface of water a piece of watchspring, which floats there as do the needles; then, after having spread on the liquid a light layer of a fine powder, one impresses, by means of the remote action of a bar magnet, a rotational movement to the piece of spring, and one notes, by the displacement of the powder grains, that all the surface of the water turns at the same time. Rumford had made an experiment which seemed to also indicate a movement of all of a liquid face; but one could see the influence of foreign causes there; that of Prechtl is, on the contrary, perfectly clear.

Pichard has presented[169], in 1824, on the phenomenon of the floating needles, the observations among which only one is of the interest for our work: the author made float thus more than forty needles, which, while being juxtaposed, formed a small raft.

The same year, Gillieron also treated the question of floating needles [170]; he adopts the ideas of Rumford, but does not bring any new argumentthe subject of § 123, de Maistre gives the same explanation as Link of the appearance of film on the surface of water.

§ 287. Mr. Artur, in a work[171] which appeared in 1842, comes, by theoretical considerations that it is needless to reproduce here, to look at the surface layer of all liquids as having more density and more cohesion than the interior, and as presenting, consequently, a certain resistance. According to just the laws of hydrostatics, the volume of the hollow formed on the surface of water by a needle which floats should exceed six times that of the needle, the density of steel being 7.8; however Mr. Artur estimated and had others estimate the volume of this hollow, and the highest evaluation did not carry it to triple of that of the needle.

§ 288. As I said (§ 152), Mr. Hagen also generalized, in 1845, the idea of a lesser mobility, and that of a greater density, in the surface layer of liquids; it rests on that, in a flow of water, the speed is lower on the surface than a little below, and on the production of film caps by the rise of gas bubbles; but the first of these facts refers only to water, and the second cannot be called upon, since the film caps are formed perfectly on alcohol, the spirits of turpentine, etc, liquids in which the surface layer is, on the contrary, we now know, more mobile that the interior.

§ 288bis. In the Report (year 1865) about which I spoke in § 160bis, Mr. Marangoni, after having pointed out that, when a drop of a liquid is likely to spread out over mercury, it does so only slowly in spite of the strong tension of mercury, described a curious experiment about or one can infer that this last liquid has an energetic surface viscosity: he deposits on the surface of mercury a small piece of wet paper, and notes that it is impossible to drive it by the breath of the mouth.

§ 289. In 1866, Mr. Nägeli[172] concludes from experiments that he made on water capillary tubes, that such a column opposes a certain resistance to the movement, independent of the friction of the totality of the column against the interior wall of the tube, and which can come only from the surface layer of the top; he consequently claims, in this layer, a viscosity larger than inside. He attributes to this surface viscosity the

[168] *Theorie der Krystallisation* (Gehlen, Newspaper für die Chemie, Physik und Mineralogie, vol. VII, p. 455).

[169] *Considérations sur les phénomènes que présentent de petites aiguilles coudre, posées doucement et dans une situation horizontale, sur la surface d'une eau tranquille* (Biblioth. Univ., vol. XXV, p. 273).

[170] *Sur les mouvements de certains corps flottants sur l'eau* (ibid, T XXVI, p. 190, and T XXVII, p. 207).

[171] *Théorie élémentaire de la capillarité*. Paris.

[172] *Ueber die Theorie der Capillarität* (Bullet. de l'Acad. des Sc. de Munich, year 1866, vol. I, p. 597).

remarkable fact studied by Mr. Jamin[173], and consistent in that a mercury or water capillary tube subdivided in a great number of small partial columns, requires a considerable force for its displacement. If that is the true cause of the resistance of the subdivided columns, it is necessary that this resistance does not appear in the liquids of my second category; however, Mr. Jamin says expressly that it is null for alcohol and oil; I will reconsider this question soon.

Mr. Nägeli tries to explain the larger viscosity of the surface layer on the basis of the theory of Mr. Clausius on the nature of liquids; he consequently also looks at the fact as general, i.e. as intrinsic to all liquids.

§ 290. Lastly, in 1866 still, Mr. Stanislas Meunier[174] was led by his experiments in claiming, like Artur and Hagen, a higher density on the surface of all liquids. The experiments concerned consist in suspending a vertical solid cylinder in a such manner that it is immersed partly in a solvent liquid; after some time, the cylinder is found cut in two on the level of the surface of the liquid, and the lower portion, which is only partially dissolved, falls to the bottom of the vessel. However, whatever the cause of this singular phenomenon, surface viscosity seems not to have any share; I have noted, in fact, that a rosin cylinder is cut perfectly by spirits of turpentine, although, in this liquid, surface viscosity (§§ 268 and 274) is less than the interior viscosity.

Such is, to my knowledge, the sum of what was done before 1870 with regard to surface viscosity. One now understands the principle stated in § 282, a principle deduced from direct experiments, presented in detail, and which differs in essential point from those of the physicists whose researches I have summarized[175].

§ 291. The principle in question being, I believe, put out of doubt, let us resume the study of the relations between surface viscosity and tension.

To be able to clearly appreciate these relations in various liquids, it would be necessary to have a precise means to determine the values of surface viscosity numerically, as one determines those of the tension. This precise means I sought in vain. After having learned of the opinion stated by Mr. Nägeli, I believed that one could employ resistances in divided capillary tubes; but, after many tests, I gave up this process; also I will not describe the apparatus which I used myself; I will say only that resistances were measured using a water pressure gauge.

By carrying out the tests in question, I soon recognized that the enormous resistances noted with regard to water by Mr. Jamin, are due, not to surface viscosity, but to the difficulty which besets water columns driven in a capillary tube whose interior wall is imperfectly wet: when wetting is complete, the divided water columns express only very low resistances. I managed to obtain a good wetting by filling the capillary tube with a very concentrated caustic potash solution, leaving it in this state for twenty-four hours, then emptying it, and carefully washing the interior with distilled water; a few fingers of this liquid was then introduced there, and, immediately before evaluating resistance, I made the column move back and forth several times. Under these conditions, and with a tube whose internal diameter was approximately 1/3 of millimetre, the resistance corresponding to 100 fingers was found to be equivalent simply to the pressure a height of water of 6mm, 4; the temperature of the room was of 17°. In my apparatus, one could hardly increase the number of the fingers beyond 70; but as resistance must obviously be proportional to the number of fingers, one calculated by a

[173] *Mémoire sur l'équilibre et le mouvement des liquides dans les corps poreux* (Comptes rendus, 1860, vol. L, page 172). See also, for more details, *Société chimique de Paris, leçons de chimie et de physique professées en 1861 par MM. Jamin, Debray, etc*, Paris, 1862.

[174] Comptes rendus, volume LXIII, page 265; also see the newspaper *Le Cosmos*, 1868, 3rd series, volume III, pages 635 and 666.

[175] For research after 1869, to see the articles listed in § 508 under n° 1, 5, 7, 25 and 35.

proportion that which corresponded to 100 fingers.

If, instead of taking measurement immediately, one waited two minutes, resistance became much larger: it was found, in this circumstance, always for 100 fingers, of 44mm approximately. It is that, in consequence of their strong concave curvature, the end surfaces of the columns exert, on the layer of water which wets the tube between them, an energetic suction which quickly makes this layer disappear; as I am not certain as resistance 6mm, 4 is not yet a little too large, because, during the small adjustments which measurement requires, the absorption of the layer in question had already started.

IN 1861, Mr. Bède had shown[176] by clever experiments, that when a liquid is raised in a capillary tube wetted beforehand, the thin layer which moistens the wall above the column does not remain, and the experiment above confirms this result fully.

Now, if the low resistance expressed by water in the case of a good wetting were due, in all or part, to the surface viscosity, one would find, under the same conditions, resistances much higher with albumin and saponin solutions, and that is what took place indeed: for a saponin solution of 1/60, resistance corresponding to 100 fingers was 90mm; and here still, after having waited two minutes, it had considerably increased, and reached, for this same number of fingers, about 440mm; the temperature was 18°. As for the albumin solution (see the fifth note of § 248), its resistance should have been less than that of the saponin solution; however it was shown, on the contrary, much more considerable: while introducing into the tube two fingers , I managed to move them only by means of a pressure of approximately 37mm, which gives, for 100 fingers, 1850mm; temperature 18°.

The weakness of the resistance of water, the errors which can result from the fast absorption of the dampening layer, finally the disagreement between the results above and those of the experiments with the magnetized needle with regard to the albumin and saponin solutions, hardly allow, one sees, to attribute a sufficient confidence to the process of the divided capillary tubes; I thus return from there to that which I reported in my 8th series, although it gives only an approximation undoubtedly rather rough, and one cannot employ it with all liquids.

§ 292. It is founded on the comparison of the respective durations of the course of the needle magnetized on surface and inside the liquid.

In the liquids which have an extremely energetic surface viscosity, such as albumin and saponin solutions, the duration on the surface is infinite, so that the ratio to the interior duration is also infinite; in liquids of the second category, where surface viscosity is, on the contrary, very weak, the ratio of the external duration at the interior duration is only one fraction; finally in liquids such as water, where surface viscosity is moderate, the ratio is larger than unity, but finite. Thus, although the ratios in question obviously cannot serve as exact measures of surface viscosities, one must recognize that they depend on it, and one can claim that when they present a notable difference from one liquid to another, there is also, in general, an difference of the same sense between the surface viscosities of these liquids.

That said, let us seek the values of the ratio in question for all the liquids of the first and third category for which we measured the properties.

For distilled water, the ratios deduced from the experiments of §§ 262 and 275 are 1.94, 1.91 and 1.96; moreover, when I compared the saponin solution with distilled water (§ 278), the average durations on surface and inside this last liquid were respectively 4.99″ and 2.66″, from which the ratio 1.88; the average of the four ratios is thus 1.92.

[176] *Recherches sur la capillarité* (Mém. de 1'Acad. de Belgique, volume XXX of Mém. couronnés et des savants étrangers, p. 143 of the Report).

In the calculation of the ratio relating to glycerin of Price, I employed, for the surface, only the two old durations first indicated in the § 263; former experiments had learned to me, indeed, that with this liquid, the durations on surface are always somewhat decreasing, because undoubtedly of a small absorption of water vapor, which one does not obviate completely by the glycerin coating applied inside the bell. One has thus, for the ratio in question, the value 1.85.

One has in the same way, by the elements given in § 264 to 266:

for sodium carbonate solution	1.75,
— that of potassium nitrate . .	1.85,
— that of calcium chloride . .	1.74.

A the regard of the liquids of the third category, one obtains:

for the household soap solution, by the results of the two series of § 275, ratios 1.87 and 1.78, average	1.82,
for the household soap solution of (§ 276)	1.85,
— that of rosin soap (§ 277)	1.63.

As for the albumin and saponin solutions, we know that the reports/ratios which relate to them have as of infinite values.

§ 293. Now let us examine all these ratios more closely, and see what consequences one can deduce. the ratio 1.85 for glycerin of Price is not much lower than the ratio 1.92 for distilled water; however, it would not have to be concluded that the surface viscosity of glycerin is close to that of water, the enormous difference of interior viscosities of these two liquids introducing a considerable element of error from the point of view of the comparison of the ratios. Indeed, because of the energetic resistance opposed by the interior viscosity of glycerin, the needle does not preserve, as well on the surface as inside this substance, as a very small portion of its direct force; but consequently a not very intense surface excess becomes very large relative to this weak remainder of force, and consequently must decrease much the speed of the needle on the surface; however the viscosity of the surface layer being able to be regarded as equal to the interior viscosity plus the positive excess, one sees that with a very strong interior viscosity and a very small positive excess, this last could double or triple without the total surface viscosity changing much, while the ratio of the durations would undergo, on the contrary, very great variations. Thus, as I said, in the case of a very viscous liquid, the ratio of the durations does not provide any more an immediate indication on the intensity of surface viscosity.

A simple means was presented to verify these deductions and to be assured if the great value of the ratio for glycerin is illusory. This means consisted of: adding water to glycerin to reduce interior viscosity sufficiently, and to submit the mixture to the testing of the needle. However, with a mixture of equal volumes of glycerin and distilled water, the durations were on the surface, 10.93″, and, inside, 7.07″; the ratio is thus 1.54; it is, one sees, extremely below the 1.92 pertaining to water; and as interior viscosity is about of the same order as in our other liquids, the ratio above becomes comparable with those of the latter and we can conclude from it that the surface viscosity of the mixture in question is very notably lower than that of water.

Let us turn to the surface layer of this same mixture being necessarily composed, as the remainder of the mass, of equal volumes of glycerin and of water, the ratio 1.54 found above can be looked at as hardly deviating from the average between the two values that would be obtained, on the one hand, if the surface layer only of the liquid were made of pure glycerin, and, on the other hand, if this same layer were made of pure water; however, in this second case, the ratio would move away obviously little from the 1.92 corresponding to water, the interior viscosity of our mixture not exceeding enough that of water to introduce a quite notable change. If, according to that, we preserve, for the second case, the value 1.92, and if we indicate by x that which the first would give, we will be able to pose $\frac{x+1.92}{2} = 1.54$, from which $x = 1.16$. Such are thus roughly the ratio of glycerin of Price when one removes the influence of the strong viscosity of the interior of the mass, and it teaches us that it is necessary to look at the surface viscosity of this substance as being, actually, much less than that of water.

§ 294. The ratio 1.63 of the rosin soap is also rather lower than that of water; however experiments of § 248, by showing us that this liquid, at least such as I prepared, gives caps which never have a colorless phase, made known to us that its surface viscosity, although with positive excess, is not very energetic; there is thus here agreement between the indications provided by the ratios and those which one deduces from results more definitely interpretable.

On another side, the ratio 1.16, that we obtained in an indirect way and that we were led to regard as giving an idea of the true surface viscosity of glycerin of Price, is again much more below that of water, and however the glycerin caps, even those which lasted a long time (§ 246), did not express any general coloring; moreover, the ratios 1.75 and 1.74 of calcium chloride and sodium carbonate solutions are lower also than that of water, although lesser quantities; however, while in the water caps which were rippled with red and green the white phase lasted a maximum of only 13″, the sodium carbonate gave caps which burst only after 26″ without showing a trace of colors, and the calcium chloride caps were rippled only after 100″ at least (ibid.); with regard to the three liquids above, there thus seems to be a contradiction between the indications of the ratios and those of the caps; but I will show that this contradiction is only apparent.

I drew attention (§ 280) to a small influence of interior viscosity in the films of the first and the third category; however, it is reasonable to attribute to this influence the disagreement above; it is understood, indeed, that, in the glycerin caps, strong interior viscosity slows down enough the descent of the liquid, in spite of the little of energy of the surface viscosity compared with that of water, so that the rupture takes place before the appearance of any rippled effect. The duration of the course of the needle inside the sodium carbonate solution has (§ 264) to some extent whole 4″, while at the interior of water (§ 262 and 275), the whole part was never but 2″; the interior viscosity of the solution in question thus exceeds that of water, which explains why, after 26″, the caps did not have any rippled effect yet; finally the interior viscosity of the calcium chloride solution is larger still, since the whole part of the duration is (§ 266) 8″, which explains the 100″ of colorless phase.

Let us take now, among the liquids with the regard of which we could evaluate the ratios, those for which interior viscosity is very close to that of water. There are three of them: the solutions of potassium nitrate , household soap and soft soap of household; for each one of them, indeed, the whole part of the duration inside is, as for water, of 2″. The ratios 1.85, 1.82 and 1.85 which belong to them respectively differ relatively little from that of water, from which we will infer that surface viscosities of these same liquids also approach that of water; however, in water caps, the white phase which preceded the rippled effect was from 10″ to 13″, in those of household soap it was from 6″ to 20″, in those of soap my of household, 5″ to 14″ (§§ 246 and 248), and one obviously can, through their irregularities, recognize that they are of the same order. As for the potassium nitrate caps, they did not give a rippled effect, but their persistence not having exceeded 6″, we are unaware of if their white phase would not have been similar. The agreement between the ratios and the caps thus reappears when interior viscosities are appreciably equal. Thus, as I have claimed, the contradiction that I announced with regard to the three liquids is not real; it comes simply from the influence of interior viscosity.

As another example of this influence, I will point out that, in the caps of albumin solution, the colorless phase was much longer than in those of saponin solution, although (§§ 278 and 281) the surface viscosity of the first of these liquids is less energetic than that of the second; it is that the contrary takes place with regard to interior viscosities, the durations of the course of the needle inside these in liquids having respectively to some extent whole 9″ and 2″. As I pointed out, in § 258, the long duration of the white phase in the caps of albumin solution shows, already then, as the surface viscosity of this solution gains much over that of calcium chloride solution; but I had the right of it, because if, in these two liquids, the white phases differ considerably, in addition the interior viscosities are brought much closer, the whole parts of the duration inside being respectively 8″ and 9″.

§ 295. Before making use of our ratios, again let us present a remark. Since these ratios become infinite for very intense but finite surface viscosities, like those of the albumin and saponin solutions, one must in infer that they vary according to a law faster than surface viscosities; however, made abstraction of the excessive ratios of saponin and albumin, all are less than that of water; if thus we always take this last liquid as reference, and if, adopting the ratio 1.92 which belongs to it to represent its surface viscosity, we want to conclude from the ratios of the other liquids with the surface viscosities of those, it seems that they should all be regarded as too weak, and that, to deduce the viscosities in question from them, one should increase them a little, and more especially as they are smaller. But, on the other hand, in all our liquids, the interior viscosity exceeds that of water; for some the excess is extremely weak, and for others it is notable (I exclude glycerin here, to which I will return); the reasoning that we made (§ 293) with regard to the immediate report/ratio of glycerin, is thus applicable to all, i.e., so that they could represent surface viscosities, it would be necessary, according to this same reasoning, to decrease them a little, and more especially as interior viscosity is stronger. To adapt our reports/ratios to surface viscosities, it would be necessary consequently to make them undergo two small corrections in contrary directions, which would be compensated thus partially.

In order to better appreciate the things, let us place compared to the reports/ratios the whole parts of the duration inside; we will have this manner the table which follows:

LIQUIDS.	ENTIRE PARTS OF DURATION	REPORTS
Water	2″	1.92
Solution of household soap	2	1.82
— of soap of household	2	1.85
— of potassium nitrate	2	1.85
— of sodium carbonate	4	1.75
— of soap of rosin	4	1.63
— of calcium chloride	8	1.74

It shows us initially that the three ratios close to that of water belong to liquids whose interior viscosity is also very close to that of water; two opposite corrections that each one of them should undergo would be thus both extremely small, and, after their partial compensation, the ratios in question would be, undoubtedly, hardly modified. For the three others liquids, the ratios being more notably lower than that of water, and interior viscosities being a little stronger, the corrections would be both larger, and consequently, after their compensation, the ratios would not be much affected.

As for glycerin, the ratio 1.54 that the experiment gave us directly for a mixture with equal volumes of glycerin and water, is still more below that of this last liquid; but, in addition, like the whole part of the duration inside is 7″, one can also claim a more or less approximate compensation, and consider the ratio in question as corresponding without too much error to the surface viscosity of the mixture; however, this viscosity must be an average between that of pure glycerin and that of pure water; to have the ratio corresponding to the first, it is thus necessary to remake the calculation of § 293 identically, and one thus finds the number 1.16, which can consequently be adopted to roughly represent the relative value of the surface viscosity for the glycerin of Price.

§ 296. After all this discussion, one will grant me, I hope, that in the absence of means of more precise measurement, we can, without too much temerity, regard our ratios as expressing, in an approximate way, relative surface viscosities of liquids, excluding, of course, infinite ratios, and by taking for glycerin the corrected ratio 1.16. It thus only remains to compare with the surface tensions the viscosities thus evaluated.

Only, so that these new ratios are just simple fractions, we will represent by 100 the surface viscosity of water; a simple proportion will give us then, for each liquid, surface viscosity in the same system of units. For glycerin, for example, we will say: $1.92 : 1.16 = 100 : y$, from which $y = 60.42$. With the numbers thus obtained, we will form the two following tables:

FIRST CATEGORY.

LIQUIDS.	SURFACE VISCOSITIES	TENSIONS OF THE FILMS	RATIOS OF VISCOSITIES TO SURFACE TENSIONS
Water[a]	100,00	14,60	6,85
Glycerine of Price	60,42	8,00	7,55
Solution saturated with sodium carbonate	91,14	8,56	10,65
Solution saturated with potassium nitrate	96,35	11,22	8,59
Solution saturated with chlorure of calcium	90,62	11,06	8,19

[a]One saw (§ 169) that by employing a new process, Mr. Quincke found, for the surface tension of water, teh value 8.253, which would give, for the tension of the films of this liquid, 16.506; Mr. Quincke looks at his procedure like more exact than those of his precursors; but as it did not determine the tensions of the others liquids which enter my tables, and as besides we need here only relative values, I kept for water that deduced from the old methods. moreover, if the value 16.506 were adopted, it would do nothing but make the smaller still the ratio pertaining to water.

THIRD CATEGORY.

LIQUIDS.	SURFACE VISCOSITIES	TENSIONS OF THE FILMS	REPORTS OF VISCOSITIES SUPERFICIAL TO TENSIONS
Solution of soap of Marpail at 1/40	94,79	5,64	16,81
Solution of soft soap of household at 1/30	96,35	6,44	14,96
Solution of soap of colophane containing potash	84,89	7,68	11,05
Solution of saponin with 1/100	Nongiven, but extremely strong.	8,74	Nongiven, but very large.
Solution of albumin	Id.	11,42	Id.

§ 297. One thus sees it with the inspection of these two tables: initially, the ratios of surface viscosity to the tension are all larger with regard to those of our liquids which belong to the third category, i.e. which gives voluminous bubbles and a foam, than with regard to those which belong to the first and neither give bubbles consequently to the opening of a pipe, nor much of foam; moreover, except for only one, excess is considerable.

In the second place, among the liquids of the first table, that for which the ratio has the highest value is the sodium carbonate solution; also, of these five liquids, it is that which provides, by agitation in a bottle, the apparent foam: it reaches one centimetre height, and takes more than one hour to disappear completely; one can thus conjecture that if the solution saturated with sodium carbonate is unsuitable for forming bubbles on the opening of a pipe, it is closer to giving some than the four others liquids. Moreover, if the liquids of this first table refuse to inflate in bubbles on a widened opening, they give small ones at the end of a narrow tube; however, by using a glass tube of almost 6mm of external diameter and 1mm internal diameter, and while operating at a temperature of 19°, the formed bubbles had the diameters which follow; each one of them is largest which was shown in a rather considerable number of successive tests:

Water	7mm, 5,
Glycerin	8,
sodium carbonate	14,
potassium nitrate	8,
calcium chloride	9;

and one sees that the sodium carbonate bubbles are much the largest.

In the third place, the liquid of the second table which presents the smallest ratio is the rosin soap solution, and it is also that which provided me the smallest bubbles: I carried out several successive preparations of this liquid with the same substances, in the same proportions, and by employing the same process, but, I do not know why, these preparations appeared less and less good; the solution with which the tests with the needle (§ 277) were carried out, tests from which I deduced the number which, in the table, represents surface viscosity, gave me bubbles of only 9 centimetres at the maximum, while with the solutions of household soap and of household soap, I obtained 25 centimetres, and, with those of albumin and saponin, 13 centimetres. The results of our two tables thus agree well with the theory exposed in the §§ 258 and 259. There is, moreover, a small uncertainty with regard to the rosin soap tension; the 7.68 registered in the table was not measured with the same solution, but on another, which resulted from a preceding preparation, and which gave bubbles reaching 12 centimetres, although with difficulty, if my memories are exact.

One will undoubtedly notice the little difference between ratios 10.65 and 11.05 pertaining respectively to the sodium carbonate solution, which are not inflatable in bubbles on the opening of a pipe, and to that of rosin soap, which gave a certain diameter. But this still is a consequence of our theory; indeed, according to our tables, surface viscosity is less in the second of these liquids than in the first; however it follows from the stated remark at the end of § 259, that if, with this rather not very energetic surface viscosity, the ratio 11.05 allows the formation of bubbles of poor size, this same ratio, and, with stronger reason, the slightly less ratio 10.65, can better allow it with a more intense surface viscosity.

One also understands, due to the same remark, why the bubbles of the saponin and albumin solutions did not exceed 13 centimetres, although with regard to these liquids the ratios of the surface viscosity to the tension are considerable; it is that, in consequence of the energy of the surface viscosities, the ratios must be very large to bring the possibility of voluminous bubbles, and that they are undoubtedly not it enough.

Finally, the pointed out remark also explains why, while the maximum diameters are appreciably the same ones for these two liquids, the ratio is larger for the second; the surface viscosity of this one is more intense still than that of the first. I believe, moreover, that with the saponin solution, I did not reach the true maximum diameter: at the time when I sought this diameter, by employing the solution of the best saponin sample (§ 278), a solution which was used for measurement of the tension, I was unaware that to develop the largest bubbles, the liquid had to be perfectly limpid: if it had been made such, I would have probably pushed the diameter a little further.

Some of the liquids of the first category tested from the point of view of the caps could not be tested, we know, by means of the needle; however, with the regard of most significant of them, sulphuric acid, we can make sure, in another manner, that the theory is satisfied. If it were not, it would be necessary that the ratio of surface viscosity to the tension had a considerable value; however the tension of films of sulphuric acid is very strong: it is equal to 12.88, and approaches, one sees, that of water; a great ratio would consequently require a surface viscosity much more energetic than water; but, as I said at the end of § 246, those caps of the acid in question which are only a few millimetres in diameter often last much longer, and express colors: after a colorless phase from one half-minute to one minute approximately, they are rippled with pink and green, and, in the most durable, appear then other colors; now if, due to its strong interior viscosity, the sulphuric acid joined a very energetic surface viscosity, these colorings could obviously occur only much longer after white phases; the sulphuric acid thus has a rather low surface viscosity, though with positive excess, and consequently the ratio is small.

Finally, although the iron peroxide acetate solution could not be submitted either for testing with the needle, it is easy to show that it also satisfies the theory. The tension of its films is 10.2, i.e. rather strong; however, if one does not take account of the red and green which appear temporarily at the bottom of the majority of caps to then vanish (§ 248), and which are probably due to the small quantity of free acid acetic that this compound always contains, the observation shows (ibid.) that the colorless phase is very long, and that thus the surface viscosity must be very intense; the ratio of it with the tension thus has itself a high value; only it is probably not it enough so that the bubbles can reach a large diameter.

§ 298. Now that we know, for almost all our liquids, the approximate value of the ratio of surface viscosity to the tension, we can announce some new examples of small influence of interior viscosity; they concern either the duration of the white phase or the total duration of the caps. It is, indeed, obviously to this influence which should be attributed the 80″ maximum persistence of the glycerin caps, and the 229″ of the caps of the solution saturated with calcium chloride, in spite of the smallness of the ratios; that also, undoubtedly, determined the 142″ caps of the solution saturated with tartaric acid, this liquid being extremely viscous; finally it explains why the persistence of the caps of the saponin solution is far from reaching that of caps of the albumin solution, although the ratio is much larger with regard to the first.

§ 299. The results at which we arrived will enable us completely to return reason of the remarkable properties of the bubbles of glyceric liquid, and the agreement of the explanation with the phenomena will bring new arguments the support of our theory.

In the first place, let us seek the approximate value of the surface viscosity of the glyceric liquid. When one prepares this liquid with soap, the best proportions are those which I indicated (§ 100), being 2.2 volumes of glycerin of Price for 3 of household soap solution at 1/40; however, one can claim that, in this mixture, surface viscosities are distributed in the ratio of volumes; if thus one takes, in the tables of § 296 the values of respective surface viscosities of the two ingredients, being 60.42 and 94.79, the surface viscosity of the mixture will be equal to $\frac{2.2\times 60.24+3\times 94.79}{2.2+3} = 80.25$; it is, one sees, of much lower than that of water.

As for the tension of the liquid, it does not differ in an appreciable way of that of the soap solution of which it is partly made. Indeed, the tension of a liquid film can (§ 158) be represented by the expression $\frac{rp}{2}$ or $\frac{pd}{4}$ in which p is the pressure exerted by a bubble of the same liquid on the air that it imprisons, and d the diameter of this bubble; however we saw (§ 121) that with regard to the glyceric liquid, one has, at ordinary temperatures, $pd = 22.56$; one deducts $\frac{pd}{4} = 5.64$ from them, a value which is also that of the tension of a film of our soap solution [177]. This identity should not surprise: according to the research of Dupre, the tension of a soap solution hardly varies by very considerable changes even in the proportion of water[178], and undoubtedly the same thing takes place when one dilutes the solution with glycerin.

In the glyceric liquid, the ratio of the surface viscosity to the tension is thus equal to $\frac{80.25}{5.64} = 14.22$; but, with a surface viscosity not very intense and a high ratio, the liquid in question must be necessarily let develop very large bubbles, and experiment confirms it.

§ 300. In the second place, we point out (§ 108) that when a bubble is created with a good glyceric liquid, the film, after having gradually attenuated until at a certain point, then little by little a new thickness begins again, and returns in general, before bursting, to the red and the green of the last orders. I showed that this retrograde progress is due to the glyceric liquid absorbing moisture from the air, and I announced (§ l09) that I would study more closely the cause of the phenomenon; that is what I will now do.

As of the moment when the bubble is formed, the film which constitutes it is obviously subjected to two different actions, being that of gravity, which tends to thin

[177] Mr. Van der Mensbrugghe found, for the glyceric liquid (see the first note of § 162), a value a little higher, being 6; but the liquid which was used was old, and had consequently undergone more or less deterioration.

[178] Dupre advances an explanation of this remarkable fact; he exposes, in his Memoires and his work (page 376 of this), the conditions which make it possible for a substance to dissolve without chemical action in a liquid; he arrives at the singular deduction that, in certain circumstances, the dissolved substance tends to go on the surface of the solution and he indicates as an example the soap solution: he determines, by means of his hydrometer (§ 161), the tension of an extremely weak soap solution, and finds it very little different from that of a strong solution, while using himself the process of jets (ibid), he finds it equal to that of pure water; it is that, following, him, in the first case the soap went on the surface, and that, in the second, the continual movement of the liquid is opposed to this sorting.

it while immediately making the molecules slide from the top to bottom, and, that of the absorption, which, tends, on the contrary, to thicken it. That said, the progress of the colors shows that the cause of thinning is initially dominating, but that, later, it is the cause of thickening which prevails; thus there is a time in existence of the bubble where these two causes are counterbalanced, i.e. where the film gains as much as it loses. However, one cannot explain, with less than one particular cause, why equilibrium between the gain and the loss does not continue to remain; indeed, the thicknesses through which the film passes again then are equal to those which it had before; but, at these former times, it went thinning; how thus it will be conceived that at the same thicknesses it is not thinned any more, especially if one reflects that while becoming more aqueous, the liquid becomes more fluid? In truth, it also becomes less dense; but as the density of the nonfaded glyceric liquid does not exceed a tenth approximately that of pure water (§ 122), it can test only a very weak reduction, a reduction compensated besides by the increase in fluidity.

§ 301. Here, moreover, is an experiment which proves that the density switching is not at all the cause of the phenomenon: a horizontal wire ring is attached by a fork under the arm of a support provided with fixing screws; one raises to this ring a cup containing the glyceric liquid in which one immerses it, then one lowers the cup rather quickly; the ring is then occupied by a film from which a drop remains suspended; if this film is quite horizontal, it is clear that the drop is held exactly in its middle; in the contrary case, one brings it to horizontal by means of support screws. That done, the film is burst, and one carries out another by the same process; only one lowers initially the cup by an insufficient quantity so that the film catenoid which develops between the ring and the surface of the liquid becomes unstable (§ 227) and one keeps it in this position during a time that one determined by a preliminary test; the film catenoid attenuates then gradually, and, by the later lowering of the cup, it will fill the ring in a perfectly horizontal plane form, without a suspended drop.

This film is initially colorless, but, after a few minutes, one sees it being variegated with red and green, then, later, taking a yellow color strewn with small spots of another color; later still, the yellow is replaced by blue, then by indigo, then by crimson, after which the colors retrogress, so that at the end the red and green variegation of the last orders reappears.

The ring which gave me the best success was only 2 centimetres in diameter; with larger rings, of 7 centimetres, for example, there was a good beginning of retrogradation of the colors, but the film always burst before the return to the red and the green; I must say, moreover, that the liquid employed was not excellent. Still, let us indicate an essential precaution: weldings of the points where the ring sticks to the two branches of the fork and that of the point where it is closed must not present any projection inside this same ring; when there are such projections, the film loses its plane form in their vicinity; it shows, in these places, the systems of colored bands which occupy a rather great extent, and it bursts much earlier.

In this experiment, one sees, the film also starts by being thinned up to a certain point, then thickening; however, as it is plane and horizontal, the variations of density do not obviously play any role, from which it necessarily should be concluded that they are the same without influence with regard to the bubble, as I have claimed. As for the thinning of the horizontal plane film, it results from the ceaseless attraction operated by the strongly concave surfaces of the small mass which attaches this film to the ring, and our experiment offers an curious example of this kind of action.

§ 302. Now, in the case of the bubble, as that of the plane film, since a new resistance develops from the progressive absorption of moisture, one is constrained to recognize that this resistance is generated in the two surface layers, or, in other words, that while the interposed liquid becomes more fluid, the surface layers become it less. Thus, indeed, is explained the liquid going down with difficulty and increasing slowness, and the film thickening freely by absorption.

But the increase in surface viscosity by a progressive addition of water necessarily supposes that the surface viscosity of the originating liquid is much lower than that of water, and that is what we actually found (§ 299).

But while the surface viscosity of the film which constitutes our bubble becomes increasingly strong, the tension changes very little (ibid), so that the ratio is growing.

Thus, on the one hand, because of the continuous absorption of water vapor, the film can, in no phase of its existence, arrive at being very thin, and, on the other hand, the ratio between the surface viscosity and the tension remains large enough to make tearings difficult until the film has assimilated a very great proportion of water. These two circumstances, it is seen, fully explain the long persistence of the bubble.

We stop one moment on the cause which finally brings the rupture. The ratio of the surface viscosity to the tension is admittedly growing; but one can claim that it does not grow enough compared to this viscosity, so that it ends up being insufficient for the maintenance of the film. There is, moreover, another cause to be assigned to the rupture: one knows that the very weak soap solutions break up spontaneously, which one recognizes when they are cloudy. This decomposition takes place after a variable time, but I believed I noticed that it occurs much earlier and for much less proportions of water when the solution was made hot: thus, for example, I had prepared, the same day and with the same soap, two solutions, one hot at 1/50, the other cold at 1/100; after cooling the first, I diluted them both up to 1/500; that which had been done hot clouded immediately, and the other remained limpid; one could bring the latter then, without it deteriorating, up to 1/1000, then to 1/2000 and, the following day, it was still limpid. Let us add however that another solution, also made cold and brought to 1/2000, was clouded less than one hour after its formation. However, in my various preparations of glyceric liquid, the soap solutions had always been made hot; one can thus believe that when the film which constitutes the bubble absorbs a great quantity of water, the soap which it contains breaks up, and consequently the bubble must obviously burst.

§ 303. I said (§ 106), in connection with the bubbles of glyceric liquid created in a closed vessel, that, to obtain the most duration, dimensions of the vessel were to be considerable relative to those of the bubble; and, indeed, I, had uselessly tried using a vessel of small capacity. I showed, moreover (ibid), that one went much further if one placed beforehand pieces of calcium chloride at the bottom of the vessel; but I added that one should not desiccate the atmosphere too much. The explanation of these characteristics appears extremely simple to me: a right relation is necessary between the cause of thinning and the cause of thickening; when the film finds much water vapor to be absorbed, the colors retrogress too early, and the liquid becomes aqueous in rather less time so that the bubble bursts; when, on the contrary, the quantity of vapor is insufficient, either because the vessel is small, or because the atmosphere is too dessicated, the film attenuates more, and breaks thus earlier by accidental causes.

§ 304. It remains to me to explain the singular fact of the liquids which provide a thick and tough foam, but refuse to develop in bubbles on the opening of a pipe (§§ 249 , 279 and 281). Let us suppose a liquid has a very strong surface viscosity; so that one can form notable bubbles of them, it will be needed, according to the remark of § 259, that the ratio of this viscosity to the tension is also very large; however let us imagine that it is not enough, but that it is very close to the limit beyond which the creation of the bubbles would start. Such a liquid, although not giving bubbles or while giving bubbles whose diameter exceeds that of the opening only by a few millimetres, will form, by agitation, a copious and durable foam. Indeed, since, under the conditions that we assign to it, our liquid is almost able to inflate in bubbles on an opening of 2 centimetres approximately, it must be formed easily in very small sheets such as those of which foam is composed; moreover, in consequence of the energy of surface viscosity, these sheets can be thinned only with extreme slowness; finally, because of this slowness, and of the relative weakness of the tension, tearings must be extremely rare in the sheets in question, in spite of the attraction operated by the small concave masses by which they are bordered; foam will thus be formed in abundance, and will persist a long time.

But albumin and saponin solutions which presentthe property with which we occupy ourselves, were appreciably in extreme cases of the generation of the bubbles, and their surface viscosities were sufficiently strong, since these solutions resulted from the mixture of liquids with enormous surface viscosities with water, which has already rather intense surface viscosity; these same solutions were consequently under the conditions as have just been discussed, and one will easily admit that it is the same for the gum arabic solution of § 249.

§ 305. As I pointed out at the end of § 288, for the liquids of our second category give, like all the others, from the film caps on their surface, one must conclude that it is not with the intrinsic viscosity of the surface layer that it is necessary to resort to explain the simple generation of the films: I tried to show, by the contents of Chap. VI, that when it is only about this generation in itself, without having regard with dimensions and persistence, one completely explains the phenomenon by interior viscosity and cohesion, but the whole of the current Chapter shows that, for the development of large and rather durable films, such as those of soap water, interior viscosity has only a very secondary influence. As for cohesion, it varies, one knows, in the same direction as the coefficient of the sum of the curvatures in the expression of the capillary pressure, a coefficient which, according to the research of Mr. Hagen and Dupre, is nothing other than the tension[179]; however the latter being much weaker in soap water than in pure water, it is necessarily the same cohesion; consequently it is not the intensity of this one which makes possible the creation of large bubbles, and, for this creation, properties of a very different nature must intervene: I hope to have, if not rigorously shown, at least made extremely probable, that these properties are surface viscosity and the tension; that, so that a liquid is easily extended into large-sized films and from a certain persistence, it is necessary: 1° a surface viscosity which notably surpasses the interior viscosity; 2° a relatively weak tension; 3° an all the more large relationship between these two elements which the first is itself more energetic.

[179] Several physicists admit that cohesion is proportional to the product $h\rho$ of capillary height by the density; in this case, as the tension is itself proportional to this product, cohesion and the tension would vary not only in the same direction, but still in the same ratio.

CHAPTER VIII.

Additional causes which influences the persistence of the liquid films. – Film shapes of very great duration. – History concerning liquidfilms.

§ 306. When one produced films with a liquid meeting the conditions pointed out above, their persistence is influenced by a certain number of additional causes which I will review.

The first consists of the small shocks which the agitations of the ambient air and the vibrations propagated by the ground communicate to the films. These small shocks undoubtedly act by overcoming the inertia and the resistance of friction of the molecules; they hasten thus the descent of these last, and, consequently, accelerate thinning; moreover, they cause, as I have claimed several times, the rupture of the very attenuated portions. That is partly why films are maintained in general much longer in a closed vessel; then, indeed, one of the causes of shocks, the movements of the air, is removed.

§ 307. A second cause is evaporation, when the liquid is susceptible. Evaporation, as I showed (§§ 253 and 255), produces two opposed effects, of which one tends to accelerate and the other to slow down thinning, because it withdraws without delay matter from the films, in addition the molecules which it removes are those which, pertaining to the extreme faces, would go down most quickly and would more or less share their excess speed with the subjacent molecules. The facts on which I was based would seem to indicate that the second effect, that of deceleration, prevails in general, from which would result the singular consequence that evaporation is rather favorable than opposed to persistence; however let us see:

The facts in question bring us back to water films, and the others with films of the second category having the opposite colors; however the first always burst before attenuating much, and, in the others, the attenuation of the meanest portions, i.e. the lower, stops soon, one saw, either by a continual surge of liquid coming from the thicker higher portions, or, on the contrary, according to Mr. Van der Mensbrugghe, by a surge of the liquid of the vessel going up without delay towards these higher portions, because of an excess of tension of those (§ 256); but now let us consider a film of the third category, a film where thinning can progress without obstacle, for example a soap bubble deposited on a ring. The effect of gravity becomes much less in such a film as it is attenuated (§ 109); consequently the quantities of liquid which, in successive equal times, would depart, under the action of gravity, the top of this film, would be decreasing progressively with the attenuation; but the successively removed quantities, in the same times, by evaporation, are appreciably equal; thus, it follows that if, at the beginning of the existence of the film, the second of these quantities of liquid may be only a fraction of the first, it becomes greater later, and then necessarily evaporation activates thinning. One even understands that, when the top thickness of the film becomes extremely tiny, evaporation can, by itself, cancel it at a point, and thus cause the rupture.

One sees, according to this discussion, that, in the films of the third category, the moment of rupture is hastened by evaporation; also our caps of household soap solution (§ 248) which, in an atmosphere saturated with water vapor, persisted several hours, lasted only four to five minutes when they were produced in the closed bottle, but without saturation of its atmosphere; if one had formed them in the entirely open air, no doubt they would have burst earlier still.

By removing evaporation by a process a little different from mine, Dr. Reade[180]

[180] *On a permanent soap bubble, illustrating the colors of thin plates* (PHILOS. MAGAZ., 1837, new series, vol. XI, p. 275).

produced plane soap water films being preserved beyond 24 hours. Here is how he operated: he introduced a small quantity of soap water into a lengthened flask that he then heated to 100° with a bath. When he supposed that the vapor produced inside had expelled all the air of the flask, he stoppered it hermetically, then, after having let it cool, he formed a transverse plane film there. By this means, one sees, the film is, like our caps, in a space saturated with water vapor; if thus the flask is set vertically, so that the film is horizontal and that thus the action of gravity is eliminated at the same time as evaporation, the only remaining thing to thin this film and to bring the rupture of it is the action of the concave surfaces of the small mass which borders it, plus the small shocks of the ground; it consequently must, like that of § 229, persist a very long time.

The glyceric liquid, we know, not only does not emit vapor, but absorbs, on the contrary, the moisture of the ambient air, and that is (§ 302) partly why the films of this liquid have such great persistence, even in the open air.

§ 308. In the third place, in the particular case of the glyceric liquid, the temperature has a notable influence; indeed, when one employs this liquid in winter, its films show persistences much more unequal than in summer.

§ 309. In the fourth place, since gravity immediately pulls the liquid to the bottom of the films, it is clear that by removing, in one way or another, the action of this force, one must increase persistence. The condition in question is filled obviously, as I pointed out several times, with regard to a plane and horizontal film; but, to judge its effectiveness, it is necessary to compare, from the point of view of the duration, such a film with a film tilted or vertical, formed of the same liquid and having the same dimensions. I carried out this comparison on films produced in the open air, in wire rings 7 centimetres in diameter, with the household soap solution at 1/40. For the horizontal film, I employed the process of § 301; as for the vertical ring, it was carried by a simple stem extruded from a point of its contour in the prolongation of a diameter, and attached under the bracket by the other end of this stem; to form the film, I held under it a vessel full of the liquid, in which I immersed it entirely, and then lowered the vessel. With each ring, the experiment was repeated twenty times.

In the horizontal ring, the films persisted 16″ to 30″, and the average was 25″; in the vertical ring, the extreme values were 9″ and 18″ and the average 13″. Thus, among the additional causes with which we occupy ourselves, it is necessary to include the position, or rather, the slope of the films.

With some liquids, one cancels the action of gravity by developing the films within another of the same liquid density, and then the slope is immaterial; thus the oil films, which attenuate so quickly and last but little in the air (§ 247), acquire, on the contrary, a great persistence when they are generated in the alcoholic liquid (§§ 218 to 224).

§ 310. In the fifth place, the assemblies of films are always maintained much less longer than the formed shapes of a single film, such as the spherical bubbles and the surfaces with zero mean curvature formed in simple closed contours. The explanation of this fact rests on considerations which I have already explained (§§ 219 and 220), but that I will reproduce in a more complete manner here.

A liquid film adherent to a solid wire is, we know, joined to this wire, or rather to the liquid layer which wets it, by a small strongly concave mass with transverse curvature, which exerts a ceaseless pull on the liquid of the film; a cause, moreover, for progressive thinning, and, consequently, for the destruction of the shape. This cause exists in any adherent film shape with a wire or solid wire; but, in the systems formed inside the polyhedric frames, a cause of comparable nature and more energetic is added to it further. All these systems, indeed, contain liquid edges, which come four by four to meet at liquid points, and of which several stick by their other end to the solid wire; however, on these edges as at their extreme points, there is also, we know, a small strongly concave surface. Those which belong to the edges are only, as those which run along the solid wire, concave in one direction, and, consequently, their action to thin the films is of the same order; but, at the meeting points of the liquid edges and at the points where these edges end at the solid wire, small surfaces are concave in all directions, so that their capillary suction is approximately twice stronger still, and thus the surge of the liquid towards these same points must be much larger. In the systems in question, the thinning of the films must thus be faster, and consequently these systems must last less longer, as experiment shows. However, when they are formed in the air, one notices no gradual increase in thickness either in the liquid edges or in their meeting points; but, as the liquid flows there, it is pulled by gravity to the bottom of the system.

The persistence of each one of these systems is, moreover, very variable, which one understands easily, because, in consequence of their complication, small accidental causes can act sometimes more, sometimes less, to bring their destruction; thus, in general, that which is maintained longest, is simplest, such as the tetrahedron.

Thus the combination of films in a system has a number of additional causes which modify persistence.

§ 311. In the sixth place, I recalled (§ 244) that the films persist in general more especially as they are less large. Although the fact was already indicated for the case of soap bubbles (§ 321), I do not believe it useless to recall some results obtained by me:

1° I created, with the glyceric liquid, film systems in two similar triangular prismatic frames, of which one had all its edges 7 centimetres long, and the other of half the size. Each one of these frames was fixed, by the tail of its fork, under a bracket, so that its side edges were vertical, and, to form the system, I used the same process as with regard to the vertical ring in the experiments of § 309. With each frame, I repeated the observation seven times; persistences appeared very variable, with the large frame, the longest were $32'$ and $37'$, and, with the small one, $60'$ and $75'$.

I also tested, in the same way, the tetrahedral systems of two frames, one of 8 centimetres edge, the other of 5; I also repeated the observations several times; but, for the large frame, the moment of rupture escaped my attention, except with regard to the longest persistence, which was 2 hours; for the small one, the longest persistence was 2 hours 36 minutes.

2° I produced, still with the glyceric liquid, horizontal plane films in two wire rings, one of 7, the other 2 centimetres in diameter; in the first, the maximum duration was one hour approximately, and, in the second, it exceeded 12 hours.

3° A bubble 10 centimetres in diameter, inflated with the household soap solution, was deposited on a ring in the interior of a vessel of which the atmosphere was saturated with water vapor; it persisted about an hour. I substituted there a bubble of the same liquid, but of half the diameter, and that disappeared only after 2 hours. It was seen besides (§ 107) that bubbles 30 centimetres in diameter, formed by Mr. Böttger with his liquid with the palm oil soap, persisted only 5 to 10 minutes, while bubbles of approximately 8 centimetres could last 20 hours.

The larger duration of films of lesser dimensions is, as I said, that which usually takes place; but sometimes this influence of size does not appear: I made, for example, several series of observations on films of soap solution produced in rings of 10, 7, 2 and 1 centimetres diameter, and the average durations did not present notable differences.

If persistence decreases in general when the films increase in size, that holds simply, I think, because the larger a film is, the more there are chances that one or the other of its points yields to some cause of rupture; but it is necessary to take account of another influence, which acts in the opposite direction: the thinning due to the pull of small concave surfaces which run all along the contour of a film is necessarily all the more slow, with equality of contour, when the film has more surface area, and, with equality of surface area, that it has less contour; one easily deduces that, in a circular ring, the slowness of thinning grows with the diameter. If thus this last influence acted alone, persistence would be increasing with the size; however, it is understood, it can be made, in certain circumstances, that the two contrary influences of which I speak compensate more or less.

§ 312. In the seventh place finally, it is still necessary to account for the nature of the solid to which a film adheres, and the surface quality of this solid: we know, for example, that if one did not oxidize the wire rings or frames, the films of glyceric liquid that one creates on them break immediately, or have only a very short duration. I announced, in §§ 124 and 125, another fact of comparable nature: a bubble of glyceric liquid of 2 centimetres diameter, inflated on the opening of a glass tube and locked up in a small bottle, was maintained for 24 hours, while other bubbles of the same diameter, formed of the same liquid and locked up in the same bottle, but inflated with the opening of an oxidized iron tube having the same external diameter as the tube of glass, only persisted to a maximum of 14 hours.

Abbot Florimond[181] announced that one can inflate much larger soap bubbles with a glass pipe than with a clay pipe; he attributes this difference to the clay absorbing or retaining the liquid, so that the incipient spherical film develops only at the expense of its thickness, while with the glass pipe, the liquid slips easily to the extreme edge of the opening, and the bubble acquires a certain volume thus before the film which constitutes it thins.

§ 313. It follows from this examination of all the additional influences, that with a film of a given size, one will obtain the greatest persistence if this film is plane, horizontal, attached by all its contour to a glass wall, protected from any evaporation, sheltered from agitations of the ambient air, and, as much as possible, from vibrations propagated through the ground. However all these conditions were satisfied with regard to the film with glyceric liquid 7 centimetres in diameter about which I spoke in § 229; it persisted 18 days, and probably burst only in consequence of a shock through the floor. The same conditions were satisfied with regard to the soap water films in the experiment of Reade (§ 307), and, we saw, the persistence of these last was more than 24 hours.

§ 314. The beauty of the film shapes of glyceric liquid naturally inspires the desire to have the same shapes completely permanent. For one of them, the sphere, one achieves this goal with molten glass; one knows, indeed, that one can blow spherical bubbles out of glass, bubbles which, once cooled, keep indefinitely; one can even attenuate the film which constitutes them so much that it shows the colors; but the creation, with the same substance, of other shapes, especially those which consist of an assembly of films, would offer difficulties, and, in any case, would be not very convenient.

The first idea which arises is to employ a liquid whose films solidify by simple cold evaporation, such as collodion, an albumin solution, etc; but, with such liquids, I arrived at some results only by limiting myself to shapes of very small dimensions. The liquid which gave me the best success is a solution of gutta-percha in carbon bisulphide: with this solution, I obtained a very pretty small system in a cubic frame whose edges were two centimetres long; this small system was preserved several months, after which it was reduced spontaneously to powder. I tried unsuccessfully with a frame of side three centimetres. The solidified gelatine shape about which I spoke in § 141, adhered to a frame whose sides were, as Mr. Schwarz was good enough to inform me,

[181] Newspaper *Cosmos*, 1862, vol. XX, p. 72.

approximately three centimetres and half long; but this shape consisted of a single film, and Mr. Schwarz produced it only one particular time. To try to arrive, in the case of assemblies of films, at results of some size, I said to myself that it was necessary to resort to substances which, as well as glass, are liquid only hot, and to seek of them one which met the double condition to not require a very high temperature to be melted, and to let itself develop into films of sufficient extent in the molten state.

One will see, further, that Morey obtained, with resin, lengthened bubbles reaching the size of an egg, made of a film thin enough to show colors, and which appeared to be indefinitely permanent.

Mr. Böttger[182] found that with a mixture of 8 parts of rosin and one part of purified linseed oil, melted in a bain-marie and maintained at a temperature of approximately 97°, one easily inflates large bubbles which persist a long time; only he does not indicate their duration. With this same mixture, Mr. Rottier tested, at my request, the creation of the film system of a triangular prismatic frame whose bases had 4 centimetres on a side, and of which the height was 7 centimetres; the system was always formed very well, and certainly kept its integrity a great number of hours; but one always found, the following day, one of the films perforated.

Mr. Mach[183] obtained, with melted purified rosin, without mixture of oil, the film system of the regular tetrahedron in a frame of 5 centimetre on a side; but he did not observe the duration of it, the experiments which he had in mind requiring that he leave the system.

I succeeded in a nearly complete way by means of a mixture of one part of pure gutta-perched and 5 parts of rosin, maintained at a temperature of approximately 150°. I had purified the rosin beforehand by melting it at a sufficient temperature, and waiting until the small quantity of spirits which it still contained was released in the form of small bubbles, and when all the solid impurities had fallen to the bottom. Mr. Donny agreed to do the experiment at his laboratory: the frame was that of a cube of side 5 centimetres; the film system was not formed without a certain difficulty, and, when it was obtained complete, it was more or less irregular; but, by then keeping it for a few moments in a heated drying oven at approximately 70°, and turning it in various directions, one saw the irregularities being erased. In the system such as it was shown after this last operation, the films were extremely transparent, but the edges were not very smooth, and differed, under this report, from each other. This same system was very solid, and was preserved for more than two years, I think; after this time, a light shock reduced it to fragments, from which it should be concluded that the constitution of its films had slowly deteriorated. I believe that one would succeed better still and that progressive deterioration would be less, if one employed a little stronger proportion of rosin. Let us point out here the curious creation described by Mr. Mach (§ 210bis) of the system of the regular tetrahedron out of rubber films.

§ 315. Let us finish this part of our work specially devoted to liquid films with a brief discussion of work that, to our knowledge, has been done on these same films apart from our own research, until the end of 1869.

One reads in the *Petites chroniques de la science* of Mr. Henry Berthoud[184] “the museum of the Louvre has an Etruscan vessel of highest antiquity, coming from the Campana collection, and on the sides of which are represented children who are blowing on pipes and having fun making soap bubbles.”

It thus appears that the ancients knew the complete film spheres obtained by blow-

[182] *Beiträge zur Physik und Chemie*. Frankfurt A. M., 1838, p. 13.
[183] *Ueber die Molecularwirkung der Flüssigkeiten*. (Bullet. of Acad. of Vienna, 1862, vol. XLVI, 2nd section, p. 125).
[184] Year 1866, p. 265.

ing at the end of a tube; nevertheless their works do not contain, that I know, any references to these bubbles; they speak only about the film caps developed on the surface of water:

> Intumuit sic, ut pluvio perlucida cœlo
> Surgere bulla solet ...
> (OVIDE,*Métamorph.*, liv. X, v. 732.)
>
> ... offensa bulla tumescit aquæ.
> (MARTIAL, book VIII, epigr. 33, v. 18.)

§ 316. According to an assumption advanced initially by Halley, visible water vapor, that which constitutes clouds and fogs, would be made of very small hollow bubbles, to which he gave the name of vesicles. This assumption, founded mainly on the apparent lightness of the clouds and the fact that one never observes a rainbow in a cloud which is not resolved into rain, is well-known to physicists, and consequently I will limit myself here to mention it; set in vogue by Saussure, it has been strongly fought since, and today it hardly has any partisans left (§ 235)[185].

§ 317. Boyle appears to be the first who directed the attention of the scientists to the colors of thin films, and especially of liquid films. In a work[186] of 1663, he writes as follows:

"To show the chemists that one can make appear or disappear colors where there is neither increase nor change of the sulfurous, saline or mercurial principles of the bodies, I did not resort to the iris produced by the glass prism, nor to the colors which one sees, on a serene morning, in those dewdrops which reflect or refract suitably towards the eye the rays of the light; but I will point out to them what they can observe in their laboratories: because if a chemical essential oil or concentrated spirit of wine is shaken until bubbles develop at its surface, those offer brilliant and varied colors which disappear all at the moment when the liquid which constitutes the films falls down in the remainder of oil or spirit of wine; one can thus make it so that a colorless liquid shows various colors and loses them in one moment, without increase nor reduction in any of its hypostatic principles. And, to say in passing, it is worthy of remark that certain bodies, either colorless, or colored, being brought to a great thinness, acquire colors which they did not have before; indeed, besides the variety of colors that water made viscous by soap acquires when it is inflated in spherical bubbles, terpentine, when air in a certain manner is insufflated there, provides bubbles variously colored, and, although these colors disappear as soon as the bubbles burst, those would probably continue to express varied nuances on their surface, if their texture were sufficiently durable."

Boyle quotes as an example of these permanent colors those which he saw on extremely thin films of blown glass.

§ 318. In 1672, Hooke communicated to the Royal Society a curious Note[187], of which here is almost a whole translation:

"Several small bubbles were inflated, by means of a small glass tube, with a soap solution. One observed easily that at the beginning of the blowing of each one of them, the orbicular liquid film, which imprisoned a sphere of air, was white and limpid, with no appearance of color; but, after some time, the film thinning by degrees (part of its substance going down to the bottom and another part dissipating itself in the air by evaporation), there is born on its surface all the varieties of colors which one can

[185] To consider the current state of the question, see the inaugural dissertation of Mr. Kober, published in Jena in 1872, and entitled: *Ueber die angebliche Bläschenform de Wassers bei seiner Condensation.*
[186] *Experiments and observations upon colors.*
[187] Birch, *History of the Royal Society*, vol. III, p. 29.

observe in the rainbow... After these colors had undergone their last changes, the film started to appear again white, and then, in this second white film, there appeared, at the top and at the bottom, holes, which increased gradually in diameter, and several of them merged, until at the end they became very large. It was singular to see how these holes were driven here and there, by the movements of the ambient air, on the sphere of imprisoned air, without the bubble losing its orbicular form or falling. It is singular also that after that, when the bubble bursts, its rupture takes place with a species of explosion, by dispersing its parts in a kind of dust or fog. It is more singular that these portions of the bubble which appear like holes, by being driven back and forth on the surface of the air sphere, change form, and from circular become elliptic or affect corrugated shapes. It is more singular still that, though it is very certain that the enclosing air and the enclosed air have surfaces, however, by any of the means of which I made use, they presented neither the reflection to me nor the refraction which the other parts of the imprisoned air express. It is rather difficult to imagine what curious web or invisible body could thus maintain the shape of the bubble, or what species of magnetism could prevent the liquid film from falling or parts of the enclosing air and the enclosed air from uniting."

§ 319. Also in 1672, Newton, among other experiments on the recombining of the light, described the following[188]: one projects a solar spectrum on the wall of a darkened room, and one exposes to the light that it reflects a white foam formed on a liquid; if one observes this foam very closely, or better through a magnifying glass, one distinguishes, on each small film, the image of the colors of the spectrum; but if one moves away, the foam appears entirely white. Or even if, after having produced foam on soap water, one waits until the films start to burst, those which remain show, when one looks at them closely, spontaneous tints in varied colors, but from afar only white is seen.

All physicists know that Newton used soap bubbles in his admirable research on the colors of thin films. The experiments that he carried out by this means, and which are described in his *Optics*[189] (year 1704), are too well known for me to point them out here; I will insist only on the following points: Newton employed, not complete bubbles, but film caps resting on liquid; he observed the black spot at the top, the small colored spots which go up and go down on the cap, as well as the little black spots which climb up to the top spot, with which they merge; he noted the appearance of the blue of the 1st order only with one solution very charged with soap, and, in this case, he sometimes saw the blue in question invading all the cap; finally, one can infer from his description that the uniformity of color, and consequently the uniformity thickness of the film, were shown sometimes also for colors other than the blue of the 1st order.

§ 320. In 1730, Gray[190], wanting to assure himself that one could conduct electricity with liquids, suspends with insulating wire a pipe whose opening is turned downward, then, after having inflated a soap bubble on this opening, he approaches the open end of the stem of the pipe with a tube of glass electrified by friction, and he notes that a light body placed beforehand under the bubble is attracted by it.

§ 321. Leidenfrost, who, as one knows, discovered the phenomenon of the spheroid state of liquids, devotes a great portion of his Mémoire[191] where he expounds on this subject, to a detailed study of soap bubbles. This work, published in 1756, and about

[188] *Answer to some considerations upon his doctrines of light and colors* (Philos. Transact., vol. VII, p. 5084).

[189] Book II, 1st part, obs. 17 to 24.

[190] *Philos. Transact.*, vol. VI, part 2, p. 19.

[191] *De aquae communis nonnullis qualitatibus tractatus.* Duisburg.

which I have already said several words in § 147, is a singular mix of clever experiments and judicious deductions with some observations which must be inaccurate, and of the odd opinion whose error is obvious today.

If Leidenfrost deals with soap bubbles, it is, one would hardly believe it, partly to provide a later proof in favour of a proposal which he previously supported, that water can pass to a solid state without the action of cold. For him, indeed, a soap bubble, when one removes with the finger the drop which remains sometimes suspended there, i.e. the liquid exceeding the precise quantity necessary to its formation, has the properties of solids: 1° it has by itself, like them, a given shape; 2° just as one contains a liquid in a bottle of glass, in the same way also one can contain in the bubble, not a liquid, because of the brittleness of the film, but tobacco smoke, for example, smoke which remains there perfectly imprisoned as in an envelope of glass; 3° the bubble, removed from excess liquid, is dry, because it does not wet the finger which touches it; 4° finally, if one gently deposits on such a bubble a small water drop, it, far from mixing with the substance of the film, slips to the bottom, as it would slip on glass, and fall then or be removed with the finger. According to that, as a solid could not run, Newton must have been mistaken by attributing the colors in the bubble to the film being thinned by the gradual flow of the liquid which constitutes it.

This solid nature of a film is explained in the following manner: in liquids, molecules are attracted on all sides, so that they are also mobile in all directions, while, in solids, there are particular centers of attraction which make the molecules group in a certain way, as one sees in crystals; from whence it is enough for a certain movement, from a certain direction impressed on the molecules of a liquid, to cause the arrangement which makes the body pass to a solid state. Thus spiders and caterpillars, while expelling their silk, in a common direction, the molecules of a liquid substance change into solid matter, and that is also what occurs when a bubble is inflated.

In connection with a bubble full of smoke, Leidenfrost said: ".... on a bubble thus made opaque by means of an interior smoke, the colors described by Newton are reflected with much more clarity, so that the bubble resembles a brilliant star; but all this glory disappears at the moment of rupture: the fetid smoke which escapes then arises from the ordure with which the bubble was filled, and the latter thus offers to us a striking emblem of the splendid poverties of humanity."

He announces the great elasticity of bubbles, which always regain their spherical shape spontaneously as soon as the external cause which had made them lose it has suddenly ceased.

One owes to him the observation of the significant fact that bubbles persist much longer in a closed vessel than in the open air: his were approximately 5 centimetres in diameter, he inflated them inside a bell jar, and they were maintained beyond one hour. He attributes this great persistence to the bubbles being then withdrawn from the agitations of the ambient air and all the accidental causes of rupture. He states, moreover, anonther fact, that the bubbles last longer especially as they are smaller: he made some which hardly had 1/3 millimetre diameter, and they were preserved more than two days in the open air and during the summer.

Also, he was the first to notice that when, after have inflated a bubble at one end of a tube, one let open its other end, the bubble decreases gradually in size, with an accelerated speed, until vanishing, by expelling the air it contains through the tube; he says, moreover, that if one filled the bubble with smoke, one sees the latter leaving the tube like a chimney. He concludes that the sphere constantly makes an effort to contract. He adds the following observations:

The bubble, at the beginning of its formation, as long as it does not show sharp colors, is at the same time so soft and so tough, that one can penetrate and withdraw a solid point with impunity, even a blunt one; the opening is always spontaneously closed again. But the more the colors become clear, the more the film becomes rigid, so that if it is pierced, it breaks. It is in the black spots that this rigidity is extreme: there the least contact of a point of a needle causes rupture, then the bubble bursts with a perceptible noise, and is dissipated in an infinity of very small projected parts on all the sides up to three or four feet of distance. This phenomenon is noted best in a sunbeam; it is completely similar to that which Prince Rupert's drops present. Thus the bubble, in addition to its contractile force, has at the same time an opposed force, an explosive force. This last force always acts from inside outwards, because if inside a bubble one inflates another, rupture of this one bursts the external one, while if the external one breaks first, it leaves the other perfectly intact. There are thus in a bubble, two contrary forces, one centripetal, which resides especially in the colorless portions, the other centrifugal, which has its seat in the colored portions, and which is at its maximum in the black spots.

The explosive force is all the more intense as the solution contains a stronger proportion of water, because the bubble bursts all the more early and all the more launches far the particles into which it is dissolved; at the same time the contractile force is all the more weak. On the contrary, the more soap there is, the less the explosive force has intensity, and the more energetic is the contractile force[192]. From there follows the consequence that the explosive force comes from water, and the contractile force from the soap, or rather of the oil of this last, bubbles varying in persistence according to the nature of the oil which was used in the composition of the soap.

As soon as the film which constitutes a bubble passes to a solid state, it produces there a separation of its elements, and then it is formed of three superimposed membranes; the external one consists of the oily part of the soap; it is what has the contractile force; it protects the two others against rupture, because while it still extends on the totality of the bubble, it is difficult to break. But soon this external membrane loses its evenness, opens at the top, goes down while becoming gradually thicker downwards, and thus leaves uncovered the higher part of the whole of both others; one must conclude from this progressive descent and this accumulation at the bottom, that the membrane in question is not truly solid; finally it is to that the colors are due. The intermediate membrane, which is saline and partly earthy, always appears white, but without much brightness; it opens then in its turn, and exposes portions more interior. This one is of an extreme transparency, reflects no color, and is, so to speak, invisible, and appears as a black spot; it is entirely aqueous.

To establish that the external membrane is of an oily nature, Leidenfrost bases himself especially on the successive appearances which the colored portions have in a bubble inflated with a solution containing little soap; he affirms that, when all the substance of this membrane is gathered at the bottom of the bubble, its appearance shows that it is made of an oily matter. He indicates, moreover, the following experiment: if one soaks in the solution the opening of a broad tube, then one withdraws it, one finds it occupied by a plane film, and if one places this film vertically, one is not long in seeing there being born the colors which express in an indubitable way the separation of the three membranes. Leidenfrost thus produced plane films, and observed the colored bands. It is again, he says, for the same reason that the colors appear earlier and more brightly at the low temperatures of winter, oil separating easier in the cold. In his opinion, if one employs a liquid in which the oily, saline and aqueous parts are joined

[192] These last observations are obviously partly inaccurate: the contractile force, i.e. the tension, cannot increase, and must rather decrease when the proportion of soap (§ 299) is increased; Leidenfrost, who does not give any measurement in this respect, judges undoubtedly more or less of intensity of the force in question only by more or less persistence of the bubble.

with more force than in soap water, so that the separation of the oily element cannot be carried out, one does not distinguish any more colors. As examples of such liquids, he quotes especially the saliva of a fasting healthy young man, and a soap solution to which one added a little spirit of wine. He infers that, most probably, perfectly pure water can never give colored films.

He indicates as a final proof of the error made, according to him, by Newton, with regard to the generation of the colors, that the black spots, instead of shading imperceptibly in the white which surrounds them, are clearly rimmed on their borders, and that they are born not only on the top of the bubble, but also on the sides.

He measures, by a simple means independent of the colors, the thickness of the film at the time when the bubble has been just formed. He uses, to inflate it, a tube of a thermometer, and finds that while employing only the quantity of liquid which rises in this tube by the capillarity, the bubble, whose maximum diameter is two inches, does not carry any suspended drop; he regards the thickness of the film as being then uniform, and he evaluates it according to the diameter of the bubble and the weight of the liquid which the tube contained; he obtains in this manner 1/15624 of an inch (approximately 1/600 of a millimetre).

Always starting from his principle of the three membranes and the idea that most of the interior is not formed of pure water, he calculates, by the same process, the thickness of the latter, knowing the proportion of water in the solution, and finds this thickness equal to 1/17577 inch (approximately 1/670 of a millimetre); however, as its bubbles have their maximum diameter, so that they burst and are reduced in a kind of dust if one continues to blow, he concludes that, until this limit of thinness only, the water molecules can remain united, and that thus the diameter of one of these molecules does not exceed the value above. He deduces from the same method still that the diameter of the molecules of oil is not higher than 1/303851 of an inch (approximately 1/12000 millimetre). Leidenfrost thus had the idea to seek upper limits of molecular diameters.

He claims that the film which constitutes a bubble has pores of a notable size, and he tries to prove it by the two following observations, which are obviously erroneous: when one starts to inflate a bubble, most of the air that one made enter escapes by these pores, because, if one blows with force, a perceptible draught is felt outside the bubble; introduced smoke does not pass thus to the outside, but if a bubble which does not contain that air is maintained above the flame of a lamp, the black smoke of this one penetrates through the film, and makes the interior air opaque.

Leidenfrost sees films and bubbles everywhere: for him, the atmospheric air is composed of small oils, or rather of small aqueous plates; it is a kind of foam which rose from the surface of water; finally, animals and plants are formed from small soap bubbles and small tubes of the same matter. One will allow me to overlook the reasons with which he supports such opinions.

§ 322. In a Report probably written in Swedish at an earlier time, but which appeared in French[193] in 1773, Wilke describes what one observes on soap bubbles inflated at a very low temperature. He writes as follows: “If one forms these bubbles in a enough cold air for them to freeze, one sees small particles of snow which condense and float freely on the bubble, in the form of small stars.... The time most suitable to blow the bubbles is the moment when the soap water starts to freeze. The stars appear initially in the form of small points, from where one then sees rays leaving little by little. These stars are usually hexagonal. One sees the same star passing through a series of different shapes, whose majority were already observed in natural snow.... The

[193] *Sur la forme de la neige* (JOURN. DE PHYS. DE L'ABBÉ ROZIER I, p. 106).

more the mixture is clear and the soap dissolved, the more the stars are delicate and numerous; they grow then promptly, and the bubbles burst. Those which are done with a thicker mixture, are less spangled; but they last longer, and they are better observed, though the shapes are less distinct.... Those who will repeat these experiments will discover there an infinity of small curious and amusing details. ”

§ 322bis. In 1782, Cavallo[194] communicated to the Royal Society of London an experiment consisting of inflating soap bubbles with hydrogen, bubbles which rose in the air. They were, as he says, the first airships.

§ 322ter. Let us pass to the current century. In 1819, Belli has mentioned[195] the following fact: when a small water jet is launched obliquely upwards by an opening located under the surface of water, all the space between this surface and the curve of the jet is occupied by a liquid film, and the jet rises less high than if the opening opened in the air.

§ 323. In 1820, Morey announced[196] that one can inflate melted resin bubbles whose dimensions reach those of an egg, and which have colors. In his opinion, one obtains thus, in general, a line of bubbles of which each one is attached to the following by a thin string; he adds that he has preserved some for eight months without them undergoing deterioration. He tells then that a small girl ran one evening towards him and showed him a similar perfectly regular line of from 22 to 23 small bubbles, having each one approximately a third of inch (8mm) length and a quarter of inch (6mm) of width; the thin intermediate strings had, in length, less than one eighth of inch (3mm). Morey declares that he does not have any idea of the cause which produces this alternative succession of bubbles and strings. We will further occupy ourselves with this below (§ 501).

§ 324. The years 1819 to 1844 provide us a succession of extremely remarkable Memoirs of Fusinieri[197]. These Memoirs contain, like that of Leidenfrost, excellent observations and a not easily acceptable theory. Here, with regard to the liquid films, is a summary of the research in question; I write it, not according to the same Memoirs, not having had them at my disposal, but according to a detailed talk that Mr. Dal Pozzo di Mombello gave, in a work printed in Foligno in 1866, and entitled: *La Dinamica molecolare secondo Fusinieri e Reichenbach*.

Fusinieri, whose work I did not know when I carried out my experiments on the small film caps, had clearly recognized the inversion of the colors in the caps of some liquids developed in the open air, the extreme speed with which this kind of coloring occurs, the shimmering movements of the zones, larger persistence, for these same liquids, when evaporation is free, etc

He was the first to form large plane films: in order to study the succession and the arrangement of the colors, he immerses in a soap solution the edge of a glass bell, which can have up to 15 centimetres in diameter, then withdraws it, and finds it occupied by a plane film; he lays down the bell close to a window, so that the film is vertical and well lit, and he excludes, by means of black screens, any foreign light. Under

[194] *The elements of natural and experimental philosophy.* London, 1803, vol. IV, p. 319.

[195] *Di alcuni fenomeni prodotti net moto de' liquidi dail' attrazione molecolare*. (Journ. de Brugnatelli, 2nd decade, vol. II, p. 232).

[196] *Bubbles blown in melted rosin*. (Journ. of Silliman, 1st series, vol. II, p. 179).

[197] *Ricerche sui colon delle lamine sottili e sui loro rapporti coi colon prismatici* (Journ. de Brugnatelli, 1819, vol. II, p. 319).

Memoria copra i fenomeni chimici delle rolls sottili (ibid, vol. IV, 1821, pp. 133,209, 287, 380 and 442).

Della forza di repulsione che si sviluppa fra le parti dei corpi ridotti a minime dimensioni, ossia del calorico di spontanea espansione in lamine sottili (ibid, vol. VI, 1823, p. 34).

Corne la forza repulsiva delta materia attenuata agisca all'atto della rottura di bolle o lamine piane di soluzione di sapone (Ann. delle scienze del Regno Lombardo-Veneto, vol. XIII, 1844, p. 213 and app.).

these conditions, the phenomena are of a very great regularity, and Fusinieri describes carefully the formation and the succession of the colored horizontal bands, as well as the appearance and the extension of the higher black segment. For these experiments, he also produces plane films of soap water in a metal ring from 5 to 7 centimetres in diameter, which he installs vertically under a glass bell, in order to shelter it from agitations of the air; when this ring does not have more than 5 centimetres, the black ends up invading the totality of the film.

He examines the films of a great number of liquids, as well when they are produced by spreading out on another liquid, as when they have their two faces in air. For this last condition, he forms them, on the one hand, in the state of small caps, and, on the other hand, in the plane form within small metal frameworks; he studies with a meticulous care all the characteristics which these various films present, and from the whole of his observations he deduces the theory of which we will give a brief account.

Let us say initially that he names *wedge-shaped films* those whose two faces are approaching so as to form a very acute angle; such as, for example, in a vertical film where a black segment has formed, the portion made up of the whole of the colored zones; he names *capillary corners* the small masses with transverse concave curvature which attach either a plane film to the solid framework, or a film cap to the surface of the liquid. These terms admitted, here are the principal points of the theory in question, with some of the facts on which it is based:

A droplet of an oil or of a combustible liquid which does not mix with water (essential oils, carbon bisulphide, etc), extends out into a thin film as well on a surface of mercury as on water; only, all things being equal, the phenomenon is carried out with less speed on mercury; but, in both cases, it is too fast to attribute it to gravity; one cannot regard it either as due to an attraction of one of the liquids for the other, because this attraction would act only in normal directions on the surface of the subjacent liquid, and not in that of the surface plane; moreover, it should be exerted with more energy, and to give rise thus to a faster and larger extension on mercury, because of its high density, while it is the contrary that one observes. It thus should be recognized that the extension results from an interior cause to the liquid of the droplet, from a spontaneous tendency to dilation, a tendency which can happen only with the development of heat; if one wants, this tendency is born from the ordinary repelling power of heat, which acts more freely in a mass thus attenuated.

When a film or a portion of a film is wedge-shaped, the force in question drives out the liquid molecules towards the edge of the corner, in normal directions to it, and, all things equal, with all the more energy when the corner is less acute.

If no obstacle is opposed to this action, it produces simply the extension of the film; it is the case of a liquid droplet deposited on another liquid. If some cause blocks the extension, different phenomena occur: in certain circumstances, visible currents of liquid are detached continuously from the edge of the corner following the directions indicated above; such is the case of a film cap presenting opposite colors; in such a cap, there are two corners opposite one another, being the capillary corner which connects the cap to the liquid on which it rests, and the colored portion of the film, of which the thickness is growing toward the the top; each one of these corners tends to drive out a current of liquid towards the other, but the capillary corner being much less acute, its action wins, and there is one current, directed upwards; it is this which generates the higher corner, and supports it against the action of gravity; it is also what repairs the loss of liquid unceasingly that the upper corner loses by evaporation.

If one covers with a glass plate the cup where the cap was developed, and if things are laid out so that the top of this cap is only at a small distance from the plate, the inversion of the colors disappears, and the cap bursts earlier: it is because the emanated vapor of the film cannot escape any more, and exerts, by its intrinsic heat, an action neutralizing the expansive force of the film, so that gravity takes again its rights. This influence of the presence of a solid plate above the cap comes, notice, to the support of the theory of Mr. Van der Mensbrugghe (§ 256) on the inversion of the colors.

In other circumstances, not only the later extension of an already wedge-shaped film is blocked, but there is, moreover, an obstacle to the emission of the currents above; it is what takes place, for example, with regard to a vertical or tilted soap water film, when the black zone develops, this zone opposing a resistance to the production of the currents in question. Then, in the wedge-shaped portion, the expansive force of heating not being able to cause neither the general later extension nor an expulsion of currents, causes partial extensions alternating with swellings: such are the tadpoles; the head of those is made of a small thinned circular portion surrounded by a small projecting pad, which itself is surrounded by a thinned narrow ring, and their tail presents the same alternations of depressions and projections. As well in the depressed portions as in the inflated portions, the expansive force of heating draws aside the molecules, and consequently the whole tadpole is specifically lighter than the surrounding liquid; thus all have an upward movement in the film.

Fusinieri again applies his principle to the fact of the instantaneous disappearance of the liquid films which break. According to him, when a film breaks, and thus the bond of the viscosity, which maintained the liquid in a film state, is removed, the expansive force, directed before in the direction of the surface of the film, acts in all directions, transforms the liquid into vapor, and gives the molecules violent movements of projection, accompanied by decomposition with releases of gas; one assures oneself of this release in what, after the rupture, one observes, on the solid edge to which the film was attached, a certain quantity of tiny hollow bubbles.

Fusinieri points out that one cannot attribute this kind of explosion to the compression of the air imprisoned in a bubble, because the same phenomenon occurs with regard to plane films, and besides one does not see how an envelope also not very resistant would be able to contain gas compressed enough to for such a purpose.

Certainly, the theory of Fusinieri cannot be allowed; but it is clever, is presented with skill, and is supported with a crowd of small facts of which we could only indicate the most salient. The true theory of all the phenomena of movement which appear in liquid films does not exist yet; but the physicist who will want to seek it, will have to necessarily study the work of Fusinieri, if he does not want to expose himself to describing as new already known characteristics.

Let us quote, in finishing this analysis, a significant and curious observation: Fusinieri produces a vertical film of a concentrated enough soap solution, within a rectangular metal framework of 8 centimetres base and 2 height, and he covers it with a glass bell.

Under these conditions, in addition to a series of movements and changes of colors which offer small colored portions, he notes the following phenomenon: when the black zone develops, it is contiguous to a white zone; but from its higher edge small yellow droplets go down followed by long tails, the white is destroyed thus gradually, and the black is then bordered by the first order yellow; this is destroyed in its turn in the same way, but the downward droplets are crimson; the following zones undergo, one after the other, a similar destruction, until the black is contiguous to a third order zone. This point reached, the colored portion of the film is thinned by the action of gravity, the colors of the first orders reappear there, then are successively destroyed as previously.

§ 325. In 1829 or 1830, Pfaff[198] studied, like Wilke, the effects of congelation on

[198] *Ueber die krystallinischen Verhältnisse des Dunst—Blättchens* (Mem. of Acad. of Munich, 1829-30, p. 77).

soap water films. During one very cold winter, he develops, while operating in a heated room, a transverse film inside a bottle from 4 to 7 inches (10 to 17 centimetres approximately) in diameter, hermetically stoppered; he then transports this bottle outside a window, where the temperature is approximately 10° below zero, and he observes the formation of small crystals of ice in the film. He sees initially its colors being disturbed, then he attends the generation of the crystals, which are shown resting on a black bottom. Pfaff is devoted to conjectures, useless to report here, about the nature of this black bottom which is made up, as he notices, by part of the uncrystallizable solution at the temperature where the experiment is done; but it is to be noted that he notes that such films can persist several days. He could not observe congelation in the largest film, that of 7 inches in diameter, this film always bursting before the appearance of crystals.

§ 326. I said (§ 149), that, in a Report of 1830, Dr. Hough appears to be brought to the idea of the tension of liquid surfaces, only on the basis of the spherical shape of liquid drops and soap bubbles; that this idea leads him to that of a pressure exerted on the interior air by the film which constitutes a bubble or a cap, but that by seeking the law which connects this pressure to the diameter, he is completely mistaken.

I add here that he observed the small mass with concave transverse curvature which furnishes the edge of the film caps; he noted, moreover, that these caps express attractions and apparent repulsions of floating light bodies.

§§ 230 to 232 contain the summary of the beautiful experiments of Savart, published in 1833, on the development of large films of various forms by the impact of the continuous part of a liquid jet against a small solid disc, and by the mutual impact of the continuous parts of two directly opposite jets.

§ 327. In his work on cohesion[199], published in 1835, Frankenheim claims that if a series of horizontal solid plates, of which only the higher one is fixed, are laid out the one under the other with interposed layers of water, the semi-liquid column thus formed will be able to have a rather big length without ceasing to remain suspended on the fixed plate: if, for example, the layers of water have only 0.1mm thickness, equilibrium will be maintained as much as the sum of the weights of all the plates, except the higher, and of all the layers of water, will not exceed that of a column of of water of the same diameter and 150mm height. Frankenheim compares then with this arrangement in layers alternatively solid and liquid, the interior constitution of the formed films of a liquid to which one added some solid parts, such as soap water films, although, as he notices, the analogy is not complete; according to him, they are the solid particles held in suspension in the dissolved liquid, and not particles, which give to this liquid the property to be developed into films; they are used as points of support by the liquid, like the solid plates above; also they should be neither in too large nor in too small proportion; thus, to give the largest bubbles, water should contain neither too much nor too little soap. Frankenheim compares also these films with membranes and vessels of organic origin, which, he says, are originally semi-liquid and solidified later by the loss of the liquid and the bringing together of the solid parts.

I recalled in § 239 the results obtained, in 1836, by François with regard to the film with oblique rectilinear borders that I had described, and which is formed when a liquid escapes from a narrow rectilinear slit bored in the side wall of the tank from it bottom to just above the level.

§ 328. In 1836 and 1837, Mr. Draper made known curious experiments on the passage of gases through liquid films. The first, which appeared in the American Journal of medical sciences and in the Journal of the Franklin Institute, consist in inflating, with

[199] *Die Lehre von der Cohesion*. Breslau; §§ 69 to 76.

a certain gas, a soap bubble in an atmosphere of another gas; the bubble then increases or decreases gradually in diameter, and the phenomena stops when the composition of gases on the two sides of the liquid membrane become the same. The gases employed are, for example, nitric oxide inside and nitrogen outside; in this case, the bubble is decreasing. The author varies the experiment in the following manner[200]: it is made so that a plane soap water film occupies the opening of a small bottle of a capacity of approximately 60 cubic centimeters, then one places this bottle in an atmosphere of nitric oxide; after a few seconds, one sees the film bulging towards the outside, and, in one or two minutes, constituting most of a sphere 6 centimetres in diameter.

I reported, in § 307, the process of Dr. Reade (year 1837) to make very durable soap water films, the process reducing to developing these films in an atmosphere formed only of saturated water vapor.

In 1840, Dr. Reade returned[201] to these same films: he seeks to prove, by a succession of experiments, that the various colors of the liquid films do not come from differences in thickness. For example, after having produced a film by the process in question, he inclines the bottle which contains it, and waits until it becomes entirely black; then he gives to the bottle a certain movement of comings and goings in the horizontal direction, and soon the film is strewn with a multitude of white points, which meet in colored bands, which being formed almost simultaneously, cannot, he says, be generated by a thinning fluid.

The author starts from there to claim, for the cause of the colors in general, a theory which we do not have to reproduce.

§ 328bis. Brewster has studied[202], in 1841, curious appearances, which appear when one lights by polarized light a liquid film presenting colors in rings or other shapes, and one varies the direction of the plane of polarization, the angle of incidence on the film, the nature of the liquid forming the film, etc. When the film rests on a solid or liquid surface, as in the case of the spreading out of an essential oil droplet on water, Brewster notes disappearances and successive reappearances of the rings, with passages of systems or portions of systems of rings with black center to systems or portions of systems with white center, and vice versa, etc. When, on the contrary, the film has its two faces in the air, the phenomena above are not shown: the shapes conserve the arrangement of their colors; if they are rings, they always have a black center, and there is of another disappearance only that which occurs when the light strikes the film under the angle of polarization of the liquid. The author gives the theoretical reason of all these phenomena.

Finally if one receives in the eye not the reflected beam, but the beam transmitted by the film, and if this film showed by reflection a uniform color, one observes, by using an analyzer, either the colored zones, or a system of rings with black center or white center; if the film is thick enough to appear colorless, it depolarizes the transmitted beam.

I mentioned, in § 314, the large and very persistent bubbles that Mr. Böttger inflated (year 1838) with a molten mixture of rosin and linseed oil.

§ 329. In 1843, Marianini wrote[203] about an interesting experiment, where occurs, moreover, a fact similar to those announced by Mr. Draper: one drops a soap bubble, inflated with the mouth, in a broad glass test-tube filled to two thirds approximately

[200] *Gaseous diffusion* (Philos. Magaz., new series, vol. XI, p. 559).

[201] *Remarks on the permanent soap film and on thin plates* (ibid, vol. XVII, p.32).

[202] *On the phenomena of thin plates of solid and fluid substances exposed to polarized light* (Philos. Transact., 1841, p. 43).

[203] *Sur un phénomène offert par les bulles de savon flottant sur le gaz carbonique* (Ann. de Chim. et de Phys. de Paris, 3rd series, vol. IX, p. 382).

with carbon dioxide; after some oscillations, this bubble remains suspended; but soon one sees it increasing in diameter, and going down progressively, until it bursts. While bursting, it launches in all directions a quantity of small droplets which will sprinkle the walls of the vessel. Marianini draws from this last fact the conclusion that the gas contained in the bubble is compressed.

It is expressed thus at the beginning of the article "to make sensible the great difference in density which exists between atmospheric air and carbon dioxide, one has had for a long time, in the courses of physics, the following experiment." The clever idea of making a soap bubble float on carbon dioxide thus appears not to be due to Marianini; I am unaware of who is the author, and at which time it was proposed.

§ 330. It was seen, in §§ 116 , 118 and 151, that Mr. Henry, in a verbal communication of 1844, looks at the tension of liquid surfaces as determining the spherical shape of film bubbles by the condition of the minimum of surface area; that he makes depend on the same causes the pressure exerted on the interior air, pressure which he states is inversely proportional to the radius of the bubble; he indicates as a curious demonstration of the tension and the resulting pressure, the fast shrinking of the film and the intense draught that one receives in the face when, after having inflated a large bubble at the end of a broad tube, one removes it from the mouth; that he measured this same pressure using a pressure gauge with water; finally, that he was slightly mistaken in attributing all the tension to external surface of the bubble.

Mr. Henry had made, shortly before, a first communication concerning approximate measurements of the cohesion of liquids: he sought to evaluate this cohesion in soap water "by weighing the quantity of water which adheres to a bubble of this substance immediately before the rupture, and by determining the thickness of film by the observation of the color that it has, according to the scale of thin films of Newton." I translate here literally the passage from the report, because it is not clear. Mr. Henry concludes from his experiments that the cohesion of water, far from being as weak as was believed, rises to several hundred pounds per square inch, and is probably equal to that of the ice.

I add to what I said relative to the second communication, that, in his measurements of the pressure by means of the pressure gauge with water, Mr. Henry estimates in the same manner the thickness of the film immediately before the rupture, and also arrives, by this mode of experimentation, at approximate values of cohesion, values which are about those that he had deduced from the weighings. He employed, said the report, to measure the tenacity of the film, several other methods, whose general results were still the same.

§ 331. In 1845, Mr. Melsens[204] managed to form small hollow mercury bubbles; he makes fall, from a sufficient height, on a bath of mercury covered with a layer of water from 4 to 5 centimetres thickness, a stream of this last liquid, in such a manner that the bubbles of air are entrained with enough force to penetrate under the surface of the metal; these bubbles then, while going up, are covered with a thin film of it, and come, in this state, to float on the surface of the water, where they persist long enough to be easily observed; the diameter of the largest can reach 15mm.

One easily understands why, in this experiment, the bubbles of air which go up on the surface of mercury are not restricted to develop film segments of a sphere, but continue their upward progress by forming complete film spheres: each bubble of air, after having formed a mercurial cap, remains subjected, on behalf of the water, to an upwards push which is transmitted to it by the intermediary of mercury, and makes it overcome the capillary pressure exerted on it from top to bottom by the raised cap; the

[204] *Comptes rendus*, vol. XX, p. 1658.

mercurial film, which cohesion prevents from breaking, is then obliged to continue to develop and finally completely surround the bubble of air.

A remarkable fact again observed by same the physician[205], is that these hollow mercury bubbles are transparent in their thinnest part; the light which traverses them takes a color of a slate-colored blue. In the time when Mr. Melsens made known this last result, Faraday had not yet published the experiments by means of which he formed such thin films of a great number of metals, which all let pass the light, and gold was the only one that he had been able to attenuate enough to make transparent.

I analyzed in § 153 a work of Mr. Hagen published in 1849, where this scientist attributes the limitation of the liquid discs of Savart to the tension of the two faces of the film, this tension giving rise to a force directed in the contrary direction of the movement of the liquid.

§ 332. In 1852, Mr. Eisenlohr[206] developed large and beautiful Newton's rings, by quickly turning circular soap water films in their planes and around their centers. He generates these films, by a suitable agitation, in a bell jar whose diameter can reach 12 centimetres, after having, according to the process of Dr. Reade, boiled off the totality or almost the totality of the interior air, and having stoppered the bell jar hermetically; he then gives it a fast movement of rotation around a vertical axis passing through the center.

In this experiment, one soon sees being formed, in the common center of the rings, a black circular spot, which grows, and which is clearly rimmed on its border by one of the first order colors. The author tries to explain this abrupt jump, already announced by Leidenfrost (§ 321), by putting forth the assumption that the black portion has only the thickness of a single molecule, so that, in the passage to the contiguous ring, the increase in thickness is much more considerable relative to the thickness of this black portion, than in the passages between the various rings.

At the end of § 230, I said a word about the modifications that Mr. Tyndall made, in 1854, to the films of Savart resulting from the impact of a liquid jet against a solid obstacle.

§ 333. Magnus, in the first part of his *Hydraulic research* (§ 234), published in 1855, also deals with the liquid discs of Savart. He considers the matter in about the same way as Mr. Hagen, but, not employing calculation, he always regards the film as decreasing in thickness from the central part to the edge; he introduces, like Mr. Hagen, an obstacle in the liquid disc to produce a notch there, and he announces, in this respect, some facts that it is significant to note: the drops formed at the two edges of the notch spring much further that those which emanate from the remaining part of the contour of the disc; moreover, the first leave in the tangential directions to the curves of the edges from where they are driven out.

I spoke, in § 234, of the research of the same scientist on the phenomena resulting from the impact of the continuous parts of two jets which meet by forming an angle between them, phenomena which produce films as well.

I reproduced, in § 118, the determination given by Mr. De Tessan (year 1856) of the value of the pressure of the air imprisoned in a vesicle of water vapor, if these vesicles existed. It is, I think, the first theoretical evaluation of the pressure inside a film sphere of a given diameter and made of a given liquid, although this evaluation is too small by half, as I pointed out; one should not take account of those of Dr. Hough, which are absolutely false.

§ 334. I translated, in § 242, part of the Note of Mr. Gladstone on foam (year 1857),

[205] *Journ. l'Institut*, 1845, n° 605, p. 279.

[206] *Bericht über die XXIXste Versanmlung deutecher Naturforscher und Aerzte,* p. 87. – See also the Treatise on physics by the same author; the edition that I consulted is that of 1860.

a Note where the author claims that all liquids are likely to give, by agitation, film caps on their surface, but that faculty to foam appears to be sui generis and to depend on no known property.

Mr. Gladstone points out, moreover, that the foam produced on a colored liquid is always a clearer color than the liquid itself, and he adds that, in certain cases, this color is very different from that of the liquid; for example, the foam of a solution of cochineal red is a pale bluish crimson; he explains these effects by unequal absorption of the various rays which make white light, in a thin film and a thick layer of the liquid.

§ 335. Mr. Tyndall[207] (same year 1857), while immersing his hand in sea foam in a rough and wet time, found that this foam had the temperature of blood, while the sea water from where it came was very cold. He attributes the heat in question to the masses of air, before forming foam, being strongly compressed between waves falling on each other.

§ 336. In 1857 also, Mr. Van der Willigen[208] proposed to explain the abrupt jump between the black and the contiguous white in a soap water film, an explanation which coincides, nearly, with an assumption of Leidenfrost; he regards as probable that, in the film, a separation of the oily part of the soap takes place; that it slides on a layer of aqueous nature and produces the colored bands, while the exposed portion of this aqueous layer constitutes the black segment.

§ 337. I reported, in § 156, the formula given, without demonstration, in 1858, by Sir W. Thomson to represent, according to the tension of the liquid, the pressure exerted by a film bubble on the interior air. Sir W. Thomson then deduces from calculation the consequence that, when a liquid film develops, it cools, although by an extremely small quantity. Taking as example water, he indicates, for the tension, without saying where he got this value and without mentioning the temperature, 2.96 grains per inch of length, which, translated into milligrams per millimeter length, makes 7.57. Supposing then that a quantity of water of the weight of a grain is extended in a film of 16 square inches, he finds that this film cooled by approximately 1/320 of a degree centigrade.

§ 338. In 1861, Mr. Graham, in his famous Memoir on dialysis[209] gives an apparent explanation of the facts of endosmosis which liquid films present (§§ 328 and 329); he writes thus: "the described separation thus is more or less similar to that which one observes in a soap bubble inflated with a gas mixture composed of carbonic acid and hydrogen. No gas, as such, can cross the aqueous film; but the carbonic acid being water soluble, is condensed and dissolved by the aqueous film, and thus becomes able to pass to the outside and to spread itself in the atmosphere, while hydrogen being insoluble, or very close, in water, is maintained on the other side of the film, inside the bubble "

§ 339. In 1861 also, Mr. Faye, after having done me the honor of repeating my experiment on the film systems on iron wire frames in front of the Academy of Science of Paris[210], described an experiment consisting of agitating, using a wire ring, oil and soap water in a glass vessel; each time the ring passes from the soap water to the oil, it carries a film of the first of these liquids, which, by the movements impressed by the ring, gives rise to a film bubble completely full of oil and swimming in this last liquid. Continuously beating the liquids, these film spheres multiply and are subdivided in increasingly small and increasingly many spherules of comparable nature, until the

[207] *Remarks on foam and hail* (PHILOS. MAGAZ., 4me series, vol. XIII, p. 352).

[208] *Ueber die Constitution der Seifenblasen* (ANN. of Mr. Poggendorff, vol. CII, p. 629).

[209] *Liquid Diffusion applied to Analysis* (Philos. transact. 1861, vol. CLI, Part 1, p. 183).

[210] COMPTES RENDUS, vol. LIII, p. 463. – This article is preceded by a Note written by me on the film systems, a Note at the beginning of which, the name of Abbot Moigno is, by a cause needless to mention here, substituted for that of Mr. Faye.

mixture becomes an emulsion. Mr. Faye thinks that one can make application of this phenomenon to certain questions of physiology.

It was in 1861 also that I received the letter where Mr. Van Rees was so kind as to communicate the procedures by means of which he at will changes the position of the central flat film in the film system of the cubic frame, and causes the formation of the interior film polyhedrons (§ 203).

The new principle concerning the film systems of the prismatic frames (§ 202) was reported to me by the same scientist in a second letter written in 1862.

§ 340. I reported, in § 312, the remark of Abbot Florimond (year 1862) on the maximum diameter that soap bubbles can take when one employs, to inflate them, a glass pipe instead of a clay pipe.

Mr. Florimond remarks, moreover, that the broader the opening of the widening out of the tube, the larger also is the diameter of the bubbles, provided that the tube itself is not too narrow. I am convinced that by attaching a glass funnel from 10 to 15 centimetres of opening to a tube 2 centimetres in interior diameter communicating with a blower, and making use of it to inflate bubbles with a good glyceric liquid, one could give to these bubbles enormous dimensions. I find, moreover, in the continuation of the passage of the *Petites chroniques de la science* quoted in § 315, that Mr. Vivier, a celebrated musician, obtains gigantic soap bubbles by blowing in a horn made out of paperboard, a horn which, undoubtedly, is extremely widened. It will be further seen that Mr. Böttger also obtained very large bubbles by employing a broad opening.

Misters Minary and Lord described, in 1862 also, their experiment of small complete film bubbles, generated by the sharp agitation of sulphuric acid with olive oil, an experiment which I recounted in more detail in § 237.

§ 341. In the Note to which I referred in § 314, a Note published in 1862 still, Mr. Mach, on the basis of the fact that my film systems do not satisfy the general condition of equilibrium, since they have, on the liquid edges, surfaces with strong curvature in the transverse direction only, while surfaces of the films have zero mean curvature, gives the opinion that the study of these systems could lead to significant consequences regarding the laws of molecular attraction in the liquids. According to him, I should have sought to explain the fact in question by claiming that the thickness of the films is lower than the double of the radius of the sphere of attraction, which supposes that I look at these same systems as being in a state of complete equilibrium; however what I had said in my 2nd Series is that, in a system where films with zero mean curvature are thus attached to masses with concave curvature, equilibrium is only apparent, or rather exists only in the general form of the system; that, in consequence of the differences in capillary pressure, the films continuously send their liquid to these masses, and consequently keep thinning; finally, that the system *tends* towards a state of equilibrium in which the films would have a thickness less than the double of the radius of the molecular attraction, but that this equilibrium appears not to be able to be reached, films always bursting before. Today, moreover, I give up, in consequence of the remark of Mr. Quincke quoted in § 165, the idea of the theoretical possibility of a complete final equilibrium.

Mr. Mach believes that he will be able to profit from the comparison of thicknesses of films of different liquids (probably at the moment of their formation); it is to this end that he created, as I said in the paragraph referred to above, the film system of the regular tetrahedron, by employing molten rosin; he also made small films with a solution of an alkaline silicate, films which were solidified by the evaporation of water. These various films, having been detached from the solid wire, Mr. Mach weighed, and measured the surface of each one of them, then, knowing moreover their densities, he calculated their average thicknesses. He found in this manner that the average thickness of the films of the silicate solution, in the liquid state, was 0.142mm, and that that of the rosin films was of 0.027mm.

§ 342. In 1862 also, Mr. Kaul published an article[211] also relating to my film systems. He shows the need for the equality of the angles between the films which end at the same liquid edge and between the liquid edges which end at the same liquid point, by employing a method which reduces to that which I had expounded in my 6th Series, and which I did not reproduce in the current work because one arrives much more simply at the result by the tensions. He then points out that if the frame which one withdraws from the liquid consists simply of two plane polygons having a common side, and if the planes of these two polygons form between them an angle less than 120°, the system obtained is composed of two curved films being based respectively on the free contours of the two polygons, and a third film, in the shape of crescent, based on the common side, linked with the first two by a curved liquid edge; he concludes that films similar to the crescent always tend to occur in the various frames, but that their form is deteriorated by the other films of the system, and he believes that on the basis of this principle and of the laws concerning the angles, one can envisage what will be the system which will appear in a given frame. My son Felix (same year 1862) caused the formation of large film bubbles while launching soap water obliquely in the air, an experiment which I quoted at length in § 235.

§ 343. In a memoir[212] of 1863, Mr. Lord indicates some curious experiments concerning the pressure exerted by a hollow bubble on the imprisoned air: he makes it so that two bubbles of glyceric liquid are respectively inflated at the two ends of the same tube; the tube is provided, for this purpose, with a junction for blowing; the apparatus is built so that one can establish, or at will stop, the communication between the two halves of the tube. When this communication is closed as well as the opening for blowing, the bubbles do not undergo any change of dimensions; but when it is opened, the opening for blowing remaining stopped, the bubbles persist in the same state only if their diameters are equal; in the contrary case, one sees the smallest decreasing with an acceleration, until vanishing, the excess of its pressure driving out its contained gas to the larger, which increases thus in volume. The author varies the experiment by modifying the apparatus so as to be able to inflate one of the bubbles inside the other.

As I said in § 235, Mr. Van der Mensbrugghe (year 1864) extended the experiment of my son, by showing that, by the same process suitably employed, one can force a great number of liquids, perhaps all, to swell in complete hollow bubbles.

§ 344. In 1864 still, Mr. Laroque[213] proposed to study the constitution of a water jet launched vertically from top to bottom from a circular opening, when the liquid of the vessel is actuated by a gyratory movement around the axis of the opening. The vessel was cylindrical and of great dimension; the opening, bored in the center of the bottom, was one centimetre in diameter; the rotational movement was impressed on the liquid by a means which the author indicates. Among the observations of Mr. Laroque, I must quote the following one here: Under a sufficiently reduced load, a hollow formed in the middle of the surface of the liquid of the vessel, and after having reached the opening, penetrated into the jet, and the jet, for a certain distance, became a film; it was composed then of bulges and hollow necks occupying fixed positions. With a load of 15 centimetres, there were three of these bulges, whose upper two, of regular form, had each one 8 centimetres length and 16 millimetres width; the third was a little smaller

211 *Ueber die Plateau'schen Figuren* (SITZUNGSBERICHTE DER KOENIGSBERGER GESELLSCHAFT, vol. III, p. 7).
212 *Etude sur la forme globulaire des liquides*, thesis presented at the Faculty of Science of Besancon.
213 *Ann. de chim. et de phys.* of Paris, 4th series, vol. I, p. 276.

and less regular; below, the jet scattered in drops. Only, according to the figures which accompany the Report, the film which constituted all this portion of the jet was much less thin in the necks than in the bulges.

I indicated, in §§ 204 and 210, the principal results at which Mr. Lamarle arrived in his Report (years 1864 and l865) on my film systems.

As one saw, he shows the mathematical laws that I had found, and he studies in a very special way the closed film polyhedrons with curved faces which one produces in the middle of the film systems of the frames.

I quoted, in § 160bis, the experiment of Mr. Marangoni (year 1865), consisting of piercing a soap water film by liquid jets.

§ 345. One saw, at the end of § 118, that Mr. Tait calculated, in 1866, the pressures to which the air would be subjected inside water vapor vesicles of given diameters.

The same year, Mr. Tait communicated to the Royal Society of Edinburgh[214], certain results concerning the films. The relation that the *Proceedings* give is too brief, but a letter that the author made me the honor of addressing makes it possible for me to summarize them in a little more complete way.

In the first place, Mr. Tait seeks by which modifications necking happens when a bubble is detached from an opening (§ 228). For that, he inflates, by means of a widened tube held the opening in top, a bubble of glyceric liquid, with a mixture of air and coal gas; it is arranged so that the bubble has a slight tendency to go up, and that it separates from the opening with the least possible speed; he then notes, in so far as the speed of the phenomenon allows it, a result envisaged by him, being that at the moment of closing, the meridian line of the neck presents two facing points of inflection which get joined. Neither the report nor the letter mention according to which theoretical insights Mr. Tait expected this result. I will reconsider further the phenomenon.

In the second place, Mr. Tait showed how one can join together two bubbles into only one, or to split a bubble into two or several others; I am unaware of the procedures that he indicates, but I have assured myself that if, after having deposited on a ring a soap bubble of from 5 to 6 centimetre in diameter, one lowers on it a second bubble of the same diameter inflated with the opening of a pipe, both frequently merge together without partition and that at the same time the resulting single bubble spontaneously separates itself from the pipe, to remain on the ring.

With the glyceric liquid, two bubbles of the diameter above always gave a partition; the diameter should have been approximately one decimetre. As for the separation of a bubble into two others, it is carried out in the experiment of § 113.

In the third place, Mr. Tait directs, in an obscure room, a beam of solar rays on a large bubble of glyceric liquid. This beam, after having partially reflected from the posterior part of the bubble as from a concave mirror, converges consequently in a focus from where it diverges again to then cross the front part of the bubble; a portion of the light thus reflected is received on a white screen suitably placed and will paint there the colors of the thin liquid films with their successive modifications; Mr. Tait assures that this spectacle is extremely beautiful.

Finally, the author claims that if one looks through a prism at the small image of the sun reflected, in an obscure room, by a bubble of glyceric liquid, one distinguishes perfectly, in the produced spectrum, the obscure bands of interference known under the name of bands of Wrede.

§ 346. Mr. Broughton[215] (same year 1866) recalls the following fact, well known to him, that, in a soap bubble, the portions which, seen from a certain distance, appear of a uniform color, show, when they are examined more closely, a crowd of small bands

[214] *Proceedings of the Roy. Soc. of Edinb.,* vol. V, 1865-66, p. 593.
[215] *On some properties of soap-bubbles* (PHILOS. MAGAZ., 4th series, vol. XXXI, p. 228).

of varied and brilliant colors; it is rare that one finds there a space of a square millimetre which does not contain several of these small bands, and the apparent uniformity is due simply to the prevalence of the small bands of a given color. Mr. Broughton deposits on a ring a small bubble of glyceric liquid with sodium oleate, and, when a black spot was formed at the top[216], he observes it and its surroundings using a compound microscope, the bubble being lit by a sharp and suitably directed light; he then notes the production of a great number of small very varied colored shapes and very mobile, offering, he says, a spectacle of the greatest magnificence.

Mr. Broughton then tries to determine, by a particular method, the average thickness of the film which constitutes a bubble: he inflates the bubble with a mixture of hydrogen and air, by varying the proportions of this mixture and the diameter of the bubble until, removed from the drop which ordinarily adheres to it, it floats in the atmosphere without any tendency to go up or go down; then knowing the diameter of this bubble, the density of the liquid, and the proportion of the interior gas mixture, he deduces, by means of a formula, the weight of the bubble and the sought average thickness. For example, a bubble of 90mm diameter, inflated with a mixture of 1 vol. of hydrogen and 16 vol. of air, met the requirements, and Mr. Broughton arrived, for the average thickness of the film, at the value 0.000965mm.

§ 347. In the 5th, the 6th and the 7th of his Memoirs *Sur la théorie mécanique de la chaleur* (of 1865 to 1868), Dupre, we know, treated, by new methods, certain questions relating to liquid films. As one saw in § 161, in addition to simple experiments by the means of which he notes the existence of the tension in the films, he arrives at several general results concerning this force: he points out that the tension is independent of the thickness of the film, at least as long as this thickness is not below a certain extremely small limit; he establishes that the tension decreases, but rather slightly, when the temperature increases, and he announces a fact which shows this variation in the films; he seeks the laws from which follow the speed of withdrawal of a film which bursts, and that which governs the progressive reduction in the diameter of a bubble when the tube of blowing is left open; finally one of the many processes which he describes to evaluate the tension of liquid surfaces in general, is founded on the measure of the pressure to which the air imprisoned in a bubble is subjected.

I add here a curious experiment described in the same work: if one drops from a moderate height a small ball of cork on a horizontal plane film of glyceric liquid, the film is crossed, but does not burst and preserves its integrity. To know what occurs in this circumstance, Dupre fixes the ball of cork at the end of a needle, and, holding the latter in hand, he passes the ball slowly through the film; he sees the latter then being inserted, to form an increasingly deep pocket, then this pocket necks above the ball, the neck closes, separates in two, and the plane film is restored. The phenomenon is thus completely similar to that which takes place when one separates a bubble from the tube which was used for inflating it (§§ 224 and 228).

Mr. Van der Mensbrugghe made (§ 139), in 1866, the application of the general principle of § 128 to the creation, out of film, of a surface with zero mean curvature of which Mr. Scherk had found the equation in finite co-ordinates.

I gave, in § 162, the substance of another Note of Mr. Van der Mensbrugghe, (year 1866), concerning: 1° new processes for the evaluation of the tension of films; 2° laws which govern the form taken by a flexible thread inserted in a liquid film, when one burst the portion of film which it intercepts. I referred, in the same paragraph,

[216]The appearance of this black spot on a bubble of glyceric liquid appears singular to me: I made at home an innumerable quantity of bubbles of this liquid prepared either with soap, or with sodium oleate, and similar spots never appeared, no matter how long was the persistence. The bubbles of Mr. Broughton were extremely small, and it was perhaps to this circumstance that the production of the black spot was due.

to the report of Mr. Lamarle on this Note, a report where is announced a law which had escaped Mr. Van der Mensbrugghe, as well as the need for a deformation, in the greatest number of the cases, of the film surface under the action of the thread which was tightened. Lastly, at the same place still, I quoted a last Note, published in 1867, in which Mr. Van der Mensbrugghe verifies the conclusions of the report above, and indicates an interesting experiment on the tension of a vertical plane film.

§ 348. In 1866 or 1867, Mr. Böttger[217] developed, with a concentrated decoction of bark of Quillaya (wood of Panama), extremely large bubbles, persisting a long time and displaying sharp colors (he indicates neither the diameter nor the duration), while using of a funnel of opening from 7 to 8 centimetres. It is known that the bark of Quillaya contains saponin.

§ 349. Liquid films were the subject, in 1867, of three Memoirs of Brewster. The first[218] is devoted to the colors of the films: the author studies with a meticulous care all the arrangements, all the changes of the colors in question, and all the singular phenomena which they present, such as the production and the movements of the small spots in the shape of tadpoles, etc. Besides these details, which could not be summarized, here are the most salient results:

1° One produces a plane film in the opening of a drinking glass; the glass is held so that this film is vertical, then, when the colored bands are well developed there, one gives the glass a rotational movement as fast as possible around its axis; all the bands remain horizontal.

2° The film being placed horizontally and offering various colors, one blows on its surface through a narrow tube, in the direction of a diameter; one sees at once being formed, on each side of this diameter, a system of Newton's rings; these two systems turn quickly, and in contrary direction, around their respective centers. If the breath is directed not according to a diameter, but according to a small chord, there is only one turning system of Newton's rings. In all, the colors of the first orders are towards the center. Finally if one continues to blow, the rings disappear gradually.

3° Brewster states an idea similar to those of Leidenfrost and Mr. Van der Willigen (§§ 321 and 336): according to him, the colors of a soap water film would not result from the various thicknesses of the film itself, but from a particular matter which floats on this film. What appears to him most probable, is that the matter which produces the colors is formed of one of the ingredients of the solution, separated from it by a kind of secretion, which takes place only when the liquid is in a film state. It is based mainly on the following facts: If one examines the surface of a soap solution or that of the glyceric liquid, even when the vessel is not very deep, one observes no coloring there, and it reflects the images of the objects as water or glass would; but as soon as the liquid is spread out in a thin film, its surface becomes temporarily unequal and does not reflect any more the images but imperfectly; moreover, when the colors develop, all their variations and all the movements which occur there hold with the hypothesis in question; finally, if one blows on the film or one passes there a wet feather of the same liquid, one brushes the colored material, and one scatters the colors. Observations contained in a second Memoir,[219], observations which treat especially film systems on frames, were undertaken by Brewster with an aim of simple recreation, and offer relatively few new results from the scientific point of view; some, however, have interest: such is that which I reported in § 187, and which consists in employing,

[217] *Jahres-Bericht des physikalischen Vereins in Frankfurt am Main,* 1866-1867, p. 67.

[218] *One the colours of the soap-bubble* (TRANSACT. OF THE ROYAL SOCIETY OF EDINBURGH, vol. XXIV).

[219] *On the shapes of equilibrium in liquid films* (TRANSACT. OF THE ROYAL SOCIETY OF EDINBURGH, vol. XXIV).

as a solid frame, the whole of two equal rectangles which intersect at the midpoints of two opposite sides and which are mobile around their points of intersection; it was seen how, by means of this frame, one completes the verification of the instability of an equilibrium film system in which more than three films lead to the same liquid edge. It is in the same Memoir that is described the process of which I spoke in § 206 for the creation of several film systems with a tiny quantity of liquid.

Finally, here is a third result of still the same Memoir: Brewster thought to produce a film with one of the openings of a tube in the shape of a truncated cone: when, after having immersed the broader opening in the glyceric liquid, one withdraws it, it is necessarily occupied by a film; but that is starts at once moving in the interior of the tube towards the smallest opening, and stops only when it reaches this last. I point out that one can regard this phenomenon as an effect of the tension: the film constantly making the effort to decrease its area, it satisfies this tendency while working towards the small opening.

Finally, in a third Memoir[220] related to appearances of small alcohol films, of volatile oils or nonvolatile, etc., Brewster produces these films by posing a drop of the liquid to be observed either on an opening of 5mm in diameter at most, bored in a solid plate, or on a small ring; the drop initially frms a biconcave lens, and, when one places the plate or the ring vertically, the major part of the liquid descends, and leaves a film. With a sufficiently volatile liquid, like alcohol, one waits some time before uprighting the plate or the ring; evaporation alone then transforms the biconcave lens into a plane film occupying all the opening almost. Brewster observes the films produced either by transmission, or by reflection; in the first case, he sees few colors, but he discerns currents affecting the forms and the odd movements; in the second, he notes there, in addition to the same currents, varied and mobile systems of Newton's rings.

§ 350. We find in 1867 still a remarkable experiment by means of which Mr. Chautard[221] makes manifest to a whole audience the magnetism of oxygen: with the opening of a clay pipe kept motionless by a support, he inflates, with oxygen, a bubble of glyceric liquid; this bubble is placed above and close to the poles of an electromagnet; by magnetizations and successive demagnetizations of this last, it takes an especially oscillatory movement, very visible when it is strongly lighted.

§ 351. In a curious Note[222] from 1868, Mr. Tait arrives, on the basis of the properties of bubbles, at a theorem of pure mathematics, a theorem which can be stated as follows:

The cube of the sum of the squares of several numbers is greater than the square of the sum of the cubes of these same numbers.

In effect, if one respectively designs several bubbles having for radii R, r', r'', etc, and if one imagines that all these bubbles are merged into only one, of which we will indicate the radius by R, the surface of the latter will necessarily be, under the terms of the tension, less than the sum of surfaces of the first, which will give the inequality:

$$r^2 + r'^2 + r''^2 + \ldots > R^2;$$

but as each originating bubble had a curvature stronger than the resulting single bubble, and exerted consequently on the interior air a more intense pressure, the volume of the

[220] *On the motion and colours upon films of alcohol, volatile oils, and other fluids* (TRANSACT. OF THE ROYAL SOCIETY OF EDINBURGH, vol. XXIV).

[221] *Experiénces relatives au magnétisme et au diamagnétisme des gaz* (COMPTES RENDUS, T LXIV, p. 1141).

[222] *Notes on an Inequality*(PROCEEDINGS OF THE ROY. SOC. OF EDINBURGH, vol. VI, 1867-68, p. 292).

single bubble must exceed the sum of the volumes of the others, from whence this second inequality:

$$r^3 + r'^3 + r''^3 + \ldots < R^3;$$

but, from these two inequalities, one deduces the following one easily:

$$(r^2 + r'^2 + r''^2 + \ldots)^3 > (r^3 + r'^3 + r''^3 + \ldots)^2.$$

§ 352. In 1868 still, Mr. Cauderay[223] announced soap bubbles as being extremely sensitive to electric attractions and repulsions, and indicated a succession of curious experiments very suitable to be carried out in public courses.

For example: "... if one charges the machine at the time when one blows soap bubbles in the vicinity, they will be attracted at a distance of 30, 40 or 50 centimetres, and even well beyond if the machine is rather powerful. One sees the bubbles then precipitating highly on the electric conductor and breaking there; sometimes the bubbles resist the shock, they stick to the conductor, and become charged there; they are then at once pushed back and attracted either by experimenter, or by the ground, on which they fall, in general, only after making a series of jumps, during which the electricity of the bubble combines with that of the ground...."

If one deposits on the conductor a series of bubbles, "at the time when one starts the glass plate of the machine moving, they will lengthen initially in the shape of ellipse, then will be detached from the conductor to fly in all directions, with a tendency however to move toward the people placed around the machine."

Let us cite further the following passage, where it is a question of showing that the static electricity appears only on the outside surface of the bodies: "... if, on an isolated disc (made out of metal) one blows concentric bubbles, when the machine is charged, the external bubble alone is influenced, it becomes deformed more or less according to the intensity of the charge, while the interior bubbles preserve all their half-spherical form."

Mr. Cauderay recommends arming the machine with a longer conductor, so that the experiments can be made at a rather long distance from the insulating supports; without that, these supports are soon moistened by the droplets which the bubbles project while bursting.

§ 353. Mr. L. Dufour (year 1869) had the happy idea to substitute a water film for metal cloths for the observation of the constitution of flames[224]: water is launched from a slit, under a suitable pressure, so as to form a horizontal film; using this film, Mr. Dufour cleanly cuts a flame at an arbitrary point of its height. "The hot gases and the carbonaceous particles are pulled in by the water. While placing the eye above, one sees extremely well the hollow cone of the flame, the luminous wall, etc.... nothing prevents one from prolonging the observation as one wants, to see as long a time as wanted, and to even employ a magnifying glass."

§ 354. In 1869 also, Mr. Boussinesq[225] subjected to calculation the shapes of the meridian line of the films of Savart produced by the impact of a liquid jet against a small solid disc (§ 230).

I mentioned, in § 165, the opinion stated by Mr. Quincke, in the same year, on the impossibility of the existence of a liquid film of which the thickness is less than double the radius of the molecular attraction; in § 166, interesting experiments of Mr. Lüdtge (same year), consisting of the spontaneous substitution of a film of one liquid

[223] *Effets de l'électricité statique sur les bulles de savon* (BULLET. DEL LA SOC. VAUDOISE DES SC. NATURELLES,, vol. IX, p. 655).
[224] *Bullet. de la Soc. Vaudoise des Sc. Naturelles,* vol. X, p. 181.
[225] *Reports*, vol. LXIX, pp. 45 and 128.

for a film of another liquid; finally, in §§ 167 and 168, the phenomena which occur, according to the observation of Mr. Van der Mensbrugghe (still the same year), when one deposits on the surface of pure water a soap bubble or a saponin bubble, as well as curious effects about substitutions of films.

§ 354bis. In 1869 still, Mr. Kessler indicated[226] a simple manner to carry out the experiment of the soap bubble of floating on carbon dioxide (§ 329): the gas is produced in a small bottle with a release, and it is brought in the nozzle of a broad funnel; it accumulates thus in the widened part, and it is there that one deposits the bubble[227]

§ 355. I spoke only incidentally, in this history, of the thin films resulting from the extension of a liquid droplet on another liquid; such a film, indeed, is of a very diferent kind than those with which I occupied myself: it is in contact on its upper face with the air, and, by its lower face, with the second liquid; it is not free to take various forms, and constitutes simply a thin layer lying on the horizontal plane surface and underlying liquid. One will be able to interrogate, with regard to these films, the Memoir of Mr. Van der Menshrugghe summarized in § 167; one will find there the indication of the various physicists who paid their attention to the subject in question, and a short analysis of their researches[228].

§ 356. As we have seen, Leidenfrost claims (§ 321) that, in a soap bubble, the oil of the soap separates and goes to the external surface of the film, where it gives rise to the colors, and he is led to this opinion by successive appearances which the bubble has; Mr. Van der Willigen also claims (§ 336) a separation of the oily part of the soap, to explain the abrupt jump which one observes, in a sufficiently thin film, between the black zone and the contiguous white zone; finally Brewster, basing himself, like Leidenfrost, on the aspect of the phenomena of coloring, claims, in the same way, that the colors are the result of a matter secreted by the films and which comes to extend on their surface (§ 349). The idea that a substance, either the soap, or one of its ingredients, separates from the solution and comes to extend on the faces of the film, was thus given three different times by good observers; however, as I recalled in the second note of § 299, Dupre sought theoretically to show the possibility of a fact of this kind, and quotes, in support of his assertion, an experiment which seems conclusive.

All that I will add here are the following remarks: in the first place, in my experiments relating to the small film caps (§§ 246 to 249), there were never definitely bordered black spots except on films of solutions of various soaps; when, with others liquids, olive oil, for example, the top of the cap took a final color close to the black, this color shaded imperceptibly into that of the surrounding zone; in the second place, the black films, whenever the evaporation is null, persist a very long time (§ 229), and, according to some tests which I made, the films of alkaline solutions being far from durable, it is difficult to claim, with Mr. Van der Willigen, that the black zone is formed of the alkaline part of the soap; I regard as more probable that it is the soap itself which, tending, in accordance with the fact advanced by Dupre, to be expelled from the solution, pushes back the colored part of the film, and thus comes, in a solution much more concentrated, to form the black zone. This opinion is supported by the experiment of Newton (§ 319), in which one does not observe blue spots which should always precede the black, when the solution contains a very strong proportion of soap; then, indeed, on the assumption that I propose, there is necessarily less difference in

[226] *Vorlesungsversuche* (BULLET. CHEMICAL SOCIETY OF BERLIN, 2nd year p. 369).

[227] For research after 1869, to see the articles listed in § 508 under the n° 14, 15, 19, 26, 30, 32, 37, 38 and 42.

[228] Mr. Van der Mensbrugghe gave this Memoir a sequel, in which he completes his history; it is the work listed in § 508 under n° 45.

composition between the black zone and the remainder of the film, and consequently the jump must be less abrupt; finally, in the experiment of Pfaff (§ 325), after, by the action of the cold, the major part of water separates in the form of small crystals, the film is black, and can be maintained several days; however the liquid of this film obviously then consists of a very concentrated soap solution.

CHAPTER IX.

Stability of the equilibrium shapes; purely experimental study.

§ 357. Let us return to equilibrium shapes. As we saw (§ 34), it is very probable that the sphere is the only closed equilibrium shape, and thus all others have infinite dimensions in certain directions. However, as we also saw for several examples, when one tries to partially form one of these last, either with an oil mass in the alcoholic mixture, or with a thin film of glyceric liquid in the air, one recognizes in general that, when the solid terminations to which the mass or the film adheres must include between them a too extended portion of the shape, this refuses to be formed, from which one concludes that with this spacing of the terminations, it would be unstable. We now will seek the limits of stability of the majority of the shapes with which we have occupied ourselves, and especially shapes of revolution ranging between two equal bases perpendicular to the axis.

When an oil sphere is freely suspended in the alcoholic mixture, it always shows, as I already said (§ 34), a perfect stability of form: if, by movements given to the ambient liquid, one perturbs this form, the mass always takes it again exactly. A soap bubble insulated in the air also shows a permanent and stable form: if one runs up against it with a taut wool fabric, and that the shock is light enough not to make it burst, one sees it to be flattened more or less against the fabric, then to rebound in the manner of an elastic ball, by taking again its sphericity. Thus the sphere is a shape of steady equilibrium in its complete state, and consequently, with stronger reason, any portion of a sphere is stable.

The sphere thus does not have a limit of stability, in the sense that I gave to this expression: i.e., whatever the extent of a portion formed of a sphere relative to the whole sphere, this portion is always in a state of steady equilibrium; it is what one sees verified, for example, with regard to an adherent mass on a solid disc (§§ 14 and 15), with regard to the bases of a cylinder formed between two rings (§ 40), etc.

I will also cite the small surfaces which top the mercury column and the alcohol column respectively in Rutherford's maximum and minimum thermometer. These surfaces being very small, the action of gravity on their form can be regarded as negligible; also that of mercury appreciably constitutes a convex segment of a sphere, and that of alcohol a concave half sphere. However, as pointed out by Mr. Duprez[229], it is the stability of the latter which is the true cause of the retreat of the enamel pointer when the temperature drops, and I will add that it is also to the stability of the end surface of the mercury that it is necessary to attribute its action to advance the steel pointer when the temperature rises.

§ 358. The fact of the absence of limits of stability being independent of the radius and, consequently, the curvature of the sphere, it is also true when the radius becomes infinite, or, in other words, when the surface of the sphere becomes a plane. The plane thus does not have a limit of stability either, which means that it can be formed in a solid contour of an arbitrary extent, without ceasing being stable.

§ 359. The experiments of §§ 45 and 46 showed us that when, in a liquid cylinder, the length notably surpasses triple the diameter, equilibrium is unstable, and the shape separates spontaneously into two unequal portions. Now let us examine the matter in more detail.

Let us employ the solid system of fig. 19, a system made up of two facing vertical discs; in that which was used for my experiments, the diameter of the discs was 30mm,

[229] *Note sur la cause qui s'oppose l'introduction d'un liquide dans un vase à orifice étroit* (BULLET. OF ACAD. OF BELGIUM, 2nd series, vol. XV, p. 11, 1863).

and the distance between them was 108mm, so that the ratio between the length and the diameter of the liquid cylinder which would extend from one disc to another, would be equal to 3.6. This system being introduced into the alcoholic mixture, I made adhere to the whole of the two discs, by the process indicated in § 46, a mass of oil too large to constitute the cylinder in question, then I absorbed the excess using a small syringe. As in the experiments of § 45, the shape started to change spontaneously before the cylindrical form was reached; but I could, using a small artifice, push the experiment further, and manage to form an exact cylinder[230]. It appeared to persist for a moment; then it started to be necked in part of its length and bulge in the other, like the vertical shapes, and the phenomenon of the disunion was completed in the same manner, by giving rise to two final masses of different volumes.

I repeated the experiment several times, and always with the same results; only separation happened on one side sometimes, sometimes on the other of the middle of the length of the shape. Moreover, if the phenomenon takes place in a nonsymmetrical way compared to the middle of the length of the shape, either horizontal or vertical, symmetry always remains, on the contrary, around the axis; in other words, during all duration of the phenomenon, the shape does not cease being of revolution. Let us add here that, in the horizontal shape, the respective lengths of the portions narrowed and bulged appear equal between them.

One thus sees now that the mode of deformation of these cylinders is the result of a property which is inherent in them. It results, moreover, from the experiment above, that ratio 3.6 is still higher than the limit of stability, so that the exact value of this must be between numbers 3 and 3.6.

§ 360. For a rough approximation of the limit in question, I also made use of mercury cylinders of small diameter formed in the air by the following process: I placed, on a horizontal glass plate, two copper wires of approximately a millimetre thickness and a few centimetres length, directed in the prolongation one of the other, but leaving between their facing ends, ends which were amalgamed, an interval from 7 to 8 millimetres; then I deposited in this interval a mercury globule whose diameter did not exceed two millimetres; I then brought the wires closer until the two small amalgamed faces came to touch the globule and to which adherence was established. Then I made slip one of wires in the direction of its length, in order to stretch the liquid globule and to try to convert it into a cylinder. When the volume of the globule was sufficiently small, I obtained thus, indeed, a cylinder which preserved its form in a permanent way. If, on the contrary, the volume of mercury exceeded a certain size, the small mass always separated into two parts before the cylindrical form was reached. By modifying the volume of the globule, I tried to arrive at the greatest spacing of the amalgamated faces for which the formation of the cylinder was possible, and I could recognize that it was higher than triple, but lower than the quadruple of the diameter of this cylinder.

This experiment presents some difficulty, because, during the trial-and-error which it requires, mercury dissolves the copper and loses its fluidity[231]; however, with a little

[230] See, for that, how it is necessary to proceed in the extraction of the oil excess. One initially makes the operation progress with a suitable speed, until the shape starts to become deformed; then one puts the end of the nozzle of the syringe along the higher part of the mass, while going from the thickest portion towards the other: this weak action is enough to bring back towards the latter a small quantity of oil, and to thus restore the symmetry of the shape; then a new absorption is carried out, the shape is still regularized, and one continues thus until one reaches exactly the cylindrical form.

[231] I think that this reduction in fluidity takes place in a way sensitive only to the surface of the small mass: the copper which is combined with mercury, being in a state of extreme division, combines with oxygen in the surrounding air, from which results, on the surface of the liquid, the gradual formation of a thin oxide film. Consequently, the small mass of mercury, as the oil masses when they overlap with the film which was

practice, usually one manages to operate rather quickly to avoid this problem.

We will later find, using the theory, the exact value of the limit of stability of the cylinder, and we will verify it by experiments more precise than the preceding ones.

§ 361. In the unstable cylinders that we have just formed, the ratio between the length and the diameter was not very considerable; but what would happen if one had suddenly obtained cylinders very long relative to their diameter? However, one can, under certain conditions, form shapes of this kind, exactly or nearly cylindrical, and we will see what are then the results of the spontaneous breaking of equilibrium.

Here is the mode of experiment that I adopted for this purpose, and which enabled me to arrive at certain laws of the phenomenon: I initially will describe in a brief way the apparatus and operations, and I will add then the necessary details.

The principal parts of the apparatus are 1° a rectangular plate of glass, 25 centimetres long and 20 wide; 2° two strips of the same glass, 13 centimetres long and 2 broad, and from 5 to 6 millimetres thick, perfectly straight and polished on their edges; 3° two pieces of copper wire approximately 1 millimetre thick and 5 centimetres long; these wires must be quite straight, and one of the ends of each one of them must be cut neatly flat across, then carefully amalgamed.

The plate being placed horizontally, one sets flat on its surface, and parallel to its long sides, the two strips of glass, so as to leave between them an interval of approximately a centimetre; then one introduces into this space the two copper wires, placing them in a straight line parallel to the strips, and so that the amalgamed ends are facing one another at distance of a few centimetres. That done, one deposits between these same ends a quite pure mercury globule, from 5 to 6 millimetres in diameter, then one brings closer the two strips of glass until they come to touch the wire, so that then they leave nothing more between them but an interval equal in width to the diameter of these same wires.

The small mass of mercury compressed thus laterally, is obliged to lengthen and go on its two sides towards the amalgamed surfaces. If it does not reach them, one slips a wire towards it, until contact and adherence are established. Then one slips the wire in the contrary direction, so as to move them away one from one another, which causes a new lengthening of of the small liquid mass, and a reduction in its vertical dimensions. By acting with care, and by accompanying the operation with small taps given with the finger on the apparatus to facilitate the movements of the mercury, one manages to extend the small mass until its vertical thickness is equal to its horizontal thickness everywhere, i.e. with that of the copper wire. The mercury thus forms a liquid wire of the same diameter as the solid wire to which it is attached, and a length from 8 to 10 centimetres. This wire, considering the smallness of its diameter, which makes the action of gravity insensible compared to that of molecular attraction, could be regarded as exactly cylindrical; so that one will have, in this manner, a liquid cylinder having a length from 80 to 100 times its diameter, and attached by its ends to solid parts, a cylinder which preserves its form as long as it remains imprisoned between the strips of glass.

Things being in this state, one sets weights on the parts of the two copper wires which extend beyond the ends of the strips, in order to maintain these wires in quite fixed positions; then finally, using a means that we will indicate below, one vertically removes the two strips of glass. At the same moment, the liquid cylinder, free of its obstacles, is transformed into a series of many isolated spheres, lined up in a straight line along the direction of the cylinder which gave them birth. Usually the regularity of the system of spheres thus obtained leaves something to be desired: the spheres show

the subject of § 17, must lose little by little its tendency to take a fixed shape of equilibrium, and consequently to appear less fluid.

differences in their respective diameters and the distances which separate them, which undoubtedly comes from small accidental causes dependent on the mode of operation; but sometimes the differences are so tiny, that one can then regard the regularity as perfect. As for the number of spheres corresponding to a cylinder of a given length, it varies from one experiment to another; but these variations, which are due also to small accidental causes, remain between not very wide limits.

§ 362. In this experiment, the transformation is carried out with too much speed for one to observe the phases well; but the phenomena presented to us by our cylinders of oil, cylinders larger and less lengthened, the formation of a juxtaposed bulge and necking, equal, or nearly so, in length, the gradual increase in thickness in the bulging portion and the simultaneous thinning in the narrowed portion, etc, enable us to conclude that, in the case of a cylinder whose length is considerable compared to its diameter, events occur in the following way: the shape starts by changing in a manner to offer a regular and uniform succession of bulging portions, separated by slightly narrowed portions of the same length, or nearly so; this change, initially very small, grows more and more, the narrow portions slimming gradually, while the bulging portions increase in thickness, and the shape not ceasing being of revolution; finally necks break, and the parts of the shape thus completely isolated the ones from the others each take the spherical form.

We must add here that the end of the phenomenon is accompanied by a remarkable characteristic, about which we did not yet speak; but as it constitutes, so to speak, an additional part of the general phenomenon, we return to its description later (§ 375).

§ 363. Now let us complete the description of our apparatus, and add some details concerning the operations.

The glass plate having to be brought to a perfectly horizontal position, it is carried, for this purpose, by four feet with screws.

At each end of the lower surface of the strips of glass, is stuck a small transverse band of thin paper, so that the strips of glass resting on the plate via these small papers, their lower surface is not in contact with the surface of the plate. Without this precaution, the strips of glass could contract with the plate a certain adherence, which would introduce an obstacle during the vertical removal of these same strips. Those carry, moreover, on their upper surface and within 6 millimetres of each one of their ends, a small vertically mounted screw, the point at the top, fixed well to the glass with cement, and rising 8 millimetres above its surface. These four screws are intended to receive nuts, being useful to fix the strips to a framework and to help in removing them.

This framework is made out of iron; it is composed, firstly, of two rectangular plates 55 millimetres long, 12 wide, and 3 thick. Each of them is bored perpendicular to its large face by two holes placed in such manner that by setting each one of these plates transversely on the ends of the two strips of glass, the screws with which these last are provided can engage in these four openings. The screws being long enough to extend beyond the openings, one can then adapt small nuts to them, so that by tightening those, the glass strips are fixed in an invariable position with respect to each other. The openings have a form lengthened in the direction along the iron plates; in this manner one can, after having loosened the nuts, increase or decrease the distance between the two strips of glass without being obliged to remove the plates. On the middle of the upper surface of each plate is mounted a vertical stem 5 centimetres high, and the upper ends of these two stems are joined together by a horizontal stem, the middle of which has a third vertical stem, directed upwards, and 15 centimetres long. This last stem has a square section, and its thickness is 5 millimetres. When the nuts are tightened, it is seen that the strips of glass, iron plates, and the fork which joins them together, constitute a rigid system. The long vertical stem is used to direct the movement of this system; for this purpose, it passes loosely through a 5 centimetres high channel of the same cross-section and bored in a part which is mounted in a quite fixed way, by a suitable support, at 10 centimetres above the glass plate. Lastly, the bored part is provided laterally with a pressure screw, which makes it possible to tighten the stem in the sleeve. Using this provision, if all the whole of the apparatus were worked carefully, the two bands of glass, once the small nuts are tightened, will be able to be manipulated with perfect simultaneity, and always identically in the same direction perpendicular to the glass plate. When the liquid cylinder is well formed, and the weights are set on the free portions of the copper wires, one passes a finger under the horizontal branch of the fork, and one raises the mobile system a suitable height above the glass plate; then one maintains it at this height by tightening the pressure screw, in order to observe the result of the transformation of the cylinder.

The amalgamization of the ends of the copper wire always extending a little onto the convex surface, one coats this surface with a varnish, so that the amalgam covers only the small plane section.

It would be impossible to judge, with the plain eye, the precise point where it is necessary to cease moving the copper wires apart so that the liquid reached the cylindrical form. In order to solve this problem, the length of the cylinder is given in advance, and one marks this length, by two thin features, on the side surface of the one of the strips of glass; then one determines, by calculation, according to the known diameter of wire, the weight of the mercury globule which must form a cylinder of this diameter and desired length; finally, by means of a sensitive balance, one makes the globule intended for the experiment have this weight exactly. Then just stretch the small mass until the ends of the copper wires it lies between reach the marks traced on the glass.

When a series of experiments is made, one can use the same mercury several times by joining together, following each observation, the isolated spheres into only one mass. However, after a certain number of experiments, mercury seems to lose its fluidity, and the mass is always divided at some point, despite every possible precaution, before it stretches the wanted length, a phenomenon which comes from the solid wire yielding a little copper to the mercury (note of § 360). It is then necessary to remove this last, to clean the glass plates and the strips, and to take a new globule. One is sometimes also obliged to renew the amalgam on the wire.

§ 364. With the assistance of the apparatus and processes above, I carried out a succession of experiments on the transformation of cylinders; but, before reporting the results of them, it is necessary, for the interpretation of those, to consider the phenomenon a little more closely.

Let us imagine a liquid cylinder with a length considerable relative to its diameter, and attached by its ends to two solid bases; let us suppose it carrying out its transformation, and consider the shape at one time of the phenomenon before the separation of the masses, i.e. when this same shape is still composed of bulges alternating with narrowings. Surfaces of the bulges being outside the original cylindrical surface, and those of narrowing being, on the contrary, inside of this same surface, we can conceive in the shape a series of plane cross-sections perpendicular to the axis, and having a diameter equal to that of the cylinder; these cross-sections will constitute obviously the boundaries which separate the bulging portions from the narrowed portions, so that each portion, either narrowed or bulged, will be bordered by two of them; moreover, the two solid bases being necessarily among the number of the cross-sections in question, each one of these bases must occupy the end of a narrowed or bulged portion.

That said, three assumptions are presented relative to these two portions of the shape, i.e. those which rest respectively on each solid base. First, we can suppose that these portions are both bulged. In this case, each narrowing will send into the two bulges which are immediately adjacent to it the liquid that it loses, the movements of transport of the liquid will be carried out in the same manner in all the extent of the shape, and the transformation will be able to take place with a perfect regularity, by giving rise to exactly equal in diameter and also spaced isolated spheres. But this regularity will not extend to the two extreme bulges: because each one of those being bounded on one side by a solid surface, it will receive liquid only from the narrowing located on its other side, and will acquire, consequently, less development than the intermediate bulges. In these circumstances, one would thus find, after the termination of the phenomenon, two portions of sphere respectively adherent at the two solid bases, and presenting each one a diameter a little less than that of the isolated spheres arranged between them.

In the second place, we can suppose that the extreme portions of the shape are one a narrowing and the other a bulge. Then the liquid lost by the first which cannot cross the solid base, will be necessarily driven out entirely into the closest bulge, so that this one receiving on only one side all the liquid necessary to its development, it will have nothing to receive on the opposed side, and so, consequently, all the liquid lost by the second narrowing will go in the same way into the second bulge, and so on, until the extreme bulge. The distribution of the movements of transport will be thus still uniform in all the shape, and the transformation will be able to be also carried out in a perfectly regular way. The regularity will obviously extend even to the two extreme portions, at least as long as the narrowings will not have reached their greatest depth; but, beyond this point, it will not be completely regular any more as follows: because then independence being established between the masses, each bulge, except for that which is based on the solid base, will be enlarged from its two sides at the same time, to pass to the state of an isolated sphere, by absorbing both adjacent half-narrowings, while the extreme bulge will be able to enlarge only from one side. Thus, after the termination of the phenomenon, one would find, on one of the solid bases, a portion of sphere of a diameter not very much lower than that of the isolated spheres, and, on the other base, a portion of sphere much smaller, coming from the half narrowing which remained attached there.

Finally, in the third place, let us suppose that the extreme portions of the shape are both narrowings, which, after the termination of the phenomenon, would leave attached to each solid base, a portion of sphere equal to the smallest of the case above. In this case, to fix ideas, let us start with one of these extreme narrowings, for example of that of left. All the liquid lost by this first narrowing being driven out into the contiguous bulge, and being sufficient for its development, let us suppose that all the liquid lost by the second narrowing does the same with respect to the second bulge, and so on; then all the bulges, except for the last on the right, will take simply their normal development; but the bulge on the right-hand side, which receives, like each of the others, on behalf of the narrowing which precedes it the quantity of liquid necessary to its development, receives, moreover, the same quantity of liquid on behalf of the narrowing which is based on the closest solid base, so that it will be bulkier than the others. It is thus seen that, for the case in question, the opposite actions of two extreme narrowings introduce into the remainder of the shape an excess of liquid. Whatever other assumption that one makes on the distribution of the transport movements, it will always be necessary, whether the excess of volume is distributed on all the bulges at the same time, or whether it increases only the dimensions of one or two of them; but the first of these assumptions is obviously inadmissible, because of the complication which it would require in the transport movements; the second would thus have to be admitted, and then the isolated spheres would not be all equal. Thus this third mode of transformation would necessarily bring by itself a cause of irregularity, and, moreover, it would not allow a uniform distribution of the transport movements, since there would be opposition, with regard to these movements, at least in the two extreme narrowings.

One must thus regard as quite probable that the transformation will be arranged according to one or the other of the first two modes, and never according to the third, i.e. things will be arranged so that the shape which changes, has as extreme portions, either two bulges, or a narrowing and a bulge, but not two narrowings. In the first case, as we saw, the movement of the liquid of all narrowings would be carried out on two sides at the same time; and, in the second, this movement would take place for all in only one and the same sense. If such is really the natural arrangement of the phenomenon, it is understood, moreover, that it will be preserved when it would be disturbed from its regularity by small foreign causes. However, it is confirmed, as we will see, by experiments relating to the mercury cylinder: although the transformation of this cylinder seldom gave a perfectly regular system of spheres, I found, in the large majority of the results, either each solid base occupied by a mass not very much lower in diameter than the isolated spheres, or one of the bases occupied by a similar mass and the other by a mass much smaller.

§ 365. Let us name, for short, *divisions* of the cylinder the portions of the shape of which each one provides a sphere, either when we consider these portions in thought in the cylinder, before the beginning of the transformation, or when we take them during the achievement of the phenomenon, i.e. during the modifications which they undergo to arrive at the spherical form. The length of a division is obviously the distance which, during the transformation, extends between the neck circles of two neighbor narrowings, and it is, consequently, equal to the sum of the lengths of a bulge and two half narrowings. According to that, let us see how the length in question, i.e. that of a division, will follow from the result of an experiment.

Let us suppose a perfectly regular transformation, and let λ be the length of a division, l that of the cylinder, and n the number of isolated spheres found after the termination of the phenomenon. Each one of these spheres being provided by a complete division, and each of the two extreme masses by a portion of a division, the length l will be composed of n times λ, plus two fractions of λ. To estimate the values of these fractions, recall that the length of a narrowing is exactly or appreciably equal to that of a bulge (§ 359); however, in the first of the two normal cases (§ preced.), i.e. when the adherent masses remaining at the bases after the termination of the phenomenon are both of the large kind, each one of them comes obviously from a bulge plus half a narrowing, and, consequently, from three quarters of a division; the sum of the lengths of the two portions of the cylinder which provided these masses is thus equal to one and half, and there will be, in this case, $l = (n+1.5)\lambda$, or $\lambda = \frac{l}{n+1.5}$. In the second case, i.e. when the extreme masses are one large and the other small, the latter comes from a half narrowing, or a quarter of a division, so that the sum of the lengths of the portions of the cylinder corresponding to these two masses is equal to λ, and that, consequently, one will have $\lambda = \frac{l}{n+1}$.

The respective denominators from these two expressions representing the number of divisions contained in the overall length of the cylinder, it follows that this number will always be either a whole number simply, or a whole number plus a half. In addition, since the phenomenon is governed by fixed laws, it is understood that, for a cylinder of a given diameter, formed of a given liquid, and placed in given circumstances, there exists a normal length that divisions tend to take, and that they would take rigorously if the overall length of the cylinder were infinite. If thus it happens that the overall length of the cylinder, although limited, is equal to the product of the normal length of divisions by a whole number or by a whole number plus a half, nothing will prevent divisions from taking exactly this normal length. If, on the contrary, which will take place in general, the overall length of the cylinder does not fill one or the other of the two preceding conditions, one must believe that divisions will take the possible length most approximate to the normal length; and then, all else equal, the difference will be obviously much less as the divisions are more numerous, or, in other words, as the cylinder is longer. One must also believe that the transformation will adopt that of the two modes best to attenuate the difference in question, and it is again what experiment confirms, as we will see soon.

As I already said, though the transformation of the mercury cylinder is almost always laid out according to one or the other of the two normal modes, the result is seldom very regular; it thus should be admitted that small accidental disturbing causes make, in general, the divisions formed in the same experiment unequal in length; but then the expressions obtained above obviously give, in each experiment, the average length of these divisions, or, in other words, the common length that divisions would have taken if the transformation happened in a perfectly regular way by giving rise to the same number of isolated spheres and with the same state of the extreme masses.

Finally, since the third mode of transformation does arise, i.e. since it happened sometimes that each base was occupied by a mass of the small kind, if one wants to disregard the particular cause of irregularity inherent in this mode (§ preced.), and to seek the corresponding expression for λ, it is enough to notice that each extreme mass comes then from a half narrowing, or from the quarter of a division, which will give obviously $\lambda = \frac{l}{n+0.5}$.

§ 366. I now will report the results of the experiments. The diameter of the copper wire, and consequently of the cylinder, was 1.05mm; I initially I gave to the cylinder a length of 90mm, and I repeated ten times the experiment, while annotating, after each one of them, the number of the isolated spheres produced and the state of the adherent masses at the bases; then I calculated, for each result, the corresponding value of the length of a division, by means of that of the three formulas of the preceding paragraph which referred to this result.

I made ten new experiments then, by giving to the cylinder a length of 100mm and I calculated in the same way the corresponding values of the length of a division.

Here is the table of the results provided by these cylinders, and of the values that one draws for the length of a division. Each of the two series gave me one perfectly regular result; I indicate it by the sign * placed beside the corresponding number of isolated spheres.

LENGTH OF CYLINDER, 90mm			LENGTH OF CYLINDER, 100mm		
Number of isolated spheres	Masses adherent to the bases	Length of one division	Numbers of isolated spheres	Masses adherent to the bases	Length division
		mm.			mm.
10	Two large.	7.83	11	One large and one small.	8.33
* 12	Id.	6.67	14	Two large	6.45
12	Two smalll	7.20	14	Id.	6.45
15	Two large.	5.45	14	Id.	6.45
14	Id.	5.81	* 14	One large and one small.	6.67
11	Id.	7.20	13	Id.	7.14
11	Id.	7.20	11	Two large	8.00
12	One large and one small.	6.92	14	One large and one small.	6.67
13	Two large.	6.21	13	Two large.	6.90
11	Id.	7.20	10	Id.	8.69

As one sees in this table, initially, the various values which one obtains for the length of a division do not differ enough from each other so that one can ignore a tendency towards a constant value whose uniformity is affected only by the influence of small accidental causes.

In the second place, of the twenty experiments, it only happened one time that the adherent masses at the bases were both of the small kind.

In third place, the two perfectly regular results identically gave the same value for the length of a division; this value, expressed in an approximate way with decimals, is 6.67mm; but its exact expression is 6 2/3 mm: because the operation to carry out consists, in the case of the first series, of the division of 90mm by 13.5, and, in the case of the second series, of the division of 100mm by 15. As the two lengths given to the cylinder are considerable relative to the diameter, and that, consequently, the numbers of division are rather large, this value 6 2/3 mm must constitute very closely, if not rigorously, the normal length of divisions. One sees, moreover; that, to give to divisions this very approximate or exact value of the normal length, the transformation chose, on the one hand the first mode, and on the other hand the second mode.

§ 367. Let us quote other examples of the spontaneous transformation of a very long liquid cylinder compared to its diameter:

Physicists know that when one makes pass, through a thin horizontally stretched wire, an electric discharge of sufficient energy, one sees initially the wire reddening to white, then being dissolved in a great number of separate globules which fall, and of which one can note, after their cooling, their rounded form.

Secondly, a cotton yarn from 20 to 25 centimetres long is stretched between the two ends of a wooden arc, of which it forms the chord; one fills with oil a large dish, and one immerses there the yarn, which must have been beforehand quite impregnated with the same liquid, then one withdraws it with a suitable speed, keeping it in a horizontal position. At the time when it leaves the oil, this constitutes around it an appreciably cylindrical envelope of small diameter, which changes at once, in a manner with very close to regular, into a great number of small masses separate from each other and strung on the yarn like pearls; if the wire is 25 centimetres long, one counts there nearly one hundred of these pearls. I do not need to point out that the liquid pearls in question are not spherical; the action of the yarn lengthens the pieces a little, and in fact they are small unduloid bulges. This experiment is due to my son Félix[232], who varied it various manners.

Finally, I can't resist the desire to report a clever process which was suggested to me by Mr. Donny, to create with a great regularity, in the alcoholic liquid, an oil cylinder very long compared to its diameter, and to observe the transformation of it.

In the center of the bottom of a cylindrical glass vessel from 7 to 8 centimetres in diameter and 60 high, is glued a small iron disc one centimetre in diameter and a few millimetres thick; a glass or iron tube one centimetre in interior diameter occupies the axis of this vessel, and loosely embraces, by its lower end, the small iron disc; this tube extends above the top of the vessel, and contains a piston whose stem, of a sufficient length, is attached by its upper end to a fixed support; the piston thus maintained is one or two centimetres below the opening of the vessel; it can neither go up nor to go down, but to make it traverse the length of the tube, it is enough, one sees, to give to teh tube an upward movement; this movement is guided by suitable parts, in order to be carried out without oscillations; finally, the tube being descended, let us suppose that it is filled with oil to the piston, and that the vessel is full with alcoholic liquid.

Things being thus laid out, let us make the tube rise with a speed that preliminary tests will have determined; the oil, which the piston completely prevents from rising, will remain very whole in the ambient liquid, where it will have to constitute a regular cylinder extending from the small iron disc at the lower end of the tube, and having a length at least fifty times its diameter. This cylinder will start at once to change, and the phenomenon will have to be achieved with a great regularity.

I did not test this process, but its success appears very probable to me; the only real difficulty would consist, I think, in the equalization of both densities, because, with such a large height, an extremely weak difference between these densities could deteriorate the regularity of the oil shape right from the start even before the transformation.

One can stretch thin glass filaments without them being converted into small isolated masses, because the temperature of the substance is not raised enough to bring it to the liquid state: it is simply made syrupy, which introduces already a great resistance to the transformation, and, moreover, as the filament is formed, it is solidified by the cold of the ambient air. Likewise, if the spider and the silkworm produce their threads, it is that, undoubtedly, the matter emitted by their spinnerets has originally a rather strong viscosity, and that, in consequence of the extreme tenuity of these filaments, the material in question is coagulated at the time of its exit.

§ 368. Here must be placed a significant remark, of which we will make application later: the phenomenon of conversion into isolated spheres is not the result of a property belonging exclusively to the cylindrical form; it occurs with regard to any liquid shape which has one dimension considerable relative to both others: for example, in the experiment of § 222, if one bursts in its middle either of the two plane films which result from the spontaneous disunion of the catenoid, the oil which constitutes this film quickly withdraws in all directions towards the metal ring, along the length of which it forms a pretty liquid ring; however, this ring, whose transverse dimensions are extremely small, is not long in being transformed spontaneously into a succession many of small isolated masses strung along the wire and which, without the presence of this wire, would be exact spheres.

Let us say, in passing, that the liquid ring above, at the time when it has just been formed, forms the part of the nodoid generated by a node of the meridian line, if this node approaches a circumference of a circle (§ 75); with a metal ring of 70mm diameter, the meridian cross-section of the liquid ring is appreciably circular, and its diameter

[232] *Sur la transformation d'un cylinder liquide en sphères isolées* (BULLET. OF ACAD. OF BELGIUM, 1867, 2nd series, vol. XXIV, p. 21). See also the article listed in § 508 under n° 30

is only from 2mm to 3mm.

§ 369. Let us continue the research into the laws of spontaneous transformations of long cylinders; one will see, in chapter XI, why we give to this part of our work a so wide development.

One must regard as obvious a priori, that two cylinders formed of the same liquid and placed in the same circumstances, but different in diameter, will tend to divide in a similar way: i.e. the respective normal lengths of divisions will be between them in the ratio of the diameters of these cylinders.

In order to verify this law by experiment, I got copper wire of exactly double the diameter of of the first, and equal, consequently, to 2.1mm, and I carried out with this a new series of ten experiments, while giving to the cylinder a length of 100mm. This series provided me also one perfectly regular result, which I indicate, as previously, by the sign * placed by the corresponding number of isolated spheres. Here is the table relating to the series in question.

Number of isolated spheres	Masses adherent at the bases	Length of a division
		mm.
7	Two small	13.33
6	Two large	13.83
6	One large and one small	14.28
7	id. id.	12.50
* 6	Two large	13.33
6	id.	13.33
6	One large and one small	14.28
8	id. id.	11.11
8	Two small	11.76
6	One large and one small	14.28

While stopping with the second decimal, one has here, as one sees, for the length of a division with the perfectly regular result, the value 13.33mm; but as the operation which gives it consists of the division of 100mm by 7.5, the value expressed in a complete way is 13 1/3 mm. This is very close to, if not exactly, the normal length of divisions of this new cylinder; however this length of 13 1/3 mm is precisely double the length 6 2/3 mm which belongs to the divisions of the cylinder of § 366; these two lengths are thus indeed between them in the ratio of the diameters of the two cylinders.

The perfectly regular result of the table above having had a mass of the large kind at each base, it follows that, to make it possible for divisions of the current cylinder to take their normal length or the most approximate possible length of the latter, the transformation had to be arranged according to the first mode; while with regard to a cylinder less than half the diameter, and having the same overall length 100mm, the transformation had been laid out according to the second mode. Here still, the case of two masses of the small kind at the solid bases is the least frequent, although it was shown twice.

Finally, the various values of the length of a division are concordant in the two series relating to the first diameter, and express well, consequently, the tendency towards a constant value; one even sees that the normal length is that which is generally reproduced.

§ 370. According to the law that we have just established, when the nature of the liquid and the external circumstances do not change, the normal length of divisions is proportional to the diameter of the cylinder; or, in other words, the relationship between the normal length of divisions and the diameter of the cylinder is constant.

The cylinder of § 366 had, as we saw, a diameter of 1.05mm, and the normal length of its divisions was very close to 6.67mm; consequently, when the liquid is mercury and the cylinder rests on a glass plate, the value of the constant ratio in question is, to a good approximation, 6.67/1.05 = 6.35.

Let us examine now if the external circumstances and nature of the liquid influence this ratio, and start with the first.

Our cylinder of mercury must have, on all the line whereby it touches the glass plate, a small adherence to this plate, adherence which must block the transformation more or less. To discover if this resistance influenced the normal length of divisions, and, consequently, the ratio between this and the diameter of the cylinder, a simple means arose: to increase this same resistance. In order to arrive at this result, I laid out the apparatus so as to remove only one of the strips of glass, so that the liquid shape remained then in contact at the same time with the plate and the other strip. I repeated ten more times the experiment, employing copper wire of 1.05mm diameter, and giving to the cylinder a length of 100mm. The results were as follows:

Number of isolated spheres	Masses adherent at the bases	Length of a division.
		mm.
9	One large and one small	10.00
8	Id.	11.11
9	Id.	10.00
8	Id.	11.00
11	Two small	8.69
8	One large and one small.	11.11
8	Id.	11.11
8	Two large	10.53
8	One large and one small	11.11
6	Two large	13.33

One sees that the various values of the length of a division are all, only one excluded, notably higher than all those which refer to the same cylinder diameter whose surface touches glass by only one line (§ 366). It thus should be concluded that, all else equal, the length of divisions grows with external resistance, and that, consequently, under the action of a similar resistance, this length is necessarily larger than it would be if the cylinder had its convex surface entirely free.

In the series above, no result appeared extremely regular; but it is understood that the average of the values of the third column will approach the normal length of divisions. It, moreover, confirms the tables of §§ 366 and 369: if one takes in the first the respective averages of the values of the two series, one will find for one 6.77mm, and for the other 7.17mm, quantities whose first is almost equal to the length 6.67mm, which can be regarded as the normal length, and from which the second does not differ considerably; and if one takes in the same way the average relating to the following table, one will find 13.15mm, a quantity very close the length l3.33mm, which also can, in the case of the second table, be regarded as the normal length. However, the average corresponding to the table above is 10.81mm; consequently, in the case of two-contact systems, we will have for the approximate value of the ratio between the normal length of divisions and the diameter of the cylinder, 10.81/1.05 = 10.29, while, in the case of only a one-contact system, we found only 6.35. Thus, ultimately, the relationship between the normal length of divisions and the diameter of the cylinder increases under the effect of an external resistance.

§ 371. In the complete absence of a similar resistance, the ratio should not thus any more depend only on the nature of the liquid. But I say that there is a limit below which this same ratio cannot go down, and which is precisely the limit of stability.

Let us imagine a liquid cylinder of sufficient length compared to the diameter, ranging between two solid bases, and carrying out its transformation with a perfect regularity. Let us suppose, to fix ideas, that the phenomenon happens according to the second mode, or, in other words, that the extreme portions of the shape are one a narrowing and the other a bulge; then, as we saw (§ 364), the regularity of the transformation will extend to these last portions, i.e. the narrowing and the bulge extremes will be respectively identical with portions of the same kind of the remainder of the shape. That said, let us take the shape at one time of the phenomenon where it presents yet only narrowings and bulges, and again consider the cross-sections whose diameter is equal to that of the cylinder (ibid). Let us start with the narrowed extreme portion; the solid base on which this rests, and which constitutes the first of the cross-sections in question, will be, as we showed, the origin of this narrowing; then we will have a second cross-section as the origin of the first bulge; a third as the origin of the second narrowing, a fourth as the origin of the second bulge, and so on; so that all the cross-sections of an odd nature will occupy the origins of narrowings, and all those of an even nature, origins of bulges. The interval ranging between two consecutive cross-sections of an odd nature will thus contain one narrowing and one bulge; and since the shape starts with a narrowing and ends in a bulge, it is clear that its overall length will be divided into a whole number of similar intervals. Under the terms of the exact regularity that we supposed in the transformation, all the intervals in question will be equal in length; and as the moment when we consider the shape can be taken arbitrarily from the beginning of the phenomenon until the maximum of deepening of the narrowings, it follows that the equality of lengths of the intervals remains for all this period, and that, consequently, the cross-sections which bound these intervals preserve for this same period perfectly fixed positions. Moreover, the parts of the shape respectively contained in each interval undergoing identically and simultaneously the same modifications, the volumes of all these parts remain equal between them; and as their sum is always equal to the volume of the liquid, it follows that, from narrowings, each one of these partial volumes remains invariable, or, in other words, that no portion of liquid passes from an interval into the adjacent intervals. Thus, at the moment when we consider the shape, on the one hand the two cross-sections which bound the same interval will have preserved their positions and their diameters, and, on the other hand, these cross-sections will have been crossed by no portion of liquid. Things will thus have occurred in each interval absolutely in the same manner as if the two cross-sections which bound it had been solid discs. But, between two solid discs, the transformation cannot take place if the relationship between the distance which separates these discs and the diameter of the cylinder is smaller than the limit of stability; thus the relationship between the length of our intervals, and the diameter of the cylinder cannot be lower than this same limit. However, the length of an interval is obviously equal to that of a division: because the first is, according to what we saw above, the sum of the lengths of a bulge and a narrowing, and the second is the sum of the lengths of a bulge and two half narrowings (§ 365); thus the ratio between the length of a division and the diameter of the cylinder cannot be less than limit of stability; and we will remark here that the conclusion is also true, that divisions cannot take exactly their normal length.

§ 372. Let us move on to what relates to the nature of the liquid. Interior viscosity generates necessarily a resistance which must also make the transformation less easy; and since external resistances increase the length of divisions, it is to be supposed that interior viscosity acts in the same way, and that, consequently, all else equal, the ratio with which we occupy ourselves grows with this same viscosity. However, this cannot exert a quite considerable influence; indeed, the resistance which it opposes to relative displacements of the molecules decreases quickly, we know, with the speed of these displacements; however, when the transformation starts, it is with a tiny speed, which then accelerates; one must thus admit that at the beginning of the phenomenon, resistance due to interior viscosity is low, and consequently there is never a great lengthening of the divisions[233]. Finally, surface viscosity undoubtedly has also some influence, at least with regard to the liquids of the first and the third category (Chap. VII).

§ 373. It follows from this discussion concerning resistances, that the smallest value which one can suppose for the ratio between the normal length of divisions and the diameter of the cylinder, corresponds to there being at the same time complete absence of external resistance and viscosities; and, according to the demonstration given in § 371, this smaller value would be at least equal to the limit of stability. However, as all the liquids are more or less viscous, it follows as very probable, that, even on the assumption of the vanishing of any external resistance, the ratio in question will always exceed the limit of stability; and since this is greater than 3, this same ratio will be, with stronger reason, always greater than 3.

It is to be believed that the smallest value considered above, i.e. that which would be the ratio in the case of a complete absence of interior as well as external resistance, would be equal to the limit of stability, or would exceed it very little. Indeed, on the one hand, the ratio approaches this limit as resistances decrease, and, on the other hand, for the little that the ratio exceeds it, the transformation becomes possible (§ 371); one thus does not see a reason for the ratio to differ from it appreciably if resistances were absolutely null. It is besides what the results of our experiments tend to confirm. Initially, indeed, since the ratio pertaining to our cylinder of mercury goes down from 10.29 to 6.35 while passing from the case where the cylinder touches glass by two lines to that where it touches it only by only one, it is clear that if this last contact could itself be removed, which would let only the influence of viscosities remain, the ratio would become much lower than 6.35; and as, on the other hand, it must exceed 3, we can well claim that it would be at least ranging between this last number and 4, so that it would approach near the limit of stability. If thus it were possible to also cancel viscosities, the new waning which the ratio would undergo then would probably bring it exactly to the limit in question, or at least up to a value which would differ excessively little from it.

Thus, on the one hand, the smallest value of the ratio, that which would correspond to a complete nullity of resistances, would not differ or would hardly differ from the limit of stability; and, in addition, under the influence of viscosities the ratio pertaining to mercury would move away only a little from this smaller value. It is thus seen, at least with regard to mercury, that the influence of viscosity, external as well as interior, is actually weak, and, according to what we mentioned above, it must be about the same with regard to other liquids.

It follows that in the absence of any external resistance, the values of the ratio respectively corresponding to various liquids will not be able, in spite of the differences in viscosities, to move away much from the limit of stability; and as the smallest whole

[233] An experiment described in my 2nd Series had led me to exaggerate the influence of viscosity over the length of divisions; I recognized, later, that the experiment in question presented, in this respect, a cause of error; this is why I did not reproduce it in the current work.

number above this is 4, we can adopt this number as representating, on average, the probable approximate value of the ratio in question.

On the basis of this value, calculation gives, for the ratio between the diameter of the isolated spheres which result from the transformation and the diameter of the cylinder, the number 1.82, and for the ratio between the distance of two neighbor spheres and this same diameter, the number 2.18.

We will later see (§ 406) that the theory explains perfectly the influence of resistances over the length of divisions.

§ 374. Yet another consequence arises from our discussion. To simplify, let the diameter of the cylinder taken as the unit of length. Then the ratio between the normal length of divisions and the diameter will express this normal length itself, and the ratio which constitutes the limit of stability will express the length corresponding to this limit. This being appropriate, let us take again the conclusion at which we arrived at the beginning of the preceding paragraph, a conclusion which we will state, consequently, here by saying that for all liquids the normal length of divisions always surpasses the limit of stability; we point out, in the second place, that the sum of the lengths of a narrowing and a bulge is equal to that of a division (§ 371), and, in third place, that the length of a narrowing is equal, or about, to that of a bulge (§ 359). However, from the whole of these proposals it follows that, when the transformation of a cylinder starts to happen, the length of only one portion, either a narrowing or a bulge, is necessarily greater than half of the limit of stability; and consequently, the sum of the lengths of three contiguous portions, for example of two bulges and intermediate narrowing, is greater than one and a half this same limit. Thus, finally, if the distance between the solid bases is between one and one and a half the limit of stability, it is impossible that the transformation gives rise to three portions, and it will not be able, consequently, to produce but one bulge juxtaposed with one narrowing. It is, indeed, as we saw, always in this manner that things occurred with regard to the cylinder in § 359, a cylinder which was obviously under the condition above, and one now has explained the asymmetry of its transformation.

§ 375. In finishing § 362, we announced that we have still to describe a remarkable fact which always accompanies the end of the phenomenon of the transformation of a liquid cylinder.

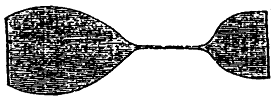

Fig. 97

In the transformation of the large cylinders of oil, either imperfect, or exact (§§ 45 and 359), when the narrow part is thinned considerably, and separation seems about to take place, one sees the two masses ebbing quickly towards the rings or the discs; but they leave between them a cylindrical filament which still establishes, for a very short time, the continuity between them (fig. 97); then this filament is dissolved itself in partial masses. Generally it is divided into three parts, whose two extremes will merge with the two large masses, and whose intermediate part forms a spherule a few millimetres in diameter, which remains isolated in the middle of the interval which separates the large masses; moreover, in each of the intervals between this spherule and the two large masses, one sees another spherule much smaller, which indicates that the separation of the parts of the filament above was carried out in the same way by taperings; fig. 98 shows this final state of the liquid system.

In the case of our mercury cylinders, the resolution into spheres is achieved too quickly for one to see the formation of the filaments; but one always finds, in several of the intervals between the spheres, one or two very small spherules, from which one can conclude that separation was carried out in the same mode.

Now that we know all the stages which the transformation of a liquid cylinder must follow into isolated spheres, we can represent it graphically; fig. 99 shows several of the successive forms through which the liquid shape passes gradually, starting from the cylinder until the system of isolated spheres and spherules. This figure refers to the case of complete external freedom; consequently, according to the probable conclusion which ended § 373, the ratio between the length of divisions and the diameter was taken equal to 4.

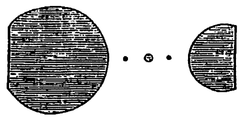

Fig. 98

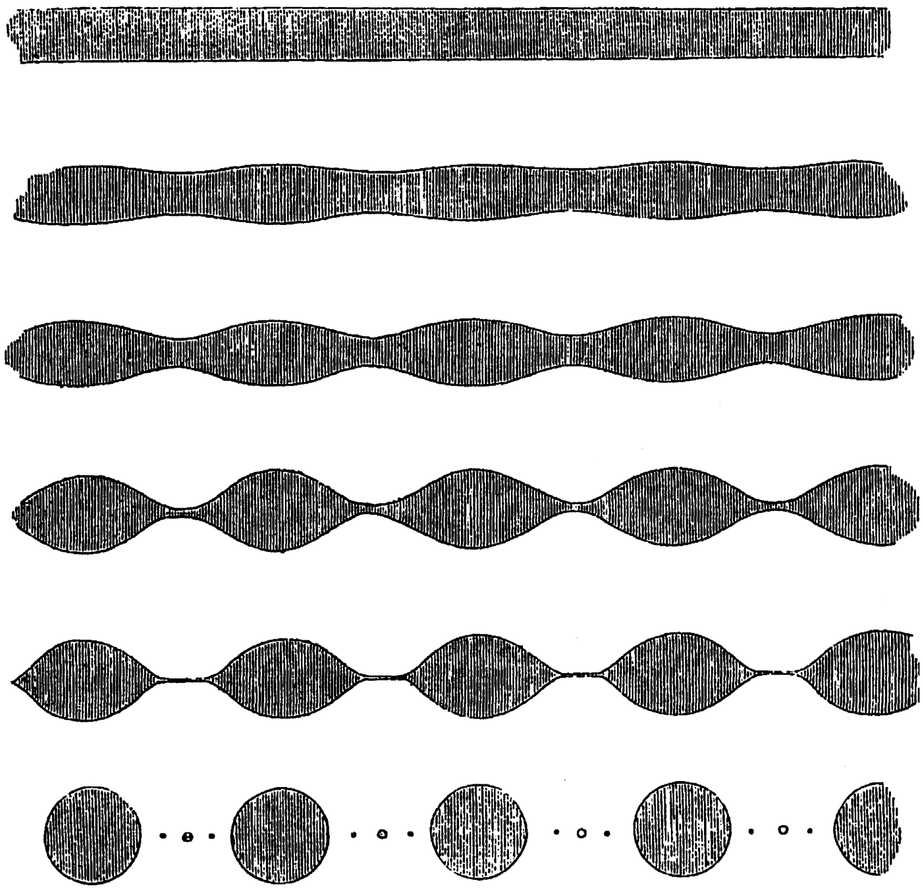

Fig. 99

The phenomenon of the formation of the filaments and their resolution into spherules is not limited to the case of the breaking of the equilibrium of liquid cylinders; except for rare exceptions about which we will speak later, it appears whenever one of our liquid masses, whatever its shape, is divided into partial masses; for example, when the experiment from §§ 222 and 368 is carried out carefully, and the liquid rings which are formed after the rupture of the two plane films are converted into isolated masses, there is, in each interval between those, a smaller mass, which is a spherule; only these small masses are relatively bulkier than that which is shown after the transformation of our short oil cylinders; moreover, the duration of the transformation of the liquid rings in question is several seconds; one thus easily notes the formation of the filaments and the conversion of each one of them into spherules.

The phenomenon occurs in the same way with liquids subjected to the free action of gravity, although it is then less easy to note. For example, if one soaks in ether the round end of a glass rod, and then one vertically withdraws it with care, one sees, at the moment when the small quantity of liquid which remains adherent to the rod separates from the mass, an extremely small spherule roll on the surface of the latter.

§ 376. Let us try to explain the generation of the filaments. There is an obvious analogy between this generation and that of films; indeed, a film in general starts to be born, we know, when two opposed concave surfaces are approaching gradually, and are close to touching; and, in the spontaneous separation of a full cylinder into isolated masses, the filaments start to be formed when the meridian cross-sections of the shape are not very distant from touching by the tips of their concave parts; however, we will see that this analogy under the conditions of the production is related to an analogy between the causes.

When an unstable liquid cylinder passes gradually to the state of separated masses, narrowings can deepen only by driving out their liquid into the bulges. That being so, let us consider, at a time of the phenomenon before the appearance of the filaments, a cross-section of a narrowing close enough to the middle so that while passing from one side to the other the diameter remains appreciably the same; the quantity of liquid lost, in a given interval of time, by the section ranging between these two cross-sections, a quantity which necessarily crosses in this same time the cross-section considered, is obviously measured by the difference of volumes of two cylinders having for common length the distance from the two cross-sections, and for radii those of these cross-sections at the beginning and the end of the time in question. If we divide the total duration of the transformation into equal and very small intervals, and if we indicate by r the first value of the radius, by α the reduction which it undergoes in one of these small intervals of time, and finally by l the distance between the two cross-sections, the volumes in question will be respectively $\pi r^2 l$ and $\pi(r-\alpha)^2 l$; and as α is necessarily very small, which makes it possible to neglect its square, the difference in these volumes will be reduced to $2\pi l r\alpha$. Such is thus the measurement of the quantity of liquid which, in a constant very short interval of time, passes through our cross-section; however, the speed with which this passage is carried out is obviously proportional to the quantity above and inversely proportional to the area πr^2 of the cross-section; it is thus proportional to the ratio $\frac{2\pi l r\alpha}{\pi r^2} = 2l\frac{\alpha}{r}$, or simply, by supposing constant the distance l of the two cross-sections, to the ratio $\frac{\alpha}{r}$.

Now, as the transformation is accelerating, the numerator increases initially quickly, while the denominator r decreases, so that, for this double reason, the speed of the passage of the liquid through our cross-section starts by growing according to a very fast law; but I say that this same speed ends up becoming appreciably uniform. Indeed, in the phenomenon of the transformation, the movements of the liquid molecules consist especially of relative displacements, since the shape does nothing but change form; however, as I often recalled in connection with films, viscosity opposes to these relative displacements a resistance which increases considerably with their speed; one can thus claim that, in consequence of this resistance, the speed of the passage through the cross-section considered, initially quickly increasing, takes at the end an about constant value, as happens with regard to any accelerated speed and subjected to resistances which increase much with it.

The ratio $\frac{\alpha}{r}$ thus reaches, when the radius r is sufficiently reduced, a value which can be viewed as not varying, then; but this constancy can take place only if α, which represents the speed of thinning of the middle of the narrowing, decreases at the same time as the radius r; thus this speed of thinning, which originally was very accelerated, finishes, on the contrary, by being delayed. But, in this phase of the transformation, the cross-sections of the narrowing more distant from the middle having lost less in diameter, the movement of molecules which pass there is much less blocked by the resistance of viscosity, and consequently, towards the end of the phenomenon, while thinning slows down in the middle of a narrowing and in the neighboring portions, it continues its accelerated progress in the more distant portions; however it obviously follows that the middle of the shape must take a lengthened and quasi-cylindrical form then, or constitute what I named a filament.

Let me say here that Beer presented a very different explanation of the formation of the filaments; we will later return there (§ 423).

§ 377. A curious fact is that the identity between the film shapes and the filled shapes is supported in the particular phenomenon which we have just studied; in other words, when an unstable film shape is divided spontaneously in isolated portions, the separation of those in the same way is accompanied by the formation of filaments, which are also converted into spherules, and these filaments and spherules are films like the shape from where they come. For example, in the disunion of the film oil catenoid created within the alcoholic liquid (§ 222), one distinguishes perfectly the film filament and its transformation into spherules; with the film catenoid of glyceric liquid (§ 111), one cannot observe the filament, because of the speed of the phenomena; but, at the time of the disunion, one sees a spherule of some millimeters diameter to fall on the lower film, and to rebound there for a few moments; this spherule changed then into a biconvex film lens, set in its edge in the film. The same if, after having created, by means of the glyceric liquid, a necked partial unduloid or a bulging partial unduloid (§ 113) one continues to raise the upper ring until the breaking of the equilibrium, one sees, at the moment of the disunion, a spherule a few millimetres in diameter escape from the shape and fly in the air, or fall on the bubble which is formed in the lower ring, according to more or less of the tenuity of the film which constitutes this same spherule.

§ 378. It is easy to extend to the generation of the film filaments the theory expounded above. In a full mass, the forces which produce the transformation emanate from the surface layer, and the portion of this layer which corresponds to a narrowing acts by extruding, by its contraction, the liquid which it surrounds towards the adjacent parts of the mass; in a film shape, the forces emanate from the surface layers of the two faces of the film, and these two systems of forces are added one to the other. If thus the film shape is made of oil within the alcoholic liquid, the film, while contracting, drives out the interior alcoholic liquid on both sides of the middle of the neck; our theory receives consequently its application, and there is formation of a film filament. If the film shape is formed in the air, for example with soap water or the glyceric liquid, it is the air which is extruded from the neck. In this last case, because of the extreme mobility of the molecules of gases, it seems initially that one cannot utilize any more the influence of viscosity; but it is necessary to pay attention to the fact that the phenomenon achieves a much greater speed, so that, by this excess speed, resistance to the relative movements of the molecules of the air becomes sufficient to produce the same result, i.e. the birth of a filament.

§ 379. Let us continue the experimental study of the spontaneous transformation of the cylinders into isolated spheres. Let us try to discover the law whereby the duration of the phenomenon varies with the diameter of the cylinder, and to obtain at least some indication of the absolute value of this duration for a cylinder of a given diameter, formed of a given liquid, and placed in given circumstances.

One understands initially a priori that, for the same liquid and same external circumstances, and by supposing that the length of the cylinder is always such that divisions take their normal length exactly (§ 365), the duration of the phenomenon must grow with the diameter. When it is large, the mass of each division is larger, and, on the other hand, the curvatures, on which the intensities of the shaping forces depend, are lower. It is true that the surface of each division also increases with the diameter of the cylinder, and that, consequently, it is the same for the number of the elementary shaping forces; but this increase takes place in a lesser ratio than that of the mass.

I assured myself, using the mercury cylinders of 1.05mm and of 2.1mm of diameter (§§ 366 and 369), that the duration of the phenomenon grows, indeed, with the diameter: although the transformation of these cylinders happens very quickly, one however easily recognizes that the duration relating to the larger diameter is higher than that which refers to the smaller.

As for the law which governs this increase in duration, it would undoubtedly be difficult to obtain from it the experimental determination in a direct way, i.e. by measuring times which the achievement of the phenomenon with regard to two rather long cylinders would require so that they are converted respectively into several isolated spheres. One would probably reach that point by resorting to the process indicated by Mr. Donny (§ 367); but one can arrive at the same result in a simpler way, although with certain restrictions about which we will speak soon, by the means of two short oil cylinders formed between discs, cylinders which nothing prevents us from giving large enough diameters to make easy the precise measurement of the durations. The transformation of a cylinder of this kind produces one only narrowing and one bulge; but, as in the transformation of cylinders long enough to provide several complete isolated spheres, the phases through which the narrowings and the bulges pass are the same ones for all, it is enough to consider only one narrowing and only one bulge. It is understood that the two solid systems must have relative dimensions such that the ratio between the distance between the discs and the diameter of those is the same for the two systems, so that similarity exists between the two liquid shapes at their origin and at every corresponding moment of their transformations.

Before accounting for the use of these oil shapes for the research of the law of the durations, we must present several significant remarks here. We will not have to make use (chap. XI) of the law in question except in the simplest case, where the cylinders would be formed in a vacuum or the air, and would be free from any external resistance, or, in other words, free on all their convex surface. However, our short oil cylinders are formed within the alcoholic liquid, and one can wonder whether this circumstance does not influence the ratio of the durations corresponding to a ratio given between the diameters of these cylinders. Initially, indeed, a more or less large portion of the alcoholic liquid must be moved by the modifications of the shapes, so that the total mass to drive in a transformation is composed of the mass of oil and this portion of the alcoholic liquid; but it is clear that under the terms of the similarity of the two shapes of oil and their movements, the respectively moved quantities of the ambient liquid will be between them exactly or at least appreciably like the two oil masses; so that the ratio of the two total masses will not be affected by this circumstance. It is quite probable, according to that, that this same circumstance will not influence either the ratio of the durations; only the absolute values of these durations will be more considerable.

In another case, the mutual attraction of the two liquids in contact decreases the intensities of the shaping forces; but it is easy to see that this reduction does not alter the ratio of these intensities between the two shapes. Indeed, let us imagine that at one corresponding moment of the two transformations, the alcoholic liquid is suddenly replaced by oil, and conceive, in thought, the surfaces of the two shapes, such as they were at this moment. Then the shaping forces will be completely destroyed by the attraction of the oil external to these surfaces, or, in other words, external attraction, at each point, equal and opposite to the interior shaping force. So now we restore the alcoholic liquid, the intensities of the external attractions will change, but they will obviously preserve between them the same ratios; from which it follows that those which correspond to two corresponding points taken on the two shapes, will be still between them as the interior shaping forces therefore of these two points; so that ultimately, the respective resultants of the external and interior forces at these two same points will be between them in the same ratio as the two interior forces only. Thus the attractions exerted on oil by the ambient alcoholic liquid will decrease the absolute intensities of the shaping forces, but they will not change the ratios of these intensities, and it is to be believed, consequently, that they will not have any influence on the ratio of the durations. But it is clear that this cause will much increase the absolute values of those.

By the two reasons which we have just explained, the presence of the alcoholic liquid will thus increase considerably the absolute values of the two durations; but one can claim that it will not alter the ratio of these values, so that this ratio will be the same one as if the phenomena took place in a vacuum or in the air. We will consider, consequently, the law that we will deduce from our experiments on the short oil cylinders as independent of the presence of the ambient alcoholic liquid, and it is what will be supported by the nature of this law.

During the transformation of our short cylinders, a characteristic is presented which involves a restriction. The two final masses in which such a cylinder is resolved being unequal, the smaller notably reaches its form of equilibrium before the other, so that the duration of the phenomenon is not unique. It follows that we will only be able to count the duration to the moment of the rupture of the filament; and, consequently, the ratio that we will thus obtain for two cylinders will be only that of the durations of two corresponding portions of the total transformations. Moreover, the ratio of these partial durations is precisely that which we will have to make use later.

§ 380. I carried out the experiments in question by employing two systems of discs, whose respective dimensions were between them as 1 to 2; in the first, the discs had a diameter of 15mm and were separated by a distance of 54mm, and, in the second, the diameter was 30mm with 108mm between them. The cylinders formed respectively in these two systems were thus similar, and, as one would expect, the similarity between the two shapes was maintained exactly, in so far as the eye could judge, in all the phases of their transformations.

It happened sometimes that the cylinder, seemingly well formed, did not show any persistence, and immediately started to deform; this circumstance having to be attributeted to a small remainder of irregularity of the shape, I restored at once the cylindrical form[234], and I counted time only when the shape could be maintained in this form for a few moments. But then still sometimes another anomaly arose, which consisted of the simultaneous formation of two narrowings including between them a bulge; this modification stopped after having reached a degree rather not very marked besides, and the shape seemed to remain in the same state for a notable time; then one of narrowings developed little by little more, while the other was erased, and the transformation continued then in the ordinary manner. As this characteristic constituted an exception to the uniform running of the phenomenon, I ceased counting as soon as it appeared, and I again restored the cylindrical form. I definitively did not continue to count time except whenever, after some persistence of the cylindrical form, there was one narrowing.

For each of the two cylinders, I repeated the experiment twenty times, in order to obtain an average result. After a transformation happened, I joined together into only

[234] See the note of § 339.

one the two masses to which it had given rise, and I reformed the cylinder[235], to pass to a new measurement of time.

Here are the numbers of seconds obtained; each one of them expresses the time passed since the moment of the formation of the cylinder until that of the rupture of the filament. These times were counted using a watch beating the fifths of a second.

CYLINDER OF 15mm DIAMETER.	CYLINDER OF 30mm DIAMETER
25″0	59″6
26.6	73.0
28.0	57.0
30.0	61.0
24.8	67.8
35.2	60.0
27.0	63.6
30.0	54.2
30.4	61.0
29.8	52.6
36.4	51.6
32.0	68.0
30.4	73.6
24.6	61.8
32.6	53.0
33.8	58.0
33.8	63.8
20.2	60.0
28.6	52.6
32.6	55.2
Average.. 29″59	Average.. 60″38

It is seen that the numbers relating to the same diameter do not deviate enough from each other so that one cannot regard the ratio of the two averages as approaching near the true ratio the durations. However, the ratio of these two averages is 2.04, i.e. almost exactly equal to that of two diameters. Moreover, it is obvious that, for each one of the latter, the largest of the numbers obtained must correspond to the cylinder formed in the most perfect way, and, consequently, it is probable that the ratio of these two greater numbers approaches also near the true ratio of the durations. However, these two numbers are, on the one hand 36.4, and, other, 73.6, and their ratio is 2.02, a number which differs even less from 2, the ratio of the diameters.

We can thus claim that the durations relating to these two cylinders are between them as the diameters of these same cylinders; from which we will deduce this law, that the partial duration of the transformation of a similar cylinder is proportional to its diameter.

I said (§ preced.) that the law thus obtained would provide by itself a new reason to believe that it would not change if our short oil cylinders were formed in a vacuum or

[235]I stretched, for this purpose, the large mass towards the small one, with the ring about which I spoke in § 46. But it was necessary to prevent the ring, when leaving the liquid shape, from involving with it a significant quantity of oil; for that, instead of making adhere to the large mass the totality of the ring, I left free a small portion of it, and as then its action was insufficient to extend the large mass until the other, I helped there by slightly pushing the oil with the end of the nozzle of the syringe. When after the meeting of the two masses I withdrew the ring, it gave up in the alcoholic liquid only one extremely small spherule, that besides I then joined together with the remainder of oil using the ring itself, as well as largest of the spherules due to the transformation of the filament.

the air. Indeed, the proportionality with the diameter is the simplest possible law, and, in addition, the circumstances in which the phenomenon takes place are less simple in the case of the presence of the alcoholic liquid than they would be in its absence; consequently, if the law changed first with the second, it would follow that a simplification in the circumstances would bring, to the contrary, a complication in the law, which is not very probable.

We thus can, I think, legitimately generalize the law above according to the whole of the remarks of the preceding paragraph, and to draw from it the conclusion which follows:

If one supposes a liquid cylinder formed in a vacuum or the air, long enough to provide several spheres, free on all its convex surface, and of a length such that the divisions take exactly their normal length, the time which will elapse from the origin of the transformation until the moment of the rupture of the filaments will be exactly or appreciably proportional to the diameter of this cylinder.

§ 381. Let us occupy ourselves now with the absolute value of the time in question, for a given diameter, the cylinder always being supposed formed in a vacuum or in the air, long enough to provide several spheres, free on all its convex surface, and of a length such that its divisions take their normal length.

It is clear that this absolute value must vary with the nature of the liquid: because it depends obviously on its density, the intensity of its shaping forces, and finally on its viscosities.

The experiments which we have just reported give with regard to oil only one extremely distant upper limit: it is what initially results from the two causes that we announced in § 379 and which are due to the presence of the alcoholic liquid; but to these two causes joins a third which we must make known. If one imagines an oil cylinder formed under the above conditions, the sum of the lengths of a narrowing and a bulge will be probably more considerable with regard to this cylinder than with regard to our short oil cylinders having the same diameter: because, due to the great viscosity of oil, this sum must undoubtedly exceed more in our short oil cylinders the length which corresponds to the limit of stability. However, one can pose in theory, that, all else equal, an increase in the sum of the lengths of a narrowing and a bulge tends to make the transformation faster, and, consequently, shorten the total and partial durations of the phenomenon. Indeed, for a given diameter, the more the sum in question moves away from the length which would correspond to the limit of stability, the more the forces which produce the transformation must act with energy; moreover, immediately below the limit of stability the transformation does not happen, so one can then regard the duration of phenomenon as infinite, from which it follows that when one passes beyond this limit, the duration passes from an infinite value to a finite value, and that, consequently, it must decrease quickly starting from this same limit; finally, it is also what the results of experiment confirm, as we will later show in § 407. Thus, at the time same as it would be possible to form in the vacuum or the air one of our short oil cylinders, and to eliminate, consequently, the two causes of delay due to the presence of the alcoholic liquid, the duration relating to this cylinder would still exceed that which would refer to an oil cylinder of the same diameter formed under the conditions as we supposed.

§ 382. But if, by counting the absolute duration in the case of one of our short oil cylinders, we obtain with regard to this liquid only an upper limit much too high, the mercury cylinder of § 369, a cylinder which is formed in the air, and of which the length is sufficient compared to the diameter so that divisions have exactly or with very nearly their normal length, will provide us, on the contrary, with regard to this last liquid, a lower limit probably more approximate.

Initially, in the case of this cylinder, whose diameter was, as we said, 2.1mm, the transition is not carried out in a so short time that one cannot estimate with some exactitude the total duration of the phenomenon; I say the total duration, because in such a fast transformation, it would be quite difficult to catch the moment of the rupture of the filaments. To approach as much as possible the value of this total duration, I had recourse to the following process.

I regulated, by successive tests, the beats of a metronome in such a manner that by raising at the precise moment of a beat the system of the glass strips pertaining to the apparatus which is used to form the cylinder, the following beat appeared to coincide with the termination of the transformation; then, after having still assured myself several times that this coincidence appeared quite exact, I determined the duration of the interval between two beats, by counting the oscillations carried out by the instrument during two minutes, and dividing this time by the number of the oscillations.

I found thus, for the interval in question, the value $0.39''$. The total duration of the transformation of our mercury cylinder can thus be estimated roughly as $0.39''$, or, more simply, as $0.4''$.

But this cylinder is not free on all its convex surface, and its contact with the glass plate must influence its duration, directly as well as by the increase which it determines in the length of divisions. Thus let us examine from this double point of view the influence in question.

The direct action of the contact with the plate is undoubtedly quite weak, because, as soon as the transformation starts, the liquid must be detached from the glass in all the intervals between the bulging parts, so as to touch the solid plane only by one series of very small surfaces belonging to these bulging parts; consequently, if only the direct action of the contact of the plate were eliminated, i.e. if one could make it so that the cylinder was free on all its convex surface, but that divisions which are formed there took the same length as before, the total duration would hardly be decreased.

There remains the effect of the lengthening of divisions. The length of divisions of our cylinder is equal to 6.35 times the diameter (§ 370), while, on the assumption of a complete freedom of the convex surface, this length would be most probably less than 4 times the diameter (§ 373); however, under the terms of the principle established in the preceding paragraph, this increase in the length of divisions involves necessarily a reduction in the duration, a reduction all the more considerable when it is in the vicinity of the limit of stability; consequently, if one could make it so that the lengthening in question did not exist, the total duration would be very notably increased.

Thus, the suppression of the direct action of the contact of the plate would produce in the total duration only a very slight reduction; and the cancellation of the lengthening of divisions would determine, on the contrary, a very notable increase in this same duration; if thus these two influences were eliminated at the same time, or, in other words, if our cylinder were free on all its convex surface, the total duration of its transformation would be very notably higher than the direct result of the observation.

Now, the quantity that we have to consider is the partial duration, and not the total duration; but, in the same circumstances, the first must be not very lower than the second: because when the filaments break, the masses between which they extend already approach the spherical form; consequently, in virtue of the conclusion obtained above, we must admit that the partial duration with which we occupy ourselves, i.e. that which would refer to the case of complete freedom of the convex surface of the cylinder, still notably exceeds the total duration observed, which is $0.4''$.

On the basis of this value of $0.4''$as constituting the lower limit corresponding to a diameter of 2.1mm, the law of proportionality of the partial duration to the diameter will immediately give the lower limit corresponding to another arbitrary diameter: it will be found, for example, that, for ten millimetres, this limit would be $\frac{0.4'' \times 10}{2.1} = 1.9''$, or more simply $2''$.

Thus if one supposes a mercury cylinder one centimetre in diameter, formed in a vacuum or the air, long enough to provide several spheres, free on all its convex surface, and of a length such that its divisions take their normal length, the time which will elapse since the origin of the transformation of this cylinder until the moment of the rupture of the filaments, will notably surpass two seconds.

§ 383. Let us return, for one moment, to our theory of the generation of the filaments (§§ 376 to 378). An immediate consequence of this theory is that the higher is the speed with which a narrowing deepens, the less the filament must be thin; however, as for a bulge considered above, a narrowing must deepen all the more quickly as it is longer; if thus, in consequence of particular circumstances, a narrowing pertaining either to a cylinder, or to another shape, is very lengthened, the filament to which it will give rise will be thick, and the principal spherules resulting from its conversion will be able to not differ much in diameter from the two extreme masses; it is even possible that, considering its length, it provides more than one large spherule; we will see a remarkable example of these cases. If, on the contrary, one narrowing is very short, the filament will be very thin, and consequently a very small spherule, or it may even form neither filament nor spherule; an experiment of Mr. Tait (§ 345) offers an example of this last case, and we will indicate another in § 504.

Moreover, since a filament transforms as soon as the ratio between its length and its diameter reaches the limit of stability of a cylinder, it follows that a very short filament will have to also be very thin to transform, and that if, at the time when it has been just developed, it does not have a rather small diameter, it will change only after having been sufficiently thinned; while a very long filament, a filament which, according to what precedes, is originally thick, will be able to transform itself into this state. Thus there is a second reason of the difference of the results provided respectively by a short narrowing and a long narrowing.

§ 384. It is not useless to present here, in short, the whole of the facts and of the laws which the described experiments in what precedes led us to establish with regard to unstable liquid cylinders.

1° When a liquid cylinder is formed between two solid bases, if the ratio of its length to its diameter exceeds a certain limit whose exact value lies between 3 and 3.6, the cylinder constitutes a shape of unstable equilibrium.

The exact value in question is what we name the *limit of the stability of the cylinder.*

2° If the cylinder has a considerable length compared to its diameter, it converts spontaneously, by the breaking of equilibrium, into a series of isolated spheres, equal in diameter, also equally spaced, having their centers on the line which formed the axis of the cylinder, and in the intervals of which are arranged, along this same axis, spherules of various diameters. But each solid base retains adherent on its surface a portion of a sphere.

3° The progress of the phenomenon is as follows: the cylinder starts by gradually bulging in portions of its length located at equal distances from each other, while it is thinned in the intermediate portions, and the length of the bulges thus formed is equal or to very close to that of the narrowings; these modifications continue to develop more and more, while being carried out with an accelerated speed, until the middles of the narrowings became very thin; then, from each one of these middles, the liquid is withdrawn quickly in the two directions, but still leaving the masses joined together two by two by an appreciably cylindrical filament; then this filament undergoes the same modifications as the cylinder; there are in general only two narrowings formed, which include, consequently, between them a bulge; each one of these small narrowings is converted in its turn into a thinner filament, which breaks at two points and gives rise to a very small isolated spherule, while the bulge above is transformed into a larger spherule; finally, after the rupture of these last filaments, the large masses take a completely spherical form. All these phenomena are achieved in a symmetrical way compared to the axis, so that, for their length of time, the shape does not cease being of revolution.

4° We name the *divisions* of a liquid cylinder, the portions of this cylinder of which each one must provide a sphere; we consider these portions either in the cylinder before they started to take shape, or we take them during the transformation, i.e. while each one of them changes to arrive at the spherical form. The length of a division measures, consequently, the constant distance which, during the transformation, lies between the middle circles of two neighboring narrowings.

We name, moreover, *normal length of divisions,* that which divisions would take if the cylinder to which they belong had an infinite length.

In the case of a cylinder limited by solid bases, divisions take the normal length when the length of the cylinder is equal to the product of this same normal length by a whole number or by a whole number plus a half.

Thus, if the second factor is a whole number, the transformation is laid out in such a manner, that, during its achievement, the shape finishes at one end in a narrowing and at the other in a bulge; if the second factor is composed of a whole number plus a half, the shape finishes at each end in a bulge.

When the length of the cylinder fills neither one nor the other of these conditions, divisions take the length most approximate possible to the normal length, and the transformation adopts that of the two arrangements above most suitable to achieve this goal

5° For a cylinder of a given diameter, the normal length of divisions grows with resistances which obstruct the transformation; it varies thus, but undoubtedly rather slightly, with the nature of the liquid; viscosities, interior as well as surface, constituting these resistances. If there is an external resistance, such as the contact of the convex surface of the cylinder with a solid plane, this resistance consequently also increases the normal length of divisions. In all the statements which follow, we will take the simplest case, which is that of the absence of any external resistance; in other words, we will always suppose the cylinders in a vacuum or the air, and free on all their convex surface.

6° Two cylinders different in diameter, but formed of the same liquid, and having lengths such that divisions take in each one of them the same normal length divide in a similar way, i.e. the respective normal lengths of divisions are in ratio between them as the diameters of these cylinders.

In other words, the nature of the liquid not changing, the normal length of divisions of a cylinder is proportional to its diameter.

It is the same, consequently, for the diameter of the isolated spheres to which normal divisions convert, and for the length of the intervals which separate these spheres.

7° In consequence of interior and surface viscosities, the ratio between the normal length of divisions and the diameter of the cylinder always exceeds the limit of stability. It would undoubtedly be reduced to this limit if the liquid were free from shearing viscosity.

8° One can adopt 4 as the average value of this ratio in various liquids. According to that, one has, for the probable approximate value of the ratio between the diameter of the isolated spheres which result from the transformation and the diameter from the cylinder, the number 1.82; and, for that of the relationship between the distance from two neighbor spheres and this same diameter, the number 2.18.

9° The time which passes from the origin of the transformation until the moment of the rupture of the filaments is exactly or appreciably proportional to the diameter of the cylinder.

10° For the same diameter, and divisions having always their normal length, the absolute value of the time in question varies with the nature of the liquid.

11° In the case of mercury, and for a diameter of one centimetre, this absolute value is notably higher than two seconds.

12° When a cylinder is formed between two solid bases brought sufficiently close so that the ratio of the length of the cylinder to its diameter lies between one and one and a half the limit of stability, the transformation produces one narrowing and one bulge; one obtains then for final the result, only two portions of spheres unequal in volume and curvature, respectively adherent at the solid bases, plus the interposed spherules.

§ 385. We have just studied in experiments what relates to the stability of liquid cylinders; let us pass to the catenoid.

We saw (§ 60) that the partial catenoid of maximum height, when it is formed within the alcoholic liquid, filled with an oil mass, is always perfectly stable, but one must regard it as being at its limit of stability. We procede, as I announced (§ 111), to clear up this puzzle.

For that, let us start by describing, with their curious characteristics, the experiments relating to the search for this maximum catenoid. Let us point out initially what we already said (§ 62), which is: that the external diameter of the rings was 71mm; that, in all the experiments in question, after having formed a cylinder between these rings, one removed oil from the mass gradually; finally that one stopped the extraction from time to time to observe the shape. Here now are the results:

1st EXPERIMENT. Separation of rings, 55mm. One reduces by degrees to a fraction of a millimetre the sagitta of the segments of a sphere which constitute the bases; then, during an interruption of extraction, a singular phenomenon occurs: the shape undergoes a small spontaneous modification; the convexity of the bases increases quickly, until the sagitta takes again a value of approximately 2.5mm, and consequently the narrowing formed between the rings is thinned somewhat, then all stays perfectly stationary. By further extracting oil with care, the sagitta increases until nearly 3mm; finally, following a new extraction, the shape is divided in the ordinary manner by the middle of the narrowing.

2nd EXPERIMENT. Separation of rings, 49mm. The bases end up losing any appreciable curvature, then there is, as previously, spontaneous transformation: the bases become again slightly convex, with a sagitta of approximately 1mm. A new extraction brings the disunion.

3rd EXPERIMENT. Separation of rings, 47mm. The bases still appear to be levelled completely, and the shape persists in this state. Later extractions initially seem to have no other effect than to deepen the necking, without the bases ceasing being plane; then a small convexity is reformed, but not spontaneously: it is born and increases as one extracts; when the sagitta is about 1.5mm, the disunion happens.

4th EXPERIMENT. Separation of rings, 45mm. The bases become initially plane, then slightly concave; the sagitta of this concavity grows until roughly 2mm, then one observes a spontaneous transformation again; concavity changes into a convexity, whose sagitta is almost a millimetre. The action of the syringe then causes the disunion.

5th EXPERIMENT. Separation of rings, 43mm. The bases are made plane, then concave, and the sagitta of concavity gradually reaches 4mm or 5mm; finally the shape is divided.

§ 386. Let us see what these experiments teach us. Initially, notice it is not easy to consider the precise point where the bases of the shape are made plane, an extremely low curvature escaping the sight. From there is born some uncertainty in the determination of the limiting height of the catenoid; fortunately the characteristics which we announced provide a more exact means of appreciation.

In the fourth experiment, one necessarily passes through plane bases, since the curvature, convex that it was, becomes gradually concave by the progres of the extraction of the liquid; but is it the same in the second and the third, where one appeared to also arrive at planes? Let us try to clarify this question. The first, the second and the fourth experiment have in common that there was a small modification or spontaneous transformation of the shape, while in the third this phenomenon did not take place; and this modification was decreasing from the first to the second, to disappear in the third and to reappear in the fourth. One must believe, according to that, that the third experiment forms a kind of passage from this side and to beyond which the small spontaneous transformations appear; but the effect was shown, in the first experiment, when the bases still had a visible curvature, and, in the fourth, when they had taken one in the opposite direction; it is thus quite probable that, in the second, at the moment when one saw being born the spontaneous transformation, the bases still preserved a real curvature, no matter that it was too weak to be distinguished, and that it is only in the third, where the distance between the rings was 47 millimetres, where one reached with the completely plane bases. If, in this third experiment, the bases considered to be plane appeared to start to lose this state only after the extraction of a very notable quantity of liquid, that obviously holds with the difficulty mentioned above to distinguish the point clearly where the curvature vanishes.

Thus, with our rings 71 millimetres in diameter, one can claim that the distance of 47 millimetres differs very little from that for which one starts to obtain rigorously plane bases, and 47 is appreciably 2/3 of 71; it is in this manner that using the experiment only carried out on a full catenoid, I found 2/3 for the very approximate value of the ratio between the height of the limiting catenoid and the diameter of the bases.

§ 387. In the second place, let us draw attention to the small spontaneous transformations considered by themselves. Until now, when we saw one of our liquid shapes changing, and to pass thus from an unstable equilibrium to a steady equilibrium, alteration was deep, the mass separated into two or several parts, and the final result of the phenomenon was always composed of spheres or portions of spheres; here nothing is similar: alteration is small, the mass is not divided, and the final result is a shape which moves away little from the first, at least in the portion formed, and which can be of the same nature. In the first experiment, for example, an unstable partial unduloid is transformed into another unduloid not very different, and it is undoubtedly the same in the second.

Moreover, which is more remarkable still, the comparison of the first two experiments seems to indicate that the unstable unduloid and the stable unduloid into which it converts become arbitrarily close as the distance from the rings is closer to the maximum height of the catenoid.

These facts give us the key to the puzzle relating to the stability of the filled limit catenoid. When, the rings being at the distance which corresponds to this catenoid and a cylinder being formed between them, one makes act the small syringe, the shape becomes initially, we know, an unduloid which, in consequence of extraction, tends from there gradually towards the catenoid; but the third experiment shows us, moreover, that if, after having reached this limit, one continues the operation, the shape becomes again imperceptibly an unduloid which, progressively with extraction, moves away from this same catenoid. If thus the partial catenoid of greater height constitutes the passage between the stable partial catenoids and the unstable partial catenoids, it constitutes, in addition, the passage between a continuous family of stable unduloids and another continuous family also of stable unduloids. Such is obviously the reason for the marked stability of the partial catenoid of greater height created in a filled state; also, as we saw (§§ 111 and 222), when one forms the catenoid in a film state, and that thus the formation of any other shape is made impossible, it loses its stability as soon as the maximum height is given it.

This explanation, however, leaves still some darkness on the idea of a very stable shape at the same time at its limit of stability; I thus will make it more complete. As I have just pointed out, it can happen, in certain cases, that the stable shape towards which goes an unstable shape which becomes deformed spontaneously, is more and more brought closer to this one as the distance between the bases is decreased, and finally merges with this unstable shape for a given value of the distance in question, in a manner that then the shape is necessarily stable; but it is really at its limit of stability, in the sense that if one tries to form it on a longer portion of its meridian line, it will not be maintained. Only the new form which it will take will differ as little from the first as this one will little exceed the limit, so that if one were hardly beyond this limit, the change in form will be very tiny.

Such is undoubtedly the case of the catenoid; if, with a full mass, one managed to form one of them for which the meridian catenary extended beyond the points corresponding to the maximum height, it would constitute the more necked of the two possible catenoids between the same bases, and consequently it would be unstable (§§ 58 and 60); and one can conclude from the 4th and the 5th experiment of § 385, that its spontaneous deformation would convert it into a nodoid or a unduloid, but this change in form would be all the more small as the catenoid would exceed less the limit in question; finally, if it is even with this limit, there will be no change of the whole, and the shape will be stable. We will see a second example hereafter of the same kind.

§ 388. Let us come to the unduloid. The stability conditions of this shape are different primarily according to whether its middle is occupied by a narrowing or a bulge.

In the first case, the limit of stability does not have anything absolute: when the ratio between the distance and the diameter of the bases is rather large, and that, by the gradual extraction of the mass, one approaches the point where the shape would be divided spontaneously, this one presents (§ 55), in addition to the narrowing, two adjacent portions of bulges; in this case thus the meridian line of the partial unduloid at its limit of stability extends much beyond the two points of inflection; but it is not the same any more for a lesser interval between the bases, as in the unduloids close to the limiting catenoid; in the latter, the meridian line is far from reaching the points of inflection; indeed, the catenoid can (§ 61) be regarded as a unduloid in which the points of inflection of the meridian line are located at infinity, and consequently, in a partial unduloid brought very close to the limiting catenoid, an unduloid which, according to the experiments of § 385, is itself very close to its limit of stability, the points of inflection must be located extremely far beyond the bases of the shape.

Happily, things are all different in the second case, i.e. for a partial unduloid whose middle is a bulge: then the limit of stability can be expressed in a way very simple and always the same; indeed, experiment led me (§ 52) to claim that the liquid shape is at its limit of stability when it ends with the middle circles of two consecutive narrowings, or, in other words, at the time it is exactly made up of a bulge between two half narrowingss. We will reconsider this point (§ 409).

§ 389. There remains still, for the equilibrium shapes of revolution, that we refer to the stability of the nodoid. My procedures create, we saw, the portion generated by a part or the totality of a node of the meridian line (§§ 63 to 68), that is to say, the portion generated by an arc of this line turning its convexity towards the outside (§ 71). We would have thus to seek the limit of stability in these two cases of the partial shape.

In the first, experiment showed us (§ 65) the stability extending at least from the circumference generated by the apex of the node to the two circumferences where the elements are perpendicular to the axis of revolution; however there is necessarily a category of nodes for which stability stops on this side of the last circumferences thus characterized. Indeed, let us conceive a nodoid very close to the catenoid (§ 77), and consider in particular one of the nodes of its meridian line; this node will be, we know, very lengthened, so that the points where the tangents are perpendicular to the axis of revolution will be at a very large distance from the axis compared to that of the apex of the node and compared to the interval between them. If thus one could form between two discs the portion of the shape generated by the part of such a node from the apex up to the points in question, these discs would be brought very close relative to their radius, and the narrowed shape would penetrate strongly between them. But, between two discs thus placed, my procedures never give a narrowing from which the meridian line differs little from a half circumference, as fig. 36 and 37 show (§ 65). Between two sufficiently close discs, there are consequently two theoretically possible necked shapes, leaving one and the other the edges of the discs where their respective meridian lines have their elements laid down on the radii of these discs, and penetrating unequally between these same discs; however, as the less necked is always the only one which is created, I concluded from it that the more necked would be unstable, i.e., for this one, stability ceases inside the circumferences where the elements are perpendicular to the axis.

According to that, one must, it seems to me, claim as very probable what follows:

1° In the shape less necked, the limit of stability is beyond the circumferences located at the edges of the discs, so that, to create this shape close to its limit, a different procedure is needed. That of § 67 makes it possible to form, in relief, the portion generated by the totality of a node; in truth, the shape is not stable, but it persists long enough so that one can conclude from it that, if it is not at its limit of stability, it is at least very close: it is not doubtful that, instead of a ring of simple iron wire, one employed a band of iron not very broad and curved cylindrically, so that the meridian line of the liquid shape reached not the crossing point of the node, although close, this shape, which would much exceed the circumferences in question, would be stable[236]. In the shape more necked, on the contrary, the limit of stability is on this side of these same circumferences.

2° As the spacing of the discs becomes larger, the two shapes approach each other, and it is the same for their respective limits of stability; finally, for a certain maximum value of the spacing, these two shapes coincide, like their limits of stability, which are then at the edges of the same discs. I am led to believe that this last case is that of the nodoid whose meridian line is generated by the rolling of an equilateral hyperbola, and I think, moreover, beyond the spacing in question, there is no more possible necked nodoid between the same discs.

If these conjectures are true, the necked nodoid would have, in a simple particular case, a clearly definite limit of stability.

§ 390. In the second case of form, i.e. in that where the shape is generated by a convex arc towards the outside, we saw (§§ 71 and 114) that by gradually bringing closer the two discs, one reaches a point beyond which the shape, either filled, or film, loses its form of revolution, the oil mass or the film going more on one side of the axis of the system; it was also seen that at the smallest distance from the discs where the shape preserves its regularity, the elements of the meridian arc at the points where it

[236] By summarizing my 4th Seiries in the ANN. DE CHEM. AND OF PHYS. OF PARIS (3rd series, vol. LIII), I said, page 37, that the shape obtained in a wire ring, is at its limit of stability; this assertion is obviously too positive.

leads to the two discs, seem to be, or with very nearly, perpendicular to the axis. One could suppose, according to that, that the limit of stability of the bulging partial nodoid corresponds to the extreme elements of the meridian arc being perpendicular to the axis; but I sought to decide the question by new experiments.

I initially measured the diameter of the discs exactly; it was, for one, 71.38mm, and, for the other, 71.82mm, average 71.60mm. I then made a sufficient oil mass adhere to the whole of these two discs in the alcoholic liquid, then the upper disc gradually was lowered, and I stopped it at the point beyond which the bulged shape started to lose its form of revolution. That done, I measured with the cathetometer the interval between the two discs, or rather the distance ranging between the upper face of the upper disc and the lower face of the lower disc, since they were the edges of these two faces from which left the free surface of the liquid shape; I carried out this operation on two opposite sides of the axis, and I found the two values 63.95mm, and 64.08mm, average 64.01mm; finally, by laying out the cathetometer horizontally, I measured the equatorial diameter of the shape, and I obtained 118.67mm.

But, by taking as data the diameter of the discs and the equatorial diameter of the mass, Mr. Lamarle agreed to calculate for me, by means of elliptic functions (§ 84), the distance which should have existed between the solid edges from which left the liquid shape so that on these borders the elements were perpendicular to the axis, and he thus found 51.9mm, a value which is only eight tenths approximately of the measured distance 64.01mm.

I then repeated the experiment with a lesser oil mass. Here the distance from the discs was, on average, 39.63mm, and the equatorial diameter of the shape 101.17mm; the value of the distance between the discs deduced from calculation, in the case of the true horizontality of the extreme elements, was 32.10mm, which constitutes also eight tenths of the measured value.

It follows obviously from this constant dissension between the appearance of the shapes and the results of calculation, that, in these shapes, the extreme elements of the meridian arc still made, actually, a notable angle with the prolongations of the radii of the discs, and that if, in the simple aspect, one could believe this null angle, that held with the great difficulty of a similar appreciation. One must, I think, to conclude from there that, in the bulging partial nodoid, the limit of stability is in on this side of circumferences where the elements are perpendicular to the axis.

§ 391. In the experiments which I have just described, when, after having lowered the upper disc until the last limit where the liquid shape is kept regular, one still lowers this same disc by a very small quantity, the side transport of the mass is also very small, and remains such as long as the disc remains in the same position; it increases by a later lowering, and is shown all the more marked as the lowering is larger.

The bulging partial nodoid thus offers a new example of a permanent liquid shape to us although being at its limit of stability, and the phenomenon is explained as with regard to the filled catenoid: it is that the stable shape to which this nodoid would convert spontaneously if it were beyond its limit, is all the more brought closer as the nodoid is supposed more close to this limit, and coincides finally with it in extreme cases.

Let us add a last note: when the cylinder, the necked unduloid, the bulging unduloid, the catenoid and the necked nodoid reach or exceed their limit of stability, and, consequently, alter spontaneously, the phenomenon is achieved without the liquid shape losing its form of revolution, and the resulting stable shape is still of revolution around the same axis; but, as one has just seen, the bulging nodoid makes an exception: during its spontaneous deformation, the shape is asymmetrical, and it remains such after the completion of the phenomenon. Another example of asymmetry already presented itself to us in the spontaneous deformation of a node of nodoid carried out in relief in a wire ring (§ 67).

§ 392. In this research into the limits of stability of the shapes of equilibrium of revolution, we always have supposed the finite shape has two cross-sections perpendicular to the axis and equal in diameter. But it is clear that one could adopt other terminations and that then the limits of Stability would be different: one could, for example, still take for the bases of the shape two cross-sections perpendicular to the axis, but give them, except in the case of the cylinder, unequal diameters. One saw (§ 91) that Mr. Lindelöf analytically treated, for this ratio, the question of the catenoid; but I had arrived before at a remarkable result that I had made known in my 11th Series; here: when one takes the neck circle for one of the terminations, the catenoid does not have a limit of stability any more, i.e. the second base can as be far from the first as is wanted, without the shape tending to alter spontaneously.

To show it, let us conceive a finite partial unduloid with on one side the neck circle of a narrowing, and, on the other side, the equator of one of the two bulges between which, in the infinite shape, this narrowing would be included; the unduloid thus formed will be very stable, since, by preserving the first base, it would be necessary, to reach the limit of stability, to move back the second to the neck circle according to (§ 52). Let us imagine now that, the first base, which is the neck circle, remaining constant, one varies the unduloid in question in such a manner that it converges gradually towards the catenoid; our second base, i.e. the equatorial cross-section of the bulge, will be growing and moving away from the first, and the shape will obviously preserve its stability; finally, in extreme cases of these variations, or, in other words, when the cross-section in question is infinitely large and is infinitely moved away, the unduloid, which will not have been able to lose its stability, will be a half catenoid to infinity extending starting from the neck circle; if thus one takes where one wants, on this half catenoid, a cross-section perpendicular to the axis, and one makes it the second base of the shape, this shape will be always necessarily stable.

In order to verify this deduction, I took, for the second base, the wire ring 20 centimetres in diameter, employed in the experiment of § l86bis and, for the neck circle, another ring, whose diameter was only 3.5 centimetres; this one was carried by a fork whose tail was fixed under a mobile horizontal arm along a vertical stem. I wet this small ring with glyceric liquid, then I produced a film of the same liquid in the larger, and set this last on its feet, so that the film was horizontal; the support which supported the small ring was then placed so that this small ring was above the large and the centers from both were on the same vertical; I then lowered the small ring until it came to be in contact with the film, then it was gradually raised. The film adhered at the same time to the two rings, necessarily took the form of a portion of a catenoid, and I thus could manage to make initially vertical the element of the catenary meridian which ended in the small ring, then to make it return towards the axis in a visible way, so that the shape presented a beginning of narrowing; in other words; I could not only reach the half catenoid, but even to exceed it a little, without the shape losing its stability.

This last result agrees, as much as one can judge in the absence of precise measurements, with the numbers of the table of § 91. One sees, indeed, by this table, that, whatever the ratio of the diameters of the bases, the catenoid, at its limit, always has a neck circle; but it is seen, at the same time, that as the diameter of the upper base decreases, that of the neck circle approaches it, so that if the diameter of the upper base is only, as in my experiment, 0.17 of that of the lower base, narrowing must be rather slightly showed. I could not, moreover, observe this narrowing just a little on this side of the height limit of the catenoid, since, at this limit, the shape was divided.

In the article where it gives the table in question, Mr. Lindelöf shows that his formulas establish, like my reasoning, the absence of a limit of stability with regard to the catenoid of which one of the bases is the neck circle.

The numbers of the table, at least those of the 2nd column, are likely precise experimental verifications; only they would not be without difficulties, because it would be necessary to find the means of making the planes of the two rings exactly parallel and of rigorously laying out the two centers on the same perpendicular to these planes.

§ 393. Equilibrium shapes which are not of revolution also have, and for the majority without any doubt, their respective limits of stability. Only it is necessary, for each one of them, to also make a convention with the regard of the solid system in which one includes it.

I will initially quote, as an example, that of the surfaces mentioned in § 137, that I created in a film state; as we saw (§ 138), one obtains a stable portion of this surface in the solid system which I chose, when its height is equal to its width; but it is not any more in the same way when the height is quadruple the width.

I will also cite the helicoid of Mr. Lamarle which I formed with oil in the alcoholic liquid (§ 132). It was included, I remember, between cross-sections perpendicular to the axis of the shape; however it showed a well decided stability when the solid cross-sections were distant a quarter of a pitch; but I did not manage any more to form it between two cross-sections a half pitch apart, which indicates that at this length it is unstable.

I was led to believe that the skew helicoid with central axis does not have a limit of stability, at least at the time which it is included, in the film state, in a solid system composed of a portion of the axis and a spiral attached to it by straight segments (§ 130); indeed, that which I formed had two complete whorls, and it was perfectly stable; however, according to a remark of Mr. Schwarz[237], one easily shows the exactitude of this conjecture, on the basis of a known result of the calculation of the variations.

[237] See the article listed in § 508 under the n° 37.

CHAPTER X.

Stability of equilibrium shapes; theoretical study, and experimental checks.

§ 394. In the preceding chapter, we had recourse only to experiment; now let us see what theory can teach us with regard to the stability of our shapes.

A few months after the publication of my 2nd Series, where I had described the experimental study of the stability of the cylinder, Mr. Hagen essayed[238] to apply calculation to the research of the limit relating to this shape. For that, supposing a liquid cylinder whose form is very slightly altered in a manner so that it presents a succession of equal bulges and narrowings of very small magnitude, Mr. Hagen claims that the meridian arcs of these bulges and these narrowings can, without significant error, being compared to arcs of a circle. He calculates, on this assumption, the capillary pressures exerted at the respective tops of a convex arc and a concave arc, and finally he seeks the limit of stability on the basis of the consideration that the difference of the two pressures above must be positive on one side of this limit, and negative on other side; he arrives thus at the value $2^{3/2}$ i.e. at the number 2.8284.

§ 395. In an article[239] in response to this Note, I have shown that the method employed by Mr. Hagen, although clever, could give only a value more or less far away from true, because the meridian arcs of the bulges and narrowings are not arcs of circles, and that in substituting for the latter of the arcs of a sinusoid, obviously closer to those of the real curve, one obtains a notably different result.

I announced that I had arrived, using a rigorous method, at the exact value of the limit in question, and that this exact value is the quantity π, i.e. the ratio of the circumference to the diameter, or 3.1416. I will now set forth this method; the principle on which it rests was provided to me by Mr. Lamarle.

§ 396. Let us suppose a horizontal oil cylinder formed between two discs within the alcoholic mixture, and short enough to be stable, without being, however, too far from the limit. If, by slightly pushing the liquid in greater quantity towards one of the discs by means of the nozzle of the small syringe, one causes the artificial formation of a bulge and a narrowing, and if this modification of the shape does not exceed a certain degree, the mass when released then takes again the initial cylindrical shape spontaneously. But if the alteration exceeds the degree in question, it progresses then spontaneously, and the transformation is completed.

But, at the precise degree of alteration which separates the tendencies for these two opposed purposes, the mass must obviously be indifferent to the one and the other; there must thus be a state of equilibrium there, although this equilibrium is unstable; and as the shape is then still of revolution and it is composed of a bulge and a narrowing, it forms necessarily a portion of unduloid. In the second place, since this partial unduloid constitutes the degree of alteration where will begin the spontaneous tendency to a major alteration, it must deviate less from the initial shape, i.e. the cylinder, as the cylinder is closer to its limit of stability. Lastly, when the cylinder is exactly at this limit, the partial unduloid must coincide exactly with it, since then the weakest trace of a bulge and a narrowing must be enough to bring about the spontaneous transformation.

§ 397. The principle above being admitted, let us apply calculation to it. Let us take again the expression of the general condition which must be satisfied by the meridian lines of equilibrium shapes of revolution, which is (§ 36)

$$\frac{1}{M}+\frac{1}{N}=C,$$

[238] *Ueber die Auflösung flüsiger Cylinder in Tropfen* (ANN. OF MR. POGGENDORF, year 1850, flight. LXXXI p. 559).

[239] *Ueber die Gränze der Stabilität eines flüssigen Cylinders* (ibid, ibid, p. 566.)

an expression where M is the radius of curvature and N is the normal. In the case of the cylinder, the meridian line being a straight line, M is infinite everywhere, which reduces the formula to $\frac{1}{N} = C$, from whence $N = \frac{1}{C}$; however as the line in question is parallel to the axis, the normal N is the radius of the generated cylinder, from whence it follows that this radius is equal to $\frac{1}{C}$.

Let us recall, moreover, that the general expression above, put in the differential form, can be integrated one time (§ 84). If one takes the axis of revolution as the x axis, this integral, which represents our meridian line, becomes:

$$\frac{y}{\sqrt{1+p^2}} = \frac{Cy^2}{2} + C', \tag{8}$$

p indicating the differential coefficient $\frac{dy}{dx}$, and C' being the arbitrary constant of integration. For a cylinder, the slope is null everywhere, thus making $p = 0$. Solving for y, one has:

$$y = \frac{1}{C} \pm \frac{1}{C}\sqrt{1-2CC'}.$$

It is clear that here y is the radius of the cylinder, and since this radius is simply equal to $\frac{1}{C}$, the arbitrary constant C' must be given so as to cancel the radical $\sqrt{1-2CC'}$, i.e. it is necessary to make $C' = \frac{1}{2C}$.

That said, let us imagine a cylinder formed between two discs of radius $\frac{1}{C}$, and suppose the distance between these discs is such that the cylinder is beyond its limit of stability, but extremely close to it. Then the partial unduloid which corresponds to it will hardly differ any; in other words, the meridian arcs of the bulge and narrowing will be almost identical with the line $y = \frac{1}{C}$, and it will be the same for the meridian arcs for all the other bulges and narrowings of the complete shape, i.e. infinitely prolonged beyond the discs. In this circumstance, consequently, the ordinate y will vary very little on all the extent of the meridian line, and the slope p will remain always extremely small.

Let us introduce these conditions into the equation [1], and, for that, parallel transport the x-axis above its original position by a quantity equal to $\frac{1}{c}$, so as to make it coincide with the generator of the cylinder. Thus let us replace y by $y + \frac{1}{C}$, and let us not forget that, in the transformed equation, y will represent the ordinate measured starting from the new x-axis, so that, in all the curve, y will remain, like p, extremely tiny. Let us expand, moreover, the radical $\sqrt{1+p^2}$; we will be able to neglect all the powers of p higher than the second, and we will have thus, instead of the radical in question, the quantity $1+\frac{1}{2}p^2$. Thus making these substitutions, equation [1] will become, the reductions being carried out,

$$2C^2y^2 + C^2p^2y^2 + 2Cp^2y + (1+2CC')p^2 = 2(1-2CC'). \tag{9}$$

Finally, because of the smallness of y and of p, let us neglect the terms of the 4th and the 3rd degree, $C^2p^2y^2$ and $2Cp^2y$, and the equation will be thus reduced to

$$2C^2y^2 + (1+2CC')p^2 = 2(1-CC'). \tag{10}$$

The error which we will make will be all the more tiny as the unduloid will approach more the cylinder, and the conclusions we will draw from this equation, for the case where the unduloid merges with the cylinder, will be rigorously exact.

Writing, in this same equation, $\frac{dy}{dx}$ instead of p, and solving for dx, it becomes:

$$dx = \sqrt{\frac{1+2CC'}{2}} \cdot \frac{dy}{\sqrt{1-2CC'-C^2y^2}},$$

which gives, by integration:

$$x = \frac{1}{C}\sqrt{\frac{1+2CC'}{2}} \cdot \arcsin\frac{Cy}{\sqrt{1-2CC'}}. \tag{11}$$

I do not add an arbitrary constant, because I take for the origin one of the points where the curve cuts the x-axis, which requires that the equation is satisfied by making there at the same time $y = 0$ and $x = 0$.

Such is thus the approximate equation of the meridian line of the unduloid in question, all the more exact equation as this unduloid is more closely coinciding with the cylinder. This same equation solved for y becomes:

$$y = \frac{\sqrt{1-2CC'}}{C}\sin C\sqrt{\frac{2}{1+2CC'}} \cdot x \tag{12}$$

It is the equation of a sinusoid, and it is seen that the points where, after having left the origin, the curve again will cut the x-axis, are at distances from the origin successively equal to

$$\frac{1}{C}\sqrt{\frac{1+2CC'}{2}} \cdot \pi, \qquad \frac{1}{C}\sqrt{\frac{1+2CC'}{2}} \cdot 2\pi, \qquad \text{etc.}$$

However the second is obviously the length of a portion of the unduloid made up of a bulge and a narrowing; by indicating it by l, we will thus have

$$l = \frac{1}{C}\sqrt{\frac{1+2CC'}{2}} \cdot 2\pi.$$

When the unduloid merges with the cylinder, this length will be, under the terms of the principle of the preceding paragraph, that which corresponds to the limit of stability of this cylinder, and it will be then rigorously exact; however, when the shape becomes a cylinder, the radius of that is, as we saw, represented by $\frac{1}{C}$, and one has at the same time, as we also saw, $C' = \frac{1}{2C}$; if thus one indicates the radius by r, one will have $C = \frac{1}{r}$ and $C' = \frac{r}{2}$. Substituting these values in the expression for l, one obtains finally, for the precise length which corresponds to the limit of stability of the cylinder,

$$l = 2\pi r,$$

from whence one deduces

$$\frac{l}{2r} = \pi.$$

Thus a liquid cylinder ranging between two solid bases is exactly at its limit of stability when its length, or the interval between its bases, is equal to its circumference, or, what amounts to the same thing, when the ratio of its length to its diameter is equal to π.

I will set forth later another method by means of which I arrive, without any calculation, at the same result, starting from a principle of Mr. Delaunay (§ 82); but I will state it only after what relates to the limit of stability of the unduloid.

§ 398. Beer, in the first of the two Memoirs where he subjects to calculation part of the results of my experiments[240], also arrives at the quantity π; here is how. Supposing a liquid mass adherent with a sufficiently long solid cylinder, it necessarily has, for its meridian line, that of a portion of unduloid, a portion which is composed of a bulge

[240] *Ueber die Oberflächen rotirender Flüssigkeiten im algemeinen, insbesondere über den Plateau'schen Rotationsversuch* (ANN. OF MR. POGGENDORF, 1855, vol. XCVI, pp. 1 and 210).

and two half narrowings. He shows then, using an artifice of calculation, that if one decreases gradually the equatorial radius of the shape, the distance between two extreme points of the meridian line converges towards a value equal to the circumference of the solid cylinder, a value that it reaches when the equatorial radius is equal to that of this cylinder, or, in other words, when the liquid mass is reduced to an infinitely thin layer on the surface of this same cylinder. Then, after talk of a result which does not refer to the current subject, he comes to a passage which I translate here; I inform the reader that Beer represents by 2 the distance above.

"Let us imagine an infinitely long oil cylinder placed in diluted alcohol, and make it undergo uniformly, in all its length, a small alteration such that the surface remains of *minimæ areæ*[241]. This surface will be obviously a surface of revolution whose meridian line will be composed only of equal curves of the kind considered above, being connected between them. The cylinder thus acquires a regular succession of narrowings alternating with bulges. For a very small deformation, the depression and the projection of these narrowings and bulges are also very small, and the cylinders to which the curves in question would be tangent deviate also very little from the original surface of oil. From there it follows that the distance between two narrowings all the more advances towards the limit of $2y_4$ found above for the originally cylindrical surface, and consequently towards the value of the circumference of the latter, as the supposed deformation is weaker. The limit $2y_4$, independent of the nature of the liquid, is obviously nothing but the limit of the stability of a liquid cylinder withdrawn from gravity, a limit observed and measured by Mr. Plateau. This physicist found that by taking as unit the diameter of the cylinder, the limit in question is included between 3 and 3.6. Mr. Hagen arrived, by a theoretical way, at the value 2.828, to which Mr. Plateau opposes the following remark: *If one replaces the radius of curvature used by Mr. Hagen by that of the top of the arcs of a sinusoid, one obtains then, for the value of the limit of stability the quantity* π. And, indeed, according to what preceded, this last quantity is the exact value."

By the expression: the distance between two narrowings (*zweier Einschnürunqen*), it is necessary to understand the distance from the middles of those; and as this interval includes a bulge and two half narrowings, it is equivalent in length to the whole of a bulge and a narrowing; Beer thus sought on his part, though by a method primarily different from mine, the length of a portion of unduloid made up of a bulge and a narrowing, when this unduloid passes to the cylinder; but he regards it as obvious that this same length is that which corresponds to the limit of stability of the cylinder, but one sees a priori no necessary relation between the length of a portion of unduloid, at the moment when it coincides with the cylinder, and the stability or the instability of the cylinder. It is quite true that a cylinder, with its limit of stability, changes so as to present a bulged portion and a narrowed portion; but nothing says immediately that at the origin of this deformation, the shape belongs to the unduloid; it is a point which it was necessary to address, as I did in § 396; this search of Beer is thus incomplete, as it requires a demonstration which he does not give.

In his second work[242], he carries out the same determination by means of elliptic integrals (§§ 84 and 85), but he again does not establish the relation between the result and the stability of the cylinder.

§ 399. I undertook, using the oil cylinders formed between two solid discs within the alcoholic mixture, to check, by experiments more precise than those of §§ 45 and

[241] One will further see (§§ 413 and 418) that Beer would not employ today any more this expression in the case in question.

[242] *Tractatus de Theoria mathematica phoenomenorum in liquidis actioni gravitatis detractis observatorum*. Bonn, 1857.

359, the exact value π of the limit of stability of the cylinder. I will give the results, but first I must present here some remarks on the procedure in this kind of research.

The limit of stability of an equilibrium shape constitutes a gradual passage between two states different from this shape, and consequently experiment alone cannot determine it in a rigorous way; but it can lead to two values brought close together enough and such that, for the first, there is still unquestionable stability, while, for the second, there is already unquestionable instability. If these two values are not very different, as I supposed, their average will give with good approximation the true value of the limit.

I employed this method with regard to the cylinder. When one is not too close to the limit, there are two characters which clearly show the stability or the instability of this shape: if, the cylinder being in the vicinity of its limit and preserving its form, one produced there artificially, by pushing oil using the nozzle of the syringe, a bulge and a narrowing slightly pronounced, and the shape takes again then its original form, it is obvious that it still has a real stability; in addition if, when one tries to obtain the cylinder, i.e. while the oil mass is in excess and then is decreased to arrive at the cylindrical form, the shape already starts to alter spontaneously before this form is reached, one must conclude that the cylinder which one wants to form would be unstable. The exact limit is thus between the closest lengths where these two effects are still respectively observable.

§ 400. The apparatus of which I made use consists of two vertical discs made out of thin iron, of the same diameter, placed facing each other, and which can gradually be brought closer together or moved apart. Each one of them is carried by a large iron wire established normally at the center of its back face, and vertically folded up from top to bottom; the lower end of that which supports the motionless disc is fixed at one of the ends of a horizontal iron bar of square cross-section, and the lower end of that which supports the mobile disc is fixed at a cursor which slides without shaking along this bar. A screw maintains it parallel to the other, and a crank engages a nut holding the cursor; by turning the crank in one direction or the other, one thus obliges the cursor with its disc to move back and forth. The horizontal bar is provided with four small feet, which are attached to a rectangular lead plate being used as a base for all the system; this plate was intended to prevent, by its mass, the apparatus placed at the bottom of the vessel with plane walls, in the alcoholic liquid, from oscillating during the operations. Finally, I moved to the crank using a sufficiently long rod which one held with the hand on the other end.

The diameter of each disc was measured with the cathetometer, and I found for one 30.05mm, and for the other 30.18mm; the average 30.11mm was taken for the diameter of the cylinder; the 0.13mm between these two diameters was obviously too small to exert an appreciable influence on the results. In each experiment, the distance between the discs was measured by means of the cathetometer laid out horizontally, while aiming at the upper part of these discs; I had assured myself besides of the parallelism of their planes.

1st EXPERIMENT. I initially placed the mobile disc at 108.40mm from the other, which gave very close to 3.6 for the ratio of the length of the cylinder to its diameter, then I made adhere to the whole of the two discs an oil mass in excess, so that the shape constituted a unduloid rather strongly bulging in the middle. Then I extracted liquid gradually, while observing the shape, and this started to become deformed spontaneously when the sagitta of the bulge above was still approximately 5mm.

2nd EXPERIMENT. I brought the discs closer then, so as to bring their distance to 99.36mm, which corresponds to the ratio 3.3. The shape being become stable again, I continued to remove liquid, and the tendency to spontaneous deformation appeared only when the sagitta of the bulge was not any more but 2.5mm, about.

3rd EXPERIMENT. Distance between discs 95.75mm, ratio 3.18. The shape was again stable, and later extraction had to reduce the sagitta to less than one millimetre so that I saw the shape becoming deformed by itself[243].

4th EXPERIMENT. Distance between discs 94.53mm, ratio 3.14. It showed no tendency to spontaneous deformation when the sagitta had a sensible value, so that I arrived without difficulty at the cylindrical form; but this cylinder, left to itself, after having appeared to persist for a few seconds, started to alter with an extreme slowness initially, then gradually more quickly as the shape was divided, as usual, in a bulged portion and a narrowed portion, and the deformation continued to go until the complete disunion. I repeated the experiment several times, and always with the same results.

5th EXPERIMENT. Distance between discs 93.03mm, ratio 3.09. I easliy reached the cylindrical form, then I artificially produced a narrowing and a bulge, the sagitta of this last being about 1mm. The shape when released regained the cylindrical shape, and it was necessary, to bring about the spontaneous progress of the deformation, to change the sagitta of the artificial bulge to 3mm approximately.

6th EXPERIMENT. Distance between discs 93.65mm, ratio 3.11. I arrived in the same way at the cylinder; so that it had spontaneous progress of the deformation there, the sagitta of the artificial bulge had to lie between 2mm and 3mm. In this experiment, before forming the cylinder, one, of course, had added a little oil to the mass.

7th EXPERIMENT. Distance between discs 94.18mm, ratio 3.13. After a new preliminary addition of liquid, I still arrived at the cylinder; spontaneous progress started when the sagitta of the artificial bulge was only 1mm and a fraction.

§ 401. It is seen, in the first three experiments, that the shape expressed the character indicating that a cylinder formed between the discs would be unstable, and, from the first to the third, this character was less and less marked; finally, in this third experiment, for which the ratio was 3.18, which was already extremely close to the sought limit. The three last experiments expressed, on the contrary, the character of the stability of the cylinder, and this stability was decreasing from the fifth experiment to the seventh, for which the ratio was 3.13 and which had very low stability.

One can thus affirm, aside from any theoretical result, that the limit of the stability of the cylinder lies between the values 3.13 and 3.18, which differ between them only by 0.05; and as the cylinders corresponding to these two values have still respectively, in a clear way, the characters of stability and instability, the sought limit is notably higher than the first and lower than the second. Consequently, if the average of these same values is taken, which is 3.15, one can be certain that the true limit is not more than 0.02 away, a quantity which is only 0.006 of this average.

Thus, on the basis of only the results of experiment, one must regard the number 3.15 as being very near the value of the limit of the stability of the cylinder; however this number hardly differs from the theoretical value π, or 3.14; finally the fourth experiment shows that while placing the discs at the distance which gives this theoretical ratio 3.14, the shape presents neither one nor the other of the characters of the instability or the stability of the cylinder, i.e., on the one hand, it does not express any tendency to transformation as long as the cylindrical form is not reached, and that, on the other hand, when the cylinder is formed, it requires, to begin and achieve its transformation, no artificial alteration.

The whole of the experiments above can thus be regarded as verifying the theory

[243]The ratio of the length to the diameter is given here with two decimals, because it is recognized that one approaches the limit already. It is the same with regard to the following experiments; of course, the second decimal was reinforced when the third had been rather large; in experiments of this nature, it would be, I think, unrealistic to seek to push the precision further.

fully.

§ 402. The fifth, the sixth and the seventh experiments, i.e. those which were done inside the limit, offered a characteristic seemingly extremely singular. Each one of them was repeated several times; however, in certain cases, the cylinder, which seemed quite regular, transformed itself after a few moments: I saw take shape a bulge and a narrowing there; but those, after having reached a more or less marked degree, though always rather small, remained stationary, without progressing nor diminishing. This phenomenon, which appeared unexplainable, embarrassed me much, until I had reasoned in the following way.

When the densities of the two liquids are quite equal, a cylinder formed inside its limit must persist infinitely without any alteration, whatever its position in the alcoholic liquid, be it horizontal, vertical, or tilted; but if there is between the densities a difference, even too weak to cause in oil a visible tendency to go up or go down, if, moreover, the axis of the shape is slightly tilted, so that one of the discs is a little higher than the other, and finally the cylinder is brought very close to its limit and thus the forces which tend to maintain its form have only an extremely small intensity, it is understood that the inferiority or the excess of density of oil will transport it in greater quantity on the side of the disc higher or lower, and that consequently the shape will present a bulge and a narrowing. However, as there are here only very tiny differences between the densities of the liquids and the heights of the discs, this transport of oil will not be abundant enough so that the shape reaches the unstable unduloid (§ 396) corresponding to its length; the transformation will not be able thus to be carried out, and the small alteration of the cylinder will remain stationary.

I confirmed this explanation by experiment with the discs being placed at the distance which gives the ratio 3.11, and a cylinder being formed between them, I inclined the apparatus somewhat so that one of the discs was approximately a millimetre lower than the other, and at the same time I gave to the alcoholic mixture a sufficient excess of density to oblige the cylinder to inflect by forming an arc of significant curvature, though small, whose convexity appeared at the top; I soon saw occurring a narrowing and a bulge, this bulge being formed on the higher disc. I then established the same slope of the system in the opposite direction, I erased the narrowing and the bulge, and I saw them developing again, the bulge being formed on the other disc. Finally I made, on the contrary, the density of the alcoholic mixture a little weaker, which slightly arched the shape in the direction opposed to the preceding, and the bulge showed itself then towards the lower disc.

I will add that, in the three experiments pointed out at the beginning of this paragraph, i.e. in the three last of § 400, when the shape presented the stationary alteration about which I spoke, and which I had released for several minutes, I in general recognized, by a slight bending of the shape, a difference between the densities; this difference, initially too tiny to cause an effect sensible to the eye, had increased little by little, either by a variation in the temperature, or by the mutual chemical action of the two liquids, an action which it is impossible to cancel completely.

Finally, I will point out that in the formation of vertical film cylinders, one sees (§ 112) the influence of the weight of the film to bulge the shape in its lower half and to narrow it in its upper half, when the ratio between the spacing and the diameter of the rings starts to approach that which corresponds to the limit of the stability of the cylinder.

From all that it follows that, if one repeats my experiments on the limit of stability of the cylinder, it will be necessary to give the greatest care to the perfect horizontality of the axis of the shapes.

§ 403. Results of the fifth, sixth and seventh experiments, carried out inside the limit, and that of the fourth, carried out in the extreme case, complete establishing the fact on which I based the research of the theoretical value of this limit, consistent with the fact (§ 396) that the unstable unduloid corresponding to a stable cylinder all the more approaches this cylinder as it is closer to the limit. Indeed, the whole of these results gives, between the length and the diameter, the series of ratios 3.09, 3.11, 3.13, 3.14, which finishes with the limiting ratio, and gives at the same time, for the respective sagittas of the bulge of the unstable unduloid, 3mm, 2mm and a fraction, 1mm and a fraction, 0mm.

§ 404. Let us suppose a cylinder formed a little inside its limit of stability, and in which one produces, by the operation indicated, a bulge and a narrowing. Since it is necessarily a part of an unduloid that forms the spontaneous progress of the deformation, it is understood that if, at the time when this spontaneous progress will start, the bulge and the narrowing had, in consequence of the artificial operation which constituted them, a form and a ratio of length other than those which are appropriate for the unduloid, they would take themselves immediately to this last form and the latter ratio. Now let us recall again that this unduloid deviates less from the original cylinder as this is closer to its limit, and even coincides with it in extreme cases; let us recall, moreover (fourth experiment of § 400), that at this limit the cylinder, which becomes deformed spontaneously, is always divided in only one bulged portion and only one narrowed portion, and we will conclude that, in a cylinder at its limit of stability, the transformation is carried out invariably as if it originated in a unduloid infinitely close to this cylinder and composed of only one bulge and only one narrowing.

Equation [5] of § 397 shows that the meridian line of the shape is then a sinusoid, from which follows this second conclusion: that with the birth of the transformation of the cylinder in question, the bulge and narrowing are rigorously equal in length.

§ 405. In § 373, I concluded that an infinite cylinder, entirely free on all its surface, and formed of a liquid absolutely free of viscosities, would most probably transform so that each division, and, consequently, the whole of a bulge and a narrowing, would have the length which corresponds to the limit of stability.

But, on the one hand, I showed (§ 371) that, always in the regular transformation of an infinite cylinder or a long one, the changes of form are achieved in each one of these pairs as if it were bounded by solid bases; and, in addition, we have just seen (§ preceding) that at the start of the transformation of an isolated pair having the length corresponding to the limit of stability, the shape constitutes a partial unduloid infinitesimally different from the cylinder; the same thing will thus take place at the origin of the transformation of an infinite cylinder, in all the pairs which are formed, if they have the length above, and all these identical portions of unduloid being connected between them since each one is composed of a whole bulge and a whole narrowing, the total shape will constitute an infinite unduloid.

If thus one places himself under the more theoretically simple condition, i.e. if one supposes the liquid without viscosity, the length of cylinder infinite or an exact multiple of that which corresponds to the limit of stability, the convex surface entirely free, and any foreign cause of trouble removed, finally if one imagines that the cylinder has some very small imperfection of form, an imperfection without which it persists since it constitute a shape of equilibrium, one must believe that the transformation happens as if it started from a unduloid infinitesimally different from this cylinder.

In this case, according to the remark which finishes the preceding paragraph, the initial length of bulges is rigorously equal to that of narrowings. If there are resistances, the bulges and narrowings are lengthened, and consequently the original shape cannot be a unduloid any more; but then still, as will be seen (§ 423), the bulges and initial narrowings are most probably equal in length.

§ 406. It is rather easy besides to render comprehensible to what is due the influence of resistances over the length of the bulges and narrowings; the examination of this question will contribute at the same time to make clearer our ideas on the play of capillary pressures in the act of spontaneous transformation.

We claim that at the start of a regular transformation, when one regards the bulges and narrowings as infinitesimal, the first are really equal in length to the second; then, whatever the true nature of the meridian line, it will constitute a curve similar to the sinusoid. Let us reason by supposing that it is a sinusoid exactly. If we indicate by r the radius of the cylinder, by β the sagitta of the arches, by l the length of the chord of each one of those, we take for the x-axis the axis of the cylinder, and we make the y-axis pass through the point where one of the convex arcs starts, the equation of our sinusoid will be obviously

$$y = r + \beta \sin \frac{\pi}{l} x. \tag{1}$$

Let us take on this curve two points belonging one to a convex arc, the other to a concave arc, and placed in the same manner on these two arcs, i.e. at the same distances from the starts of these arcs. If, for brevity, we represent by γ the term $\beta \sin \frac{\pi}{l} x$ of our equation, the value of γ will be the same, except for the sign, for the two points, so that the ordinates of those will be respectively $r+\gamma$ and $r-\gamma$, which gives, according to a known formula, for the values of the two normals, $(r+\gamma)\sqrt{1+p^2}$ and $(r-\gamma)\sqrt{1+p^2}$, where p is, as always, the differential coefficient $\frac{dy}{dx}$; it should be noticed that, by the nature of the liquid shape, these normals are both positive ones; one will have to remember moreover, for the understanding of the formulas which follow, that the quantity γ, or $\beta \sin \frac{\pi}{l} x$, is taken in itself, and consequently is primarily positive. As for the radius of curvature, it is clear that its value is, except for the sign, the same for the two points; if thus q indicates the differential coefficient of the second order $\frac{d^2y}{dx^2}$, one will have, also according to a known expression, for the radius of curvature at the first point, $+\frac{(1+p^2)^{3/2}}{q}$ and at the second point $-\frac{(1+p^2)^{3/2}}{q}$.

The capillary pressure corresponding to the first point relative to the unit of area will be consequently, under the terms of the formula which I have so often pointed out,

$$P + \frac{A}{2} \left\{ \frac{1}{(r+\gamma)\sqrt{1+p^2}} + \frac{q}{(1+p^2)^{3/2}} \right\}$$

and the pressure corresponding to the second point will be

$$P + \frac{A}{2} \left\{ \frac{1}{(r-\gamma)\sqrt{1+p^2}} - \frac{q}{(1+p^2)^{3/2}} \right\},$$

P being always the pressure of a plane surface, and A a positive constant whose value depends on the nature of the liquid.

Let us subtract the first of these expressions from the second; we will have thus, for the excess of the pressure of the point of the concave arc over that of the point of the convex arc,

$$\frac{A}{\sqrt{1+p^2}} \left\{ \frac{\gamma}{r^2-\gamma^2} - \frac{q}{1+p^2} \right\}.$$

Since we supposed the deformation infinitesimal, the slope p is infinitely small everywhere, which makes it possible to replace $\sqrt{1+p^2}$ by $1+\frac{1}{2}p^2$. Making this substitution and carrying out calculations, it becomes:

$$A\frac{\gamma+\gamma p^2-qr^2+q\gamma^2}{r^2-\gamma^2+\frac{3}{2}r^2p^2-\frac{3}{2}\gamma^2p^2+\frac{1}{2}r^2p^4-\frac{1}{2}\gamma^2p^4}.$$

Neglecting the terms in p^2, γ^2, p^4, which are the infinitesimally small ones of higher order, the expression is reduced to

$$A\cdot\frac{\gamma-qr^2}{r^2}. \tag{2}$$

It remains to substitute in this expression the values of γ and of q. Then two successive differentiations of equation [1] give $q=-\frac{\beta\pi^2}{l^2}\sin\frac{\pi}{l}x$; but as we have affected the quantities of the proper signs which depend on the parts of the curve to which these quantities belong, it is necessary here to disregard the negative sign; and, indeed, if we want to replace, in the expressions of the two pressures, the general value $\frac{q}{(1+p^2)^{3/2}}$ of the inverse of the radius of curvature by its value relating to our sinusoid, we could leave with that of q the minus sign brought by the differentiation, only by choosing this same sign between the two whose denominator can be affected because of exponent $3/2$, without which the terms which represent the opposite of the radii of curvature would not have any more, in the expressions concerned, the signs which are appropriate for the question; however that amounts to disregarding these signs and taking q and the denominator in an absolute way. Substituting thus, in the expression [2], for γ and q their respective absolute values $\beta\sin\frac{\pi}{l}x$ and $\frac{\beta^2\pi^2}{l^2}\sin\frac{\pi}{l}x$, one obtains finally, for the measure of the difference of the pressures, corresponding to two points similarly located one on a convex arc and the other on a concave arc,

$$A\left\{\frac{1}{r^2}-\frac{\pi^2}{l^2}\right\}\beta\sin\frac{\pi}{l}x. \tag{3}$$

In this expression, the factors A and $\beta\sin\pi lx$ are, we know, primarily positive, so that the sign of the total quantity will depend on that of the factor $\frac{1}{r^2}-\frac{\pi^2}{l^2}$; the difference of the pressures will be thus positive if

$$\frac{1}{r^2}-\frac{\pi^2}{l^2}>0,$$

or, which amounts to the same thing,

$$2l>2\pi r,$$

i.e. if the sum of the lengths of a bulge and a narrowing exceeds the circumference of the original cylinder, and that in all the extent of the arcs, except at their very ends, where the factor $\beta\sin\frac{\pi}{l}x$ disappears. It is seen, moreover, that the difference in question increases starting from these ends until the middles of the arcs. Thus, initially, when a cylinder formed between two solid bases exceeds the limit of stability, but by relatively little length, it will have to give only one bulge and one narrowing, the pressure corresponding to an arbitrary point of the meridian arc of narrowing overriding that which belongs to the point similarly placed of the meridian arc of the bulge.

Let us consider now a second cylinder of the same diameter and formed of the same liquid, but longer, always with the condition that there is one pair there, and suppose it has the same degree of deformation, i.e. whose bulge and narrowing have the same sagitta as in the first shape; we will pass thus from a sinusoid to another more lengthened, but of the same sagitta, which amounts to increasing l without changing either β or r or A. If we respectively take two corresponding points on these two sinusoids, such that their x-coordinates are between them like the chords of the arcs, the ratio $\frac{x}{l}$ will be the same for these two points, and consequently the factor $\beta \sin \frac{\pi}{l} x$ of expression [3] will have the same value, as will A; but the factor $\frac{1}{r^2} - \frac{\pi^2}{l^2}$ will increase while passing from the point of the first shape to the point of the second, more especially as one will have given to l a greater increase. The prevalence of the capillary pressures of all the points of narrowing over those of the points of the bulge is thus all the more strong as the pair is lengthened more; and as we can apply to each pair formed in the transformation of a long cylinder what we have just found with regard to an isolated pair, it follows that, in the transformation of such a cylinder, the more the bulges and narrowings are lengthened, the more the forces which drive the phenomenon are intense. Accordingly, when, in an infinite or very long cylinder, the transformation is obstructed by resistances either external or interior, these resistances, unless they are not too energetic, could be overcome by a lengthening of the pairs; however it is conceived that the transformation is prepared so as to produce this effect, and that it lengthens so much more the pairs to which resistances oppose more of an obstacle.

In truth, the calculation above is based on the assumption that at the beginning of the phenomenon, the meridian line is a sinusoid; but, as I pointed out, if, actually, it is not such, it very probably has much analogy with this curve, to which it is reduced besides, we know, when the pairs have the length corresponding to the limit of stability; if one knew the exact nature of it, and the preceding calculation were applied to it, one without any doubt would get the same result.

§ 407. Initially, indeed, one arrives there by the considerations set forth in § 381, considerations from which it follows a priori that when a cylinder formed between two solid bases has a small enough length to give one pair, it must change more quickly as its length exceeds the limit of stability more; and since, in the regular transformation of an infinite or long cylinder, events occur with regard to an arbitrary pair as if it were bordered by solid bases, it follows that, in an infinite or long cylinder, the transformation will be also all the faster as the pairs are longer; but a faster transformation supposes more intense forces; the differences in pressure in question in the preceding paragraph thus increases with the length of the pairs.

Moreover, one can resort to experimental checks: it is enough, for that, according to what I have just said, to create, with the same liquid, cylinders of the same diameter exceeding their limit of stability more and more, and to measure, for each one of them, the duration of the transformation. However, that is what was carried out at the same time as the experiments of § 400: in the first three, after having observed the point where the shape started to alter spontaneously, I continued the extraction of the liquid, until, by employing the small operation indicated in the note of § 359, i.e. by constantly regularizing the shape by means of the nozzle of the syringe, I had reached the cylindrical form; then I release the shape, and I timed, with an ordinary watch, the approximate duration of the transformation; I also estimated this duration in the fourth experiment, where the cylinder was at its limit of stability. I found thus for the limit ratio 3.14 between the length and the diameter, an 11 minutes duration; for ratio 3.18, a 4 minutes duration; for ratio 3.3, a 2 minutes duration; and, for the ratio 3.6, a duration of 1 minute.

In repeating the fourth experiment, it happened several times that the duration was only 5 to 7 minutes; but it is seen that it was always higher than all the others.

§ 408. If, in the expression [3] of § 406, there is $\frac{1}{r^2} - \frac{\pi^2}{l^2} < 0$, or $2l < 2\pi r$, which makes it negative, one will conclude from the mode of reasoning employed, that the capillary pressures of all the points of narrowing will be lower than those of the points of the bulge, and more especially as the pair will be shorter. In this case, consequently, if the pair is single and is bordered by two solid bases, the liquid mass will tend to regain the cylindrical shape; in other words, the cylinder formed between the bases in question will be stable, and its stability will be all the more marked as its length will be less.

Finally, if one has $\frac{1}{r^2} - \frac{\pi^2}{l^2} = 0$, from whence $2l = 2\pi r$, the difference of the pressures is null in all the extent of the pair, so that, if there is one pair ranging between solid bases, the mass will tend neither to return to the cylindrical shape, nor to move away some more; the cylinder formed between these bases will be thus then at its limit of stability, as we already knew.

This manner of arriving at the limit of stability of the cylinder is nothing, one sees , but the method of Mr. Hagen (§ 394), but corrected in substituting for the arcs of a circle the arcs of a sinusoid, and by evaluating the pressures over the entire length of these arcs, instead of only doing it at their tops.

§ 409. We now concern ourselves with the unduloid. The stability conditions of this shape change, we saw (§ 388), according to whether its middle is occupied by a narrowing or by a bulge. The application of a theoretical method to the first case would undoubtedly be quite difficult; but it is not the same with the second:

When one forms a partial unduloid of this second kind by making adhere, within the alcoholic mixture, a mass of oil on the convex surface of a solid cylinder rubbed with oil beforehand (§ 47), it is obvious that the surface of this mass cannot, at its ends, form an angle with the thin oil film which wets the solid surface beyond these same ends, and thus the surface of the shape must be tangent to the layer in question, from whence it follows that the meridian line finishes precisely at the minimum of distance to the axis; however, we know the unduloid thus formed is stable, and one draws the rigorous conclusion that the limit of stability is not inside the mid-circles of two narrowings. It should now be shown that it is not either beyond that.

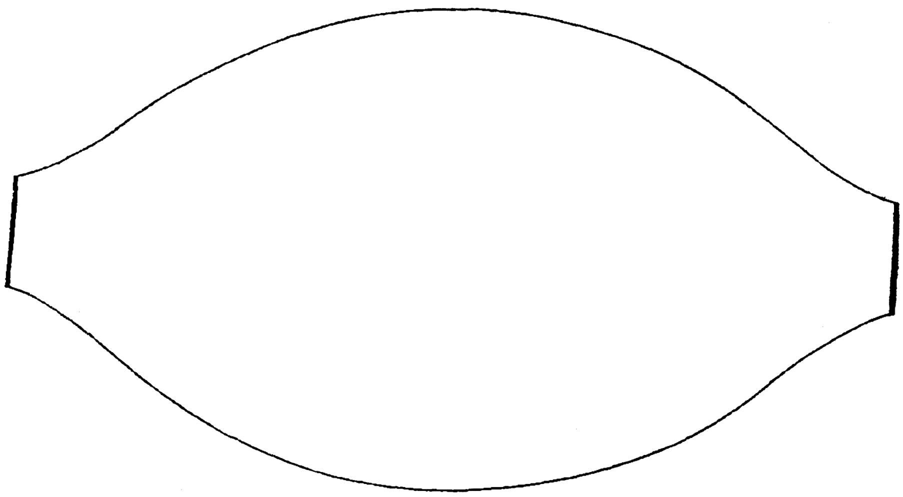

Fig. 100

For that, let us take again the creation of our unduloid between the two small discs (§ 52); let us leave with the oil mass a sufficient excess so that the last elements of the meridian line do not reach yet parallelism real or apparent with the axis, but are not very far from it (fig. 100); this unduloid will consequently be stable. Then let us push the oil slightly with the nozzle of the small syringe towards one of the discs; we will thus decrease or we will entirely erase the portion of narrowing which was beside this disc, while we will make more marked the portion of narrowing leading to the other disc. However it is clear that events will occur here as with regard to the cylinder; i.e. if the artificial deformation does not exceed some degree, the shape when released will regain its original shape, but, if one goes beyond this degree, it will spontaneously continue to alter in the same direction, until its complete disunion. There is thus here also, at this precise degree of deformation, a shape of unstable equilibrium (§ 396); and as, during the artificial deformation and the subsequent spontaneous deformation, the oil shape will be always maintained of revolution and that its meridian line will present always at least a point of inflection, the unstable shape of equilibrium in question is necessarily another unduloid.

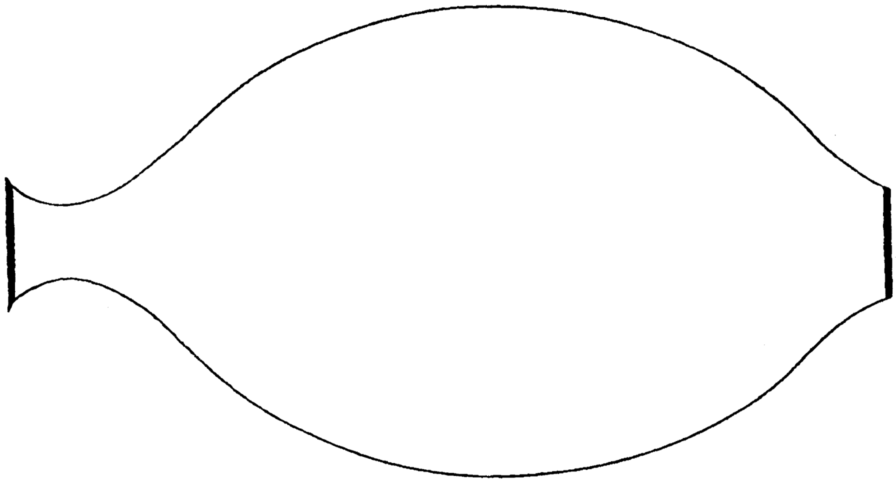

Fig. 101

This unstable unduloid, that, to shorten the language, we will call the *conjugate unduloid*, will be, according to what precedes, asymmetrical compared to the middle of the interval of the two discs, i.e. will have its bulge brought closer to the one of these discs than the other (fig. 101). Finally, this same unduloid will deviate obviously less from the original unduloid as that is closer to its limit of stability, and, at this limit, will coincide exactly with it.

Now let us notice that, according to the mode of generation of the meridian line of the unduloid by the focus of a rolling ellipse (§ 82), this line necessarily has perfect symmetry on the two sides of a maximum of distance to the axis, and that thus, in an infinite unduloid, there is rigorous symmetry of both sides of the equator of a bulge. It follows that if one includes between two equal discs a bulge of unduloid with portions of two adjacent narrowings, and if these portions both go beyond their respective mid-circles (fig. 102), the two discs are rigorously at equal distance from the equatorial section of the bulge. So that this section does not occupy the precise middle of the interval of the discs, it is necessary obviously that one of narrowings goes beyond its mid-circle, and that the other does not reach to it; that is the case of the conjugate unduloid (fig. 101); in this unduloid, only one of two narrowings presents a mid-circle.

These principles established, let us conceive a unduloid of the kind considered in this paragraph, and precisely at its limit of stability; let us imagine it mathematically

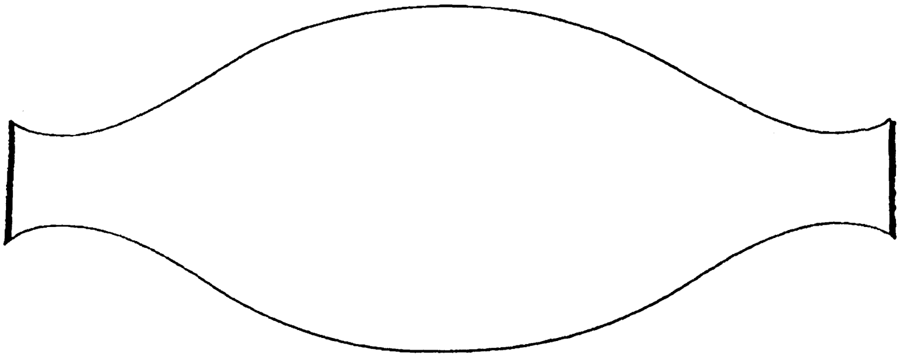

Fig. 102

perfect, so that it is maintained, and suppose that its narrowings go beyond their mid-circles, in a manner that it is similar to that of fig. 102. Then let us add a very small quantity of liquid, which will make a stable shape of it, but close to its limit. Since we control the added volume, we can make it rather tiny so that the stable unduloid produced differs as little as we want from the original unduloid, and consequently so that the conjugate unstable unduloid differs as little as we want from the original unduloid; if thus this one, i.e. the unduloid at its limit of stability, extended beyond the mid-circles of its narrowings, we could always, by an addition of a sufficiently small liquid and a tiny transport of the bulge towards one of the discs, to arrive at a conjugate unduloid whose narrowings would still extend beyond both theirs mid-circles; however, according to what I showed higher, that is incompatible with the nature of the conjugate unduloid. Our unduloid at its limit of stability cannot thus finish beyond the mid-circles of its narrowings, and since it cannot either, as I showed at the beginning of this paragraph, finish inside, it really finishes at these same circles, as the experiment had indicated to me.

I will add that Mr. Lindelöf, to whom I communicated this result, told me he arrived at it on his part by calculation; the experiment, the reasoning and the analysis thus agree to establish it.

§ 410. I now set forth the method announced at the end of § 397, by which I arrive without any calculation at the exact value of the limit of stability of the cylinder.

It follows from the generation of the meridian lines, that the portion of the meridian line of the unduloid ranging between a minimum of distance to the axis and the following minimum, corresponds to a whole revolution of the rolling ellipse; thus the partial unduloid generated by this portion, which is the unduloid at its limit of stability, has a length equal to the periphery of the ellipse in question; however, when this ellipse becomes a circle, the unduloid becomes a cylinder, and consequently the cylinder, at its limit of stability, has a length equal to the circumference of the travelling circle; but this circumference is obviously equal to that of the cylinder; thus the limiting cylinder has a length equal to its own circumference; thus finally, in such a cylinder, the ratio of the length to the diameter has as an exact value the quantity π.

§ 411. We saw (§ 387) that the catenoid of maximum height is actually at its limit of stability, and we also know that when one forms it in a film state, it is converted into two plane films as soon as this maximum height is reached; however, in consequence of the calculation of Goldschmidt (§ 80), with this same maximum height, the ratio of the spacing of the bases to their diameter is equal to 0.6627. By employing some care, I could subject to an experimental verification this precise value.

For that, I substituted for the wire rings two iron bands bent cylindrically, one centimetre high and two millimetres thick, having their facing edges cut outside in bevel at an angle of approximately 45°, so that the edges of these two bevels, edges to which the film is to attach, were sharp. These two bands or rings were worked with a lathe; the diameter of the circumferences formed by each edge above was found to be exactly the same for both[244], and equal to 71.02mm. Fig. 103 represents, in real size, the vertical section on the left side of the system. These same rings were carried like the wire rings, the lower by three small feet and the upper by a fork; only these feet and this fork were more solid. Just as in the experiments of §§ 110 to 114, the vertical stem with which one could screw the tail of the fork, using a suitable adapter, to the end of the glasses of a cathetometer; one could thus raise vertically the upper ring, and at the same time measure with exactitude the distance its sharp edge had separated from that of the lower ring.

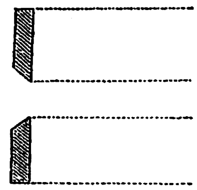

Fig. 103

Things being thus laid out, I proceeded in the following way: I set, by its feet, the lower ring on a shelf with fixing screws and under the upper ring, held fixed, as I said, by the cathetometer; I made quite horizontal the edge of the first, by means of the fixing screws and a small spirit level; the second then was lowered, and, using a pliers, I slightly curved the tail of the fork in one direction or another, while sliding at the same time, by small quantities, the lower ring on the shelf, until the two edges were exactly superimposed. Then, after having reassembled the upper ring, I wet with glyceric liquid the beveled edges of the two rings, making use of a brush for that, then I lowered the upper ring again until very close to the other, and I filled the circular groove formed by two bevels with the same liquid, by stroking there the brush well soaked. That done, I raised the upper until a millimetre approximately inside the quantity indicated by the theory, and that I had determined beforehand; then I made act, with extreme care, the screw of gradual movement, stopping at the precise time of the rupture of equilibrium, i.e. at the moment when the shape narrowed quickly to be divided in its middle and to be converted into two plane films, as with the wire rings.

The diameter of the sharp edges being, as I indicated above, 71.02mm, I was to have (§ 80), for the height of the limiting catenoid and consequently for the spacing of these edges corresponding to the rupture of equilibrium, 71.02mm $\times$ 0.6627 = 47.06mm. Out of seven times that the experiment was carried out, the reading with the cathetometer gave six times identically the same value, which was 46.97mm, and once 46.92mm, which hardly deviates from the preceding one. One must thus regard the value 46.97mm as being that which the experiment gives; it only differs from the theoretical value 47,06mm by 0.09mm, a quantity which does not reach two thousandth of the theoretical value. I will draw attention here to another verification of the theory. We saw (ibid.) that, according to the calculations of Goldschmidt, when the spacing of the bases exceeds the limit, there is no more, as surfaces of revolution with zero mean curvature being based on these bases, than two planes which occupy them respectively; however we indeed know, by the experiment above and those of §§ 111 and 222, that at the time when, by the gradual spacing of the rings, one reaches the theoretical limit exactly, or very closely, the catenoid film between them is transformed spontaneously into two plane films.

The discussion contained in § 78 and the theorem of Mr. Delaunay (§ 82) and completed by Mr. Lamarle (§ 83) have, moreover, shown that, among surfaces of revolution, the catenoid and the plane are the only ones of zero mean curvature.

[244] I measured these diameters by means of the cathetometer, by placing for that the rings in a vertical position.

Let us recall here that the limiting catenoid is defined (§ 80) by this simple property, that the extreme points of its meridian catenary are those for which prolonged tangents also would touch the opposite catenary meridian, or, what amounts to the same thing, intersect in the center of the shape.

We also saw (§ 89) that the catenoid in question enjoys this other simple property, that its volume is half of that of the cylinder of the same bases and of the same height, a property which we also checked by experiment (§ 90).

Finally, this same catenoid has a last property, which is to be unique; we know, indeed, that, for any variation of the bases lower than the limiting spacing, there are always two possible catenoids being based on these bases and narrowing unequally, catenoids which differ less from each other as one approaches the limit, and of which the less narrowed is the only stable one.

§ 412. We would have still to seek, by theory, the precise limits of stability of the nodoid in the various cases examined in §§ 389 and 380; but here, as with regard to the narrowed partial unduloid, the principles which we applied to the cylinder and the bulging partial unduloid could not be employed. It is the same as for the shapes mentioned in § 393.

§ 413. Now let us try to penetrate deeper into the essence of the phenomena.

Let us conceive a liquid shape of equilibrium created in a solid and mathematically perfect frame; then the capillary pressure will be rigorously the same at all the points of the surface layer, and the shape, whatever its extent, will be maintained as long as an external cause will not come to disturb it. Let us suppose that a very small deformation is artificially given to it; thus altered, it will in general cease being in equilibrium, and consequently the pressures respectively corresponding to the various points of its surface layer will not be exactly equal any more; if thus one releases it, it will tend to leave this new state. That said, two cases are possible: either the shape tends to return to its original form, or it tends to move away some more. If the first case takes place, whatever the nature of the small deformation, the shape is stable; if, on the contrary, the second case arises either for a small arbitrary deformation, or for a small deformation of a determined nature, the shape is unstable.

But one can consider the stability and the instability of the liquid shapes from another point of view, also general, whose idea was suggested to me by a passage of one of the Memoirs of Beer, a passage which I reproduce below.

The geometers claimed, as result of analysis, that the surfaces represented by the equation $\frac{1}{r}+\frac{1}{r'}=C$, i.e. surfaces whose mean curvature is constant, are also those which, containing a given volume, have a minimum area. But if it were necessary to accept this principle without restriction, it would follow that any partial liquid figure of equilibrium bordered by a solid system would necessarily be stable, any portion representing the complete shape to which it belongs: the unduloid, for example, would preserve all its stability with an arbitrary number of bulges and narrowings between its two solid bases.

Indeed, the surface layer of the mass being really, we know (chap. V), in a state of tension, it constantly makes an effort to tighten itself; if thus, in the state of equilibrium, its area were always a minimum, an arbitrary very small deformation would increase this area, and consequently the surface layer would make an effort to take again its original dimensions and to restore the equilibrium shape. Also, Beer seeks to modify the principle stated by the geometers: in the second of the two Memoirs which he published on my experiments[245], he expresses it in the following way:

"A liquid in the state of equilibrium and withdrawn from any foreign influence

[245] Voir the third note of § 398.

enjoys the property that the variation of its surface is always null... this surface is thus of the nature of those that one names minimæ or maximæ areæ but, for a surface, maximæ areæ will correspond obviously to an unstable equilibrium, while to a surface minimæ areæ a steady equilibrium will correspond."

However, if one reflects, one will easily be convinced that a surface containing a given volume could not constitute a maximum in an absolute way, because one could always find modes of small deformation which would increase it: if it is imagined, for example, that the shape furrows in grooves such that the sum of those which are in hollow compared to the original surface is equal in volume to the sum of those which are in relief, so that the total volume did not change, it is clear that surface area will have received thereby a notable increase, however thin one supposes the grooves in question. It is, undoubtedly, for this reason that the geometers regarded surfaces with constant mean curvature as having always each one a minimum of area. Besides, experiment confirms this impossibility of an absolute maximum, and it will, moreover, give us the solution of the difficulties above:

When one creates between two discs, in the alcoholic liquid, an oil cylinder exceeding very little its limit of stability, if, before the spontaneous deformation starts to be shown, one slightly pushes the cylinder in its middle using a spatula covered with fabric, so as to bend the shape by a certain quantity, then that the spatula is removed, one sees the shape returning to the cylindrical form, more or less altered by the birth of the bulge and narrowing; from whence it should be concluded that a cylinder exceeding its limit of stability is nevertheless stable still and consequently minimæ areæ, compared to the deformations which would bend it. It follows that the surface of a liquid shape of equilibrium which exceeds its limit of stability, is still minimæ compared to certain deformations, while it is maximæ compared to others.

Experiment shows, moreover, that a given liquid shape of equilibrium included in a given solid system, and exceeding its limit of stability, always identically transforms in the same manner as the cylinder, for example, one bordered with two solid discs always splits in bulging portions alternating with narrowing portions, and, when no disturbing cause intervenes, the respective lengths of the bulges and narrowings, at an arbitrary time of the phenomenon, are always the same under the same conditions of the experiment; in the bulging partial unduloid ranging between two equal discs, the bulge always goes towards one of the bases, so that one of two narrowings is erased by degrees, while the other deepens until its disunion; etc.

But these facts appear to lead to a second consequence: whether, beyond the limit of stability, the surface is maximæ only compared to one mode of deformation, or, if it is maximæ areæ compared to several, there are certain conditions which determine the choice of the mass among those, so that only one deformation is likely to progress.

§ 414. To still make more obvious the deductions which precede, I will study the cylinder from the point of view of the variations which its surface area undergoes when one alters its form a little, without changing the volume that it contains; this shape, indeed, easily lends itself to such an examination.

Let us imagine a liquid cylinder of any length compared to its diameter and between two solid bases, and imagine that a finite deformation is given to it, but very small, the only condition being that the areas of all the plane sections parallel to the solid bases remain the same as in the cylinder. Such a deformation is acceptable because it does not alter the volume of the mass; consider, indeed, two of these infinitely close sections; the volume of the liquid section that they include will be equal to the product of the area of one of them by the distance which separates them, and since this surface is equal to that of a circular section of the cylinder, the volume in question will be equal to that of a section of the same thickness belonging to the cylinder; finally the total volume of the shape being the sum of volumes of all the sections obtained by cutting this shape by an infinite number of planes infinitely near and parallel to the bases, and the numbers of these sections being the same before and after the deformation, this, as I have claimed, does not makes any modification to the total volume in question.

Now let us consider, in the deformed shape, one of the sections above. If the cross-sections which include it are not circular, their perimeters will be larger than that of the sections of the cylinder, since of all the plane curves which contain the same surface, the circumference of circle is shortest; the small surface zone which links these perimeters and which belongs to the free face of the shape, will be thus, for this reason and, moreover, because it is composed in general of oblique elements in the planes of the two sections, larger than the small zone belonging â a section of the cylinder. If the two sections are circular, they will be, by the condition assigned to the surfaces, identical to those of the cylinder, but their centers will not be in general exactly facing one another, so that the small zone which links the two perimeters will also be composed of oblique elements, and will also consequently be larger than that of a section of the cylinder. Accordingly, as the number of the sections is the same in the deformed shape and the cylinder, the sum of surfaces of the small zones of the first will override the sum of those of the small zones of the second; thus finally, which is the same thing, the free surface of the deformed shape will be wider than that of the cylinder.

Thus, by the nature of the small deformation, if it is such that the areas of the sections parallel with the bases did not change, it increases the extent of the free face of the mass; in other words, the surface area of a cylinder is a minimum by ratio for all the small deformations of this kind.

Among these same deformations is obviously that which consists of a simple inflection, and we saw, indeed, that a slightly bent liquid cylinder returns spontaneously to the form of revolution.

§ 415. Now let us suppose a small deformation which changes the areas of the sections parallel to the bases. Then, since the total volume, or the sum of the volumes of all the sections, is invariable, it is necessary that, among the sections, some have larger areas and others smaller than the area of a section of the cylinder; it is necessary consequently that the shape has bulging portions and thinned portions. Thus let us see if, in this state, the surface of the shape must still exceed that of the cylinder, or if it can be less.

In order to make the question accessible to calculation, let us imagine that the deformed shape is itself of revolution, and that it has as a meridian line a sinusoid. As the deformation must be supposed finite, although very small, it is understood that the axis of this sinusoid will not be able to coincide with the generator of the cylinder: so that volume remains the same, the bulges will have to cover less of the original cylindrical surface than the narrowings, which penetrate more inside; the axis of the curve will thus be brought a little closer to the axis of revolution than the generator of the cylinder; we will indicate by μ the small difference in these two distances. Then, by taking for the x-axis the axis of revolution, and by placing the origin where the sinusoid cuts its axis and where starts a convex arc, if l is the length of the chords of the arcs, β the sagitta of these same arcs, and r the radius of the original cylinder, one will easily find that the equation of our sinusoid is:

$$y = r - \mu = \beta \sin \frac{\pi}{l} x. \tag{1}$$

Let us seek first the relation between μ and β necessary so that the volume does not change. Our liquid shape being bordered by two solid discs, let us suppose that the first of these discs leaves a bulge, and that on the second a narrowing is given; we will be able to then divide, by sections of the same diameter as the discs, the shape in a whole number of equal parts containing each one a bulging portion and a narrowed portion; only, in consequence of the noncoincidence between the axis of the sinusoid and the generator of the cylinder, one understands that, in each pair thus formed, the bulging portion does not constitute a complete bulge, and that at the end of the narrowed portion the beginning of the bulge is added which follows it. However, all these pairs being equal, and the sum of their volumes representing the total volume of the mass, it follows that the volume of each one of them is equal to that of the portion of the cylinder originally ranging between the same sections; it will thus be enough, to establish that total volume did not change, to seek the expression of the volume of a pair, and to equalize it with that of the volume of the corresponding portion of the cylinder.

But one can substitute for the pair in question another pair bordered by two sections having for radius the distance $r-\mu$ of the axis of the sinusoid to the axis of revolution, sections from which the first pass through the point where is born a convex arc, and of which the second passes through that where finishes the following concave arc; it is seen, indeed, that while thus acting, one adds a small portion at the first end of the pair considered initially, but that one cuts off at the other end an identical portion. This new pair will be composed thus exactly of a complete bulge and a complete narrowing, and will lend itself without difficulty to calculation.

The general expression of the volume of a body of revolution bordered by two sections perpendicular to the axis, is, as we know, $\pi\int y^2dx$. To apply it to our pair, it will be enough to replace there y by the value which the equation [1] gives. One has in this manner

$$\begin{aligned}\pi\int y^2dx &= \pi\int\left(r-\mu+\beta\sin\frac{\pi}{l}x\right)^2dx\\ &= \pi\left\{\left[(r-\mu)^2+\frac{\beta^2}{2}\right]x-\frac{2\beta l}{\pi}(r-\mu)\cos\frac{\pi}{l}x-\frac{\beta^2 l}{4\pi}\sin\frac{2\pi}{l}x\right\}+C.\end{aligned}$$

Now let us take this integral between the limits of the pair in question, i.e. of $x=0$ to $x=2l$; we will obtain

$$2\pi\left\{(r-\mu)^2\frac{\beta^2}{2}\right\}=2\pi r^2l+\pi l(2\mu^2-4r\mu+\beta^2).$$

Such is the expression of the volume of the pair; however that of the of the same portion length $2l$ taken in the cylinder is $2\pi r^2l$; so that these two volumes are equal, it is necessary consequently that one has

$$2\mu^2-4r\mu+\beta^2=0.$$

Solving for μ, it becomes:

$$\mu=r\pm\sqrt{r^2-\frac{\beta^2}{2}}.$$

Observing that, as μ must be very small, it is necessary to take the radical with a minus sign, developing this radical, and neglecting the powers of β higher than the second because of the smallness of this quantity, there is finally

$$\mu=\frac{\beta^2}{4r}. \tag{2}$$

It is the relation sought[246] between μ and β.

Let us pass on to the surface area. This is represented, as generally is known, in the case of bodies of revolution, by $2\pi\int yds = 2\pi\int y\sqrt{1+\left(\frac{dy}{dx}\right)^2}\cdot dx$; however y is given by equation [1], from whence one deduces also $\frac{dy}{dx} = \frac{\beta\pi}{l}\cos\frac{\pi}{l}x$. One will have

$$2\pi\int yds = 2\pi\int\left(r-\mu+\beta\sin\frac{\pi}{l}x\right)\sqrt{1+\frac{\beta^2\pi^2}{l^2}\cos^2\frac{\pi}{l}x}\cdot dx.$$

But, because of the smallness of β, one can develop the radical and limit oneself to the first two terms of the series; with this simplification, one finds:

$$2\pi\int yds = 2\pi\int\left(r-\mu+\beta\sin\frac{\pi}{l}x\right)\left(1+\frac{\beta^2\pi^2}{2l^2}\cos^2\frac{\pi}{l}x\right)\cdot dx.$$

Carrying out the multiplication, neglecting the term in β^2, and also integrating between the limits $x=0$ and $x=2l$, one obtains:

$$2\pi\int_0^{2l} y\,ds = 4\pi l(r-\mu)\left(1+\frac{\beta^2\pi^2}{4l^2}\right),$$

an expression into which it is necessary to introduce the condition [2] relating to volume; thus making $\mu=\frac{\beta^2}{4r}$, and neglecting the term in β^4, it becomes finally, for the value of the surface of our pair,

$$4\pi rl+\pi\beta^2\left\{\frac{\pi^2 r}{l}-\frac{l}{r}\right\}. \qquad (3)$$

But the surface area of the of the same portion of length taken in the cylinder is $4\pi rl$; the surface area of our pair will be thus larger or smaller than that of the portion of the cylinder, according to whether there is

$$\frac{l}{r}<\frac{\pi^2 r}{l} \quad \text{or} \quad \frac{l}{r}>\frac{\pi^2 r}{l},$$

inequalities from whence the following ones are drawn:

$$2l<2\pi r \quad \text{or} \quad 2l>2\pi r.$$

But $2l$ is the length of the pair, and $2\pi r$ the circumference of the cylinder; if thus the length of the pair exceeds the circumference of the cylinder, the surface area of this pair will be less than the portion of cylinder having the same length; however, the surface of our pair being equal to that of the pair originally considered, and the whole deformed shape being composed of pairs identical to this last, it follows that, in the condition above, the total free surface of the deformed shape will be less than the total free surface of the cylinder.

§ 416. Thus, when the cylinder is sufficiently long compared to its diameter, its surface is a maximum with regard to the small alteration which would share the shape in portions alternately bulging and narrowed, of suitable form and length; however

[246] This relation shows that if the deformation is supposed, and, consequently, the sagitta, infinitely small, as we did in the calculation of § 406, the quantity μ, i.e. the interval between the axis of the sinusoid and the generator of the cylinder, is second-order, and consequently disappears in front of β; this is why, in the calculation which we have just cited, we made coincide the two lines concerned.

we know that such is, indeed, the mode of spontaneous transformation of an unstable liquid cylinder; theory and experiment are thus again mutually verified.

In truth, we are not certain that at the beginning of the transformation of a liquid cylinder exceeding its limit of stability, the meridian line of the shape is rigorously a sinusoid; but this condition is not essential: when the last inequality of the preceding paragraph exists, the total surface of the shape decreased by a finite quantity, although very small, and consequently one can obviously, without cancelling the difference completely or making it pass in a contrary direction, modify up to a certain point the meridian line so that it constitutes an exact sinusoid.

Moreover, one can, without the inequality in question ceasing to hold, allot to the pair all the lengths higher than the circumference of the cylinder, provided that they are at the same time aliquot parts of the distance from the two bases; consequently, when the cylinder exceeds sufficiently its limit of stability, if, on the one hand, there is (§ 414) an infinity of small deformations with regard to which its surface is still a minimum, there is, on the other hand, several small deformations with regard to which this surface is a maximum.

§ 417. Now let us suppose the length of the cylinder not very considerable so that the spontaneous transformation gives rise only to one pair, i.e. partitions the whole shape in only one bulging portion and only one narrowed portion. In this case, $2l$ will represent the overall length of the cylinder; if this length exceeds the circumference, the surface area of the cylinder will be a maximum with regard to the small deformation, and this deformation will progress. If, on the contrary, the length of the cylinder is less than its circumference, the surface of this cylinder will be a minimum with regard to the small deformation, and this deformation will have to be erased. Finally, if the cylinder has a length equal to its circumference, the deformation, provided that it is supposed extremely little, or, with more exactitude, infinitely little pronounced, will not alter the extent of surface, and consequently will not have any tendency to progress or to be erased; however we know, indeed, that a liquid cylinder whose length is equal to the circumference is precisely at its limit of stability, and when inside this length it is stable, so we must conclude that then its surface is a minimum with regard to any species of very small deformation.

§ 418. The discussion contained in the preceding paragraphs thus establishes, relative to the liquid cylinder, the following principles:

1° However large is the interval between the solid bases compared to their diameter, the surface of the cylinder ranging between them is always minimæ areæ with regard to certain small deformations.

2° For any interval between the bases exceeding their circumference, the surface of the cylinder, though minimæ areæ with regard to the small deformations above, is, on the contrary, maximæ areæ with regard to some other small deformations, among which is that which progresses according to the experiment.

3° For any interval between the bases less than the circumference, the surface of the cylinder is minimæ areæ in a complete way, with regard to any kind of small deformation.

The analogy of the phenomena observed obviously makes it possible to extend these principles to other equilibrium shapes, and we will deduce this general conclusion from it:

When a shape of equilibrium has a limit of stability, it is only inside this limit that its surface is minimæ areæ in a complete way, i.e. it is less than all close surfaces containing the same volume and bordered by the same solid system; beyond the limit in question, the surface of the shape is still minimæ areæ with regard to certain deformations, but it is maximæ areæ compared to another at least, which the molecular forces make progress.

It is thus necessary to relatively restrict in this direction the principle claimed by the general information of the geometers on surfaces whose mean curvature is constant: the majority of these surfaces are completely minimæ areæ only between some limit, beyond which they are minimæ areæ with regard to certain variations, and maximæ areæ with regard to other variations.

It is hardly necessary to point out that if, in the calculation of § 415 and what follows, I supposed the deformation finite though extremely little pronounced, it is that I reasoned from the physical point of view, i.e. that of the liquid shapes formed; but it is clear that from the purely mathematical point of view, nothing prevents one from supposing the infinitely small deformation, and that one would still arrive at the same conclusions; only, in the case of the cylinder, the meridian line of the altered shape should then be an exact sinusoid, which is indifferent for the theory, and the term $\pi\beta^2\left\{\frac{\pi^2 r}{l}-\frac{l}{r}\right\}$ of the expression [3] of the cited paragraph would represent the second variation of the surface.

§ 419. If thus one wanted to treat a priori, and only by calculation, the question of the limits of stability of equilibrium liquid shapes, the problem would consist in seeking, for each surface represented by the equation $\frac{1}{r}+\frac{1}{r'}=C$, the limits between which it is minimæ areæ in a complete way, that is to say less than any other close surface containing the same volume and having the same terminations; these terminations should be characterized besides in advance in a sufficient way. If calculation is practicable, there will be thus a general method for the determination of the limits of stability in question.

This research does not appear to me devoid of interest, even from the purely mathematical point of view; it would probably present very large difficulties, and I leave to the geometers the care to test it. We saw, in this chapter, that while being helped at the same time by experiment and theory, the question is solved clearly and in a simple way in several cases, by means of particular methods.

Let us add that it is easy to explain maintaining the stability of the sphere (§ 357); one knows, indeed, that the surface of this body is, in an absolute way, the smallest possible surface which can wrap a given volume. As for the plane, its stability is, as I showed (§ 358), a necessary consequence of that of the sphere.

§ 420. A point remains to be examined. It was seen that the surface of a sufficiently long liquid cylinder compared to its diameter, is maximæ areæ with regard to several small deformations, and the analogy makes it possible to think that the same thing takes place for other shapes of equilibrium; moreover, we know that an arbitrary shape of equilibrium exceeding its limit of stability always undergoes the same spontaneous deformation in the same experimental circumstances; it is thus necessary to recognize, at least in the cylinder, the existence of a theoretical condition which determines the choice of the mass among all the deformations which would decrease its surface.

One would be tempted to believe, at first glance, that since surface area tends to decrease immediately, the molecular forces choose the deformation which makes this waning the largest possible. But it is not thus; indeed, the deformation which would produce the greatest waning of surface must be unique for a given shape; however, in the spontaneous transformation of the cylinder, the length of the pairs varies, we know, for the same diameter and the same distance from the bases, with viscosities of the liquid and other resistances. Moreover the expression [3] of § 415 shows that at the beginning of the transformation, the greatest reduction in the surface would correspond to the value of l being the largest possible, and consequently with that where, whatever was the spacing of the bases relative to their diameter, there would be formed between them one bulge and one narrowing; and one cannot object that that holds with the nature of the curve that I took for meridian line, and which is perhaps not the true one; I carried out a calculation similar to that of § 415 for two other lines, which were, first, a broken line which would generate a succession of equal truncated cones joined together alternately by their large and their small bases, and, in the second place, a line made up of equal arcs of circles alternatively convex and concave towards the outside, i.e. on the meridian line supposed by Mr. Hagen (§ 394); however, in both cases, I still found that the greatest reduction corresponded to the biggest length of each bulging or narrowing portion; it is thus quite probable that this result is general, and that thus it takes place for the true meridian line.

§ 421. The condition which fixes the choice between all the small deformations from which would result a reduction of surface area, must, consequently, be sought elsewhere, and one arrives there, I think, by the following considerations:

When, by an unspecified cause, a liquid mass subjected to only molecular actions does not constitute a shape of equilibrium, and that thus the pressures corresponding to the various points of its surface are unequal, it necessarily tends to equalize these pressures, and then the liquid is driven out immediately from points of stronger pressure towards those of less pressure, until the inequalities completely disappear. The shape thus changes such a manner that at every moment the pressures are as not very different as the conditions of the phenomenon allow; in other words, at every moment, the form of the mass always has the greatest possible analogy with a form of equilibrium.

Now let us suppose a liquid shape exceeding its limit of stability formed between solid terminations. It will have necessarily a crowd of small irregularities imperceivable to the eye, and arising at the same time from the process of its formation, whatever it is, from the small movements inseparable from this process, etc, so that it will constitute a shape of equilibrium only seemingly, and will be, actually, in the case above. However, among these irregularities, some will be such as, if they existed alone, they would increase surface area, and others will be such as they would decrease it; consequently the first will tend to be erased, and the second, on the contrary, will tend to progress; but, under the terms of what precedes, the molecular forces will choose among these last those which will allow the modified mass to deviate the least possible from another shape of equilibrium, and will make them progress while regularizing them.

One can express this principle differently: since the forces which produce the transformation are the differences in pressure, one can say that the phenomenon is arranged so as to be achieved with the least possible expenditure of force.

§ 422. Let us apply these considerations to the cylinder. Let us suppose a liquid cylinder of a considerable length relative to the diameter, formed by an unspecified means between two solid bases. We saw that an irregularity consisting of the division of the shape into thicker and thinner portions alternately, could decrease surface area; on the other hand, the shape of equilibrium closest to the cylinder is the unduloid, which is composed of portions alternately bulging and narrowing; if thus our principle is true, the molecular forces will choose, among all the small irregularities of the cylinder, sufficiently spaced narrowings, they will regularize them, by growing them more and more, and they will arrange the shape so as to as much as possible approach an unduloid. Accordingly, if the liquid were completely free from viscosity, and that it had at the same time an absence of external resistances, so that the transformation could be carried out with entire freedom, finally if the distance between the bases were an exact multiple of their circumference, the phenomenon would go as if it originated in a unduloid infinitesimally different from the cylinder.

But I had already arrived (§ 405) at this last result, by basing myself on experiment and reasoning of another nature; two primarily different methods thus contribute to establish it, and consequently the principle set forth in the preceding paragraph, an obvious principle in itself and whose result in question rises immediately, can, I think, being regarded as sufficiently shown.

§ 423. If the interval of the solid bases is not an exact multiple of their circumference, so that the pairs cannot take the length which is appropriate for the unduloid, i.e. their minimum length, or if it is about a real liquid, in which case there are resistances, the pairs will be lengthened more, we know; but, always under the terms of our principle, the shape, at the origin of the transformation, will approach as much as possible an unduloid, and one can admit that it will be what would become an unduloid if it were simply stretched in the direction of its length. If it is indeed thus, the originating meridian line will be still a sinusoid, as we supposed, and, consequently, the length of the bulges will be still rigorously equal to that of narrowings.

In his second Report, Beer states, about the modifications through which a long cylinder gradually passes, an idea similar to that which I gave forth above; he thinks that, throughout all transformations, the mass, which cannot assume one shape of equilibrium any more, is worked in a manner, not of always moving away the least possible from such a shape, but of constantly presenting an ensemble of equilibrium shapes; in other words, according to him, during the achievement of the phenomenon, the shape is composed of bulging portions of unduloid alternating with cylindrical portions, these last decreasing more and more in diameter while they increase in length, and first bulging more while shortening itself, until, in the cylindrical portions, there is equality between the length and the circumference; these same portions constituting unstable cylinders then, change in their turn; from whence spheres and spherules.

This idea, not easily acceptable a priori, is, moreover, in dissension with what the experiments of § 400 show, when the ratio between the spacing and the diameter of the solid bases equals or exceeds very little the limit of stability, so that the transformation is carried out with enough slowness so that one can observe the changes of the meridian line well, this last form always has a perfectly continuous curve, until the middle of narrowing has only a small thickness; only then one sees the cylindrical filament developing with speed. Also Beer, to whom I had announced my remark, recognized his error[247]

§ 424. Assemblies of liquid films have also their stability conditions, and we saw (§ 210) how Mr. Lamarle subjected this topic to a precise theory. I will finish this chapter with a summary of a work of Mr. Duprez[248] on a phenomenon in which the capillary pressures combine with the action of gravity to produce curious effects of stability and instability.

One knows that a vessel full of liquid whose neck is sufficiently narrow, can be inverted, with the opening opened, without the liquid escaping from it, and one simply attributes this fact to the atmospheric pressure exerted upwards on the opening; however Mr. Duprez recognized that with suitable precautions, one can maintain the liquid thus suspended in a vessel whose opening is by no means narrow: he managed to support water with an opening of 19.85mm diameter. To obtain this result, it is necessary that the surface of the liquid in the opening is plane and quite horizontal, a condition that Mr. Duprez realizes by means of a clever apparatus.

It was difficult to understand how the physicists had stopped with the idea of the atmospheric pressure as the single cause of phenomena of this kind; indeed, if this direct compression supported the liquid in a narrow opening, it should obviously support

[247] *Ueber die Transformation of the flüssigen Cylinders* (ANN. OF MR. POGGENDORFF, flight. CII, p. 320).

[248] *Sur un cas particulier de l'équilibre des liquides* (MEM. OF ACAD. FROM BELGIUM, vol. XXVI, 1851, and vol. XXVIII, 1854).

it in an opening of an arbitrary diameter; thus why is there a limit which one cannot exceed?

At the time when Mr. Duprez made his observations, I had dealt already with the questions relating to the stability of liquid surfaces; I was not long in finding the principles which are used as a basis for the complete explanation of the phenomenon in question here and for the theoretical determination of the limiting diameter, and I suggested these principles to Mr. Duprez, as he is pleased to recognize in his Report.

Let us suppose the surface of the liquid in the opening is mathematically plane and horizontal, and remove any accidental cause of disorder; it is clear that the liquid will be held up by the atmospheric pressure, however large is the diameter of the opening. Let us imagine now that the liquid surface undergoes an excessively small deformation which makes it concave on approximately half of its extent, and, consequently, convex on the other half; consequently equilibrium will not be able to take place from the point of view of gravity any more: because of the difference in level, the liquid of the convex portion will tend to fall and let the air enter by the concave portion; but, in addition, a plane surface is, we know, stable from the point of view of the molecular forces, whatever its size, so that, under the action of these same forces, the small deformation will tend to be erased; in other words, the capillary pressures corresponding to the convex portion will override those which belong to the concave portion, and will consequently tend to restore the plane surface; there will be thus a fight between gravity and capillary pressures.

But, for the same difference in level, the curvatures of the two portions of surface, and consequently the differences in capillary pressure, will be obviously all the lower as the diameter of the opening will be larger; there is thus necessarily a limit of diameter inside which the capillary pressures prevail, so that the liquid surface is stable, while beyond it gravity prevails and causes thus the flow of the liquid.

Accordingly, to arrive at a theoretical value of the limiting diameter, it would be enough to know, for a small difference in level, the radii of curvature of two rectangular normal sections at the respective apices of the convex portion and concave portion, according to the diameter of the opening; with these elements, one would calculate the difference of the capillary pressures corresponding to the two apices in question, one would equate it to the action of gravity, and one would solve the equation for the diameter.

But how to get these data? When the diameter of the opening is rather large so that the liquid refuses to be maintained suspended, the exchange between this liquid and air happens in a way so fast that it is impossible to observe the alteration of the original surface. Mr. Duprez overcomes the difficulty by employing my process of the immersion of oil in the alcoholic liquid: a cylindrical glass vessel 8 centimetres in interior diameter and 7 high, is introduced into the alcoholic liquid, its opening at the bottom, then filled exactly with oil using a means that Mr. Duprez indicates; this vessel is fixed at a certain height in the ambient liquid, so that the surface of oil in the opening is free, and the quantity of oil is such that this surface is plane. If the densities of the two liquids are equal, the surface of oil preserves, one understands, its plane form; but if one adds to the ambient liquid an increasing alcohol excess, one reaches a point where gravity takes again its rights, on the oil, and where stability is not possible any more; however it is easy to adjust the rather small alcohol excess so that the deformation happens with an excessive slowness, and that thus one can examine it perfectly.

By operating in this manner, Mr. Duprez noted that the deformation consists indeed in the division of the surface into only one convex portion and one concave portion, and he could, using suitable means, determine to a sufficient approximation the elements indicated higher, which leads him to the formula

$$D = 5.485\sqrt{h},$$

in which D is the limiting diameter, and h the height to which the liquid would rise in a capillary tube of a millimetre of radius.

Mr. Duprez deduced, for distilled water at room temperature, D = 21.15mm. Now, before resorting to the method above, he had arrived at the approximate value of the limiting diameter relating to the same liquid by a primarily different way: with a diameter lower than the limiting diameter, the liquid can remain suspended by having, in the opening, a convex surface or a concave surface; but if, by a suitable means one increases up to a certain point this convexity or this concavity, the liquid runs out. Mr. Duprez measured, for a sufficient number of diameters, the respective sagittas of convex and concave surfaces at the moment of the rupture of equilibrium, and he names them *sagittas of rupture*. With these data, he generally links the sagitta of rupture to the corresponding diameter by an empirical equation, and setting, in this equation, the sagitta equal to zero, he obtains the limit diameter in the case of a plane surface. He finds thus, with regard to distilled water, the value 21.44mm, a value not very different from that which results from the theoretical method.

The very satisfactory agreement of these two values, obtained by methods which do not have anything common, cannot leave any doubt about the legitimacy of the formula mentioned above, a formula according to which the limiting diameter is proportional to the square root of the capillary height of the liquid.

These same values exceed a little the diameter 19.85mm found by direct experiment; but that must be, because, in this experiment, it is impossible to avoid small causes of disorder which, when one approaches the limit, are enough to bring about the destruction of equilibrium. Mr. Duprez subjects the formula to new checks, by applying it to three other liquids: alcohol, the oil of sweet almond and sulphuric ether; he also seeks, for each one of those, the limiting diameter by the method of the sagittas of rupture and by the formula, and he still arrives at concordant results. Here, indeed, are these results:

$$\text{Alcohol} \ldots\ldots \begin{cases}\text{by the sagittas, 13.14mm,}\\ \text{by the formula, 13.48mm.}\end{cases}$$

$$\text{Oil of almond} \begin{cases}\text{by the sagittas, 15.00mm,}\\ \text{by the formula, 15.03mm.}\end{cases}$$

$$\text{Ether} \ldots\ldots\ldots \begin{cases}\text{by the sagittas, 12.02mm,}\\ \text{by the formula, 12.48mm.}\end{cases}$$

The agreement is less satisfactory for ether, but, with regard to this liquid, Mr. Duprez measured the sagittas of rupture in one only tube.

Finally, Mr. Duprez extends the same principles to the explanation of the well-known fact that it is impossible to pour a liquid in a vessel with a narrow neck: the liquid takes, in the opening, a rather stable surface so that the exchange with the interior air of the vessel cannot take place.

CHAPTER XI.

Applications of the properties of unstable liquid cylinders, or, more generally, of liquid shapes which have one dimension large relative to both others; theory of the formation of the drops at the edge of certain films; theory of the explosion of bubbles; theory of the constitution of streams of liquid launched from circular openings.

§ 425. I said (§ 153) that I would give the explanation of the generation of the drops at the edge of the liquid discs of Savart. Let us first say that Magnus, in the passage of his *Hydraulic researches* relating to these discs (§ 333), tried to solve the question: he looks at, as we saw, the film as always decreasing in thickness from the central part to the edge; he supposes that on this border the film becomes very thin and tears, and that the portions thus separated contract, by the attraction of their molecules, into many small masses, which would constitute the drops.

These ideas do not agree with the results of Mr. Hagen (§ 153), so strongly and convincingly supported, and whereby the minimum thickness would not be at the contour of the disc; moreover, they explain neither the presence of the rim observed by Savart on this contour, nor the higher speed of the drops launched from the edges of the notch produced in the film by the introduction of a solid obstacle, nor the tangential directions of these last drops.

§ 426. Now let us approach the announced true explanation. Let us consider what occurs immediately after the opening of the two orifices from where escape the opposite streams (§ 232), while the liquid disc grows in diameter. This disc constitutes a shape of revolution, whose meridian section has obviously a very strong curvature at its equator, i.e. at the very edge of the sheet; however, this strong curvature necessarily causes an energetic capillary pressure directed along the radius of the disc and in a contrary direction to the movement; at the edge of the disc, the liquid is thus acted on by two opposed forces, of which one tends to move it away from the center and the other to bring it closer; and from there must result an effect similar to that which takes place at the meeting of the two streams, where two opposite forces are also present, i.e. the liquid must undergo a sideways displacement; in other words, while the disc develops, the driven back liquid must form a rim all along its contour. That said, while the disc is increasing, this rim tends, on the one hand, to grow bigger by the same causes which give it birth, and, on the other hand, to be thinned in consequence of its extension according to the increasing circumference of the disc, and one can claim that these two effects are neutralized each other more or less, so that the rim varies little in size until the disc reaches its largest diameter. However, this same rim, having the form of a kind of cylinder that one would have curved around in a ring, constitutes an unstable liquid shape, and must be dissolved, during its development, into isolated masses, like the oil ring of § 368. In truth, it is connected, by its interior edge, to the liquid film; but from there is born simply a slight resistance, which, according to what we saw with regard to cylinders, must be restricted to making the divisions a little longer, without preventing the transformation.

Now the rim, due to the inertia of its total mass, cannot completely lose its speed at the same time as the portion of the film to which it immediately adheres; the small masses into which it converts will thus separate from the contour of the film, and will be launched with their small excess speed. In this moment, the capillary pressure must quickly reform a new rim, which is dissolved soon, like the first, into isolated masses, and so on. Such is, I am convinced, the true theory of the generation and the projection of the drops, and it also applies to the drops launched from the edge of the open films without aureole of the first Memoir of Savart (§ 230).

Let us see if this theory, which explains the general phenomena thus, is satisfied in the same way with all the details observed by Savart, Mr. Hagen and Magnus.

As soon as the small masses into which one of the successive rims is dissolved separate from each other, each portion of the edge between them, not having more than the small thickness of the film itself, is subjected at once, in consequence of its strong transverse curvature, to a very energetic capillary pressure; this pressure acting on points where the liquid does not have any more than a low speed of traverse, must cause a withdrawal towards the center of the disc, and a beginning of a rim; but as the small masses are still adherent to the disc by their bases and continue, due to their inertia, their translatory movement, the intermediate portions of the edge must appear indented, and each small mass should appear at the tip of a protuding wedge. However, it is precisely this aspect which Savart in his first Memoir describes, with regard to open films and without aureole, "of which free contour, *slightly notched*, threw a great number of droplets, *which leave the protruding wedges of the serrations.* "

Savart does not make the same description of the contour of the films of his second Memoir; he says only (§ 232) that "the sheets are constantly surrounded by a small round rim from where escape a multitude of droplets"; but Mr. Hagen claims that the drops *strongly stretch the film*; however, that is precisely the appearance which must result from the whole of their small movement ahead and of the small withdrawal of the intermediate portions.

At the moment when a series of drops completely leaves the disc, the protruding wedges of which I speak must be erased abruptly and converted into portions of rim; those constitute then, with the portions previously formed, a continuous rim, which continues its movement of withdrawal until, always growing bigger and thus decreasing in meridian curvature, its capillary pressure ceases to overcome the remainder of force which pushes ahead the portion of the film to which it adheres; then it starts again to go ahead while it carries out its transformation into isolated masses; then the preceding phenomena recur, and so on; the diameter of the disc must thus express a fast succession of increases and reductions, as observed it Mr. Hagen. Let us add that all these phenomena would do little to achieve a perfect uniformity all along contour: the rim, one understands, does not have, in general, the same thickness in all its extent, so that its transformation is not carried out at the same time everywhere; whence the irregularities announced by Mr. Hagen.

When, by means of a transverse wire, one makes a notch in the liquid disc, the capillary pressure must also drive back the two edges of that by forming rims there, although the presence of the latter is not announced by Mr. Hagen and Magnus. Each one of these rims constitutes also a lengthened shape which must, during the trajectory of the liquid of which it is formed, be dissolved into small masses, and these masses, as soon as they are free, must escape in the same direction as their translatory movements; thus drops are launched from the edges of the notch, with their tangential directions. Moreover, in these same rims, there is extension of the liquid, and consequently the tension cannot reduce speed; this, one understands, is only slightly diminished by the side adherence of the rims to the film, and the drops which take their origin in these rims must thus be projected much further than others, as Magnus observed.

Finally, one can show to the eyes a rim being formed at the free edge of a film, its fast withdrawal and its subsequent conversion into isolated masses; it is enough, for that, to arrange, in the experiment of § 368, so that one at least of the two plane oil films has a rather great thickness; then its withdrawal, when it is burst, is carried out with relatively little speed so that one distinguishes perfectly a rim all along the edge of the opening, a rim which, arriving at the metal ring, constitutes the regular liquid ring there that we know, and which is not long in being dissolved into small masses.

In this experiment admittedly the film is made of a liquid at rest and the rim goes from the center to the circumference, while, in the experiments of Savart, the liquid of the film is moving, and the withdrawal is carried out from the circumference towards the center; but these differences obviously do not have any theoretical bearing on the question.

§ 427. It is clear that, whenever a film presents a free edge, the strong capillary pressure on this border will cause there, as in the cases above, the formation of a rim, and it will be dissolved in the same way into isolated masses; it is what happens, for example, as I said (§ 235), with regard to the films which develop when obliquely launching the liquid into the air. In certain circumstances, the liquid which constitutes the rim is animated by a translatory movement in the direction of the length of this rim, as at the two edges of the notch produced in the liquid disc of Savart, at the edges of the films resulting from the meeting of two streams which form an angle between them (§ 234), etc.

§ 428. We will use the same principles to explain a mysterious fact described initially by Hooke (§ 318), then by Leidenfrost (§ 321), and that Fusinieri sought to explain (§ 324) on the basis of his theory; I speak about the explosion of bubbles. It follows from observation that the phenomenon is more marked when the film is very thin, and I have seen that its burst can make a perceptible noise, and convert it into a kind of liquid dust which can carry more than one meter away.

One cannot claim that at the exact moment when the film is pierced at a point, all the remainder of this film changes simultaneously into tiny droplets: such a transformation would escape any theoretical principle; moreover, it follows from a result of § 121 that the pressure exerted on the interior air by a soap bubble 5 centimetres in diameter, such as those of Leidenfrost, is equivalent to the pressure of a water column only approximately 0.04mm high[249]; however, this very weak addition to the external atmospheric pressure can cause only an insensible reduction in the volume of the air contained in the bubble, and consequently, if the film were transformed into globules on all its extent at the same time, the interior air, so little compressed, and finding, moreover, an infinity of exits at the same time, could only take, while expanding, almost no movement; consequently no explosion and no notable transport of the globules.

It thus must be, in all necessity, that conversion into globules is carried out progressively, starting from a first point, where the rupture was done. Accordingly, let us see how events must occur. As soon as a small opening is formed, it increases quickly by the shrinking of the film; but, at the same time, another effect occurs, which is the generation of a rim all along the edge of the opening, as we saw in the oil film of § 426; and, because of the great thinness of the film of soap water, the transverse dimensions of this rim are excessively tiny, it must be transformed into globules with an extreme speed. If, in the experiment of the oil film, one does not see the rim changing before it reaches the metal ring, it is because the phenomenon is slowed down considerably (§ 379) by the viscosity of the oil and especially by the presence of the ambient alcoholic liquid.

Now, in our bubble, the two effects above are accompanied by a third: once an opening exists, the bubble, we know, starts to decrease in diameter, by displacing the air which it contained towards the outside; and, although the force which acts thus is weak, it expels the air with speed, because it is continuous and increasing, and, in addition, the opening is growing: there is proof of this speed in an experiment of Mr. Henry (§ 151); however, the air thus highly expelled carries far with it the globules

[249] In § 121, it is about glyceric liquid, and not about soap water, but the tensions of these two liquids are not appreciably different (§ 299); it is the same, with equality of diameter, for the pressures exerted by their respective bubbles.

above. But as soon as this first series of globules is removed, a new rim is reformed at the edge of the increased opening, then is also dissolved in globules which are carried like the preceding, and so on. In consequence of the tenuity of the film, all these generations of series of globules, although successive, are achieved in a wink, so that the observer can note only the final result, which is the presence and the movement of liquid dust in the surrounding air.

One sees that this explanation, which rests on well established principles and facts, gives an account of all the characteristics of the phenomenon.

§ 429. Moreover, to put it out of doubt, I subjected it to an experimental test: I told myself that if, in a bubble which bursts, the production of the globules is actually carried out in successive series, it would be the same with regard to a plane film; however, in this last case, the thing could be checked, because it was enough that the film, during its rupture, was very close to a plane surface, on which the globules or droplets would mark their traces.

Accordingly, I operated as follows: I placed, on a plane and horizontal shelf, a sheet of slightly absorbent paper colored pale brown (straw paper); then I developed a film of glyceric liquid in the large wire ring which had been used in the experiments of §§ 186bis and 392. This ring with its film was maintained then for a few moments in a vertical position, so that the excess liquid accumulated in drops at the low point; these drops removed, I gently set the ring, its feet in the air, on the paper. The wire which constitutes this ring having 3mm thickness, the film found itself 1.5mm from the paper. Things being thus prepared, the film was burst, and, so that the withdrawal of it had to traverse a larger space, the rupture was generally produced in a point very closer to the interior contour of the ring. For this last operation, I did not employ, as in other experiments where films had to be broken, a filter paper with a point, because this could have depressed the film and have brought it in contact with paper; I in fact used a sewing needle reddened with a lamp: the film bursts as soon as the point of this needle touches it.

Once the film was broken, I removed the ring for better observing the traces of the droplets; now, despite the irregularities which they presented, there is clearly recognized in their arrangement several distinct series, placed one behind the other with respect to the point of rupture. The experiment was repeated fifteen times, and almost always with results of the same order, more or less quite marked; it is hard to tell two of them apart.

I proceeded in various manners. First, after having formed the film, I held it vertically until it showed well characterized red and green horizontal zones, and, when the ring was set on paper, I burst the film at the point where these zones indicated the greatest thickness. Secondly, I left the film vertical only for a very short time, then I inclined successively the ring in various directions to make the incipient zones disappear. When the ring is set down, the film is slightly bent; that because, while one lowers the ring towards the paper, the resistance of the air makes the middle of the film remain a little behind, and then the contact of the wet ring with the paper is rather intimate so that the excess of imprisoned air cannot escape; I gave it an exit by raising a little the ring on a side; as soon as it was replaced, the film was plane. Thirdly, I let it remain convex, and I waited one minute; I then saw concentric rings of various colors developing; finally, I burst the film either in its middle, or close to its edge. I reproduce here (fig. 104 and 105), at a quarter of their size, the two best results; that of fig. 104 was obtained by the second process, and that of fig. 105, by the third; the point of rupture is at *a*. Each series of droplets being formed during the fast withdrawal of the film, takes part the speed of this movement; the droplets thus will meet paper very obliquely, so that their

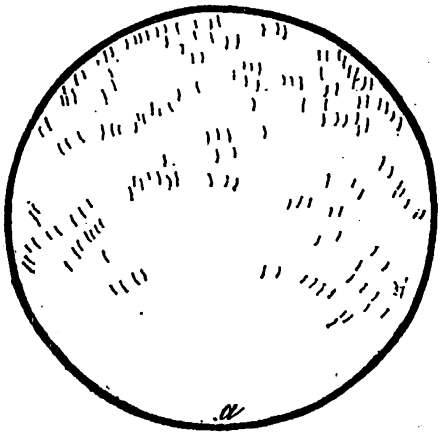

Fig. 104

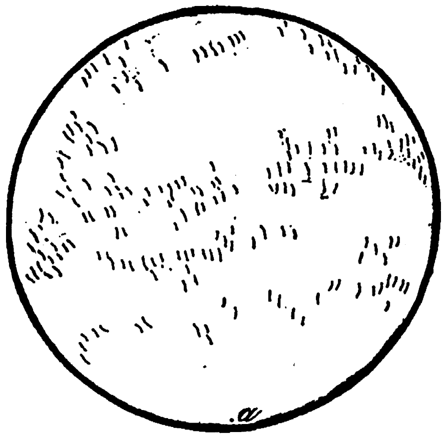

Fig. 105

traces are not round, but lengthened in the direction of the withdrawal; moreover, when one immediately observes them after their production, one recognizes that the majority present a succession of bulges and narrowings; due to their lengthened form, they started to undergo the spontaneous transformation of such shapes, and this transformation was stopped by increasing adherence to the paper which they soak. As for the irregularities and the interruptions of these sets of traces, they come obviously from the film not having an equal thickness throughout[250].

§ 430. In the Report summarized in § 167, Mr. Van der Mensbrugghe draws from our principle of the spontaneous transformation of lengthened liquid shapes, the explanation of a fact which is shown in certain cases of spreading out of one liquid on another: for example, as Mr. Tomlinson has observed[251], a droplet of lavender oil

[250]Since my work was published, Misters Marangoni and Stetanelli published the Report that I listed in § 508 under n° 38. They also studied what occurs during the rupture of the bubbles, but by a very clever process very different from mine. This process primarily consists of making the liquid fluorescent by the addition of a suitable substance, such as esculine, and causing the rupture of the bubble while it is exposed to the intermittent illumination of the sparks of an apparatus of Ruhmkorff. When several bubbles are successively subjected to the experiment, it happens, one understands, that, for one or the other of them, a spark happens during the short destruction of the film; the persistence, although very tiny, of the impression on the retina thus makes it possible to observe the bubble in a phase of the phenomenon of its rupture.

The authors as well noted, firstly, that when a bubble bursts at a point, it creates there simply an opening, which grows quickly until extending to the film; secondly, that the edge of this opening is furnished with a notched rim, similar to that of the films of Savart, and that the protruding wedge of each serration is prolonged in a curved filament towards the outside.

Up to now all agrees perfectly with my theory and my experiment: the spherules to which the rim converts, spherules driven outwards by the movement of the air, stretch the rim, and produce there the protruding wedges, from which they are detached with taperings. But the authors add that the filaments are terminated by ramifications, which are dissolved into extremely tiny droplets.

If these ramifications were real, if the authors were not misled by some illusion due to the instantaneity of the observation, the fact would be explained with much difficulty; indeed, the rim cannot apparently present thus, at various points of its length, the protruding wedges being prolonged in filaments, if it is stretched by small masses, or spherules, projected outwards; and it is hardly acceptable that it is each one of these spherules which is subdivided thus to give rise to the ramifications in question. I am led to believe that at the time of the observation, the spherules were already detached and moving away from the bubble, and that the authors, paying their attention specially to the serrations and the filaments, did not notice the spherules disseminated in the air. As, after the projection of the spherules, the filaments which they leave behind them must convert themselves into excessively small spherules, it is perhaps the latter phenomenon which the authors took for ramifications which change.

I must say, however, that the authors repeated a great number of times the experiment, and that they do not mention another variation in the aspect of the phenomenon, which is extended from the portion of film which still remains at the moment of the observation.

[251]*On the cohesion-figures of liquids* (PHILOS. MAGAZ. 4th Series, 1861, vol. XXII, p. 249).

deposited on distilled water is spread out initially in a highly colored film, then, after a few seconds, this film is subdivided into a great number of delicate filaments, which draw small curvilinear polygons whose whole has the aspect of a fine lace; however, these filaments are not long in being dissolved into series of small lenses.

§ 431. Our principles completely explain in the same way another phenomenon, which is the constitution of liquid streams launched from circular openings, a constitution that Savart so admirably studied from the experimental point of view. We will present our theory in detail, by constantly putting it in parallel with the observations of the famous physicist. Let us consider a liquid stream running out freely under the action of gravity from a circular opening bored in the thin wall of the horizontal bottom of a vessel. The molecules of the liquid interior to the vessel, which flow from all the sides towards the opening, still preserve, as one knows, immediately after their exit, their oblique directions in the plane of this opening, from which results a fast contraction of the stream starting from the opening until a horizontal section which one improperly indicates under the name of contracted section. Arriving at this section, which is not very far away from the opening, the molecules tend to take a common vertical direction, with the speed corresponding to the height of the liquid in the vessel, and they, moreover, are pulled in this same vertical direction by their individual gravity. It results that, the opening being supposed circular, the stream tends to constitute, starting from the contracted section, an appreciably perfect cylinder of an arbvitrary length; but due to the speed of the liquid, the diameter of the stream, instead of being everywhere the same, is decreasing more or less as one moves away from the contracted section.

If only the causes which we point out acted, the stream would thus simply appear increasingly threadlike beyond the contracted section, without losing either its limpidity or its continuity. But it follows from our experiments and from our principles, that such a liquid shape, whose form approaches that of a very lengthened cylinder, must be transformed into a series of isolated spheres having their centers arranged on the axis of the shape. In truth, this is about a liquid subjected to the action of gravity; but it is obvious that, during the free fall of a liquid, gravity does not put any obstacle in the way of the molecular attractions, and that those must then exert on the mass the same shaping actions as if this mass were without gravity and in the at-rest state; this is, for example, why raindrops take, in their fall, a spherical form. Only, so that the preceding conclusion was completely rigorous, it would be necessary that all the parts of the mass were animated at the same speed, which does not take place for the stream; but it is understood that, if this difference can make some modifications to the phenomenon, it could not prevent its production.

The liquid of the stream will necessarily thus arrive by degrees, during its movement, to constitute a series of isolated spheres.

But this liquid being continuously renewed, the phenomenon of the transformation must also be always renewed. In the second place, each portion of the liquid starting to be subjected to the shaping forces as soon as it belongs to the imperfect cylinder that tends to constitute the stream, i.e. as of the moment when it crosses the contracted section, and remaining then, during its progress, under the continued action of these forces, it is seen that each *division* of the stream must start to take shape starting from the contracted section, and to go down, carried by the translatory movement of the liquid, while changing by degrees to arrive at the state of an isolated sphere. However, it follows that at a given moment, divisions of the stream must be in an all the more advanced phase of the transformation as one considers them at a larger distance from the contracted section, at least until where the transformation into spheres is completely carried out. Until the distance where the separation of the masses takes place, the stream must obviously be continuous; but at a larger distance, the portions of liquid which pass, must be isolated from each other.

If thus the movements of the liquid, that of translation as well as that of transformation, were rather slow so that one could follow them with one's eyes, one would see the stream formed of two distinct parts, the upper continuous, the lower discontinuous. The surface of the first would present a succession of bulges and narrowings which would go down with the liquid, while being renewed continuously, and which, imperceivable at their origin and for a certain distance from the section contracted, would develop more and more during their translatory movement, the bulges becoming more projecting and narrowings deeper; finally, these divisions of the stream arriving one after the other, in their greater development, at the lower end of the continuous part, one would see them become detached, and tend then towards the spherical form. However, one knows, due to the beautiful observations of Savart[252] that such is, indeed, the real constitution of the stream.

Let us add that, on the same assumption of the deceleration of the movements, one would see the separation of each mass preceded by the formation of a filament which would be dissolved in smaller masses. If events occurred exactly as in cylinders, these small masses would be unequal spherules, so that each isolated sphere would be followed similar spherules; the discontinuous part of the stream would thus appear to be made up of isolated spheres of the same volume and unequal spherules arranged in the intervals between the first, the ones and the others being carried by the translatory movement, and being renewed unceasingly at the end of the continuous part. But the acceleration of the descent lengthens divisions and, consequently, the narrowings, during their progress; consequently, due to what was set forth in § 383, the filaments must, in general, be rather thick, and the largest of the masses into which they are dissolved can not be much lower in diameter than the principal masses, in which case the denomination of spherules is not applicable for them. It is indeed, as will be seen (§ 478), what experiment showed.

§ 432. Now, the translatory movement being too fast for the phenomena which occur in the stream to be grasped by direct observation, there must result certain particular appearances. Let us recall here that when a liquid cylinder is dissolved into spheres, the speed with which the transformation is carried out is accelerated, and starts by being extremely small. Because of this originating smallness, and speed of the translatory movement in the stream, the effects of the gradual transformation will be able to become perceptible only at a more or less large distance from the contracted section. Until this distance, the fast passage of the bulges and narrowings in front of the eye will be able to give rise to no sensible effect; so that this portion of the stream will appear in the form which it would affect if it did not have any tendency to divide. After this same distance, the bulges starting to take a notable development, the stream will appear to be widening, until another distance beyond which the diameter will appear about constant.

Such is, indeed, as again the observations of Savart showed, the form presented to direct observation by a stream withdrawn from any foreign influence.

Finally, one knows that starting from the opening up to the point where it starts to appear to widen, the stream is limpid, while with beyond it appears more or less turbid; and Savart explained these two different aspects perfectly, as other curious appearances which the turbid part has when one has not removed the foreign influences: the limpidity of the higher portion is due to the slight development of bulges and narrowings which are propagated there, and disorder as well as other appearances of the remainder of the stream result from the fast passage in front of the eye, initially of the bulges and

[252] *Mémoire sur la constitution des veines liquides lancées par des orifices circulaires en mince paroi* (ANN. DE CHIM. ET DE PHYS. DE PARIS, 1833, volume LIII, page 337).

narrowings becoming more pronounced, then, lower, of the isolated spheres and the interposed smaller masses.

§ 433. But we can go further. Two consequences arise immediately from our explanation of the constitution of the stream. Firstly, divisions changing during their descent, it is clear that the space traversed by a division during the time that it takes to carry out a given part of its transformation, will be all the more large as it goes down more quickly, or, in other words, that the load, i.e. the height of the liquid in the vessel, will be more considerable; from which it follows obviously that, for the same opening, the length of the continuous part of the stream must grow with the load. However, that is what the observations of Savart confirm. In the second place, since the transformation of a cylinder is all the more slow as the cylinder has a largeer diameter, the time which a division of the stream will employ to carry out the same part of its transformation, will be all the more long as the stream will have more thickness; from which it follows that, if the rate of flow does not change, the space which a division will traverse during this time, is all the more considerable as the diameter of the orifice will be larger; consequently, for the same load, the length of the continuous part must grow with the diameter of the opening, and it is again what the observations reported in the cited Report verify.

As for the laws which govern these variations in the length of the continuous part, Savart deduced from his observations, which were made by employing water streams, that, for the same opening, this length is about proportional to the square root of the load, and which, for the same load, it is about proportional to the diameter of the opening.

We will examine whether these two laws themselves also arise from our explanation.

§ 434. Let us imagine, for one moment, that gravity ceases acting on the liquid as soon as it crosses the contracted section. Then, starting from this section, the speed of traverse will be simply that which is due to the load, and which has, as one knows, the value $\sqrt{2gh}$, g indicating gravity, and h the load. This speed will be uniform, and, consequently, if the stream did not have a tendency to divide, it would remain exactly cylindrical for an arbitrary extent (§ 431). Now, all the parts of the liquid being animated at the same speed of traverse, this common movement will not be able to influence the effect of the shaping actions; so that, for example, the gradual modification of each narrowing, and the time that it will take to achieve them will be independent the speed of traverse.

That said, let us consider the infinitely thin liquid section which must constitute the mid-circle of a narrowing, from the moment when it leaves the contracted section. This section will go down with a constant speed, and, at the same time, its diameter will be decreasing, until the narrowing to which it belongs transforms into a filament, and then the section in question will occupy the middle of this filament; then the filament will divide to be converted into spherules. As we showed above, the time employed by the achievement of these phenomena, and during which the liquid section that we considered traversed the distance between the contracted section and the place that occupies the middle of the filament at the precise moment of rupture, is independent of the speed of traverse, and, consequently, if the diameter of the opening does not change, this time will be constant whatever the load. However, in a uniform movement, the space traversed during a given time being proportional to the speed, the distance above will be proportional to $\sqrt{2gh}$, and, consequently, to $\sqrt{h}$. As we will have often to make use of this same distance, we will represent it, for brevity, by D.

Now, it is easy to understand that, in our stream, the length of the continuous part does not differ appreciably from the distance D. Indeed, the continuous part finishes at the precise place where comes to occur, in each filament, the highest of its rupture points: because, at the moment when the rupture happens, all the stream above the point in question finds itself in less advanced phases of the transformation (§ 431), and still has, consequently, continuity, while all the stream below this same point is necessarily already discontinuous. Thus, on the one hand, the continuous part of the stream starts at the opening and finishes at the place where comes to occur the highest rupture point of each filament; and, in addition, the distance D starts at the contracted section and finishes at the point corresponding to the midpoint of each filament at the moment of its rupture. The continuous part thus takes its origin a little higher, but finishes a little lower, than the distance D; the difference of the origins of these two sizes and that their terminations must, consequently, be compensated partly; and as these differences are both very small, the excess of the one over the other will be, for stronger reason, so that the two sizes to which they are referred will be able, as I said, to be regarded as equal to each other without significant error.

Due to this equality, the length of the continuous part of the stream that we consider will thus follow appreciably the same law as the distance D, i.e.it will be very close to proportional to $\sqrt{h}$.

Thus, in the imaginary case of a uniform speed of traverse, we find the first of the laws given by Savart. However, it is clear that, in a real stream, speed will deviate less from uniformity as the load is more considerable; from which we can infer that, for sufficiently large loads, the length of the continuous part of a real stream will have to still follow this law appreciably. It is besides what we will show in a rigorous way.

§ 435. Let us thus place ourselves in the real case, i.e. consider a stream subjected to the action of gravity, and in which, consequently, the translatory movement is accelerated. Then, the speed which a horizontal section of the liquid carried by the translatory movement has, after an arbitrary time t,, will have as a value $\sqrt{2gh}+gt$, the first term representing the portion of the speed due to the load, the second the portion due to the action of gravity on the stream, and t being counted as from the moment when the liquid section crosses the contracted section. Recall here that due to the accelerated speed, the stream, if it did not divide, would go on slimming indefinitely from top to bottom (§ 431).

That said, let us imagine that, under the same load and by another opening of the same diameter, runs out, at the same time as the real stream in question, another stream of the same liquid placed under the imaginary condition of the preceding paragraph. That is to say, let θ be the time employed by this second stream to traverse the distance which we indicated by D, i.e. that between the moment when the liquid section which will constitute the mid-circle of a narrowing passes through the contracted section, until the moment of the rupture of the filament into which this narrowing changes. Let us make, in the expression for the speed relating to the first stream, $t = \theta$, which gives, for this speed after time θ, the value $\sqrt{2gh}+g\theta$, in other words, consider the speed of a liquid section belonging to the real stream, after the time necessary for a section belonging to the imaginary stream to traverse distance D. According to what we saw in the preceding paragraph, if the opening remains the same, this time is constant, whatever the load, so that, in the expression above, the term $g\theta$ remains invariable when one varies h whatever the value of θ, to suppose the load h rather considerable so that the term $\sqrt{2gh}$ is very large relative to $g\theta$ at the end, and that this last can, consequently, being neglected without significant error. For a value of θ which will satisfy this condition, and, with stronger reason, for all the larger values still, the speed of a section of the real stream during time θ could be regarded as constant and equal to that of a section of the imaginary stream so that, in all the space traversed by the first during this same time starting from the contracted section, the real stream, if it did not divide, would decrease very little in diameter, and could be regarded as identical with the imaginary stream also supposed without divisions.

Now, it follows necessarily from this quasi-identity, that, during time θ, all will happen in appreciably the same manner in the two streams; consequently, the time θ will be also very close to that which will be used by, in the real stream, the liquid section corresponding to the mid-circle of a narrowing, to achieve the modifications which we considered, and the space that it will traverse during these modifications could be regarded as equal to the distance D relating to the imaginary stream.

But, since the continuous part of the real stream finishes a little less low than this space, and is, consequently, included in the same portion of the stream, it still follows from the identity approached above, that this continuous part will hardly differ in length from that of the imaginary stream, and that, consequently, from the least of the loads considered above, the lengths of the continuous parts of the two streams will have to be controlled by very nearly the same law

We thus arrive finally at this conclusion, that, for the same opening, and starting from a low enough load, the length of the continuous part of the real stream must be appreciably proportional to the square root of the load.

In consequence of the preceding demonstration, the lower load in question is that under which the translatory movement of the liquid starts to remain almost uniform in all the continuous part of the real stream.

Thus, under the condition of a sufficiently low load to produce this approached uniformity, an always possible condition, the law indicated by Savart as establishing the relation between the length of the continuous part and the load, arises in a necessary way from the properties of the liquid cylinders.

To discover if this law must still be true when one employs weaker loads, it is necessary to start from other considerations; but we see as of now that if, in this last case, the law is different, it must at least necessarily converge towards the proportionality in question as the load is increased.

Let us note here that, for a given liquid, the load under which the stream starts to be under the condition that we determined, must be as much less considerable as the diameter of the opening is smaller. Indeed, since, all else being equal, the transformation of a liquid cylinder happens all the more quickly as the diameter of the cylinder is less, it follows that the value of θ will decrease with the diameter of the opening, and that, consequently, the smaller it is, the less the value of h will have to be so that, in the expression $\sqrt{2gh} + g\theta$ posed at the beginning of this paragraph, the term $g\theta$ is negligible beside the term $\sqrt{2gh}$ and, consequently, so that the stream is under the condition in question.

Moreover, as the time θ varies with the nature of the liquid, it will be necessarily the same as we consider the load.

§ 436. Now we turn to the second law, which is that which establishes the approximate proportionality between the length of the continuous part of the stream and the diameter of the opening when the load remains the same.

Let us take again, for one moment, the imaginary case of an absolutely uniform translatory movement. Then the stream will constitute, abstracted of its divisions, an exact cylinder starting from the contracted section, a cylinder which will be formed in the air, and free on all its convex surface; moreover, the translatory movement of the liquid being without influence on the effect of the shaping actions, and no foreign cause tending to modify the length of the divisions, those will take necessarily their normal length. It is thus seen that, except for the non-simultaneity of the formation of its divisions, our imaginary stream will be precisely under the same conditions as the cylinders to which the laws recapitulated in § 384 referred; consequently, if we consider in particular one of narrowings of this stream, it will have to pass through the same forms, and to achieve its modifications in the same time, as any of narrowings which would result from the transformation of a cylinder of the same diameter as the stream, formed of the same liquid, and placed under the conditions in question.

Now, the time between the origin of the transformation and the moment of the rupture of the filaments, is, according to one of our laws, exactly or appreciably proportional to the diameter of the cylinder; and it is clear that this law applies just as easily to one of narrowings in particular, or even simply to its mid-circle, as to the whole of the shape. Consequently, in our imaginary stream, the time which the mid-circle of each narrowing will employ to arrive at the moment of the rupture of the filament, will be exactly or appreciably proportional to the diameter which the stream would have if there were not divisions, i.e. with that of the contracted section. However, the cylindrical shape of the stream supposed without divisions starting only at the contracted section, it is only from there that the shaping actions start coming from the instability of this same cylindrical form. It thus should be admitted that the liquid section which must constitute the mid-circle of a narrowing, does not start to undergo the modifications which result from the transformation until the moment when it crosses the contracted section; thus, the time that we consider starts at this same moment.

But this time between the moment when the liquid section which must constitute the mid-circle of a narrowing passes through the contracted section and the moment of the rupture of the filament into which this narrowing converts, is that which we indicated by θ, and during which the liquid section traverses the distance D; in our imaginary stream, the time θ will be thus proportional to the diameter of the contracted section. On the other hand, the plain translatory movement being supposed, the distance D will be proportional to time θ used to traverse it. Therefore, due to these two laws, the distance D will be proportional to the diameter of the contracted section. Lastly, since the distance D does not differ appreciably from the length of the continuous part of the stream, this length will also be proportional to the diameter of the contracted section.

Now one knows that, in a liquid stream, the diameter of the contracted section can be regarded as proportional to that of the opening when this last exceeds ten millimetres, and that below this limit, the proportionality does not change in a noticeable way, until the diameter of the opening becomes lower than a millimeter[253]. Moreover, as this change is attibuted to influence exerted by the thickness, though very small, of the edges of the opening, it is probable that it will be made still less, by employing, as did Savart, openings widened outside, openings which can be cut so as to have extremely sharp edges. Thus, with suitably worked openings, one will undoubtedly be able, starting from a diameter equal to more than one millimetre, to admit, without notable error, that the diameter of the contracted section is proportional to that of the opening.

Accordingly, since the length of the continuous part our imaginary stream is proportional to the diameter of the contracted section, it will be also proportional to the diameter of the opening, at least starting from a lower value of this last, which is not much below a millimetre.

If now we pass from the imaginary stream to the real stream, we have only to suppose for the constant load a rather considerable value so that, in all the extent where we will assign the variations of the diameter of the opening, the condition posed in the preceding paragraph is satisfied, so that, for each value given to this diameter, the

[253] Indeed, one deduces from the results obtained by Hachette (*Ann. de chim. et de phys. de Paris*, vol. III, p. 78), that, for a diameter of opening equal or higher than 10mm, the ratio between the diameter of the contracted section and that of the opening are, on average, 0.78; that while passing from 10mm to 1mm, the ratio increases only up to 0.83; and finally, that, for a diameter equal to 0.55mm, the ratio becomes 0.88.

continuous part of the real stream has appreciably the same length as that of the corresponding imaginary stream; then the law which governs this length could be regarded as the same one in the two kinds of streams. According to the first of the two remarks which finish the preceding paragraph, one sees that if the common load meets the condition with regard to the greatest of the values that one assigns to the diameter of the opening, it is filled, with more strong reason, with regard to all the other.

We are thus led to this final conclusion: if, for the same load, one gives to the diameter of the opening increasing values, from a value not very much lower than a millimetre up to another arbitrary given value, and if the common load is sufficiently large, the length of the continuous part of the stream will be proportional to the diameter of the opening.

Thus the second of the laws given by Savart again arises, in a necessary way, from the properties of liquid cylinders; and one sees, in the same way, that if, in the case of a not very considerable common load, the law changes, it must converge towards that of Savart as one gives to this load a larger value.

§ 437. Now let us place ourselves inside the limit for which the real stream can be comparable, in its continuous part, with the corresponding imaginary stream; in other words, let us suppose the load rather not very considerable or the diameter of the opening rather large, so that, in the extent of the continuous part of the real stream, the translatory movement is not very appreciably uniform. Then also the stream will tend to be thinned top to bottom, and this thinning will become visible in the limpid portion. The question of the laws which must, in these circumstances, govern the length of the continuous part is rather complicated; however, we will try to clear up it.

Let us consider a division of the stream at the moment when its higher end passes through the contracted section. The two liquid cross-sections between which the division in question is included, start from this position with different speeds: because, in the small distance which the lower section traversed, its speed increased already a little by the action of gravity. However, it follows from this excess speed and acceleration of the movement, that the two sections will be moving away more and more one from the other as they go down, or, in other words, as the portion of liquid ranging between them lengthens gradually during its translatory movement. Consequently, as I already indicated in § 431, each division, carried with the accelerated speed of the liquid, will increase gradually in length until the moment of the rupture of the filament, and will preserve during its descent a constant volume.

In this case, it is easy to recognize two kinds of influences, acting in opposed directions, on the law which governs the length of the continuous part when one varies the load.

Initially, we recall that if the translatory movement were uniform, the proportionality with the square root of the load would be always satisfied, even starting from very weak loads (§ 434). Now, if divisions go down with the accelerated speed of the liquid, and if it is supposed that it does not result in any change in the duration of their transformation, they will traverse for this length of time a more considerable space, so that the continuous part will be longer than if the acceleration did not exist, and the excess, compared with the length which the continuous part would have in the case of uniform movement, will be notable under a weak or moderate load, while it will be negligible under a very strong load, making the movement of translation in the continuous part appreciably uniform. Accordingly, when one passes from the first of these two loads to the second, the ratio of the lengths of the continuous parts which correspond to them respectively will be brought closer to unity than it would be if acceleration were null, i.e. more approach unity than the ratio of the square roots of the loads.

But, as we saw above, divisions cannot go down in an accelerated movement without elongating at the same time, and from there is born, we know (§§ 381 and 407), a cause of reduction in the duration of the transformation. This second influence, which is the reduction in the duration of the transformation, a reduction which must be all the more marked as the speed of traverse approaches less uniformity, or as the load is weaker, acts obviously to make the law faster than the proportionality to the square root of the load, and it is consequently opposed to the first.

§ 438. In short thus, for loads less considerable than those which would make the translatory movement of the liquid appreciably uniform, in the continuous part of the stream, two opposite kinds of influences act on the law whereby the length of this continuous part varies with the load, the first tending to make this length grow less quickly than the square root of the load, and the second, on the contrary, tending to make it grow more quickly. However, due to their opposition, these two kinds of influences will be mutually neutralized in more or less great proportion; but one must regard as far from probable that neutralization is complete; which leads us to this first conclusion, that, under sufficiently weak loads, the law with which we occupy ourselves will most probably differ from that of Savart; only it would be impossible to decide a priori in which direction.

In second place, the influences of which we spoke having their cause in the acceleration of the movement of the liquid, it is clear that the action of each one of them, separately considered, decreases as the load is increased, and becomes negligible starting from the first of the loads under which the movement of the liquid becomes appreciably uniform in the continuous part. However, what remains after the mutual neutralization of the two opposite actions is necessarily less, and probably much less, than each one of them individually, from which it is to be believed that this excess will become negligible starting from a load much less large. We thus arrive at this second conclusion, that the first law of Savart will undoubtedly start to be true starting from a load which will still leave a very notable acceleration to the translatory movement liquid in the continuous part.

Finally, this result combined with a principle that we established at the end of § 435, provides us a third conclusion, which is that the load from which the stream actually starts to satisfy the first law of Savart, will be all the more weak as the opening will be smaller: because it is obvious that while passing from one opening to another, this load must vary in the same direction as that from which the acceleration of the movement of the liquid becomes negligible. But I say, moreover, that the variation in question will most probably arise in a ratio much larger than that of the diameters of the openings. Indeed, let h' be the load under which starts, for a given opening and a given liquid, the approximate uniformity of the translatory movement, and θ' the corresponding value of θ. The load h' will have to be such, as we saw, that $\sqrt{2gh'}$ is very considerable relative to $g\theta'$, or, in other words, that the ratio $\frac{\sqrt{2gh'}}{g\theta'}$ is very large. Now let us take an opening of a less diameter, and indicate by h'' the load which meets, with regard to this second opening, the same condition as h' with regard to the first; that is to say also θ'' becomes θ for the new opening. If we want that, in the continuous part of the stream which runs out of it, the movement of the liquid has the same coefficient of uniformity as in the continuous part the preceding one, we will have obviously to set

$$\frac{\sqrt{2gh'}}{g\theta'} = \frac{\sqrt{2gh''}}{g\theta''},$$

which gives

$$\frac{\sqrt{h'}}{\sqrt{h''}} = \frac{\theta'}{\theta''},$$

and, consequently,

$$\frac{h'}{h''} = \frac{\theta'^2}{\theta''^2}.$$

But the time θ is proportional to the diameter of the contracted section, and, consequently, with that of the opening (§ 436); therefore, for the ratio $\frac{\theta'}{\theta''}$, one can substitute that of the squares of the diameters of the two openings; from which it follows that while passing from a given opening to a lesser opening, the load which we consider will decrease as the square of the diameter of the opening. Now, one must regard as quite probable that the weakest load from which the law of Savart starts to hold will grow in a similar way, i.e. in a ratio much larger than that of the diameters.

§ 439. Let us pass to the other law, i.e. to that which governs the length of the continuous part when one varies the diameter of the opening. I say, initially, that this law will coincide with the second of those of Savart, when one gives to the common load the value for which the stream leaving by the largest of the openings employed would actually start to satisfy the first of these laws.

Indeed, let us first notice that, under the load in question, a load that we will indicate by h_1, the streams leaving by all the lesser openings will be, with stronger reason, under the effective conditions of the first law; it is what follows from the third conclusion of the preceding paragraph. Consequently, if we substitute, for one moment, for this load h_1 a considerable enough load to make the speed of the liquid appreciably uniform in all the continuous parts, and if we take the ratio of this second load to the preceding one, the respective lengths of the continuous parts will decrease all in the same ratio, which is that of the square roots of the two loads. However, under the largest of those, the lengths in question were between them like the diameters of the corresponding openings (§ 436); thus it will be still the same under the load h_1, and, consequently, under this load, the second law of Savart will be satisfied.

Secondly, I say that under a load lower than h_1, it will not be thus any more. To show it, let h_2 be this new load, and indicate by h_3 the load which fills, with regard to the stream leaving by the smallest opening, the same role that h_1 fills, with regard to that which leaves by the largest. We point out that h_3 is lower than h_1, and suppose h_2 lies between these two last. Then, consequently, under the loads h_1 and h_2 the stream leaving by the smallest opening will be still under the effective conditions of the first law of Savart, while, for the stream which leaves by the largest opening, these conditions start only from h_1; if thus we pass from h_1 to h_2, the continuous part of the first stream will decrease in the ratio of the square roots of these two loads; but that of the last stream will decrease in a different ratio. However, under the load h_1, these two lengths were between them as the diameters of the corresponding openings; therefore, under the load h_2 they will be in another ratio, and, consequently, the second law of Savart will not be satisfied any more, at least as for these two extreme streams of the series compared between them.

These new conclusions follow from all that: under a sufficiently weak common load, the proportionality between the length of the continuous part and the diameter of the opening do not take place any more in the total extent one assigns to the variations of this diameter; but it starts to appear when one gives to the common load the value for which the stream leaving by the larger of the openings starts to be under the effective conditions of the first law of Savart.

Now we see that these conclusions, like those of the preceding paragraph, agree with the results of the experiment.

§ 440. Savart made, on water streams removed from any foreign action, two series of observations, one with an opening six millimetres in diameter, and the other with an opening of three millimetres; the successive loads were the same ones in both series. Two tables below reproduce the results obtained, i.e. the lengths of the continuous part corresponding to the successive loads; these lengths as well as the loads are expressed in centimetres. I placed, in each table, a third column containing, compared to each length of the continuous part, the ratio of it to the square root of the corresponding load.

DIAMETER OF THE OPENING, 6mm.		
LOAD	LENGTHS of CONTINUOUS PART.	RATIO to the SQUARE ROOT OF LOADS.
4.5	107	50.4
12	126	36.4
27	143	27.5
47	158	23.0

DIAMETER OF THE OPENING, 3mm.		
LOAD	LENGTHS of CONTINUOUS PART.	RATIO to the SQUARE ROOT OF LOAD.
4.5	24	11.3
12	39	11.3
27	58	11.2
47	78	11.4

Before discussing these tables, notice here that all the lengths of the continuous part are expressed in whole numbers, which shows that Savart took for each one of them the whole number of centimetres more similar, without taking account of the fraction; moreover, as we will see (§ 487), in such streams, the continuous part immediately undergoes small variations in length; it thus follows that the lengths given in these same tables are in general only approximate.

That said, let us start by examining the table relating to the opening of 6mm. One sees that the ratio between the length of the continuous part and the square root of the load decreases considerably from the first load to the last; from which it follows that, in the case of a water stream leaving by an opening of 6mm diameter, if one makes the load rise only up to 47 centimetres, the first law of Savart is far from being satisfied. Thus, the first conclusion of § 438 is in conformity with the experiment. Moreover, the waning of the ratio establishes the direction in which the real law deviates from the law of Savart, inside the limit where it starts to be sufficiently approximate; it is seen that then the length of the continuous part increases less quickly than the square root of the load.

In the second place, according to the progress of the ratio in question, one recognizes that it converges towards a certain limit, which must be a little below 23, i.e. the value corresponding to the load of 47 centimetres. Indeed, while the load receives successive increases of 7.5, 15, and 20 centimetres, the ratio decreases successively by 14, 8.9, and 4.5 units, and this last difference is already rather not very considerable relative to the value of the last ratio; from which one must suppose that if the load were further increased, the later waning of the ratio would be extremely small, and that one would reach soon an appreciably constant limit, a limit beyond which the first law of Savart would be satisfied.

Accordingly, let us seek what is, for the stream which runs out under the load of 47 centimetres, the ratio between the speeds of traverse of the liquid at the end of the continuous part and the contracted section. By neglecting the small interval ranging between the opening and the contracted section, we will have for the speed in question at an arbitrary distance l about this section, the value $\sqrt{2g(h+l)}$; if thus l indicates the length of the continuous part, the ratio of speeds at the end of this length and the contracted section will be expressed generally by $\frac{\sqrt{2g(h+l)}}{\sqrt{2gh}}$, or more simply by $\sqrt{\frac{h+l}{h}}$. Now, in substituting in this expression for h and l the values relating to the stream with which we occupy ourselves, which are 47 and 158, we find for the ratio between extreme speeds the value 2.1. Thus, although, under a load of 47 centimetres, the stream leaving by an opening of 6mm is probably close to being under the effective conditions of the first law of Savart, the speed at the end of its continuous part is even more than double its speed at the contracted section, so that the translatory movement of the liquid is still very notably accelerated. The second conclusion of § 438 thus appears up to now to agree, like the first, with the results of the experiment.

Let us pass to the table relating to the opening of 3mm. Here, as one sees, the ratio between the length of the continuous part and the square root of the load is, very nearly, the same for all the loads; it follows that with this opening, the stream already starts to be under the effective conditions of the first law of Savart under a load of 4.5 centimetres. But, according to what precedes, with the opening of 6mm, the stream enters under these same conditions only under a load of at least 47 centimetres; thus the load from which the first law of Savart starts to be carried out, increases and decreases with the diameter of the opening, and much more quickly than this diameter; however, it is in that that the third conclusion of § 438 consists.

Finally, if, in the general expression of the ratio of extreme speeds found above, we replace h and l by the values 4.5 and 24 relating to the first stream of the table with which we occupy ourselves, we find, for this ratio, the value 2.5; this shows that for the load 4.5, for which the stream is already under the effective conditions of the law of Savart, the speed of traverse of the liquid still is very notably accelerated. Accordingly, there cannot remain any doubt any more about the legitimacy of the second conclusion of § 438.

Let us calculate now, for each of the four loads, the ratio between the lengths of the continuous parts respectively corresponding to the two openings; we will form the following table thus:

LOADS.	RATIOS.
4.5	4.46
12	3.23
27	2.46
47	2.03

This table shows that, for loads lower than 47 centimetres, the starting ratio between the respective lengths of the continuous parts of two water streams, one for an opening 6 millimetres in diameter, and the other for an opening of diameter half that, is far from being the same as that of the diameters; from which it follows that, under these loads, the second law of Savart is not satisfied. But it is seen, at the same time, that this ratio converges towards that of the diameters as the load is increased, and that, under the load of 47 centimetres, it is close to reaching it; however, according to what we saw higher, under this same load of 47 centimetres, the stream leaving by the largest of the two openings is most probably close to reaching the effective conditions of the first law of Savart. The conclusions of the preceding paragraph thus appear to agree, like those of § 438, with the results of observation. We will see, moreover, this agreement confirmed by the results obtained with water streams not removed from foreign actions.

§ 441. These foreign actions, of which we will speak again in § 455, and which consist in certain more or less regular vibratory movements transmitted to the streams, appear not to alter the laws with which we occupy ourselves considered in their general form; but they cause a shortening of the continuous parts, and produce in that the same effect as a reduction in the diameters of the openings, so that, under their influence, the laws of Savart start to be carried out starting from weaker loads.

I have just said that the complete laws which govern the continuous part appear not to be changed by the foreign actions in question; it is what one will recognize easily, if, for each series made by Savart under the influence of these same actions, series in which the openings, the loads and the liquid are the same as previously, one forms the table of the ratios between the length of the continuous part and the square root of the load. Through the small variations coming from the irregularities inherent in the foreign actions, one will see: 1° that at the opening the ratio still starts by decreasing, and converges towards a certain limit; only here the waning is less by the reason that I gave above, and the limit appears to be reached under a load lower than 47 centimetres; 2° that with the opening of 3mm, the ratio is appreciably constant.

Accordingly, the series in question can thus be also used for the discussion of the laws which govern the length of the continuous part. I will restrict myself to reproduce here two of these same series: these are those that Savart took as typical, and from which he deduced his laws; here are the tables he reports:

DIAMETER OF THE OPENING, 6mm.		
LOADS	LENGTH of CONTINUOUS PART.	RATIO of the SQUARE ROOT OF LOAD.
4.5	40	18.9
12	59	17.0
27	82	15.8
47	112	16.3

DIAMETER OF THE OPENING, 3mm.		
LOADS	LENGTH of CONTINUOUS PART.	RATIO to the SQUARE ROOT OF LOAD.
4.5	16	7.5
12	25	7.2
27	41	7.9
47	55	8.0

and one sees, from the first, that for the opening of 6mm the ratio between the length of the continuous part and the square root of the load already appears to have reached its limit under the load of 27 centimetres; the small increase which appears for the following load is undoubtedly due to the causes of irregularity which I announced. Still, let us calculate, for these two series, the ratio between the lengths respectively corresponding to the two openings, which gives us the following table:

LOADS	RATIOS.
4.5	2.50
12	2.36
27	2.00
47	2.04

It is thus under the load of 27 centimetres that the ratio between the lengths of the continuous parts reaches that of the diameters of the openings, which completes the establishment of the conformity of the conclusions of § 439 with the results of observation.

Finally, Savart made, with the opening of 3mm, a series of observations corresponding to four loads more considerable than the preceding ones, and the ratio between the length of the continuous part and the square root of the load was still shown appreciably constant; the first of these new loads was 51 and the last 459 centimetres.

§ 442. As one knows from the work of Savart, the stream makes a low sound, resulting mainly from the periodic impact of the isolated masses of which the discontinuous part is composed against the body on which they fall, and one can make this sound acquire a great intensity, by receiving the discontinuous part on a stretched membrane. By comparing the sounds thus produced by water streams under various loads and with openings of various diameters, Savart found that, for the same opening, the number of vibrations carried out in a given time is proportional to the square root of the load; and that, for the same load, this number is inversely proportional to the diameter of the opening. However, we will see these two laws again derive from our principles.

Let us resort again to the consideration of the imaginary streams. In such a stream, the length of divisions is equal, as we saw, to the normal length of those of a cylinder of the same liquids, formed under the conditions of our laws, and having for diameter that of the contracted section of the stream; thus, this length depends only on the diameter of the opening and nature on the liquid, and does not vary with the rate of flow. However, it follows that, for the same liquid and the same opening, the number of divisions which pass, in a given time, through the contracted section, is proportional to this speed, i.e. to $\sqrt{2gh}$, and consequently to $\sqrt{h}$. But each one of these divisions provides an isolated mass below, and each one of those then comes to impact the membrane; thus the number of the impacts produced in a given time is equal to that of divisions which pass, in this same time, through the contracted section, and, consequently, is proportional to the square root of the load. Now, it is easy to see that each impact gives birth to two vibrations: because the small depression that it causes in the membrane is followed by a small raising, which gives two waves; thus the number of vibrations corresponding to the sound produced is double that of the impacts, and, consequently, is also proportional to the square root of the load.

In the second place, since the normal length of divisions of a cylinder supposed under the conditions of our laws and formed of a given liquid is proportional to the diameter of this cylinder, it follows that, for the same liquid, the length of divisions of the imaginary stream is proportional to the diameter of the contracted section, and, consequently, appreciably proportional to that of the opening. However, for a given rate of flow, the number of divisions which pass, in a given time, through the contracted section, is obviously inversely proportional to the length of these divisions; therefore, if the liquid remains the same, this number is appreciably inversely proportional to the diameter of the opening. But, according to what we saw above, the number of vibrations corresponding to the sound produced is double the preceding; therefore, when the load and nature of the liquid do not change, this number of vibrations is the same, appreciably inversely proportional to the diameter of the opening.

Thus, the two laws which, according to Savart, govern the sounds made by the streams would be necessarily satisfied with regard to our imaginary streams.

Let us pass to the real streams. For a given opening, as the load is increased, the constitution of the stream approaches more and more what it would be if there were acceleration, and consequently the length of its incipient divisions converges towards that which they would take in the same case; from which it follows, due to what precedes, that starting from a less sufficiently strong load, the laws of Savart will be necessarily satisfied.

§ 443. The experiments from which Savart deduced the proportionality, for the same opening, between the number of vibrations and the square root of the load, will enable us to check a result that we simply presented as probable (§ 373), which is that, in the transformation of a liquid cylinder free on all its convex surface and of a length such that divisions can take their normal length, the ratio between this normal length and the diameter of the cylinder would be, in the various liquids, not very distant from 4.

Here now is the table of the results obtained by Savart; the loads are still expressed in centimetres.

DIAMETER OF THE OPENING, 3mm.	
LOAD	NUMBERS of VIBRATIONS PER SECOND
51	600
102	853
153	1024
459	1843

Now let us consider a division immediately after its passage through the contracted section, i.e. at the moment when its higher end crosses this section; it is what we name an incipient division. One will easily admit that for the opening of 3mm employed by Savart, and even under the least of the loads above, loads which are already strong, the stream, abstracted of its divisions, deviated extremely little from the cylindrical form, and which moreover, in the small length corresponding to an incipient division, the acceleration of the translatory movement could be regarded as negligible; consequently, under the least load, and, with more strong reason, under the following loads, incipient divisions were to take very close to the normal length of those of a water cylinder free on all its convex surface.

That said, if λ indicates the length of an incipient division, t the time that its lower mid-circle takes to traverse this length from the contracted section, and h the load, one will have appreciably:

$$\lambda = t\sqrt{2gh}.$$

Moreover, let n be the number of divisions which pass in one second through the contracted section; the time t obviously measuring the duration of the passage of one of them, one will have, by taking the second for the unit of time, $t = 1/n$, and, consequently,

$$\lambda = \frac{1}{n}\sqrt{2gh}.$$

Finally, let k be the diameter of the contracted section corresponding to the same opening; one will have, to represent the ratio between the length of incipient divisions and this diameter, the formula

$$\frac{\lambda}{k} = \frac{1}{kn}\sqrt{2gh}. \tag{d}$$

It is needless to point out that the values of h, k and g will have to be reported in the same unit of length. By taking for this unit the centimetre, the value of h will be given immediately by the table above, and it will be necessary to make $g = 980.9$; as for k, one can conclude from the results recalled in the note of § 436, that when the diameter of the opening is 3mm, that of the contracted section is very close to exactly 0.8; consequently, if we keep the centimetre as the unit of length, which will give 0.3 for the value of the diameter of the opening in question, we will have $k = 0.24$.

Substituting in the formula [a] these values of k and g, as well as those of h drawn from the table and those of n obtained by taking (§ preced.) the respective halves of the numbers of vibrations contained in the same table, we will find, for the ratio λ/k, the four following numbers:

$$\begin{array}{c} 4.39 \\ 4.37 \\ 4.46 \\ 4.29, \end{array}$$

and one sees that indeed, these numbers are very close to each other, and vary little from 4.

The average of these same numbers, which is 4.38, or more simply 4.4, thus gives us, to a good approximation, the ratio between the normal length of divisions and the diameter, in the transformation of a long water cylinder supposed without gravity and free on all its convex surface.

While determining by experiment, for another unspecified liquid, the number of vibrations corresponding to a given opening and a sufficient load also given, one will obtain in the same way, using formula [a], the value of λ/k relating to this liquid; however, Savart says that the nature of the liquid appears to be without influence on the number of vibrations corresponding to a given load and opening, and one can conclude from it that the value of λ/k would be appreciably the same, in general, with regard to the various liquids. Consequently, along respectively formed cylinders of these liquids, supposed without gravity and free on all their convex surface, the ratio between the normal length of divisions and the diameter would be also appreciably the same, and would move away little from 4, as we have claimed.

However, Savart does not indicate which are the liquids that he compared, and one must suppose that if one submitted to experiment a liquid with strong interior viscosity, such as glycerin, or with very strong surface viscosity, such as a saponin solution, one would obtain ratios a little larger.

§ 444. The partial duration of the transformation of a cylinder obviously can, as we already noticed, be counted by considering only one of the narrowings of the shape, or even simply its mid-circle, and, in addition, this duration varying, for the same diameter, with the nature of the liquid, it follows that, in the stream, the time between the moment when the surface section which must constitute the mid-circle of a narrowing passes through the contracted section and the moment of the rupture of the filament at which this narrowing converts, will also vary, all else being equal, with the nature of the liquid. However, it necessarily results that, for the same load and the same opening, the length of the continuous part of the stream will change from one liquid to another; and this conclusion is still in conformity with the results of the experiment. Indeed, Savart measured the continuous part of four streams running out in identical circumstances, and respectively formed of sulphuric ether, alcohol, water, and a caustic ammonia solution, and he found the lengths following:

Ether	90,
Alcohol	85,
Water	70,
Ammonia. . .	46.

§ 445. We occupied ourselves up to now with only streams launched vertically from top to bottom. The streams launched in directions different from the vertical are curved by the action of gravity, and, consequently, cannot be compared any more with cylinders; but they constitute shapes lengthened according to one of their dimensions, and consequently they must (§ 368) be also formed of divisions passing gradually to the state of isolated spheres; thus the constitution of streams launched either horizontally, or obliquely, must be similar to that of the streams launched vertically from top to bottom, a conclusion which agrees, indeed, with the observations of Savart.

One must believe that this analogy of constitution extends to the ascending part of the streams launched vertically upwards; only, in the case of these last streams, the phenomena are probably disturbed by the liquid which falls down.

§ 446. By describing (§ 239) the phenomena which appear when a liquid runs out through a vertical rectilinear slit and narrow opening in the side wall of the tank from the bottom until above the level of the liquid, and by describing the rim which furnishes the higher edge of the film thus generated, I said that, under the best conditions of the experiment, i.e. with a slit 2mm wide, this rim, starting from the middle approximately of its length, changes, on the right and on the left, into sprays of droplets. However, my son, while observing, through a disc bored with radial slits and turning with a suitable speed, the portion of this same rim before the place where the resolution into drops took place, consistently saw a sheaf of streams of small diameter, of which each one presents a continuation of bulges and narrowings, and which have a slight movement of oscillation in the transverse direction. One understands, accordingly, the generation of the drops, which are just the bulges above passing to the state of isolated masses; it is also understood that the oscillatory movement of the streams in question prevents one from clearly distinguishing those with the naked eye. As for the cause of this odd constitution in a sheaf of streams, it escapes me completely.

Let us note, in passing, that the rims recalled in § 427 can be regarded as composed also of streams in which the transformation into isolated masses is carried out gradually during the translatory movement of the liquid.

§ 447. The influence that the vibratory movements communicated to the vessel exert on the stream, influences that we will soon put in connection with our theory, led Savart to look at the constitution of the stream as being itself the result of certain vibratory movements inherent in the phenomenon of the flow. On that basis, Savart tried to render comprehensible how the kind of impact caused in the mass of the liquid by their emission could indeed give rise to vibrations directed normally to the plane of the opening; here is how he expressed himself on this subject:

"One imagines, indeed, that at the moment when the orifice is opened, the column of liquid which is placed immediately above it, being the first to run out, the level of the central part of the liquid must tend to drop a little, and, on the contrary, the part most external of the mass of the fluid being driven back, its level must rise by a small quantity, and thus, in consequence, all the mass must become the seat of oscillations similar to those which take place under only the influence of gravity in a siphon whose branches are straight."

Savart showed that these oscillations, which would produce *pulsations* at the opening, would involve the alternating formation of bulges and narrowings in the stream, because the portion of the latter which would leave during a pulsation directed from inside outwards would undergo a compression which would increase its thickness, while the portion which would leave during a pulsation directed from outside inwards, would undergo, on the contrary, a stretching which would thin it.

We saw, according to the description of our theory, that the constitution of the stream is a necessary consequence of the properties of liquid cylinders, properties which we studied by experiment and calculation; we thus can, I believe, omit a detailed discussion with regard to the clever ideas which we will to point out, ideas for the complete intelligence of which we refer to the same Report of Savart. We will only point out that it is difficult to admit the kind of impact supposed by Savart; that, moreover, one does not see well how the pulsations in question, after having drawn on the surface of the stream an incipient division, would cause the later development of that so as to make it pass gradually, during its descent, to the state of an isolated mass; that finally, if one wanted to disregard these difficulties, it would still be necessary to resort to additional assumptions to arrive at the laws which govern the length of the continuous part and those which give the numbers of vibrations corresponding to the sounds produced by the impact of the turbid part. Moreover, it is by borrowing from Savart one of his ideas, which becomes applicable when, by an external cause, vibrations are actually excited in the liquid, that we will find the elements necessary to explain the curious influence of these vibrations on the stream.

§ 448. Let us occupy ourselve now with this subject; and initially let us recall, in short, which are, according to the research of Savart, the modifications which the stream in the circumstances in question receives, i.e. when it is under the influence of vibratory movements. In the first fourteen numbers which follow, the streams are vertically downward.

1° The continuous part is shortened.

2° The thickness of the limpid portion appears increased.

3° Each mass which is isolated at the lower end of the continuous part is flattened initially in the vertical direction, and, consequently, its horizontal diameter is larger than that of the sphere that it tends to constitute.

4° The masses being thus released in a flattened form and tending to take the spherical form, they exceed then the latter by the effect of inertia, and lengthen in the vertical direction, to be flattened again, then to lengthen still, and so on; so that their horizontal diameter, which initially is higher than that of the sphere of the same volume, becomes then less than this last, then again larger, etc.

These periodic variations of the horizontal diameter of the masses taking place while those are carried by the translatory movement, the impression left in the eye by the fast passage of an arbitrary one of these masses must be that of a shape offering a regularly laid out series of maximum and minimum thickness, the first corresponding to the places through which the mass passed in its moments of greater horizontal development, and second with places passed in its moments of greater horizontal contraction; and as the successive masses pass either exactly, or about, through the same places in the same phases of their oscillations of form, the impressions which they produce individually are superimposed more or less completely, and the turbid part of the stream presents in a permanent way differences in thickness in question; in other words, this disturbed part is made up of a regular continuation of lengthened bulges and nodes occupying fixed positions.

When the superposition above is imperfect, each swelling offers the appearance of an assembly of layers, of which each one constitutes a kind of cone having for axis that of the stream. About half of the first swelling is formed by the passage of the bulges at the bottom of the continuous part, so that this continuous part finishes about the middle length of this same swelling.

5º The length and the diameter of the swellings are more considerable as the load is stronger and as the diameter of the opening is larger. It is the same for the diameter of the nodes.

6º The whole of these phenomena appear already when the stream is released in ordinary circumstances, i.e. when one intentionally does not excite vibratory movements in the liquid. They come, on the one hand, from the impact of the discontinuous part against the liquid into which it falls, giving birth to vibrations which are transmitted to the vessel via the air and the supports, and, on the other hand, the vessel also receives, by the supports, small vibrations due to external noises and propagated in the ground. It is only by removing, by certain processes, the vessel from these two influences, that the stream takes the aspect which is intrinsic to it.

7º But all the phenomena indicated in the first five preceding numbers become much more marked and more regular when, using an instrument, one produces, in the vicinity of the apparatus, a sound in unison with that which would result from the impact of the discontinuous part of the stream against a taut membrane. Then the continuous part is shortened considerably; the diameter of the limpid portion is still shown increased; the swellings widen while collecting more on themselves, so that the nodes which separate them are lengthened; finally these nodes appear of a less diameter.

8º In addition to unison above, other sounds, products of the same instrument in the vicinity of the apparatus, act on the stream in a similar way, but with much less energy.

Finally, there are sounds which do not exert any influence.

9º In the particular case where the sound of the instrument moves away very little from unison, the continuous part of the stream lengthens and is shortened alternately, and the ear perceives beats which coincide with these variations in length.

10º When one receives the discontinuous part of the stream on a body which can return only a given sound, it frequently happens that the vibrations of this body modify the sound specific to the stream; but that does not appear possible if the difference between this last sound and that which is appropriate for the impacted body exceeds a minor third.

When the sound of the stream is thus modified by an outside sound, it is often enough, to make it return to the tone which belongs to it, to give a slight impact to the apparatus or of a change of position to the impacted body, and it is always by abrupt jumps that this return takes place.

When the difference between the two Sounds is very small, they can be heard periodically, or even simultaneously.

11º The modifications which the stream under the influence of the vibratory movements undergoes increase further, and acquire a perfect regularity, when the sound instrument (nº 7), instead of being held at a certain distance from the apparatus, is put in contact with the walls of the vessel, and it makes a sound very intense and exactly in unison with that which is specific to the stream. Then the continuous part is shortened so much that the higher end of the first swelling almost touches the orifice, and, in addition, the superposition of the swellings formed by the individual masses (nº 4) is exact, so that one sees no appearance any more of layers.

12º This extreme regularity makes it possible to clearly distinguish the apparent shape which the passage of the spherules interposed between the masses produces, a shape which occupies the axis of the stream after the end of the continuous part; one notices there swellings and nodes, but shorter than those which are due to the passage of the masses.

13° By means of an instrument thus put in contact with the walls of the vessel, almost all the sounds can cause effects similar to those of the unison with the tone specific to the stream; but these effects are much less marked as the sound of the instrument moves away more from the unison in question.

14° Moreover, under this same condition, when the sound which is specific to the stream is not in unison with that of the instrument, it can be brought there, even when the difference between the numbers of vibrations would be large enough to constitute an interval of a fifth above the sound specific to the stream, and more than one octave below.

15° If the stream, instead of running out from top to bottom vertically, is launched horizontally, and it is in ordinary circumstances, or, in other words, that it is not under the influence of a sound instrument, but that it will strike the liquid of the vessel which receives it, its turbid part shows the swellings and the nodes, as shown, in the same circumstances, by the downward vertical streams (n° 6), and the vibrations of an instrument modify it also in the same manner.

If the stream is launched obliquely upwards, the same phenomena are still observed, as long as the angle that it forms with the horizontal does not exceed $20°$ to $25°$.

16° But beyond this term, and until $45°$ to $50°$, the discontinuous part takes other aspects: when the stream is not under the influence of the sound of an instrument, this discontinuous part is shown scattered in the same vertical plane in a kind of sheaf. Under the action of a fixed period vibration, it can happen that the sheaf is dissolved in two quite distinct jets, having each one their regularly formed swellings and their nodes; it can even be made so that, for another fixed sound, the sheaf is replaced by three jets; finally, there is always a sound which reduces the whole stream to only one jet presenting a system of perfectly regular swellings and nodes, and this sound is also that which produces the greatest shortening of the continuous part.

17° For the same load and the same opening, the number of vibrations corresponding to the sound which exerts the maximum effect over the length of the continuous part and dimensions of the swellings of the stream, is much less when the direction whereby the latter is launched forms a larger angle with the downward vertical from the orifice. The difference between the numbers of vibrations which are appropriate if the jet falls vertically and with that where it is launched horizontally, is not very considerable; but it becomes very large between this last case and that where the jet is vertically ascending.

§ 449.Now let us seek the reason for these odd phenomena. All that we say, from here to § 468, will refer to streams launched according to the downward vertical; such streams will thus, up to that point, be always represented.

One can claim, we know (§§ 405 and 423), that at the beginning, at least, of the transformation of a liquid cylinder, the length of a narrowing is equal to that of a bulge; however, this result is obviously applicable to narrowings and bulges incipient in the stream, and it follows that the respective durations of the passages of one of these narrowings and of one of these bulges through the contracted section are equal; on the other hand, a division of a cylinder or a stream lying between the middles of two neighbor narrowings, and being thus composed of a bulge and two half narrowings, the duration of the passage of a division of the stream through the contracted section is necessarily equivalent to the sum of those of the passages of a bulge and a narrowing; and since these two last are equal, we arrive at this first consequence, that the duration of the passage either of a narrowing, or of a bulge, through the contracted section, is equal to half of that of the passage of a division.

But the number of vibrations per second corresponding to the sound made by the impact of the discontinuous part of the stream against a taut membrane is, as we know (§ 442), double that of the isolated masses which come, in the same interval of time, to run up against this membrane, and this last number is (ibid.) always equal to that of the divisions which pass, in same time also, through the contracted section; thus the duration of each vibration in question is, like the duration of the passage of a narrowing or of a bulge, equal to half of that of the passage of a division, and we will deduce from it finally this fundamental conclusion:

The duration of each vibration corresponding to the sound specific to the stream is equal to that of the passage of a narrowing or a bulge through the contracted section.

§ 450. Now let us suppose that using the means indicated by Savart, one removed the stream from the influence of the vibrations coming from the fall of the liquid in the vessel which receives it, and from that of external noises; then, the stream being thus left to only the action of the shaping forces, one transmits to the vessel from which it escapes a sound exactly in unison with that which the impact of the discontinuous part against a membrane would give. The liquid which flows from the interior of the vessel towards the opening crosses it while receiving its vibrations; if thus these last are directed in the vertical direction, each portion of the stream which will pass through the contracted section while carrying out a downward vibration, increases its speed $\sqrt{2gh}$ by that of this vibration, and consequently it will contain more liquid than the portion which would have passed in the same time in the absence of the vibrations. The excess speed will tend, in truth, to communicate itself to the part of the stream located below that which we consider; but, while disregarding for one moment the shaping forces, we will have to admit at least that this lower part will oppose a certain resistance due to its inertia, and that, consequently, the excess of liquid brought by the excess speed will tend to be distributed in the horizontal direction, or, in other words, to bulge the portion to which it belongs.

That said, if the nearly cylindrical shape that the stream would take by only the effects of the translatory movement of the liquid and the circular shape of the opening was a shape of steady equilibrium, the portion which, by the action of the downward vibration, is bulged while it passes the contracted section, would exert at the same time an effort to return to its original form; from which it necessarily follows that, on the assumption in question, as the bulge forms, it would be propagated to the sections below, and on the surface of the stream it would constitute a bulging wave of a certain length, which would go with a speed which would be the sum of that of its propagation and that of the liquid. Then also the portion of the stream which passes then through the contracted section while carrying out an ascending vibration, and which, consequently, crosses this section with the speed $\sqrt{2gh}$ decreased by that of the vibration, would produce, by the contrary reasons, a narrowed wave of the same length as the bulging wave, and which would go behind it with the same speed; then would come a new bulging wave followed by a new narrowed wave, and so on, as long as the communication of the vibratory movements persists.

But, due to the instability of the cylindrical shape and the tendency of the stream to transform into isolated spheres, events will happen in a very different manner. Let us imagine that the lower end of one of the bulges which would be formed by the action alone of the shaping forces due to instability, crosses the contracted section at the precise time when a downward vibration starts in the liquid. Then, since the shaping forces push in a continuous way into this portion of the stream an excess of liquid which inflates it without it having any tendency to return on itself, one sees that the quantity of liquid brought at the same time by the additional speed due to the downward vibration will be able to be distributed in the horizontal direction, and to contribute to the formation of the bulge, without having to overcome a contrary tendency. Moreover, since the duration of the vibration is equal to the time which the portion of the stream whose shaping forces would make an incipient bulge alone spends in the contracted section, the higher end of this portion will cross the contracted section at the precise time when the vibration will finish, so that its immediate action will have been exerted on all the portion in question, and only on this portion. Lastly, since the bulge produced by the combined actions of which we speak does not have any tendency to be erased, it will not be propagated with the underlying parts, and, consequently, it will not give rise to a wave. Thus the portion of the stream considered will be more inflated, as of its formation, than it would have been it in the absence of the vibratory movements; but it will have the same length and will go down with same speed as in this last case.

After the downward vibration will come an ascending vibration, and it will decrease the speed of the passage through the contracted section. It will result, as we already said, in the portion from the stream which passes under its influence, a reduction in volume, so that this portion will tend to be thinned; but the shaping forces tending to make this same portion an incipient narrowing, thinning due to the vibration will be also carried out without meeting an opposed tendency, and, consequently, without giving rise to the formation of a wave. It is thus seen that, just as the bulge which precedes it, the narrowing thus formed by the double action of the shaping forces and the vibration will be more pronounced, but will have the same length, and will go down with the same speed, as if the stream were left to only the action of the shaping forces.

Finally, the same thing will take place with regard to all the other bulges and narrowings: due to the equality between the time which each one of these portions of the stream uses to spend in the contracted section and the duration of each vibration, all the bulges will coincide with the downward vibrations, and all narrowings with the ascending vibrations; the ones and the others will preserve consequently their length and their speed of traverse, but all will leave the contracted section more marked, or, in other words, in a more advanced phase of the transformation, than if the vibratory movements had not been produced.

§ 451. But the action of these movements will not be limited there: indeed, speeds of vibrations downward and ascending, speeds which, as we showed, change directions in the bulges and narrowings to produce a greater transverse development of the first and a greater thinning of the second, cannot vanish. In each one of these portions, at the moment when it completes its passage through the contracted section, these speeds thus changed into transverse speeds will thus continue, like acquired speeds, to be added to those which result from the shaping forces.

§ 452. So that transmitted vibrations exert with all their intensity on the incipient divisions of the stream the action described in the two preceding paragraphs, it is necessary that at the orifice they, as we imagined, are directed in the vertical direction. It would undoubtedly be difficult to show a priori that while being propagated to the orifice the vibrations take there really this direction; but Savart, who dealt so much with the communication of the vibratory movements, claims the fact implicitly: indeed, on the one hand, he supposes that these vibrations do nothing but reinforce those which are born, according to him, from the flow and which would be necessarily vertical, and, on the other hand, he says, to obtain the maximum of action, it is only necessary to give to the sound instrument a particular position. Moreover, if some difficulty there were found, it would be enough to notice that, whatever the real direction whereby the liquid molecules carry out, while crossing the opening, the vibrations which are transmitted to them, one will always be able, except in the very exceptional case where this direction would be exactly horizontal, to break up each vibration into two others, of which one is horizontal and will not influence the transformation of divisions of the stream, and the other vertical, which will exert all its action. Moreover, as we will see (§ 487), Magnus showed that the effect exerted on the stream by the transmitted vibrations depends especially on those which the bottom of the vessel carries out, and these last are naturally vertical.

We supposed, moreover, that the moment when each downward vibration starts is also that when the lower end of each bulge passes the contracted section; but if, in the first moments when the vibrations are felt, this coincidence does not take place, there will be a conflict between the actions of the shaping forces and those of the vibrations, and one understands that consequently the transformation of the stream, which, being only a phenomenon of instability, can be moved by slight causes, and will move back or advance the bulges and narrowings, so as to establish the above coincidence and to thus allow the concordance and the full freedom of the two systems of actions.

§ 453. These principles established, we will see some modifications which the stream undergoes by the influence of the vibrations.

Let us say initially that when the stream is left to only the action of the shaping forces, the speed with which the transformation is carried out remains extremely small until a rather considerable distance from the contracted section, which gives to the corresponding portion of the stream a calm and limpid aspect; in the second place, further down, the bulges taking a notable development and more rapid, the stream appears to widen, up to the point where the masses are isolated; and finally, beyond this point, the diameter of the stream, a diameter which is that of these same masses, is appreciably uniform (§ 432).

Let us take such a stream, and produce, near the apparatus, the sound considered in all that precedes. Under the influence of this sound, each division leaves the contracted section in a more advanced phase of the transformation, and, moreover, the transformation happening on the basis of this phase with a speed higher than it had done under only the action of the shaping forces, it necessarily follows that this same transformation will occur in less time; consequently each division will reach the state of an isolated mass at a less distance from the opening, and thus the continuous part will be shortened.

And since the bulges are more developed as of their origin, one sees, in the second place, that the apparent thickness of the limpid portion of the stream, a thickness which, at each point along this limpid portion, is obviously that that acquired by the bulges at the time when they pass there, will show itself increased.

In third place, the transverse excess speed that the transformation receives from vibrations and which persists as acquired speed, must necessarily make the horizontal diameter of the successive masses exceed that of the spheres which these masses tend to constitute, so that these same masses will be flattened in the vertical direction. But one understands that this horizontal extension and this vertical flatness make the capillary pressure, at the circumference of the mass, higher than that of the points close to the axis, and that from there is born an increasing resistance which ends up counteracting the transverse speed. Then the differences in pressure will act freely and the mass will return on itself to reach its shape of equilibrium, i.e. the spherical shape; but the phenomenon being carried out with an accelerated speed, will not be able to stop with this last shape, and the mass will contract in the horizontal direction, while lengthening in the vertical direction, until the increasing resistance which results from the new inequalities between the pressures counteract the acquired speed; then the mass, pulled by the differences in pressure which produced this resistance, will again return towards the spherical shape, which it will exceed again to extend a second time in the horizontal direction and to be flattened in the vertical direction, after which it will start again the same series of modifications and will continue these oscillations of form as long as its fall will last.

Thus is explained very simply, in the case of unison with the sound which would be given birth by the impact from the discontinuous part, the facts recalled in n^{os} 1, 2, 3 and 4 of § 448.

Only, since the end of the continuous part of the stream is about the middle of the first swelling, and consequently is not very distant from the point corresponding to the first of maximum thickness of the turbid part, it should be admitted that each mass reached its first phase of greater horizontal development a little before being detached completely, and at the time undoubtedly where it is connected to that which follows it by only a filament. As for the systems of films whose swellings appear when the phenomena are not completely regular, it is obviously, as Savart recognized, the result of the inaccurate superposition of several of the swellings individually produced by the successive masses: these swellings are seen then simultaneously and appear on top of each other, by the effect of the persistence of their impressions on the retina.

§ 454. It is clear that the time between two phases of stronger horizontal contraction, or, in other words, that each mass uses to carry out a complete oscillation of form, is independent of the speed of traverse; consequently the distance a mass traverses during the time in question is all the more large as the speed of traverse is more considerable; but this distance is obviously the distance which separates the middles of two nodes, or the length of a swelling[254]; this length must thus increase with the load.

Suppose, for one moment, that the stream does not divide; then, under weak loads, it will thin very much, and, even in the small length which would correspond to an incipient division, its diameter will decrease already starting from the contracted section, by a considerable quantity; moreover, the length in question finds itself decreased, because, one understands, it must be intermediate between the normal length of division of two cylinders having respectively for diameters that of the contracted section and that of the lower section of the portion that we consider. In the presence of the shaping forces, i.e. in the real case, the volume of incipient divisions is thus, by the double reason above, less under weak loads than under strong loads; but each incipient division providing one isolated mass below, the volume of these masses must grow with the load.

Now, the more volume these masses have, the larger their horizontal diameter must be at their successive maxima and minima; but these maximum and minimum diameters are respectively the diameters of the swellings and the nodes; thus the diameters of the swellings and those of the nodes must also increase with the load. Only, the increase in the isolated masses tends towards a not very wide limit: because the greatest volume that they can acquire is obviously that which they would take if the translatory movement of the liquid were uniform, i.e. that of the spheres which form in an infinite cylinder of the same liquid, and having a diameter equal to that of the contracted section.

Moreover, the air which the stream crosses undoubtedly has also some influence: it can be only imperfectly involved in the movement of the liquid, and thus opposes a certain resistance which necessarily grows with the speed of traverse, and, consequently, with the load; however, this resistance must increase the maximum flatness of the isolated masses, and block their subsequent lengthening more or less; it must thus contribute to growing the diameters of the swellings and the nodes when the load is increased.

[254]Thus Savart appears to consider the swellings all the times that he deals with their length, and we conformed to his expressions; but, actually, it is visible that the space in question is composed of a swelling and two half-nodes.

Now, if the load does not vary, but one employs a larger opening, the volume of divisions of the stream, and, consequently, that of the isolated masses, will also be more considerable; however, the larger these masses are, the slower their oscillations of form must be, and consequently the more space they must traverse in their descent, during one of these oscillations; thus the length of the swellings must grow with the diameter of the opening. As for the respective diameters of the swellings and nodes, it is obvious, according to what we pointed out above, they will grow at the same time.

One thus sees, by the contents of this paragraph, that the facts of n^{o} 5 of § 448 are necessary consequences of the theory, always in the case of the same vibration period as that of the sound specific to the stream. Let us pass to the facts of n^{os} 6 and 7.

§ 455. When the stream is not under the influence of a sound instrument, but it is received in a vessel simply set on the ground, the principal cause of the vibratory movements transmitted by the air and the supports to the vessel from which the stream escapes is the impact of the isolated masses against the liquid into which they fall; it is thus understood that, in these movements, the same vibration period must dominate as those which would result from the impact of the masses in question against a taut membrane, and consequently the action exerted on the stream is explained by what we described in the paragraphs which precede. Only the vibrations thus produced not having a great intensity, the modifications of the stream will not be able to acquire all the development of which they are likely; moreover, these same vibrations being not very regular and being accompanied by more irregular small vibrations still which come from the external noises, the phenomena must feel these irregularities, and it is indeed in these circumstances that Savart describes appearance of layers in the interior of the swellings.

Savart roughly measured, in these same circumstances, for water streams launched from two different openings and under different loads, the lengths and the diameters of the swellings as well as the diameters of the nodes. We do not believe it useless to reproduce here the results of these measurements; they are expressed by taking the centimetre for unit:

Opening 6 millimetres in diameter.

LOADS.	LENGTHS of the CONTINUOUS PART.	LENGTHS of the SWELLINGS	DIAMETERS of the SWELLINGS.	DIAMETERS of the NODES.
4.5	40	25	0.9	0.70
12	59	30	1.0	0.75
27	82	39	1.1	0.80
47	112	60	1.2	0.90

Opening 3 millimetres in diameter.

LOADS.	LENGTHS of the CONTINUOUS PART.	LENGTHS of the SWELLINGS.	DIAMETERS of the SWELLINGS.	DIAMETERS of the NODES.
4.5	16	7.8	0.50	0.28
12	25	9	0.52	0.32
27	41	13	0.55	0.36
47	55	16	0.60	0.40

We will point out here that the length of a swelling being the space traversed by a mass for the duration of an oscillation of form, and this length of time being constant in the same stream, the swellings belonging to it must increase in length starting from the first, because of the acceleration of the descent. It is thus singular that Savart, who, at another place in his Report, speaks about this increase in connection with a particular experiment, has given, in the tables above, the lengths in question as absolute; one must suppose that they refer to the first swelling of each stream. In truth, the particular experiment in which Savart observed the increased length of the swellings must have made the effect very apparent, because the first swelling was born very close to the opening.

§ 456. If, the stream falling in the same way freely into the liquid of the vessel which receives it, one makes resound near the apparatus an instrument which sounds in unison, as we supposed up to now, then, under the action of these more intense and perfectly regular vibrations, the modifications of the stream will be necessarily more marked, i.e. the limpid portion will appear a little thicker, the continuous part will undergo a new shortening, the swellings will widen and the nodes will be thinned. Moreover, the swellings formed individually by each mass will be superimposed in a more exact way, and thus will exceed less the ones the others towards their ends, so that the swellings which result from their unit will be collected on themselves, and the nodes which separate the latter will seem to have lengthened. However, such is actually, as one sees it by n° 7 of § 448, the state of the stream under the influence in question.

The phenomena would be much more regular still if the stream were originally removed from all foreign influence; and, indeed, Savart speaks about the great regularity of the swellings which appear when such a stream is received on a taut membrane, which is used then as a sound instrument giving unison.

§ 457. When the instrument which one makes resound in the vicinity of the apparatus makes a sound other than in unison with that which is specific to the stream, vibrations no longer following one another at the same intervals as the passages of the bulges and narrowings due to the shaping forces, there cannot any more be ceaseless concordance between the two kinds of action; however, since unison produces effects so marked, it is understood a priori that different sounds must cause similar effects, but less intense, and as much less as they deviate more from unison.

To try to disentangle, up to a certain point, what occurs then in the stream, let us suppose it left to only its shaping forces, and take initially the case most accessible to reasoning, which is that where the excited sound in the vicinity is very close to unison. Let us grasp by thought the moment when the middle of a narrowing due to the shaping forces crosses the contracted section precisely in the middle of the duration of an ascending vibration; then this vibration will contribute obviously with the shaping forces to deepening the narrowing. Only, if the sound of the instrument is sharper than that of the stream, and thus the vibration has less duration than the passage of a narrowing, a very small part of the bottom of it will be in a conflict with the end of the downward vibration which preceded, and an equivalent part the top will be also in conflict with the beginning of the downward vibration which will follow, since these downward vibrations tend to inflate the portions of the stream on which they act. If the sound of the instrument is, on the contrary, deeper than that of the stream, it is clear that the contest will take place for the totality of a narrowing, but that the beginning of the vibration will have been in a conflict with the higher part of the preceding bulge, and that the end of this same vibration will be in conflict with the lower part of the following bulge.

It is easy to see that after a certain number of vibrations, an identical effect will recur, i.e. the middle of an ascending vibration will coincide again with the middle of the passage of a narrowing, then it will again return after a number of vibrations equal to the preceding, and so on periodically with equal intervals. If, for example, the duration of a vibration is 99/100 that of the passage of a narrowing or a bulge, the total duration of 100 double vibrations, i.e. made up each one of an ascending vibration and a downward vibration, will be equivalent to the total duration of the passage of 99 narrowings and 99 bulges; however, it is easy to assure oneself that if one starts to count this duration at the moment of one of coincidence, it finishes also at a moment of a similar coincidence; in our example, coincidences will thus recur successively after intervals equal to the duration of one hundred double vibrations. Now let us try to follow the contests and the conflicts during each one of these intervals, or, in other words, between a coincidence and the following one.

For that let us examine what takes place at the time when the first half finishes one of these same intervals. In the example that we took, we will be then obviously still in the middle of an ascending vibration; but, if we reflect that the interval starts in the passing of the origin of a division, and includes exactly the passage of 99 whole divisions, we will recognize that the end of its first half is the moment of the passage of the middle of a division, and, consequently, the middle of a bulge; there will be thus, for this whole vibration, opposition to the shaping forces: it will be the maximum of the conflict, and it is seen that it will have been up to that point increasing, i.e. occupying increasingly large portions of the successive vibrations, to decrease then by the same degrees.

These principles stated, let us see what one can deduce:

Each narrowing for which there will be coincidence will leave the contracted section in a more advanced phase of the transformation, and thus will break at a less distance from the opening, than if one did not produce vibratory movements; but the following narrowing, which is not already any more under so favorable conditions, will be able to only break a little beyond, and the subsequent ruptures will be carried out in the same way farther and farther from the opening, until that of the narrowing for which the conflict between the two actions is at its maximum; after which events will go in the opposite direction, i.e. the successive places of rupture will go up, until a narrowing again returns to coincidence, then all will start again in the same order.

In such a stream, the continuous part must thus be being shortened and lengthening periodically; but, because of the almost equality between the respective durations of a vibration and passage of a narrowing or a bulge, it will be obviously only after a notable time that the maximum of conflict will arise, so that the gradual lengthening of the continuous part will be carried out with enough slowness so that one can follow it by eye; finally, there will be necessarily the same subsequent shortening, and so on. However, these conclusions are clearly verified by the fact of n° 9 of § 448.

As for the beats, they result from the mutual reaction of the sound of the instrument and that of the stream; because, although Savart does not say it in proper terms, one can conclude from the way in which he describes the fact in question, that the stream on which he operated fell on a taut membrane; in what precedes, admittedly we supposed the stream only subjected to its shaping forces and the vibrations due to the sound of the instrument; but the reasoning remains obviously the same if the effect of the shaping forces is activated by a unison resulting from the impact against a membrane.

Although the general aspect of the phenomenon agrees with the principles which I have just described, I think nevertheless that events do not take place, actually, in such a completely simple way; in incipient divisions where the conflict is notable, the mutual reaction of the two opposite causes undoubtedly produces more complicated effects.

§ 458. But it is especially when the sound of the instrument deviates more from unison, that these effects of reaction must decide, because then, in any incipient divisions, there is almost no contest; the conflict is notable everywhere; and as divisions of the cylinders, and, consequently, the natural divisions of the stream, are likely to vary in length from rather slight causes, one must admit that the vibratory movements will modify this length, and that thus their influence will prevail more or less over that of the shaping forces; the sounds in question must thus make the stream undergo changes similar to those which unison causes, i.e. the continuous part must shorten itself, that the turbid part must present swellings and nodes, etc. Only it is clear that the prevalence of the vibratory action will vary with the ratio between the sound of the instrument and that which is specific to the stream, and in general it will be much less as the first will move away more from the second; it is clear also that the prevalence will be accentuated more especially as the sound of the instrument will be more intense, so that sounds which, excited remotely, would seem without action on the stream, will be able to become effective if the sound instrument is put in contact with the walls of the vessel. However, all these deductions agree with the facts of n^{o} 8 and 13 of § 448; and, which confirms them more, it is the fact of n^{o} 14, which shows that, under favorable conditions, the action of the shaping forces can be brought to coincide exactly with that of the vibrations, even for a considerable difference between the sound of the instrument and that which is specific to the stream.

In the experiments of Savart on the influence of the sounds which deviate from unison, the stream was not left beforehand to only its shaping forces; it fell freely into water in the lower vessel, so that the effect of the shaping forces was more or less activated; but one understands that this circumstance could not cancel the reactions which we considered, and was simply to make them less easy.

Lastly, according to an observation of Magnus § 487, when the load is very weak, the stream is influenced by all the sounds produced in its vicinity, except only for the very acute sounds. It is because for very weak loads, its specific sound is relatively low, as are all those which do not deviate too much above, and which, consequently, the vibrations corresponding to these sounds have more amplitude, and consequently conflict with more advantage against the shaping forces.

§ 459. It would be, I believe, useless, considering the complication of the phenomena, to seek to study them with more precision; moreover, the experimental checks that one could draw from the Report of Savart would not be sufficiently clear; here are the texts of the only passages of this Memoir which refer to the facts in question:

"Of the sounds to the octave and the fifth low registers, the minor third, the superfluous fourth and the sharp octave of that which the impact of the turbid part against a reinforcing body gives, produces on the stream modifications similar to those which we come to describe[255], but, however, with much less energy; and there are sounds which do not act in any manner on its dimensions and the aspect that it presents."

And further, while speaking about a stream received at a very small distance from the opening on a thick solid body:

"One notices (just as when the stream is whole) that octaves grave and sharp as well as the fifth and the sharp minor third of the sound in question[256], also influence, but a less degree, the state of the stream."

Lastly, in connection with the modifications undergone, under the influence of unison due to the impact against a taut membrane, by a stream withdrawn from any other outside influence:

"One obtains similar results, when with a string instrument one produces various sounds in the vicinity of the tank, but always one of these sounds exerts on the stream an influence larger than all the others."

[255] I.e. to those that unison produces.
[256] Of unison

Do these passages mean that, if one deviates notably from unison, there are only the octave and the fifth low registers, the minor third, the superfluous fourth and the sharp octave which modify the state of the stream? That is very probable, because then, instead of saying: "and it is sounds which do not act in any manner etc", Savart would have said: *and all the sounds other than the preceding are without influence* etc. Does one have to interpret these same passages by admitting that the sounds which are announced there are most active after unison, and that, among the tones in the rest of the range, they have simply less effectiveness, while the others do not exert any action absolutely? But, in this case, can one believe that Savart had been expressed thus? We will point out, moreover, that the superfluous fourth, indicated in the first passage, is omitted in the second.

These so vague statements show that Savart has little studied the influence of the sounds more or less far away from unison, at least in the circumstances in question.

§ 460. To finish what remotely relates to the influence of a sound excited at a distance and different from unison, we have still to explain the facts of n^{o} 10 of § 448.

When the stream falls on a body which can give only one sound, such as a tuning fork, if one supposes, for one moment, that it does not undergo any modification in the number of the isolated masses, the vibrations due to the impact of these masses will be in general of another period than those of the impacted body, and consequently they will be able to come only from that each time that a mass reaches this body the air is expelled of them, then returns, to be expelled again on arrival of the following mass and so on; however, the sound waves produced in this manner are necessarily very weak relative to those given birth by the vibrations of the impacted body itself; moreover, while varying either the load, or the diameter of the opening, one is able to decrease as much as one wants the interval between the two sounds.

The vibrations of the instrument (or, in the current case, of the impacted body), transmitted by the air to the vessel and the liquid, not having the same duration as the passages of narrowings and the incipient bulges due to the shaping forces, there is, as we described, a variable conflict between the two kinds of action; but, if the two sounds do not move away too far one from the other, the transformation of the stream, a phenomenon likely to be influenced by foreign causes, can, under the action of the vibrations, lengthen or shorten the incipient narrowings and bulges, so that the duration of the passage of each one of them is precisely equal to that of a vibration and that the two kinds of actions agree constantly; this point reached, the sound of the stream will necessarily be in unison with that of the instrument. Only, so that the vibrations of the instrument are able to bring about this result, it is obviously necessary that they have a sufficient energy compared to the vibrations of the sound specific to the stream, since these last tend to support the normal action of the shaping forces.

But this state of the stream is a forced state, since the natural mode of the transformation is altered. Accordingly, if some cause abruptly disturbs the succession or the regular transmission of the vibrations, the shaping forces will have to become again at once dominating, and incipient narrowings and bulges will take again the length which is appropriate for the free action of these forces. One thus easily explains this characteristic of the experiment of n^{o} 10 of § 448, that a small impact given to the apparatus or a change of position of the impacted body is often enough to suddenly bring back the sound of the stream to the tone which is specific for it.

We supposed that, in this same experiment, the sound of the stream is put in unison with that of the impacted body; however, as one can conclude from the statement of the number in question, Savart does not express himself in this respect in precise terms: he says simply that the sound of the impacted body modifies that of the stream, that it changes the period of it; but other experiments which we will discuss soon allow us to attribute to these words the direction that we gave them.

§ 461. Finally, n^{o} 10 of § 448 again teaches us that when the variation of the two sounds is extremely small, these two sounds can be heard periodically or even simultaneously. Let us try to also explain these facts.

Suppose, to fix ideas, that the sound specific to the stream is somewhat deeper than that of the impacted body. In the case of exact unison, the number of the impulses of the masses in a given time would be half of the number of vibrations of the body in same time, and consequently the interval between two successive impulses would be equal to the duration of two of these vibrations; therefore, in the assumption above, the interval between two impulses will exceed a little the duration of two vibrations, and if the reaction of these vibrations on the incipient narrowings and bulges is not powerful enough to modify the length and to thus bring unison with it, the small excess duration of the intervals in question will be maintained.

That being so, let us start with the first impulse. This will make the body carry out a vibration directed from top to bottom, which will be followed by a vibration upwards; then, a little after the beginning of a new downward vibration, the second impulse will arrive; the third will act during the third downward vibration, but in a phase a little more advanced than this vibration; the fourth impulse will take place during the fourth downward vibration, and in a phase still a little more advanced; and so on, until an impulse coincides appreciably with the end of a similar vibration. Under these repeated impulses, the amplitude of the vibrations of the body will necessarily be growing, until the impulse which we considered lastly. But, always due to the small excess of duration of the intervals, the impulses which will follow will be carried out during the rising vibrations, and in the same way in increasingly advanced phases, so that after a pulse repetition frequency equal to that of the preceding ones, the body will be also struck at the time of the termination of a vibration; however, this second group of impulses will destroy obviously all that the first had done, i.e. by degrees the amplitude of the vibrations will decrease, and will end up cancelling. A third group of impulses will revive these vibrations, a fourth will cancel them again, and thus indefinitely. The sound of the impacted body must thus alternatively be reinforced and die out; on the other hand, the sound of the stream must be weaker when the masses reach the body during its downward vibrations that when they strike it during its rising vibrations, because of the difference of relative speeds, and one sees, moreover, that this last sound has its minima during the reinforcements of that of the body, and its maximum during the reductions. That said, if the vibrations of the body acquire, in their greater amplitudes, a certain energy, and if the relative speed of the impulses becomes at the same time rather small, the sound of the stream could be entirely masked in the moments of greater intensity of that of the body, to reappear and dominate in its turn in the intermediate moments; and, consequently, the two sounds will be heard periodically.

But if the body is able to carry out only vibrations of little amplitude, and if it is held at a long distance from the opening, it can be made that the relative speed of the impulses remains always considerable, so that the sound of the stream is appreciably uniform, and that of the body, at its maximum, does not have enough intensity to mask it. Then the first will not cease being perceived, and consequently, during the periods of reinforcement of the second, they will be heard both at the same time. It is undoubtedly in this direction that the words should be interpreted: *or even simultaneously*, which is borrowed textually from Savart.

§ 462. Now let us take again the case where one uses a sound instrument suitable for exact unison with the sound of the stream. If the instrument, instead of acting remotely, is put in contact with the walls of the vessel from which the stream escapes, it is clear that the vibrations communicated to these walls and propagated in the liquid will be much more energetic, and that, consequently, the modifications of the stream will have to be much more marked; moreover, it is understood that the small irregularities about which we spoke in § 455 could then be entirely unobtrusive. The contents of n^o 11 of § 448 are thus explained.

§ 463. Then also one observes (fig. 12 of § 448), in the axis of the stream, starting from the lower end of the continuous part, another system of swellings and thinner and shorter nodes, which is due, as Savart points out, to the spherules which accompany the masses. I speak of spherules, because, in the current case, the continuous part of the stream being extremely shortened, the acceleration due to gravity cannot lengthen narrowings notably, so that those give place to thin filaments, and, consequently, to true spherules (§ 383).

Here arises an apparent difficulty. When the stream is withdrawn from any vibratory action, its turbid part is made of swellings and nodes; it thus seems that, under the action of the shaping forces alone, the masses arrive at the spherical form without carrying out significant oscillations, and that oscillations of form take place only if the shaping forces are activated by vibrations; however, the mode of production of the spherules can in no manner being influenced by the vibrations, because those act directly only with the contracted section; lower than this section, their effect is limited to acquired speeds, which accelerate the development of the bulges and the deepening of narrowings, then the conversion of each one of the latter into a filament, and this filament only changes by providing the spherules, by only the shaping forces, which are born as in the unstable liquid cylinders; however, these spherules carry out oscillations of form, since the trace of their passage in front of the eye offers swellings and nodes.

In order to clear up this point, let us examine attentively what are the circumstances with regard to the spherules and the large masses. According to what was known in § 375, the filament must be generally divided in three parts, whose two extremes will meet respectively in the two large masses between which this same filament was included, while the intermediary contracts at the same time and symmetrically top and bottom, while bulging in the horizontal direction, to give the spherule with which we occupy ourselves. Due to this simultaneity and of this symmetry of action, the small portion of liquid in question reaches the spherical form towards which it tends; but it reaches it with an acquired speed, and necessarily exceeds it, so that its vertical diameter becomes less and its horizontal diameter larger than the diameter of the sphere of the same volume; from there come the oscillations of form of the spherules, and, consequently, the swellings and the nodes which result from it.

Events do not occur identically in the same way in the large mass suspended from the filament and which is isolated by rupture from it: indeed, a moment before this separation, the mass in question was already made free in its lower part by the rupture of the filament formed between it and the mass which precedes it; here thus the ruptures below and above the mass, and consequently the two contractions which tend to flatten the latter in the vertical direction, are not done completely at the same time; but the difference is necessarily extremely small, and it follows that the large masses must also carry out oscillations of form. However, it is, indeed, this that Magnus noted (§ 487); but he recognized that the successive masses are not detached exactly at the same distance from the opening, so that they do not pass through the same points in their maximum respective widening and lengthening, and that thus one cannot observe swellings and nodes. It is obvious, moreover, that these oscillations of form must be much less marked than when the stream is under the influence of a vibratory movement.

§ 464. Let us return, for one moment, to the spherules. When a filament changes, small narrowings which occur there change themselves into more delicate filaments, of which each one breaks in two points, and provides thus, by its average portion, a very tiny spherule (§ 375). These last spherules are frequently ejected out of the axis of the stream, undoubtedly entrained by the movements of the air; but as their mode of generation is the same as that of the less small spherules in question above, they must also carry out oscillations of form, and said Savart, indeed, that that takes place, although he does not indicate by which means he noted it: the trajectory described by those of these spherules which are launched out of the stream probably leaves in the eye a sufficient trace so that one observes swellings and nodes there; perhaps one can also distinguish the apparent shape resulting from the passage of those which stay on the axis.

§ 465. Now again let us produce a sound which deviates from that of the stream, but continue to place the sound instrument in contact with the vessel, so as to give more energy to the action of the vibrations. One sees, by n° 13 of § 448, that, in this case, almost all the sounds act on the stream. There still seems, in truth, to be some vagueness in these words *almost all the sounds* employed by Savart; but one cannot believe that they mean that ineffective sounds alternate with effective sounds. Indeed, let us suppose, for one moment, the inefficiency of certain intermediate sounds, and imagine that the sound of the instrument is moving away in a continuous way from that of the stream; then, when one leaves one of these ineffective sounds, it will be necessary: either the action on the stream, null that it was for this sound, gradually increases up to a certain point, which would be against the statement of the quoted number, according to which the action decreases as one deviates from unison; or this action becomes suddenly marked, which is hardly acceptable. It is thus very probable that the idea of ineffective sounds contained in the words: *almost all the sounds*, refers simply to sounds far too distant from that of the stream, which, due to the statement in question, should produce only an insensible action.

§ 466. We said that vibrations differing in period, between some limits, from those of the stream, can bring this last to unison with that of the instrument. However, the condition most favorable to the production of this result, must obviously be the contact of the sound instrument with the walls of the vessel, because of the more immediate transmission of the vibrations. And indeed, while in the case of n° 10 of § 448, the phenomenon is realizable only in an interval of minor third, here, as one sees by n° 14 of the same paragraph, it extends to intervals from a fifth above its specific sound and more than one octave below; let us add that Savart is no longer using, as in the first case, not very precise terms: he says clearly that the sound of the stream is put at unison with that of the instrument.

§ 467. A higher limit as high as the fifth seems, at first glance, in opposition to certain results of our study of liquid cylinders. Indeed, so that the sound of the stream goes up by a fifth, it is necessary that the number of isolated masses which hit, in a given time, the taut membrane, increases in a ratio of 2 to 3, and that, consequently, it is the same for the number of incipient divisions which pass, in the same time, through the contracted section; and as, under a constant load, the length of incipient divisions is obviously inversely proprotional to this last number, it follows that, from its specific sound to the fifth of this one, incipient divisions are shortened in the ratio of 3 to 2; but we know (§ 443), that when a water stream gives the sound which is specific for it, the length of its incipient divisions is equal to 4.38 times the diameter of the contracted section; if thus, by only the action of a sound instrument, the sound of a similar stream goes up by a fifth, the length of its incipient divisions will be reduced to 2/3 of the value above, i.e. with 2.92 times the diameter of the contracted section; however, this number is a little lower than the limit of the stability of the liquid cylinders, a limit which is, as we also know, equal to 3.14; however, we showed (§ 371) that when a liquid cylinder changes, the length of its divisions cannot be less than this same limit.

The difficulty is only apparent. The quoted demonstration supposes that the cylinder spontaneously starts to change, and then it is rigorously true; but it does not apply if the narrowings and the bulges are originally formed by a sufficiently energetic foreign cause. Indeed, the demonstration in question primarily consists in showing that if, in the first phases of the transformation, one considers the whole of a narrowing and a bulge, together whose length is equivalent to that of a division, all occuring in this portion of the cylinder as if its two bases were solid, so that the transformation can be established spontaneously only with a spacing of these bases at least equal to the limit of stability; but if, in a cylinder formed between two solid discs whose distance is a little smaller than the limit of stability, the transformation could not begin itself, we know (§§ 396 and 399 to 401) that it continues if it were started with a foreign cause which accumulated the liquid in a certain quantity towards one of the discs, so as to artificially cause a bulge and a narrowing. The demonstration that we pointed out cannot thus be called upon any more when, in a liquid stream, the incipient narrowings and bulges are formed by energetic vibrations. Then, if the sum of the lengths of one of these narrowings and one of these bulges, or equivalently the length of a division, is a little lower than the limit of stability, the transformation will be able to start from this abnormal mode, and the more the vibrations will be intense, the more the last sound for which will exist the possibility of the phenomenon will be high above its specific sound.

If the foreign sound is below its specific sound, and thus tends to give to incipient divisions a length necessarily higher than the limit of stability, it will not meet the kinds of resistance which we have just announced inside this limit, so that the possibility of the phenomenon will extend much further, and we see, indeed, that, in the experiments of Savart, it embraces an interval of more than one octave.

There is still another reason that the phenomenon is limited less below its specific sound than above in the same sound instrument, the amplitude of the vibrations generally increases with the gravity of the sound; however, it is understood that the more considerable the amplitude of the transmitted vibrations is, the more the excess of liquid which each downward vibration tends to push in the stream to form an incipient bulge, and the more also the subtraction of liquid which each ascending vibration tends to operate to indent an incipient narrowing. If thus, as the sound of the instrument deviates from its specific sound, either below, or above, the length of incipient divisions that the vibrations tend to form becomes increasingly higher or increasingly lower than that of incipient divisions that the shaping forces tend to form on their side, and from there is born obviously an increasingly strong conflict with these last forces; in addition, below its specific sound, the vibrations act more and more vigorously to make prevail the new fashion of transformation, and this increase in action must compensate more or conflict.

Let us note here that in the case of a very deep sound compared to its specific sound, the new fashion of transformation is not established in the same manner as in that of a sound which does not move away much from its specific sound; in this last case, indeed, because of the little difference in length between incipient divisions of the two kinds, it is quite probable that the shaping forces modify simply their own action, by lengthening or shortening incipient divisions which are appropriate to them, in order to make them coincide with those which are appropriate for the vibrations; but when the sound of the instrument is deep enough so that length of these last divisions exceeds considerably that of others, when, for example, the instrument makes the lower octave, and that the transmitted vibrations are intense enough to impose on the stream their mode of transformation, one must admit that the action of the shaping forces is completely destroyed, so that there is no more modification of the first mode, which adapts to the second, but absolute substitution of the second for the first.

In the case of the sharp fifth there are two other reasons which contribute to the change of sound of the stream: initially, after the immediate action of each vibration, the deformation must increase by acquired speeds; and, in the second place, divisions, and consequently narrowings and the bulges, lengthening during their descent, the sum of the lengths of a narrowing and a bulge, initially lower than the limit of stability, is quickly approaching this limit, so that the progress of the transformation according to the originally impressed abnormal mode becomes easier.

§ 468. Thus, the theory explains all of the phenomena resulting from the action of vibrations on streams launched according to the downward vertical, of all those, at least, that Savart describes in a precise way; but not sounds with streams launched in other directions.

And initially, since, in these streams, there is also a gradual transformation into isolated masses, the sounds must necessarily exert on them an influence similar to that which they exert on the streams launched vertically from top to bottom; n^o 15 of § 448 thus does not need explanation.

§ 469. But it is not the same for n^o 16. If all divisions, by reaching one after the other the end of the continuous part, were isolated identically in the same manner, and if all the masses started from there with the speed precisely corresponding to the translatory movement of the liquid at this point, those would describe all exactly the same trajectory, and consequently the discontinuous part of the stream could not present scattering or a sheaf; there are thus, as Savart points out, irregularities in the emission of the isolated masses from the end of the continuous part; these irregularities, moreover, must be extremely small, because the sheaf does not have a great width. I had initially thought that they came from the same causes as those in question in § 455. But if that were so, the suppression of the foreign actions should make the sheaf disappear, and thus reduce the totality of the stream to a single jet; however, the experiment did not confirm it: by employing, with regard to such a stream, the means which Savart made use in the case of the vertical downward streams, i.e. by receiving the discontinuous part on a suitably tilted thick board, and by placing soft bodies under the vessel from where the stream escaped, under that in which it went and under the supports, I could not succeed in making the sheaf undergo a notable reduction. One must infer that the irregularities are not due to vibratory movements, and that, consequently, they affect the action even of shaping forces; it is understood, indeed, considering the nature of the phenomenon of the transformation, that even slight disturbing causes must influence the perfect equality of all divisions which are born one after the other at the contracted section; we saw, for example, in the experiments of §§ 361 to 369, a foreign cause alters the equality of length of divisions of a cylinder. That said, we will show that small differences of this kind in incipient divisions of a stream launched at a suitable obliqueness, must necessarily give rise to some scattering of the discontinuous part.

Let us consider in particular two of the narrowings with the bulge which they include between them. As we know, each one of these two narrowings, initially very slightly indicated when it leaves the contracted section, deepens then gradually in the progress of the continuous part, by sending half of its liquid into the bulge; this thus receives, through its leading end, the liquid which is driven out in the contrary direction of the translatory movement, and, through its trailing end, the liquid which is driven out there in the direction with this movement, so that its speed of traverse tends to being decreased by the first of these surges and to be increased by the second. Now, although these two opposite actions are in general unequal, because the leading narrowing is, at every moment, in a little more advanced phase of transformation than the trailing one;

however, if two narrowings were perfectly identical at their respective births, and if, consequently, they identically underwent, though not completely at the same moments, the same modifications until their respective ruptures, it is obvious that after these two ruptures, i.e. at the time when the bulge will be in the state of an isolated mass, the sum of the momentum brought into this mass by the leading narrowing will be exactly compensated by the momentum brought there, in the other direction, by the trailing narrowing, and that thus this same mass will leave the continuous part with a speed exactly compensating the movement generated by translation. But it is clear that the compensation will not be whole any more if two narrowings differed at their births, if, for example, they were unequal in length: it follows that the transformation happens faster when divisions are longer, and, consequently, when narrowings are longer, that the more lengthened of two narrowings in question will deepen more quickly than the other; and as, in virtue of its excess length, it contains more liquid, it will send into the bulge a larger matter surge with higher speeds, and consequently a greater momentum. If thus this same narrowing is the trailing one, the mass will leave the continuous part with an excess of speed, and if it is the leading one, it will leave with a deficit of speed. Thus, small differences in length in incipient narrowings will have as a result to establish small inequalities between speeds of the successive isolated masses; but consequently these masses will traverse necessarily parabolas of unequal amplitude, and consequently will scatter in a vertical plane, thus forming the sheaf.

This explanation supposes that the disturbing causes do not produce, in narrowings, any irregularity in directions perpendicular to the axis of the stream; however, one saw (§ 359 and 413) that, during the transformation of an unstable liquid cylinder, the shape tends with energy to remain symmetrical compared to its axis, and one understands that it must be the same for narrowings and bulges of our curved stream, so that irregularities in a normal direction could not persist.

According to this same explanation, it is clear that there are two extreme limits for which scattering is necessarily null, which are when the stream is vertically launched from top to bottom and when it is launched vertically upwards, since, in these two cases, all the isolated masses traverse the same rectilinear trajectory; if thus one passes from the first to the second while varying by degrees the direction whereby the jet is launched, the sheaf will be able to start to be shown in a way quite distinct only starting at a certain angle between this direction and the downward vertical, and it will have to cease being quite apparent beyond another certain angle. Moreover, as long as the stream is launched in directions obliquely downward, and even in the horizontal direction, one understands that the trajectories described by the masses escaping with different speeds will not deviate enough from each other so that the sheaf appears clearly, so that the first direction which will start to make the sheaf visible, will be obliquely rising. All these conclusions agree with the facts of the number which we discuss.

We claim, one sees, that the inequalities between incipient narrowings do not depend on the direction whereby the jet is launched: and, indeed, there is no plausible reason to attribute these inequalities to the rising obliqueness of the jet. If we did not speak about it while treating downward vertical streams, it is because, in these last streams, they can give rise to no appearance of a particular kind; they then make obviously only increase a little, in the axis of the stream, the inaccuracy of the superposition of the individual systems of swellings and nodes, and they constitute thus simply an influence to add to those which are mentioned in § 455. As for the nature of the disturbing causes which produce the inequalities in question, it would undoubtedly be quite difficult to discover it; but, whatever it is, scattering of the discontinuous part of the streams launched under a suitable angle reveals to us the presence of these causes.

§ 470. Now, a stream being launched at an angle such that the sheaf is well formed, let us subject it to the influence of a sound instrument. The sound which will shorten more the continuous part will be still obviously that whose vibrations follow one another at the same intervals as the passages through the contracted section of the narrowings and bulges due to the shaping forces. But these vibrations, being perfectly regular and isochronous, will prevent, if they have a sufficient intensity, the disturbing causes from modifying the incipient narrowings; in other words, while activating the transformation, they will bring their regularity there, so that all incipient narrowings will have the same length, and thus all the isolated masses will follow identically the same trajectory (§ preced.); under the influence of this sound, the sheaf will have to thus disappear, and the totality of the stream will be reduced to a single jet presenting a quite regular system of swellings and nodes.

§ 471. As for the singular effects of reduction of the sheaf to two or three jets under the influence of other sounds, one needed, to test the explanation, to know the ratios of the sounds in question to the specific sound, ratios that Savart does not indicate. Accordingly, as these phenomena are not the least curious among those which result from the action of vibrations on liquid streams, I decided to undertake the experiment.

The opening that I employed had a diameter of 3 millimetres; it was bored in the center of a circular brass plate of 12 centimetres diameter[257], inclined so that the jet was launched at an angle of approximately 35° above the horizontal; this plate formed one of the bases of a cylindrical drum, which communicated, by a broad and short horizontal tube, with the lower part of a large vessel of Mariotte; the load was 34 centimetres; finally, the sound instrument was a violoncello, whose base I rested on the supports of the apparatus.

The sheaf being well formed, I initially sought to find its specific sound, or, in other words, that which clearly brought back the totality of the stream to a single jet with a quite regular system of swellings and nodes, and which, at the same time, gave birth to the first swelling very close to the opening. This point reached, I raised the sound of the instrument by successive semitones. Then the influence of the vibrations was decreasing: the jet started by losing its regularity, then the sheaf gradually reappeared, after which it persisted, without being reduced to two or with three jets. I returned then to the specific sound, and I reduced the sound from the instrument, from there, by semitones too. The same effects, which are deterioration of the regularity of the jet and the progressive reappearance of the sheaf, appeared; but, while approaching the lower octave, I noticed a tendency of the sheaf to change into a double jet, and, when I arrived at this last sound, the sheaf was clearly replaced by two jets with regular systems of swellings and nodes. I continued to lower the sound, and the two jets were shown in the same way, until the third below the lower octave; lower still, and as long as the double lower octave was not reached, I obtained sometimes two, sometimes three jets; only the fifth gave a single jet sometimes; finally, for the double lower octave, I observed three jets constantly. In all these cases, the jets always each had their system of swellings and nodes.

These facts are less restricted than those which are stated in n° 16 of § 448; indeed, according to this number, which reproduces the direction of the expressions of Savart, it would be only under the influence of its specific sound that the sheaf would contract into only one jet, and there would be only two other given and different sounds which would reveal respectively two jets and three distinct jets. But the absence of any indication of the ratio between these sounds and the specific sound, are enough to show that

[257]This large diameter was justified by the need for giving the vibrations of the plate a sufficient freedom.

Savart did not give all his attention to the phenomena of this kind, and that after having observed them in isolated cases, he did not seek if they were suitable for extension.

§ 472. Now let us see if the theory can explain these same phenomena. Let us start with the lower octave. For this sound, the duration of vibration is double of that of the passage of a narrowing or of a bulge through the contracted section, from which we will easily conclude that divisions which would be born under the action alone of the lower octave from its specific sound, would be double in length of those which on its side the action isolated from the shaping forces would cause. Accordingly, we can claim that each first embraces exactly the whole of two of the seconds: because, in this manner, with all the sections which finish these sets or couples, there is obviously absolute concord between the two kinds of action, the sections of which they are constituting at the same time the middle narrowings which would result from the vibrations, and the middles of narrowings due to the shaping forces. Now, let us examine what must occur, during the transformation, in any of these same pairs of divisions. This pair being composed of two whole divisions, contains two bulges which include between them a narrowing, and ends in two half narrowings; however, while whole narrowings to which these terminations belong, as we saw, are favored by the vibrations, it is clear that the intermediate narrowing is, on the contrary, in conflict, since its middle, which is the middle of the pair, corresponds to the middle of the division which the vibrations tend to produce, and, consequently, in the middle of the bulge of it; each bulge which in the stream the shaping forces give birth to, is thus adjacent to two unequally pulled narrowings. Moreover, the narrowings favored by the vibrations must lengthen under the influence of these last, since narrowings which they would produce alone would have a length twice larger, and as the length of each pair of divisions considered above remains the same as in the absence of the sound of the instrument, it follows that intermediate narrowings with the preceding, i.e. those which occupy the middles of the pairs and which are in conflict with the vibrations, must be shortened. One can thus admit that favored narrowings, although, as of their birth, they already are thinner than narrowings in conflict, however, then contain, because of their excess length, more liquid than the latter; and as, by the double reason that they are longer and that they are activated by the vibrations, they arrive more quickly at their rupture, it is seen that they will send into the bulges more matter with more speed, and, consequently, a greater momentum. All the bulges will be thus under the condition that we analyzed in § 469, and consequently the isolated masses, in leaving the continuous part, will have a small excess speed, and the others a small deficit speed. But here the vibrations, impressing their regularity on the phenomena, make identical between them, at their births, all favored narrowings, and make in the same way identical between them all narrowings in conflict, so that all the masses formed by the bulges which, in the course of the continuous part, had a favored narrowing behind, leave with the same excess speed and describe consequently the same trajectory, and all those which come from the bulges for which a favored narrowing was ahead, leave with the same deficit speed and describe another single trajectory; therefore, under the influence of the lower octave of its specific sound, the sheaf must be replaced by two separate jets.

However, it is not impossible that the sound considered made the sheaf disappear; indeed, this sound being already very low, at least with regard to the stream on which I operated, its vibrations have much amplitude, and could act with enough energy to prevent the formation of narrowings in conflict, and to thus leave in the stream only divisions which they tend to produce alone, in which case all the isolated masses would have necessarily the same speed, which is normal speed.

Let us examine, in the second place, the influence of the lower fifth of the preceding sound, or, in other words, of the double lower fifth of its specific sound. The vibrations of this double fifth being three times slower than those of its specific sound, one easily concludes that each division that they tend by themselves to cause in the stream, includes exactly three divisions due to the shaping forces. One sees, moreover, that, of the three bulges contained in this ensemble of divisions, the trailing has a favored narrowing behind it, and in front of it a narrowing in conflict; that leading, on the contrary, in front of it has a favored narrowing, and behind it a narrowing in conflict, and finally the intermediate is between two narrowings in conflict, which are identical between them at their respective births. Accordingly, the momentum will be necessarily distributed, in the isolated masses coming from these three divisions, in such a way that the posterior one will leave the continuous part with a speed higher than the normal speed, the leading one will take a speed lower than this normal speed, and the intermediate will leave with normal speed; and as, always because of the perfect regularity of the vibrations, events occur identically in the same way in each system of three divisions, it will be able to have, in the discontinuous part, only three different speeds. If thus the action of the vibrations does not entirely mask that which the shaping forces exerted freely, before its influence, the sheaf will be dissolved into three distinct jets; and if, on the contrary, the action of the shaping forces is completely dominated, which must take place more easily for the lower octave, because of the larger amplitude still of the vibrations, there will be only one jet, as we showed above.

As for the separation of the two jets under the influence of the double lower fifth, a result the experiment also gave, one can explain it in the following way. When the action of the vibrations is dominating, and that thus there are born at the contracted section only divisions which it causes, those have a big length, since each one of them holds the place of three divisions which the shaping forces would draw; however, we know that any liquid shape whose dimension is considerable relative to both others tends to be divided into isolated masses; one can thus claim that, in the divisions in question, if acquired transverse speeds are not sufficient to be opposed to it, it develops new shaping forces which divide each one of these same divisions into two others, by indenting a narrowing in its middle, and consequently, as all narrowings thus produced are obviously in conflict, the reasoning employed with regard to the lower octave shows that one must obtain two jets.

Let us notice here that the abnormal shaping forces of which it has just been the question could not form, in each great division, more than one narrowing; indeed, if they formed two of them, which would divide each great division into three small ones, these last would have the same length as those of the stream not subjected to the influence of the sound instrument; but, so that that was possible, it would be necessary that new divisions did not undergo more resistance to be taken shape than in the absence of any foreign action because one can conclude from what takes place in the cylinders, that, in any more or less similar liquid shape, the length of divisions increases with resistances; however, acquired transverse speeds cause, in our great divisions, a tendency to persevere in the mode of transformation impressed by the vibrations, which constitute a resistance to a later division.

Let us pass, in the third place, to the double lower octave. Here each division which would be born under the action alone of the vibrations, would include obviously four divisions which would result from only the shaping forces; however, if these two actions combined, it seems that one should have four distinct jets: because it is easy to see that, in three narrowings which would be formed then, the conflict would be unequal: it would be stronger for the narrowing of the middle than for the two others, so that each of the two bulges ranging between these three narrowings would receive unequal momentum on two sides, and finally that the differences would be larger for the two extreme bulges, of which each one would be between a narrowing in conflict and a favored narrowing. But, on the one hand, the vibrations in question having a considerable amplitude, it is conceived that that their action must always erase that of the shaping forces, and, on the other hand, formed divisions in this manner being very long, one also conceives, according to what we mentioned above, that there must be generated there new shaping forces which cause fractionation of it; however, by the reason of the resistance indicated in the same way above, this fractionation must give rise here to more than three parts, which, considering the distribution of the conflicts and the concord and the regularization brought by the vibrations, must only convert the sheaf into three jets.

There remains, in the fourth place, the action of the sounds ranging between the lower octave and the fifth below, and between that and the double lower octave. For these sounds, there is no more simple ratio between the lengths of divisions which would result respectively from the vibrations alone and the shaping forces alone; but one will easily admit that, under the influence of those which border, either on top, or below, the double lower fifth, and if the effect of the vibrations does not completely replace that of the shaping forces, divisions due to these last forces are shortened or lengthened a little, so as to allow, in the limits which separate them, successive systems of three of these divisions, the absolute concord of the two kinds of actions, and to thus restore the simple ratio from 3 to 1 pertaining to the double fifth; from whence the resolution into three jets. Under this same influence, as under that of the double fifth, if the vibrations are dominating, but not enough to be opposed to a later development of shaping forces, each great division born will be able to be divided into two, so that the discontinuous part of the stream will present only two jets.

One will also admit that the sounds close to the lower octave will make prevail the mode relating to the latter, and that thus the sheaf will never change but into two jets.

Finally, one will admit still that, for sounds which do not move away too much from the double lower octave, vibrations have always enough amplitude, and, consequently, enough action, to surmount ordinary shaping forces, and that at the same time divisions which they give birth to are always rather long so that each one of them must necessarily undergo a fractionation then, which will be division into three, and will be able also to divide into only two, if it encounters a greater resistance on behalf of the vibrations; from whence three jets or two jets.

As for the systems of swellings and nodes which are observed in each jet, they are the obvious consequence of acquired transverse speeds which come from the action of the vibrations.

§ 473. One can wonder why, above its specific sound and between this and its lower octave, no sound, except for those which bordered these two last, did not cause, in the experiments described in § 471, anything analogous to the phenomena which we have just studied; indeed, for the simple lower fifth of its specific sound, for example, one will find easily that the length occupied by the whole of two of divisions due to the vibrations alone, would be equal to that which the whole of three divisions due to the shaping forces occupies, so that by imagining these two superimposed sets and combining, there would be concord in two narrowings of which the terminations of the system would form part, and conflicts in two intermediate narrowings belonging to the second of the two sets considered; and, as these two conflicts would be equal, one could expect, according to our theory, to see the sheaf giving rise to three jets; finally, one could also expect, for similar reasons, the demonstration of three jets under the influence of the sharp fourth and of two jets under that of the sharp fifth of its specific sound.

But, in our theory, the appearance of one, two or three jets to replace the sheaf, supposes, as one saw, that the vibrations communicated to the liquid regularize what occurs in the stream, and that requires that they have an energy of action able to neutralize the effect of the disturbing causes which tend to establish, in incipient successive narrowings, inequalities of length not symmetrically distributed; however, all else being equal, the action of the vibrations on the stream decreasing with the amplitude of these vibrations, one understands that above the lower octave of its specific sound this action could be simply insufficient, and if it had been possible to increase, by a more immediate transmission or a better provision of the system of the opening, the amplitude of the communicated vibrations, the three sounds announced higher would undoubtedly have ceased being inactive with regard to the sheaf. It is what will become obvious, if one pays attention to how the vibrations act on the streams launched obliquely in the same way as on the streams vertically launched from top to bottom, and if one remembers that, in the experiments of Savart mentioned in n^o 14 of § 448, experiments in which all was laid out in order to give a great intensity to the communicated vibrations, the mode of transformation impressed by those replaced completely that of the shaping forces, even for sounds going until the sharp fifth of its specific sound.

We spoke about the possible influence of a change to the system of the opening; it is that indeed the opening employed in my experiments was bored in a very thin plate[258], and that, consequently, this plate vibrated perhaps with difficulty in unison with sounds which did not have a certain gravity.

§ 474. We now have only, to complete the study of the influence exerted on the liquid streams by vibratory movements, to show the connection of the theory with the facts of n^o 17 of § 448, and this connection appears less easy to establish:

Since its specific sound is also that for which the duration of vibration is equal to that of the passage of a narrowing or a bulge through the contracted section, and since, according to the experiment of Savart, the number of vibrations corresponding to this sound falls as the direction whereby the jet is launched departs from the downward vertical, it must be the same for the number of incipient narrowings and bulges, and, consequently, of the number of incipient divisions; but as the velocity of emission of the liquid is appreciably independent of the direction of this exit, the number of divisions which are born in a given time can decrease notably only by an increase in the length of these incipient divisions; thus, under the same load and with the same opening, incipient divisions must be lengthening as the direction of emission of the stream moves away more from the downward vertical.

We saw (§ 454) that, under a weak load, the length of the incipient divisions of a stream vertically launched from top to bottom is decreased by the tendency of this stream to tapering; it thus must, by the contrary reason, being increased in a vertically launched stream, upwards, and consequently, always under a weak load, it must be gradually growing from the first of these directions to the second. But, in the experiment which Savart reports, the opening was 3 millimetres, and the load 50 centimetres; this load must be regarded as strong, and, under its action, the change in length of incipient divisions due to the cause above was insensible; however, in this same experiment, from the downward vertical direction to a rising direction at the angle of 45°, the number of vibrations corresponding to the its specific sound appeared to be lowered from 600 to 355, i.e. to become close to twice less. It is necessary thus that, from the one of these directions to the other, the length of divisions almost doubled; thus the cause that I pointed out is completely insufficient.

Let us say here that Savart speaks only with much reserve about the fact in question; here is how he expresses it: "However, I have nothing to specify on this subject,

[258]It had only approximately a half millimetre thickness.

because, when the jet is launched upwards, at an angle of only 45° (the load being of 50 centimetres and the opening 3 millimetres in diameter), the impact of the turbid part against a membrane is already too weak to put it into vibration, so that it is not able any more to cause the number of pulsations, so that to seek with a string instrument which is the sound which most strongly modifies the shape and dimensions of the stream, means which being without control, cannot inspire a whole confidence any more."

It is allowed, one sees, to conceive some doubts with regard to the phenomenon; however, let us admit it in all its plenitude, and try to discover a plausible reason for the great lengthening of divisions.

Our study of liquid cylinders revealed a cause able to produce a similar effect: it is the presence of resistances which obstruct the transformation; thus let us see if, under the conditions of the phenomenon, we will be able to find the origin of a resistance of this nature.

Let us note initially that, in the spontaneous transformation of the cylinders, and, consequently, in that of the streams, the bulges do not have, by themselves, any tendency to be formed: they develop only because the liquid is driven there by the capillary excess of pressure of narrowings; they are purely passive, only narrowings are active: Now, if a stream launched in a downward direction either vertical, or oblique, did not divide, it would spread gradually in its way, we know, by acceleration due to gravity, so that the molecules, at the same time as they would be driven in the direction of the axis, would be approaching it gradually. Consequently, in a downward stream, narrowings which tend to be formed, do not meet an opposite tendency; it is understood, moreover, that they will be formed all the more easily as the stream will be closer to being vertical.

But it is not the same any more for rising directions: here the stream, disregarding its divisions, widens starting from the contracted section; in other words, the molecules, at the same time as they obey the general movement of translation, tend everywhere to move away from the axis; however, isn't it rational to admit that from there is born a resistance to the generation of narrowings, a generation which takes place by a movement in a contrary direction? And this resistance will be necessarily all the more energetic as the direction of emission of the stream will approach more the ascending vertical. One can object only that with the load and the opening employed by Savart, the molecules tend to move away very little from the axis; because, when the transformation starts, the capillary forces which produce it are extremely weak, and, consequently, it is conceived that the transverse movement above molecules must be enough in spite of its smallness, to obstruct the normal production of narrowings, and to thus force the stream to lengthen its divisions. Finally, the opposition in question taking place only with regard to the rising streams, it is only in these last that lengthening will be considerable, which agrees with the result of Savart.

Let us add that our explanation could be subjected up to a certain point to the control of experiment; in effect, if it is well founded, it is necessary obviously that, for the same opening, the phenomenon observed by Savart is much less marked as the load is stronger.

CHAPITER XII.

History of the constitution of liquid streams. Action of electricity on streams of small diameter. Laminar streams. Constitution of a gas current which crosses a liquid.

§ 475. Faithful to the progress that we followed up to here, we now will outline the research made by other physicists before 1870, at least of those which we could collect, on the constitution of liquid streams launched from circular openings.

Already in 1686, Mariotte[259] had foreseen, at least in a particular case, the spontaneous resolution of the stream into isolated masses. After having claimed that upwards vertically launched jets are widening in consequence of the progressive reduction in speed, he adds, in connection with streams vertically downward:

"By the same reason the water which runs out of a hole of 5 or 6 lines, when it is in the tank with a depth of only 3 or 4 inches, is always extending itself until it is reduced into drops when the filament of water becomes too small, from which it is seen that the filament of water would become at the end thinner than a hair; but before reaching this point, it separates and divides into drops which always accelerate their movement so that they acquire their higher speed."

As for the general fact of the turbid aspect presented, under arbitrary loads, by the second part of the stream, one attributed it, before Savart, to simple scattering produced by air resistance[260].

Savart showed that this resistance is by no means the cause of the phenomenon; using clever processes, he studied the true constitution of the stream, and made known all the characteristics such as we recalled them in the preceding chapter.

In ordinary circumstances, i.e. when the turbid part is received in a vessel where the liquid accumulates freely, Savart notes initially, by extremely simple means, the discontinuity of the portion which extends beyond the middle of the first swelling: when, after having fixed the eyes on a point of the stream close to the opening, one makes them quickly move so as to follow the translatory movement of the liquid, one distinguishes the isolated masses very well; a rod which one makes quickly pass through the turbid part lower than the middle of the first swelling, is almost never wet; finally, if the liquid is mercury, this portion of the stream is transparent, and the most delicate objects are seen perfectly through it.

To discover the cause of the swellings and the nodes, Savart lays out an apparatus producing a flow drop by drop; he sees this flow giving rise to the appearance of very regular swellings and nodes, and, by observing the way in which the drops are detached, he recognizes that it takes place by a tapering, after the rupture of which the drops are flattened in the vertical direction.

Returning then to the stream resulting from a continuous flow, he manages to show with the eyes what actually occurs in the upper half of the first swelling. For that, he makes use of a procedure founded on the persistence of impressions in the eye: a broad black ribbon crossed with equidistant white bands is driven upwards, at a uniform and suitable speed, behind a water stream rendered dark by a dye. The observer placed in front of this system, sees the stream being projected on the bottom, grayish due to the fast passage of the white bands, and, by reasons too long to develop here, distinguishes the isolated masses in the form of dark spots occupying fixed positions, as well as the bulges which furnish the bottom of the continuous part.

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259 *Traite du mouvement des eaux, 4th partie, 1r discours.* (Works of Mariotte, the Hague, edition of 1740, p. 448).

260 see CAVALLO, *The elements of natural or experimental philosophy,* London, 1803, vol. II, p. 191; BIOT, *Précis élémentaire de physique expérimentale,* vol. I, chap. XIII, p. 116 of the edition of 1824; etc.

Lastly, to note the existence of the bulges in points close to the opening, Savart makes fall on the limpid portion of the stream a thin and horizontal beam of light; the small zone thus lit alternately appears to go up and down, and these oscillations, which start to appear a little distance from the opening, all the more acquire amplitude as the illuminated zone is brought closer to the turbid part.

We summarized, in § 448, the results of the observations of Savart concerning the action exerted on the stream by the vibratory movements, and, in § 447, we gave an idea of the assumption put forth by the same scientist to explain the constitution of the stream.

§ 476. In the Report in question in § 486, Mr. Fuchs recounts a curious observation made, says he, by chance by Eperies in Hungary, a score of years before his work, which corresponds to the year 1836 approximately: he carried out experiments with an electrophorus, while a small water jet spouted out of a fountain of Heron, placed in the vicinity; however, it was noticed that this jet, which naturally separated into droplets before reaching its top, became continuous in all its length under the remote influence of the plate of the electrophorus.

One knew, moreover, for a long time the experiment of the electric watering-can: it was known that if water runs out from top to bottom by a rather narrow opening so that it left only drop by drop, the flow becomes continuous when one communicates directly to the vessel a moderate electricity.

§ 476bis. Let us mention here the curious experiment that Mr. Colladon has described[261] in 1842, and which consists in making a beam of sunlight penetrate a curved stream of water along the direction of emission of the jet. The drain opening is bored in the side wall of the tank, and an opening made in the wall opposite, with respect to the opening, is provided with a convex lens whose focus coincides with this opening when the tank contains water; the sunlight reflected horizontally by a mirror on the lens, thus forms a lengthened conical beam which crosses the water of the tank and is introduced by the opening into the stream; however, the rays of which it is composed strike everywhere the interior surface of the stream under rather large angles so that they undergo total internal reflection, so that the light remains imprisoned in all the continuous part of the jet, in spite of its curvature. But if this continuous part is received on a solid obstacle, the light is released and proclaims its glare at the place of the meeting; in the same way, if the stream is sufficiently long so that its lower portion is reduced into isolated masses, the light goes to the bottom of the continuous part, and from there escapes in sharp gleams.

§ 476ter. In 1844, Mr. von Feilitzch published, on the flow of the liquids by small openings, a Memoir[262] where he deals especially with the contraction of the stream, initial speed, and the influence of adjutages, but where he presents some considerations on the generation of the isolated masses. He attributes the formation of the bulges at the bottom of the continuous part to a kind of conflict between the tendency of the stream to constitute a lengthened cone and a tendency to conversion into drops: according to him, separation is carried out, and a drop is detached, where the section of the continuous cone placed between two bulges is the smallest possible.

One sees that Mr. Von Feilitzch more or less approached my theory, since he claims in the stream a spontaneous tendency to transformation into isolated masses.

§ 477. The apparatus with parallel white bands imagined by Savart alters, for the eye of the observer, vertical dimensions of the bulges and the isolated masses, and can-

[261] *Sur les réflexions d'un rayon de lumière l'intérieur d'une veine liquide parabolique* (COMPTES RENDUS, vol. XV, p. 800).

[262] *Ueber den Ausfluss der Flüssigkeiten aus Oeffnungen in dünner Wand und aus kurzen Ansatzröhren* (ANN. OF MR. POGGENDORFF, vol. LXIII, pp. 1 and 215).

not show these bulges and these masses with a great clearness. In 1846, Matteucci[263] had the idea to observe the stream illuminated by a strong electric spark, or better by a succession of similar sparks; he saw thus, in a perfectly distinct way, the isolated masses in the various phases of their oscillations of form. He thinks that one could, by laying out the apparatus suitably, project the image of the stream on a screen by means of a magic lantern using the sparks as the source of light.

§ 477bis. In 1848, Mr. Weisbach[264] could study, in a mine close to Freiberg, water streams launched horizontally under a load of 122 meters. I do not have to deal with the results which he obtained with adjutages, or a square opening; but one of these streams left from a circular opening, without adjutage; however, it presented this remarkable characteristic, that it was continuous only up to a small distance; the opening was approximately a centimetre in diameter, and its stream was already discontinuous at two decimetres from this opening.

According to the laws of Savart, the continuous part of a stream launched under these conditions should have had, in the absence of any disturbing cause, approximately 40 meters length; so that it was reduced to two decimetres, it was necessary thus that the stream was subjected to the action of a very powerful disturbing cause; however, one understands that, under such an enormous load, the friction of water against the edge of the opening was to be extremely intense, and to produce consequently, at this edge, energetic vibrations; these are undoubtedly the vibrations which caused the shortening in question. Savart admittedly showed that friction at the edge of the opening did not influence the constitution of the streams which he observed; but, in his experiments, the strongest load was in general only 47 centimetres. To omit nothing, let us add that, with an aim of confirming one of his laws, Savart measured the lengths of the continuous parts of launched streams, from an opening of 3mm of diameter, under loads going from 51 to 459 centimetres; the latter was already very strong compared to the opening employed, however, the continuous part of the corresponding stream did not present an abnormal shortening, but all the streams in question were received on a resonating body, and the influence of its vibrations perhaps masked that of the vibrations of the edge of the opening.

§ 478. In the Report of which I analyzed a part in § 153, a Memoir published in 1849, Mr. Hagen also deals with the liquid stream. Supposing a stream which runs out from top to bottom vertically, he considers two thin sections, or cross sections, not very distant one from other at their departure, sections whose mutual distance is growing as they go down; he shows that the surface of the portion of the stream ranging between them is also growing, but that the increase becomes much slower as the portion in question traverses further. He concludes that the molecules from the interior of the stream which must go to the surface to satisfy its progressive increase, make it more and more easily as the portion considered moves along, and thus one cannot attribute the separation of the masses to the molecules in question not having time to arrive at the surface; he points out, moreover, that the resolution into isolated masses takes place in a stream launched upwards as well as in a stream launched from top to bottom, although, in the first case, the cross sections are approaching, instead of being deviating.

He employs, for the observation of the bulges of the continuous part, a process consisting of receiving this part on a plate of glass, and looking at other side of it; the experiment is especially convenient in the upwards spouting case of a stream and for which the continuous part comes to strike the concavity of a watch glass. Mr. Hagen

[263] *Examen de la constitution de la partie trouble de la veine liquide* (COMPTE RENDUES , vol. XXII, p. 260).

[264] *Ueber den Ausfluss des Wassers unter sehr hohem Druchke* (POLYTECHNISCHES CENTRALBLATT, 14th year, p. 763).

does not say in a quite clear manner how this kind of experiment shows the existence of the bulges; he adds that he never could continue these bulges to the opening.

He makes use of still another process, applicable also to the discontinuous part: he receives, for a very short time, a stream formed of colored water on a horizontal paperboard cylinder covered with absorbing paper, and turning at a suitable speed which one can evaluate.

If it is the discontinuous part which strikes it, the cylinder carries the imprint of each one of the isolated masses, and one can thus determine the number of these masses which are detached in a given time, their relative sizes; and the distances which separate them. If it is the limpid portion, the cylinder presents the imprint of a trail where one does not distinguish any bulge, from which Mr. Hagen concludes that the formation of the drops starts by no means at the opening, but only at the point of the stream where it loses the massive aspect. I do not need to point out that this process is not so sensitive that one can draw such a deduction from it; moreover, the meeting of the limpid portion and the revolving cylinder obviously cannot give birth to notable vibrations, so that the stream is nearly withdrawn from any foreign influence; however, in this case, as I already described (§ 432), the bulges and narrowings can become perceptible only at a rather long distance from the opening.

In the streams that Mr. Hagen subjected to his observations, the masses composing the discontinuous part were not very different in diameter, and did not show under this report any regularity. The regular alternative succession of two unequal diameters arose only in only one circumstance, and these two diameters were between them approximately like 4 to 3. I gave, in § 431, the reason for which, in the downward streams, the masses resulting from the transformation of the filaments are not, in general, small spherules.

Mr. Hagen unnecessarily tried to test the proportionality of the length of the continuous part to the square root of the load: he made several series of observations with openings of various diameters, and in representing the results graphically, by taking for *x*-coordinates the successive loads, and, for ordinates, the corresponding lengths of the continuous part; he always found, even with openings of approximately 3mm, that the line obtained in this manner, instead of belonging to a parabola passing through the origin, was appreciably a line whose prolongation cut the *y*-axis at a certain height above the origin. We saw (§§ 440 and 441) with clearness the law of proportionality to the square root of the load, a law that we know to be related to the theory, is in the observations of Savart with an opening of 3mmn, even starting from the weak load of 4.5 centimetres; I do not suspect the cause of the disagreement above.

Finally, Mr. Hagen, although he could not know my theory, it having appeared in the same year, concludes that the surface stress of the stream appears to exert a great influence on the resolution into isolated masses.

§ 479. In 1851, Mr. Billet-Sélis[265] indicated two new processes for the observation of the stream. The first consists in looking at the stream through my turning disc bored with a radial slit; this process clearly showed him the principal masses and the intermediaries, and finally the bulges which, sliding along the limpid part, prepare the advent of the isolated masses; the author used it to make visible the same details in the image of the stream projected on a screen by means of a lens.

The second process is a clever modification of that of the striped ribbon of Savart: the stream runs out from top to bottom vertically in front of a large concave mirror, while passing through its center of curvature; the real image of this stream thus occupies then same the place as this stream, and the movement produced there with an

[265] *Sur les moyens d'observer la constitution des veines liquides* (ANN. DE CHIM. ET DE PHYS. DE PARIS, 3rd series, vol. XXXI, p. 326).

equal speed, but in the contrary direction.

By laying out the things so that coincidence takes place, for the eye placed in front of the system, between the well lit discontinuous part and its image, one sees the separate and motionless masses distinctly.

The Note of Mr. Billet-Sélis starts as follows:

"At a time when the clever Memoir that Mr. Plateau has just published adds to the interest which always inspired the beautiful research of Savart on the flow of the liquids..."

One will later see (§ 493) why I quote this passage.

§ 480. In 1851 also, Mr. Tyndall published[266] a Note especially relating to a phenomenon already studied by Magnus, which is the introduction of bubbles of air under the surface of a liquid into which a stream falls. This phenomenon refers only indirectly to our subject; however, I will describe the clever process here that Mr. Tyndali employs to disentangle what occurs at the place where the stream reaches the surface of water: this liquid is contained in a white basin, it is highly lit by a lamp provided with a flat wick whose prolonged plane passes through the stream; the shades of the bubbles of air, when these bubbles occur, and the shade of the more or less deformed portion of the surface of the liquid at the place where the stream penetrates, take shape clearly on the bottom of the basin.

To show the continuity of the higher portion of the stream and the discontinuity of the lower portion, Mr. Tyndall places behind the stream, in the darkness, a thin horizontal platinum wire, maintained white-hot by an electrical current; when this wire is behind the limpid part, it appears interrupted by a dark interval, but if it is behind the turbid part, one sees it shining in all its length. Mr. Tyndall employs also an electric spark, but he does not give any detail on the arrangement of the experiment, and is limited to saying that this instantaneous illumination reduced the turbid part to a series of transparent globules. He believes that it is produced by the fall of the turbid part of the stream in the water which receives it, and has its principal cause in the small successive explosions of the bubbles of air which come to burst on the surface.

Let us say, moreover, that at the beginning of his Note, Mr. Tyndall states a singular opinion: according to him, the isolated masses in which the lower part of a water stream is dissolved, attenuate more and more (probably while subdividing themselves) during their descent, and if the stream fell from a sufficient height, there would be no more, below, than a kind of dust liquid; he quotes the cascade of Staubbach, in Switzerland, as providing, on a great scale, an example of this phenomenon.

§ 481. In 1851 also, Mr. Buff wrote[267] a succession of observations on the resolution of a stream into isolated masses. He repeated the experiment of illumination by an electric spark, by using an apparatus of induction provided with a switch with which he could graduate its speed at will. He observed the shadow which the stream thus lit projected on a white screen, and this shadow was perfectly clear. It is in this manner, indeed, that the experiment must be done; Magnus, who, as will be seen (§ 487), had recourse also to an electric spark, but he looked at the stream itself, and could not distinguish the forms of the isolated masses, because, as he says it, each one of these masses is hardly visible but as a brilliant point; Matteucci, who first used a spark (§ 477), does not indicate how he observed.

The spectacle more surprising, says Mr. Buff while speaking about the use of his process, is offered by a stream spouting obliquely upwards: the shadow then produces

[266] *Phenomena of a Water-Jet* (PHILOS. MAGAZ., 4th series, vol. I, p. 105).

[267] *Einige Bemerkungen ueber die Erscheinung der Àuflôsung des flüssigen Strahis in Tropfen* (ANN. OF CHEM. AND PHARMAC. OF MM. LIEBIG, WÖHLER AND KOPP, vol. LXXVIII, p. 162).

the impression of a solid stem, which, at its inflated higher end, throws all the more spheres as the sparks follow one another more quickly.

Mr. Buff had the idea to receive in oil, at a suitable distance from the opening, the continuous part of a water stream running out under a very weak load; oil slows down still more the speed of traverse, already not very considerable, and, in this way also, one distinguishes the successive isolated masses. Mr. Buff concludes from the results of this process, that, under weak loads, almost all the drops are of the same size and follow one another at equal intervals.

Mr. Buff knows my theory, and, to subject it to an experimental test, he receives, in a glass full of water, a stream of the same liquid running out from top to bottom vertically; he raises this glass until there are no more bubbles of air generated, and notes that then the stream runs out quietly, without making any noise; moreover, by observing the shadow projected by electric illumination, he finds that, under normal conditions, the diameter of the continuous part does not undergo periodic variations until close to its end. He concludes from all that that the production of isolated masses cannot depend on the rupture of equilibrium of the cylindrical shape, rupture whose results should start to take their effect even at the opening, and develop more and more until the end of the continuous part.

But initially, as I already pointed out it, the speed with which the transformation is carried out starts by being excessively tiny, it is not a rather large distance from the opening that the phenomenon develops sufficiently to produce significant effects, such as a noise when one receives the stream in water. In the second place, the suspension of the generation of the bubbles of air by no means indicates that one is at the end of the continuous part; it indicates only that by raising the glass, one reached a point where the bulges and narrowings are not very marked. In the third place, it follows by reasons pointed out above, that, in the shadow of the stream one could distinguish the bulges and narrowings from the continuous part only below the point where this part observed directly with the naked eye seems to start to widen; it was seen, indeed, that, even if the stream is not removed from small foreign actions, Savart could not show the existence of the bulges and narrowings close to the opening, even using the very delicate process of the thin section of light. Lastly, as for the bulges and narrowings more developed at the bottom of the continuous part, Mr. Buff does not say that he regulated his electrical appliance so that the period of succession of the sparks coincided, or about, with that of the emission of the masses; however, if this approximate coincidence did not take place, the shadows of these bulges and narrowings would project themselves at different heights, and consequently, by their nonsuperposition, would simply produce the appearance of a widening of this portion of the stream; it is, moreover, what seems to be indicated by the description given by Mr. Buff of the aspect of the shadow in the case of an obliquely launched stream upwards. We know, moreover, that the presence of these same bulges and narrowings at the bottom of the continuous part was clearly noted by Savart by means of the striped ribbon, and by Mr. Billet-Sélis by means of the revolving disc; finally, as will be seen (§ 487), Magnus also observed it in the streams withdrawn from any vibratory influence.

Mr. Buff believes that the separation of each mass is carried out absolutely as in the experiment of Savart on a flow drop by drop: according to him, when, in consequence of the acceleration of the movement, the weight of the drop which is formed at the lower end of the stream becomes sufficient to overcome the cohesion of the liquid, this drop, initially preceded by a tapering, is detached, then the capillary action makes an indentation behind, which then becomes the end of the continuous part, which then causes a bulge, which is detached in the same way in the form of drop, and so on. One has the right to wonder how Mr. Buff, who took pleasure to observe the emission of the isolated masses in the case from an ascending stream, where the movement is delayed, can state such an opinion.

§ 482. In 1855, Mr. Dejean presented at the Academy of Science of Paris a Memoir[268] which, I think, was not published, and of which he gives an extract; he there states, but in a too brief way for one to understand well, singular ideas on the nature of liquids, and indicates how he makes application of them to the phenomenon of the contraction of a stream and the calculation of the flow from arbitrary openings; he explains, according to the same ideas, but always too briefly, the pulsations at the opening supposed by Savart, and claims that he arrives at the laws deduced from the experiment with regard to these pulsations, or, which amounts to the same, of the number of the vibrations corresponding to the sound specific to the stream. Finally, he adds that he explains the formation of the bulges which are propagated along the limpid part; he shows how this part is shortened under the influence of a sound produced in its vicinity, why the stream is all the more sensitive to this influence when the liquid is more compressible, and why this sensitivity increases with the diameter of the openings.

§ 483. In 1855 also, Magnus published the first part of his *Hydraulic Researches*[269]. This work contains, in addition to the curious facts which I recalled in § 234 and 333, observations on liquid streams vertically running out from top to bottom. The author claims initially that, with some care given to the regularity of the apparatus of flow, the liquid which it contains always takes, after a certain time, a rotational movement, from which results that the stream ends up being curved in a kind of lengthened helix. Magnus avoids this problem by depositing on the bottom of the vessel a system made up of four large vertical metal plates forming between them right angles, all directed towards the axis of the opening, but not reaching this opening[270].

He treats initially streams launched from noncircular openings of various shapes; he describes the singular aspects that they present, and gives a theory of these phenomena which is obviously the true one. I will not summarize this portion of the research of Magnus; it is outside my work, except the case of an opening in the rectilinear shape of a slit, a case of which I said some words in § 238.

Magnus begins then the study of streams leaving circular openings. When the opening has a sufficient diameter, such as 12mm, and one produces in the vicinity of the stream a shock, for example by striking a foot on the floor, the stream is divided close to the opening; this phenomenon comes from the vessel being put in vibration and, consequently, the liquid takes, for one moment, in the opening, a movement opposed to that which drives it out to the outside.

In this first part, Magnus attributes the separation of the masses, when all the foreign influences are drawn aside, to the acceleration in the speed of traverse, from which a tearing results in the liquid; he adds that vibrations communicated to the vessel support this tearing, in that then there are in the stream adjacent sections for one of which the speed of traverse is increased, while for the other it is decreased.

At the end of his Report, Magnus resumes the study of the introduction of bubbles of air into the liquid which receives the stream, a phenomenon with which he had already been occupied in another work; finally, he examines the case where air is introduced into the stream itself, and he describes the following fact: when the liquid of the vessel is animated by a rotational movement, its surface grows hollow in the fun-

[268] *Nouvelle théorie de l'écoulement des liquides* (COMPTES RENDUS, vol. XL, p. 46).

[269] *Hydraulische Untersuchungen* (ANN. OF MR. POGGENDORFF, vol. XCV, p. 1.

[270] A discussion was established, about this gyratory movement liquid of the vessel, between Magnus and Mr. Laroque; many experiments led this last to the conclusion that the movement in question inevitably existed in the liquid mass before the flow (see *Ann. de chim. et de phys. de Paris*, 3rd series, 1861, vol. LXI, p.345, and 1863, vol. LXVII, p. 484).

nel, and, after some time, the lower end of this depression penetrates into the stream through the opening, at least when this is not too small; the stream is then transformed, sometimes over a length of several feet, into a liquid tube.

I had thought initially that this tube constituted a film stream; but, in the Report about which I spoke in § 344, Mr. Laroque describes the same phenomenon and represents it by a shape in cross-section, which shows that the diameter of interior hollow space, where it is broadest, i.e. close to the opening, is not a third of that of the stream; in the experiment of Mr. Laroque, the wall of the liquid tube had, in this place, 3mm thickness; at least it cannot thus be regarded as a film. I will further return (§ 505) to this curious phenomenon.

This first part of Hydraulic Research does not make any allusion to my theory; but Magnus later resumed the study of the stream, as we will see (§ 487).

§ 484. In a letter[271] addressed to Magnus in 1856, Mr. Buff also deals with the streams launched from openings of polygonal form; but he reports a curious experiment relating to circular openings: when water runs out from two openings of this species located one close to the other, the two streams, instead of leaving normally in the plane of these openings, are approaching, and can even meet.

§ 485. I must mention here the report submitted by Mr. Maus, in 1856 also, to the Academy of Belgium[272] on my 3rd series, in which I apply my theory to the action of vibratory movements to the stream. Mr. Maus states doubts with regard to this theory; he writes as follows:

"I have difficulty to admit with Mr. Plateau that, in a phenomenon mainly produced by gravity, this force is completely isolated, and to attribute the configuration of the moving mass exclusively to the molecular force, which is extremely lower than gravity."

"My hesitation increased when I noticed that, to justify the elimination of gravity, Mr. Plateau regards this force as acting on the liquid stream only starting from the drain opening, without considering its action on the liquid contained in the vessel, an action which, by the way in which it attracts the liquid molecules towards the opening, exerts an effect that the phenomena known under the designation of *contraction* and *inversion* of the stream do not make it possible to revoke in doubt."

Certainly, it is the gravity which drives out the liquid and thus produces the stream; but once the liquid is animated with its translatory movement after its passage through the contracted section, the molecular forces, interior forces within the system, can obviously, as I already pointed out (§ 431), exert their action freely, whether or not they are less energetic than gravity.

I have not occupied myself with the contraction of the stream; I should have considered the action of the molecular forces only starting from the cross-section of the stream where the contraction finishes, because it is from there only that the stream takes its lengthened form.

As for the inversions which are shown in the streams leaving noncircular openings, they do not have anything in common with the bulges and narrowings which presage separation into isolated masses, since those are carried by translatory movement of the liquid, while the inversions preserve, in the limpid portion of the stream, fixed positions.

Mr. Maus prefers the theory of the pulsations at the opening; he tries to explain these pulsations by considerations different from those of Savart, and it would be difficult for me to summarize clearly: imagining the stream prolonged above the opening into the interior of the liquid, he utilizes the inertia of this ambient liquid entrianed in

[271] ANN. OF MR. POGGENDORFF, vol. C, p. 168.
[272] *Bullet. of Acad. of Belgium*, vol. XXIII, 1st part, p. 4.

the flow, and the loss of momentum which the stream by this communication of movement undergoes, and he seeks to show that the set of these two causes must produce, in the emission velocity, the increases and the alternative reductions. As for separation into isolated masses, he attributes it, like Magnus in work analyzed above, to a tearing produced by the acceleration in the speed of the liquid.

§ 486. In 1856 still, Mr. Fuchs[273] reports the observation of the influence of electricity repeated on a water jet of small diameter (§ 476). He recognized that with a rather narrow opening so that, under a load of 26 inches, the jet hardly rose to 12 inches height, the action of a stick of rubbed glass brought the continuity of the jet, even from a distance of four to five steps. He tries to explain the phenomenon by the consideration that the separated droplets, as soon as they are subjected to an electric induction emanating from a long distance, are turned so as to present the one to the other their parts of contrary electric charges, and that then they meet by the attraction of these electricities.

Later in same the year[274], he went over this explanation, and in itself the inaccuracy showed: among his arguments, he points out that if the explanation in question were true, it would be necessary that one doubled the effect by subjecting the jet to the simultaneous influence of two bodies of contrary electric charge and placed with two dimensions opposed of this jet, while the effect should be cancelled if the two bodies were of the same electric charge; however, he found that it is precisely the reverse which takes place.

He notes that if, by means of a small screen, one intercepts the action of the electrified body on the opening alone, the resolution in drops happens again, while if the action is intercepted on the stream and not on the opening, the continuity of the jet appears. He points out that the continuity of the jet is also established by a suitably moderate direct electrification.

He attributes the resolution into drops to adhesion between the opening and the liquid; to support this opinion, he employs a brass opening wetted with an oil, and then sees a small jet left to itself remain continuous in all its length. He concludes that the electric induction acts simply by destroying the adhesion of the liquid with the opening; but, in addition, he is astonished that so weak an action can cancel this adhesion, while it does not appear to affect the cohesion of the molecules in the water, cohesion which is, however, according to him, much lower still; he finds, moreover, that the phenomena are the same with a metal opening or a glass opening, i.e. conducting or not conducting of electricity.

The experiment, repeated by Mr. Logeman in front of the Provincial Society of Utrecht, caused, between the Dutch physicists, a discussion[275], but does not teach us anything significant.

§ 487. In 1859, Magnus gave the second part of his Hydraulic Research [276]. He continues his observations there on the downward vertical streams leaving circular openings; he repeats the observations of Savart relating to the effect of sounds, but he notes that under very weak loads, such as two or three centimetres, the stream is influenced by all the sounds produced in its vicinity, except for those very high. He shows, moreover, that the action of the sounds on the stream results especially from the vibrations of the bottom of the vessel; for that, he says install things in the following

[273] *Ueber das Verhalten eines feinen Springbrunnens innerhalb einer elektrischen Atmosphäre* (BULLET. DES TRAVAUX DE LA SOCIÉTÉ DES SC. NATUR. DE PRESBURG, vol. I, p. 79).

[274] *Mem. de la Société des Sc. natur. de Presburg*, p. 37.

[275] *Verhouding van eene klein fontein in een electrische atmospheer* (UTECHT, ANTEEK. PROV. GENOOTS. 1858-59, p. 18).

[276] *Ann. of Mr. Poggendorff*, vol. CVI, page 1.

way: the bottom is bored with a rather broad opening, from where leaves a thin rubber tube going down vertically and closed, at its lower end, by a metal plate in which is made the opening; this plate is of a diameter much larger than that of the tube, and rests by its edge on cushions. Now, by employing this system, which makes the opening independent of the vibrations of the bottom of the vessel, Magnus recognized that the sounds did not act any more.

To show that the swellings are formed by isolated masses which pass quickly, Magnus introduced to a small depth into a swelling, the end of a wire that he holds with the hand, and then feels the impression of an energetic movement of vibration, while when he introduces the end of the wire into the limpid part, he undergoes only the feeling of a uniform pressure; a flame brought close to this limpid part remains quiet, and is shown on the contrary agitated when one maintains it close to a swelling. Magnus used, moreover, a revolving mirror; the experiment is done in a darkened room; a portion of the stream is strongly lit, the mirror, placed at one meter of distance, turns around a vertical axis, and one observed the image of the portion in question. If this portion is a swelling, the image is composed of a series of tilted brilliant lines.

Finally, Magnus again employs the electric spark and my revolving disc. When the stream is withdrawn from any vibratory action, this last process does not show him bulges and narrowings in the limpid portion; but it makes him recognize the existence of similar bulges and narrowings increasingly pronounced starting from the point where the turbid portion starts until where the masses are isolated. He adds:

"The resolution of the stream into isolated masses thus takes place in a way completely similar to that which was already described by Mr. Plateau."

Always by means of the turning disc, Magnus finds that, even in the stream above, some of the isolated masses are flattened and others lengthened, but they do not all isolate at the same distance from the opening, and thus do not pass all at the same points in their maximum flatness and lengthening, so that the appearance of swellings cannot result from it. He notes, moreover, in the sizes of these masses, the same irregularities as Mr. Hagen (§ 478). When the stream leaves an opening of a very small diameter (less than one millimetre), and it is under the influence of a sound, Magnus sees the isolated drops follow one another in regular series of a determined number, series leaving between them larger intervals, and he says that these large intervals correspond to the ascending vibrations of the bottom of the vessel. Finally, it is arranged so as to obtain a flow of simple drops following one another rather slowly, and then he discerns the production of the filaments and their conversion into spherules.

When the stream is withdrawn from any vibratory influence, and Magnus observes the limpid portion using the turning mirror, he sees only one broad luminous space; but if the stream is under the influence of a sound, the image presents a series of brilliant oblique lines, which then shows the existence of bulges and narrowings being propagated in the portion of the stream located above the first swelling.

By approaching the limpid portion of a stream with an electrified body, Magnus sees it simply deviated towards this body; but if the stream has regular swellings, in a manner such that one distinguishes, in the axis of these swellings, the appearance of a thinner stream (n^{o} 12 of § 448), and if the electrified body is approximately in the middle of one of them, only the thin stream is deviated, because of the small mass of the drops which make it up, and are shown isolated outside the swellings. Magnus deals then with streams ascending either vertically or obliquely. He notes initially, by means of the turning disc, that things happen in the same manner as in the streams vertically downward; he notices only that the bulges that one distinguishes towards the end of the continuous part then are brought closer to each other. In the case of the obliquely ascending streams, he attributes the formation of the sheaf to the tube which carries the opening undergoing vibrations normal to the axis of the stream, vibrations which are transmitted to it, and from which results that the successive isolated masses leave the continuous part according to slightly different directions; he affirms that, under the influence of a sound, one feels with the hand these transverse vibrations of the tube. He looks at the separation of the discontinuous part of the stream into two or three distinct jets as due to the same cause, and he points out that these effects necessarily occur only for a fixed ratio between the period of the transverse vibrations of the opening and that of the emission of the isolated masses.

Lastly, during separation into two jets, observation through the turning disc makes him recognize, in the isolated masses, an arrangement such that all the masses of an odd nature describe one of the two trajectories, and all the masses of an even nature the other trajectory. In the case of three jets, he notes a similar provision. He presents, one sees, an explanation of these phenomena very different from mine; I will return there in § 495.

Magnus completes this remarkable work by studying the action of the sounds on the streams launched from noncircular openings.

§ 488. In 1860, Mr. Reitlinger[277] studied, like Mr. Fuchs, the phenomenon of the action of electricity on a water jet of small diameter. He speaks initially about old experiments of Desaguliers, P. Gordons and Nollet relating to the attraction exerted on a small water jet by an electrified body, with the variations which the flow undergoes when the jet is electrified directly, and with the scattering a jet undergoes, either by a direct electrification, or by a sufficiently strong electric induction; we do not have to occupy ourselves here with these effects. The author repeats the experiment of the continuity of the jet brought about by a not very intense direct electrification, and points out that this experiment is enough to show the error of the first explanation suggested by Mr. Fuchs, since here there is only one species of electricity communicated to the jet, and that thus polarity cannot be established in the drops which follow one another.

He subjects to the action of electricity either direct, or by influence, a small jet formed of a nonconducting liquid, spirits of turpentine, and finds that this action does not modify the resolution into drops.

He adopts the last explanation of Mr. Fuchs on the way in which the electric induction with regard to the small water jet acts, and he tries to solve the difficulty raised by this scientist himself, by claiming that, under the action of electricity, a weak electrolysis of water occurs, from which results, between the liquid and the interior wall of the opening, the formation of a very thin gas layer which cancels adhesion. To subject this new assumption to experimental test, Mr. Reitlinger employs mercury, which is at the same time electrically conducting and indecomposable, and he affirms that, even in the absence of any electric action, the jet is perfectly continuous until its apex; he employs an amalgammed opening, and sees the jet then being dissolved in drops as in the case of water; he notes, moreover, that electrification either direct, or by influence, does not make this last jet continuous. I will return (§ 494) to these experiments[278].

§ 489. We have, in 1866, the singular observations of P. Lacouture[279] on a water stream that vertically runs out from top to bottom from the opening of a tube on the base of a vessel of Mariotte, and things are regulated so that the flow takes place at very low speed; the opening of the tube has 7mm diameter, and the stream is received on a resistant plane placed at a distance from the opening which can vary from 17mm to

[277] *Ueber die Entwickelung der Electricität Springbrunnen*(BULLET. OF ACAD. OF VIENNA, vol. XXXIX, p. 390).

[278] See the article listed in § 508 under nº 17.

[279] *Les Mondes*, 2nd series, 1st year, vol. II, p. 73. See also (ibid), pp. 237, 429, and following volume p. 10, for a small discussion between the author and me.

25mm. Under these conditions, the stream, though limpid and continuous, presents a succession of bulges and narrowings definitely marked and occupying fixed positions. For a less distance, which is 8mm approximately, it happens sometimes that the flow stops suddenly: "the opening", says the author, "is then closed by a drop which one managed to isolate one moment, and whose surface forms like a membrane. The contact of a body, even slight, breaks the screen of this membrane, and the flow starts again."

These curious effects presented by an animated stream at a very low speed, appear unable to be attached to any known principle.

Already in 1819 Belli, in the Report in question in § 322ter had announced a fact more or less similar to the preceding. This fact consists of that if a filament of water is received, running out from top to bottom vertically, in a spoon filled with the same liquid and held one decimetre from the opening, one notices, in the lower part of the stream, equidistant narrowings, brought very close and motionless. By raising and lowering the spoon successively, one sees the narrowings rise and also drop, and at the same time increase and decrease length.

§ 490. In 1867, the *Philosophical Magazine* gave[280] an extract of the work of Mr. Tyndall on sound; the author speaks there about the liquid stream and the action which sound vibrations exert on it, but he hardly makes known any new results; let us note a curious experiment, however: a water stream being launched obliquely upwards so that the discontinuous part forms a sheaf, Mr. Tyndall makes resound in the vicinity two selected tuning forks in order to exert an energetic action on the stream and to make heard four beats a second; he sees the sheaf then vanishing and reproducing periodically, in synchrony with the beats. Let us add that Mr. Tyndall adopts the assumption of the pulsations, and attributes those to fluid friction against the edge of the opening.

§ 491. In 1867 also, Mr. Rodwell[281], after having described facts outside our subject, deals with the constitution of downward liquid streams, and adopts my theory fully. He insists on the generation of the smaller masses interposed between the larger in the discontinuous part, and described an experiment suitable to show in an easy way the production of the filament and its spontaneous transformation, when a liquid mass separates into two. This experiment consists in introducing, in a mixture of water and alcohol having a density not much higher than that of oil, a mass of this last liquid forming a sphere from three to four centimetres in diameter; then, when the sphere is assembled on the surface and changed there into a kind of hemisphere having its base on the level of the ambient liquid (§ 11), to heat it; the mass then lengthens from top to bottom in an approximately cylindrical shape with a hemispherical lower base, after which it is narrowed about its middle, the filament appears and is converted into spherules, and the lower mass goes down to the bottom from the vessel, while the higher stays adherent to the surface of the alcoholic mixture.

§ 492. Lastly, in 1869, Mr. Buff, in a Memoir[282] having especially for its object the intensity of the impact of a liquid stream against an obstacle, claims that one of the causes which decrease the height of a stream spouting upwards, is a capillary pressure which is exerted at the end of the continuous part each time that a mass has just been detached. In support of this opinion, he says to have noted, by observing the shadow of such a jet projected by electric light (§ 481), that each time that a mass is isolated, the continuous part undergoes a movement of retreat.

[280] *On the action of sonorous vibrations on gazeous and liquid jets* (PHILOS. MAGAZ., 4th series, vol. XXXIII, p 375).

[281] it One some effects produced by a fluid in motion (ibid, p. 99).

[282] *Versuche ueber den Stoss of Wasserstrahls* (ANN. OF MR. POGGENDORFF, vol. CXXXVII, p. 497).

He does not say that the stream of his experiments was put under the influence of a sound which would have regularized the phenomenon of the emission of the masses, and consequently the masses were undoubtedly isolated successively at slightly unequal distances from the opening; it is what results, indeed, as for the streams vertically downward, in the experiments of Savart and Magnus; and what I explained (§ 469) on the causes of the formation of the sheaf in the streams rising obliquely, makes very probable that the same thing takes place with regard to those; however, in these conditions, I wonder how, by means of his electric illumination, Mr. Buff could recognize the retreat about which he speaks.

Moreover, we know that immediately before the separation of a mass, that which follows it, being still between two filaments, must be more or less lengthened in the direction of the axis of the stream, which at the moment of the rupture of the leading filament, is flattened to then separate in its turn; this flatness produces thus indeed a small retreat of the leading portion of the mass in question. Moreover, as the narrowing which gave rise to the leading filament was at every moment in a phase a little more advanced than the trailing narrowing, it had to send its liquid into the mass considered a little more quickly than this trailing narrowing; from there a small retreat of this whole mass, or rather a slight reduction in its speed of traverse. But all these effects of retreat must be extremely tiny, and besides they are compensated exactly by opposite effects which occur then with the trailing part of the same mass; thus, apart from the irregularities of the emission of the masses, that of which we occupy ourselves will leave the continuous part with the speed of traverse of the liquid, and the height to which it will arrive, a height which measures that of the jet, could be by no means influenced by the retreats which I mentioned.

§ 493. One sees, by this history, how many clever processes were imagined to study the constitution of liquid streams; but one sees at the same time the inadmissible assumptions one had recourse to in order to explain the resolution of the stream into isolated masses: that of the pulsations at the opening, even when the stream is under the influence of a sound and that thus these pulsations actually exist, is completely insufficient to explain the progressive development of divisions, and it is much more still when the stream is under the influence of any vibratory action; as for that of the tearing caused by the acceleration of the speed of traverse, it is contradicted by the fact that the ascending streams are dissolved just as easily into isolated masses as the downward streams.

One sees, moreover, that except for Mr. Rodwell, perhaps for Mr. Billet-Sélis, and perhaps, also for Magnus in the second part of his Report, the physicists who dealt with liquid streams since 1849, the time of the first publication of my theory, did not adopt it; Tyndall, Dejean, Fuchs and Reitlinger appear to have been unaware of it, which astonishes me little, because one could not suspect the existence of it according to the title of my series; Buff and Maus knew it and rejected it while being themselves based on arguments easy to refute. Moreover, my first exposition could, with rigor, more or less lend itself to the objections of Mr. Buff; moreover, when Buff and Mauss wrote their articles, I had not established yet by calculation the absolute need for the spontaneous transformation of the lengthened liquid cylinders, and these two scientists could not regard as rather conclusive the experiments which I had described. I am quiet on the future of my theory; such as I present it today, it will be adopted, because it must be it when it is well known[283].

§ 494. But I must return to some of the work summarized in what precedes, and initially to that of Mr. Fuchs and Mr. Reitlinger, whose experiments seem, at first

[283] For research after 1869, to see the articles registered with § 508 under the n^{o} 14, 17 and 21.

glance, to accord with difficulty with my theory.

While reasoning according to it, one could believe that electricity acts by itself to be opposed to the spontaneous transformation of the jet; and the idea which arises naturally is that this electricity, while going on the surface of the liquid, determines a repulsion between the molecules of the surface layer, and decreases consequently the capillary forces. But the still new experiments of Mr. Van der Mensbrugghe prove that static electricity does not change the tension of liquid surfaces, and thus does not alter the capillary forces which emanate from these surfaces; finally, I sought to assure myself, by a direct means, if the static electricity can modify the phenomenon of the spontaneous transformation of liquid cylinders of small diameter: for that, I repeated the experiment of §§ 361 to 363, by making a metal wire communicate from the driver of an electric machine to the amalgammed ends between which extended the thin cylinder of mercury; however, as soon as the side obstacles were removed, this cylinder, which had a lower diameter than 1mm, changed just as easily, and in the same way, when it was strongly electrified, as when it was in a natural state. It thus should be recognized that the static electricity does not exert a direct action on the phenomenon.

I believe correct the idea of Fuchs and Reitlinger, according to which the fact that they studied would be due to a destruction of the adhesion of the liquid at the edge of the opening. Only I then suppose, with Mr. Reitlinger, that this destruction comes from a weak electrolysis of water, and I think that it results simply from what, under the influence of electricity, the liquid and the solid mutually pushes back. Now it remains to show how the destruction of adhesion determines the continuity of the water jet.

The thing appears extremely simple to me: let us notice initially that the opening employed was very narrow; Mr. Van der Mensbrugghe, who repeated part of the experiments, ensures me that their full success requires an opening having less than 1mm diameter. Let us notice, moreover, that the fluid friction and adhesion at the edge of a so small opening have a considerable influence, and that the water jet of Mr. Fuchs did not rise even to half the height of load; however, one will easily admit that this intense friction causes a vibratory movement in the water of the jet, and consequently the transformation must be activated. As I already pointed out, Savart, who operated under similar loads, could not note, in his experiments, any influence of this kind; but the narrowest opening of which he made use had 3mm diameter, and if one reflects that, the smaller the opening, the larger is the ratio of its contour to the sectional surface area of the stream which passes there, one will understand that the influence of friction and adhesion on the streams of Savart can have been too weak to produce an appreciable effect, while it is not thus any more with an opening of a very small diameter. The stream of Mr. Weisbach (§ 477) showed us a first example of the probable influence of fluid friction; there the opening had a rather large diameter, but the load was enormous.

From this viewpoint, electricity would not cause by itself the continuity of the small water jet; it would not remove a foreign cause which accelerates the transformation, and, in the jet thus left to only its shaping forces, the separation of the drops would not yet completely be carried out when divisions reach the top.

It is indeed what the experiments of Mr. Reitlinger prove on the small mercury jet: as one saw, when the opening was not amalgammed, and that thus there was adhesion, the jet, although not subjected to the electric induction, was continuous in all its length; it was thus in its natural state. With an amalgamamed opening, and consequently with a strong adhesion, the vibrations caused by it accelerated the transformation, as do vibratory movements in general, and discontinuity appeared; finally, the electric induction did not restore the continuity of this last jet, because, between mercury and the amalgammed opening, there was metal continuity, and consequently impossibility of a repulsion between the liquid and the solid.

The same considerations explain why the small water jet of Mr. Fuchs was continuous, in the absence of electricity, when it left by an oiled opening: adhesion and friction being much decreased, the jet took its natural state, a state in which divisions arrived at the top before their complete transformation; with the opening not oiled, on the contrary, the vibrations resulting from adhesion made the transformation faster, so that it was completed at a less distance from the opening; finally, under the influence of electricity, adhesion being destroyed, the jet again took its natural state.

It is also easy to explain the singular effects that Mr. Fuchs observed by intercepting the electric induction by means of a screen; when it sheltered only the opening while leaving all the remainder of the jet under the influence of electricity, discontinuity remained in spite of this influence, because the vibrations of the opening could freely occur; when, on the contrary, the screen sheltered the remainder of the stream, without sheltering the opening, the jet was continuous, because the vibrations did not take place any more.

§ 495. In §§ 469 to 473, I described a theory of the phenomena presented by obliquely ascending streams, on the basis of an assumption which appeared very rational to me; however, at the end of his Report, Magnus, as we saw (§ 487), explains the same phenomena with the assistance of an absolutely different assumption; thus let us see if one can decide between the two.

According to Magnus, the sheaf would be due to vibrations of the opening normal to the axis of the stream, vibrations which would be transmitted in the latter; but, as Savart observed, the sheaf is contained entirely in a vertical plane, i.e. in that which passes through the initial direction of the stream; one would thus need, which must appear not very probable, that the transverse vibrations of the opening always took place in this plane; it would be necessary, moreover, that by removing the causes of vibrations, one removed the sheaf, or, at least, one decreased the amplitude of it; however, it was seen that, in my experiments, the sheaf remained the same after I had taken all the precautions to cancel the vibratory movements. One could admittedly still find a cause of vibrations in the continual release of the bubbles of air generated by the upper tube of the vessel of Mariotte, a release which I had let remain; but, as all the other causes had been isolated, I should have seen at least the sheaf becoming narrower, which did not take place. Finally, Magnus recognized that, in the streams vertically downward and completely withdrawn from the vibratory actions, the masses do not isolate all at the same distance from the opening, which shows that, in such a stream, successive divisions, and consequently successive narrowings, are not completely identical; however, this lack of identity must obviously exist the same in the streams obliquely ascending, and I showed that the sheaf is the necessary result.

Let us pass to the conversion of the sheaf into one, or two, or three distinct jets. The assumption of Magnus leads to this conversion under the influence of its specific sound, the lower octave and the double lower octave. Indeed, in the opening, the supposed transverse vibrations must correspond to the vibrations along the axis of the stream, and, in addition, apart from any theory, it follows from the experiments of Savart that its specific sound is that for which the vibrations are of double the period of the emission of the isolated masses; if thus the sound instrument emits the specific sound, and if a mass isolates itself at the beginning, for example, of a transverse vibration at the end of the continuous part, it will be the same for all the following masses: all will leave the continuous part thus in the same direction, and there will be only one single jet. Under the influence of the lower octave, one easily sees, the masses will be launched alternately at the beginning and the end of each simple transverse vibration, and there will be consequently two jets. Finally, one also sees that, under the influence of the double lower octave, the masses will leave alternately at the beginning, the middle and the end of each simple transverse vibration, from whence three jets.

But if one tries to apply the same theory to the case of the sounds ranging between the lower octave and the double lower octave, one arrives at results in dissension with those of experiment: I pointed out that, from the lower octave to the third below, I always obtained two jets, and that between this last sound and the double lower octave, there were sometimes two, sometimes three jets; however, on the basis of the assumption of Magnus, one will find that it should occur, for the second below the octave, seventeen jets, for the third seven jets, the fourth five jets, the sixth nine jets, and for, seventh seventeen jets; for the fifth only, one arrives at one of the results of the experiment, which is two jets.

In addition, according to my assumption, the great divisions caused by the vibrations in the direction of the axis of the stream, split, after their separation from the continuous part, into two or three masses by the action of the abnormal shaping forces; however, the reality of such a fractionation is proven by the experiments of Magnus on the downward stream, outgoing from an opening of very small diameter and subjected to the influence of a sound: this stream being extremely thin, the specific sound which corresponded to it would be extremely high; it is thus quite probable that the sounds that Magnus made act on it were relatively very low, and that thus each division formed under the influence of one of these sounds was extremely long. However, it was seen that the discontinuous part of the stream in question was composed of series each including the same number of equidistant masses, these series being separated by larger intervals, and it is clear that the middles of these large intervals correspond to the middles of long narrowings of divisions due to the vibrations; it is thus, moreover, that Magnus interprets the same intervals, since he attributes them to the ascending excursions of the opening; it is clear also that the masses of each series result from the fractionation of the divisions in question. Let us add that, in the illustration which accompanies the Report by Magnus, each series includes three masses, which again agrees with my explanation. There is no reason besides so that the things observed in a downward and very thin stream, also do not occur in streams thicker and obliquely rising.

But consequently one is inevitably led to speak of my theory. Indeed, narrowings due to vibrations being born under an action more powerful than that from the shaping forces, they must break before those which these last forces cause; however, let us suppose our oblique stream subjected to the influence of a sound which changes the sheaf into two or in three jets; the period of emission of large divisions being that of the vibrations double the sound used, all these divisions will necessarily escape in the same direction, whether or not there are transverse vibrations, and since the fractionation does not take place in each large division, that after its departure, the masses which result from this fractionation will also leave originally according to the same direction; if thus they run on different trajectories, it is necessary that that comes from differences in speed.

In truth, the expressions which Magnus used appear to indicate that he saw, through the revolving disc, all the masses isolate themselves at the same distance from the opening, while, according to me, the points of separation resulting from the fractionation of large divisions are necessarily further away from the opening than the point where successively these large divisions are isolated; but it is possible that the revolving disc does not make it possible to observe this last circumstance easily, or it could escape the attention of Magnus.

I do not need to point out that the symmetry of arrangement of the isolated masses is also well explained in the two theories.

§ 496. We saw in § 344 that Mr. Laroque obtained a kind of film stream: now, if it is considered, on the one hand, that the film shapes, consisting of a single film, are subjected, as for their forms and with their stability, to the same laws as the full shapes, and, on the other hand, that, in consequence of the smallness of their mass, their spontaneous transformation, in case of instability, is extremely fast, one will understand that with less than a particular resistance to the transformation, a laminar stream can have only an extremely short continuous part, except the case of an enormous speed of translation. In the experiments of Mr. Laroque, where the liquid of the vessel was animated by a rotational movement, the cause of resistance is obvious, it is the centrifugal force of the liquid of the laminar stream; this stream admittedly presents bulges and narrowings; but they occupied fixed positions, instead of going down with the liquid as the divisions of a full stream do it. I will return soon to the stream of Mr. Laroque.

§ 497. I tried to obtain a laminar stream with a liquid which was animated by no rotational movement, by simply making this liquid run out through a narrow circular slit. The vessel, of cylindrical form, was built out of tinplate; it had 40 centimetres height, and a diameter of 10; in the middle of its bottom a circular opening of 30mm diameter was bored; a part about which I will speak below maintained in this opening a tin disc of 29mm of diameter, whose plane coincided with that of the bottom, and its center with that of the opening; in this way ran, between the respective edges of the opening and the disc, a continuous annular space of 0.5mm of width. The part which supported the disc in the opening was a bent tube with a right angle, whose end opened in the side wall of the vessel, and the other in the center of the disc; this tube thus allowed a free access to the air in the interior of the stream. Lastly, to avoid any rotation of the liquid, the vessel contained a system of four plates with right angles, similar to that which Magnus (§ 483) employed; only, as the slit was to remain entirely free, these plates did not go down to the bottom.

I expected that, this vessel being fixed on a suitable support, then filled with water and maintained full, the liquid outgoing by the circular slit would form a laminar stream which would operate its transformation at a small distance from this opening, so as to give, from there, a continual succession of hollow bubbles several centimetres in diameter. But it was not, as follows: water constituted simply a laminar bag 6 centimetres long, whose meridian line had a rather low convex curvature, and a tip from which descended a full stream. The phenomenon remained the same when I stopped the tube which ended in the center of the disc.

I told myself whereas the bag would undoubtedly be lengthened, and perhaps would manage to carry out conversion into bubbles, if one substituted for water a liquid having much less tension, and in which, consequently, the capillary pressures, which are born of curvature and which cause closing of the bag, would be much weaker; for this purpose, I employed alcohol; but there was no other modification than an increase the length of the bag, a length which reached 9 centimetres.

Thus, although the load was rather strong, although the diameter of the stream, at its exit, was considerable, and the capillary pressure due to the transverse curvature was consequently not very intense; finally, although the lamina was very thick compared with ordinary liquid films, and that, consequently, the low capillary pressure had to act on a relatively large mass, this pressure, in the absence of any centrifugal force, was enough to close the stream within 6 or 9 centimetres of the opening, according to the degree of tension of the liquid.

§ 498. I wanted to know what would occur if, while using the same apparatus, I impressed a movement of rotation to the liquid of the vessel. The system of the four plates was thus removed, and, the vessel being full of water, I introduced there a rod that I made turn, initially with a speed of approximately two turns a second. Under these conditions, I obtained a result similar to that of Mr. Laroque: the bag based on the slit had a 7 centimetres length, and, from its tip, which was not closed and constituted thus a narrowing, extended a laminar spindle of approximately 13 centimetres length and 2 of equatorial width; finally, below the lower point of this spindle, the liquid scattered in divergent drops.

I then doubled the speed of whirling of the rod; then the narrowing took a diameter equivalent about to half of that of the slit, and the spindle opened at its lower part, from the edge of which drops left while scattering; the overall length of the shape was 23 centimetres.

§ 499. Let us try to explain these results. If the gyratory movement of the liquid is not intense enough so that, at the exit of the opening, centrifugal force counterbalances capillary pressures due to transverse curvature, the stream will initially narrow, and molecules of liquid, which turn around the axis while they go down, will describe helices approaching the axis. However, during this trajectory, the horizontal component of the speed, that which constitutes the rotational movement, can be regarded as keeping the same intensity and consequently the centrifugal force will be increasing since, for a constant absolute velocity, it is inversely proportional to the distance to the axis. In addition, the capillary pressure will also increase, by the increase in the transverse curvature, and if one disregards meridian low curvature, one easily sees that this pressure will be also be inversely proportional to the distance to the axis. It would thus seem that, the two opposite forces growing according to the same law, the second, which overrode the first at the exit of the opening, must continue to override it, and thus the bag must close completely; but, as the film is tightened, it increases in thickness at the same time as it decreases in surface area; and as capillary pressures are exerted only in two layers of surface, if one considers that which corresponds to an element of the film, one sees that, driving an increasingly large mass, its action will grow, actually, less quickly than indicated by the law above; on the contrary, the centrifugal force being exerted as well on the molecules of the interior of the layer as on the surface molecules, its action will grow, in each element, by the increase in the mass, more quickly than the law in question wants it. Consequently, unless the gyratory movement is not too slow, the two opposite actions will become, at a certain distance from the opening, equal between them; but, in its movement towards the axis, the liquid, under the terms of its inertia, exceeds the point of equilibrium, so that the centrifugal action is then in excess, and, when this excess destroyed the effect of inertia, it draws aside the liquid from the axis gradually, to form the higher half of the spindle; however, in this spacing, the opposite effects of the preceding occur: the layer decreases thickness, the centrifugal action decrease, consequently, more than the capillary action, equilibrium is reached, then exceeded under the terms of inertia, so that the capillary action dominates again and brings anew the liquid closer to the axis, to form the lower half of the spindle. Finally, it is understood that, under suitable conditions, the same causes can determine the generation of other spindles below the first, and it is thus, I think, that one must explain the phenomenon observed by Mr. Laroque.

If the gyratory movement is such that at the exit of the opening, the centrifugal force neutralizes the capillary pressure exactly, equilibrium will continue to remain below; because, due to what precedes, if the liquid approached the axis notably, the centrifugal action would carry it outward, and if it deviated notably from the axis, the capillary action would become dominating. If thus, in the second experiment, one had been able to increase the number of revolutions further, it is quite probable that an almost cylindrical stream would have been obtained. Only, in this case of the equality between the centrifugal action and the capillary action, the acceleration of the descent of the liquid obviously tends to make the layer increasingly thin as one moves away from the opening, from which results a progressive reduction from the centrifugal action, and consequently a gradual preponderance of the capillary action; such a stream should thus be narrowed somewhat, in order to maintain in the layer the thickness which is appropriate for the equilibrium of the two actions; it should thus constitute not a cylinder, but a kind of very lengthened cone.

§ 500. Let us return to the flow without gyratory movement. I said myself that one would obtain probably the resolution of the laminar stream into bubbles if one injected air in its interior; I thus adapted, on a side opening of the tube which supports the small disc in the apparatus of § 497, a large bladder filled with air; there is maintained in the vessel a load of 20 centimetres, and, while the liquid left by the slit, I compressed the bladder. The liquid bag lengthened, and I noted, indeed, a succession of bubbles approximately 5 centimetres in diameter, which were renewed without delay as long as blowing lasted. When the stream was looked at from the side, these bubbles passed with too much speed for me to assure myself clearly of their existence; but I distinguished them extremely well by looking at the stream from top to bottom at a great obliqueness. Once these same bubbles were recognized, I sought, by observation from the side, to determine at which distance from the slit they were isolated, and I found, in so far as the eye could judge it, that this distance lay between 15 and 20 centimetres.

I wondered then if it would be possible to arrive at the same result without blowing, by increasing the load considerably; but I arrived at a negative conclusion.

Initially, as we saw, under a load of 40 centimetres, the laminar bag of water, in the absence of rotation of the liquid and of blowing, has 6 centimetres length; by reducing the load to 20 centimetres, i.e. to half of the preceding one, the length of bag was no more than 4.5 centimetres; however the ratio from 4.5 to 6 is 0.75, and that of the square roots of two loads is very close to 0.71; these two ratios hardly differing one from the other, and measurements of the bag being necessarily only approximate, one can claim that the length of the bag is proportional to the square root of the load.

In addition, if we indicate by h the load, by θ the time which a division of the laminar stream employs to convert into bubbles, and by e the space which the liquid traverses during this time, we will have, by a known formula,

$$e = \theta\sqrt{2gh} + \frac{g}{2}\theta^2.$$

But, for h = 0.20m, e is, we saw, ranging between 0.15m and 0.20m and, to place us under the most favorable conditions, we will take e = 0.15m. If we substitute these values of h and e, as well as the known value of g, in the formula above, we draw from it

$$\theta = 0.065'',$$

and this value must be appreciably constant, i.e. independent of the load; it gives, for the term $\frac{g}{2}\theta^2$, the also constant value 0.021.

Now, because of the smallness of this last term, one sees that e, i.e. the space traversed during the formation of a bubble, is also about proportional to the square root of the load; however, under a load of 20 centimetres, and without blowing, the bag, of which the length is only 4.5 centimetres, is much shorter than the space of 15 centimetres necessary, under this load, for the resolution into bubbles; if thus, always without blowing, one gradually increases the load, the length of the bag and the distance covered during time θ both stay about proportional to the square root of the load, the second will dominate more and more over the first, and thus one will never be able to arrive at the resolution into bubbles. One could hardly hope for success by making the layer thinner, which would decrease θ and, consequently, e; because one would decrease at the same time, and undoubtedly in such a strong proportion, the length of the bag produced without blowing.

Our laminar stream, when air was injected, had, according to what precedes, a continuous part from 15 to 20 centimetres, under a load of 20 centimetres; but this stream had, at the opening, a diameter of 3 centimetres; if the annular slit had had only the diameter of the ordinary liquid streams, 6mm for example, it follows from the laws of the transformation of cylinders (§ 384) that, under the same load, the continuous part would have been only 3 to 4 centimetres; and if the layer, instead of being thick, had had the thinness of the films which constitute soap bubbles, this continuous part would have been much shorter still.

§ 501. The string of small resin bubbles that a child showed to Morey (§ 323) can be regarded as a laminar stream into which one injects air and which is solidified during its transformation. If one wants to describe in a more precise way how such a necklace could be formed, one must admit that at the moment when the end of the tube left the molten resin and when one starts to blow, a bubble develops; but that, seized by the external cold and consequently not being able to continue to grow bigger, it tends, driven out by the air which arrives in it, to be detached from the tube, initially leaving behind it a tapering; this is solidified in its turn before being able to break, then a second bubble is formed, which is seized in the same way by the cold, then a third, and so on. It is clear, moreover, that, to get a good result, the breath must be quite regular and of a suitable intensity.

§ 502. A full liquid stream which runs out in the air is a liquid current which crosses a gas; however, one can take the opposite conditions: one can wonder what is the constitution of a gas current which crosses a liquid, and to seek if the theory envisages or explains it. That is what I will examine.

Imagine that from a tube leading vertically upwards from the bottom of a liquid, a gas that this liquid does not dissolve is released abundantly, as, for example, in the process which chemists use to collect certain gases under a bell originally full of water. The molecules of this gas, at their exit from the tube, being actuated by a vertical movement upwards, tend to preserve this same movement, and consequently the gas current tends to cross the liquid in the shape of a continuous vertical cylinder, extending from the opening of the tube to the upper surface of this liquid. But two distinct causes are opposed so the current takes this form: the first is the side hydrostatic pressure of the liquid, pressure which is increasing from the liquid surface down to the level of the opening of the tube; the second consists of the molecular shaping forces being exerted in the liquid wall which bounds the current.

Let us disregard initially this second cause, and seek what would be the shape of the current under only the combined influence of the force which pushes gas upwards and of the hydrostatic pressure of the liquid. The gas tends, as we saw, to carve in the liquid a vertical cylindrical channel; but the liquid, due to its hydrostatic pressure, should tighten this channel, while leaving its form of revolution, so that the liquid wall which limits the current would be, starting from the contour of the opening of the tube, tilted on all the sides towards the axis. Accordingly, on the assumption that we placed ourselves apart from the molecular shaping forces, if, into an unspecified point of the liquid wall, one breaks up, in a meridian plane, the vertical force of the gas molecules into two other forces, one tangent and the other normal with respect to the meridian line, it will be enough obviously, for the equilibrium of shape, that this last component is equal to the hydrostatic pressure of the liquid at the same point. However, this pressure is decreasing from the opening to the higher level, and consequently, so that the normal component of the upward force decreased as the pressure, it would be needed that the liquid wall was straightened gradually starting from the opening of the tube, until becoming completely vertical at the higher level, where pressure being null, the normal component should be also null.

One thus sees that, if the molecular shaping forces did not exist, the channel with a liquid wall would present in hollow, and upwards, a form similar to that which is presented in relief, and from top to bottom, by the seemingly smooth part of a liquid stream running out from a circular opening bored in a thin wall in the horizontal base of a vessel, and we know that, in all the extent of this smooth part, the effect of the shaping forces remains very marked; moreover, just as the smooth part of a liquid stream all the more approaches to be cylindrical as the rate of flow is larger, in the same way our channel would all the more approach to constitute a hollow cylinder as the speed of gas would be more considerable.

§ 503. But as the molecular shaping forces exert their action, events cannot happen in this manner. As I already pointed out several times, the equilibrium conditions and stability, from the point of view of the molecular forces, are the same for a liquid shape in hollow and a liquid shape in relief; however, we know, under the sway of the molecular forces, the liquid which constitutes a stream passes gradually, during its translatory movement, to the state of masses separate from each other; therefore, due to the analogy of form that I announced, the liquid shape in hollow which would be used as a channel for the gas current must pass to the state of hollow spaces separated by liquid. In other words, our gas current must, during its upward movement, convert itself into isolated bubbles, and it is indeed what takes place, as everyone knows.

However, the circumstances of the two phenomena present an essential difference, which diminishes the similarity of these phenomena themselves: in a full liquid stream, a narrowing deepens only by driving out into the two adjacent bulges the liquid which constitutes it; however, this transport in the two directions requires considerable relative displacements of the molecules, and the liquid, under the terms of its viscosity, resists these relative displacements more or less; it follows that a notable time elapses between the birth of each narrowing at the contracted section and the rupture of the filament into which this narrowing converts, and that, during this time, the narrowing traverses a rather great space, so that the stream presents a rather long continuous part.

Now, in our gas stream, a narrowing deepens by driving out into the two adjacent bulges not liquid, but gas, and it opposes to the relative displacements of its molecules a resistance incomparably weaker, from which it follows that the time which passes between the moment of the birth of this narrowing close to the opening and that of its rupture, must be also be incomparably shorter; In truth, the modifications of narrowing are carried out only by a movement of the ambient liquid; but it is visible that this movement is achieved with much less relative displacement, and, consequently, with much less resistance than that which takes place inside a narrowing of a liquid stream. Consequently, the space traversed in the translatory movement, during this same time, will be much smaller, with equality of diameter of opening and velocity of emission, for the gas current than for the liquid stream, so that at less than an enormous speed, the first will not present a notably continuous part.

§ 504. To subject these deductions to experimental proof, I ran a draught through water contained in the vessel with plane glass walls which is used for the experiments with oil and the alcoholic liquid. The current was brought by a glass tube of 5mm approximately of internal diameter, based on a gasometer, which was bent so as to go down to the bottom of the vessel, then to be raised vertically until a few centimetres above this bottom; the level of water in the vessel was at 15 centimetres above the opening of the tube; finally the air, in the gasometer, was subjected to a pressure of 130 centimetres of water.

With these conditions, which gave a considerable speed to the gas current, it, in its passage through the water of the vessel, appeared continuous to the eye; but it was far from offering the form described in § 502; that which it presented was rather not very regular; however, one could observe there kinds of swellings and nodes, as in the turbid part of a liquid stream; finally, a continual boiling took place at the place where it pierced the surface of the water.

This boiling makes it possible to conclude that the gas current, in spite of its high speed, reached the surface of water only in isolated bubbles; moreover, its apparent shape, if distant from that which it would have offered if it had been really continuous on a notable part its length, led me to believe that the bubbles were formed already very close to the opening, and that the continuous aspect of the current in all its extent was a simple illusion due to the fast passage of these bubbles, absolutely as the continuous aspect of the turbid part of a liquid stream is due to the fast passage of the isolated masses.

To return the subject some more, I lowered the level of water in the vessel until it was not any more than approximately 2 centimetres above the opening of the tube, and the boiling did not disappear at all; the isolated bubbles are thus formed actually very close to the opening, even for high speeds; in other words, the gas stream does not have a continuous part. As for the appearance of swellings and nodes, about which I spoke above, it probably comes from each bubble which develops at the opening meeting the resistance of the water of the vessel, initially flattened in a vertical sense, then, during its movement through the liquid, carrying out oscillations of form similar to those of the isolated masses of a liquid stream.

A last fact particular to the gas current is the absence of small bubbles accompanying the big ones; these small bubbles, if they occurred, would be ejected away from the current, in consequence of the larger resistance their upward movement through water would encounter, and would become thus visible; however, one seldom distinguishes any, from which it follows that when a large bubble is isolated close to the opening, the narrowing by the means of which separation takes place closes without giving rise to a gas filament; this narrowing is too short, and the absence of filament and spherules constitutes the second of the examples to which I referred in § 383.

§ 505. Let us return for one moment to the liquid tube with thick walls obtained by Magnus and Mr. Laroque in the case of a moderate rotational movement of the liquid (§ 483). It follows from what we have just described about the gas current, that, without the intervention of the centrifugal force, the air which penetrates in the stream would have, as of its entry, to convert itself into isolated bubbles, which would descend pulled by the translatory movement of the liquid while being renewed unceasingly close to the opening. The absence of this conversion is explained, I think, by the principles developed in § 499 with regard to the laminar streams: if a narrowing took shape, the liquid which would flow towards the axis, to form this narrowing, would bring with it its absolute velocity of rotation at the same time as an increase in mass, so that the centrifugal action there would become dominating, and would again move the liquid away from the axis. Narrowing cannot thus be born, and thus the air extends, in the interior of the stream, in a long continuous trail, a trail which constitutes, seemingly, an exception to the laws of equilibrium of the shapes lengthened in hollow or relief.

However, the maintenance of the gas trail requires obviously that the gyratory movement not be too slow; it is understood, indeed, that beyond some reduction in this movement, the variations of the centrifugal action cannot be enough any more to oppose to the shaping forces; as Mr. Laroque observed, during a later weakening of rotation, the trail worked itself into a succession of bulges and narrowings.

§ 506. Let us return to the gas current which crosses a liquid at rest. Its fractionation into successive bubbles starting even at the opening, explains the gurgling which occurs when one inclines a bottle full of liquid to make some pour out: an exchange is established between the liquid which runs out and the air which replaces it; but as soon as a portion of air is introduced into the neck, the shaping forces start to round it, they quickly narrow the part of it close to the opening, the narrowing thus formed breaks, and the portion of air is separate, in the state of a complete bubble, from the air which tended to follow it; the liquid thus occupies then all the opening, so that the exchange with the surrounding air is stopped, and that, consequently, the flow is temporarily stopped; then the same phenomena recurs, a second portion of air enters the neck to replace an equal portion of liquid which leaves, this portion of air is formed into a bubble like the first, the flow undergoes a new interruption by the closing of the narrowing, to start again in the same manner, and so on. The jerks which the flow of a liquid presents in the circumstances in question, thus result again from the action of the forces which tend to give to a liquid surface in contact with gas a shape of steady equilibrium. Without this action, the exchange between the liquid and the air would take place in a quiet way: the first would also leave by the lowest part of the neck in the form of a continuous current, while the second would enter by the highest part, in the form of a continuous current, and would cross, without dividing, the liquid contained in the body of the bottle: it is thus, for example, which, in the experiment of the passe-vin, the exchange of the two liquids in the small opening is done without jerks, and one sees a continuous red filament starting from this opening up to the level of water, because, due to the slight difference in the nature of these two liquids, there does not develop, on the surface by which they touch, significant shaping forces.

§ 507. It now remains to me to pay a proper tribute of recognition to the people who, in this long continuation of research, agreed to help me, by carrying out, under my direction, experiments or calculations. To the names which I quoted in § 8, I add here those of Mr. Kekule, then professor of chemistry at the University of Ghent, and Mr. Rottier, preparator of industrial chemistry at the same establishment; both agreed to prepare for me certain substances which were necessary for me.

Thanks is thus returned to these devoted friends, whose benevolent cooperation allowed a physicist struck with blindness to carry on his road with a firm step, and to bring his material contribution to the building of science.

§ 508. I finish by enumerating here the Notes and Memoirs published since the end of 1869, the time when my Histories stop, on subjects having relevance to mine.

1º M. Tomlinson: *On the motions of certain liquids on the surface of water* (PHILOS. MAGAZ., 4th series, 1870, vol. XXXIX, p. 32).

It is on the question of the surface viscosity of the liquids.

2° Mr. Quincke: *Ueber Capillaritäts-Erscheinungen an der gemeinschaftlichen Oberflache zweier Flüssigkeiten* (ANN. DE M. POGGENDORFF, 1870, vol. CXXXIX, p. 1).

It is, in-extenso, the Report of which I analyzed an extract in § 169.

3º M. Paul du Bois-Reymond : *Ueber den Antheil der Capillaritat an den Erscheinungen der Ausbreitung der Flüssigkeiten.* (Ibid., p. 262).

4° Sir W. Thomson: *The size of atoms*. (Journal NATURE, 1870, vol. I, p. 551).

5º M. Van der Mensbrugghe: *Sur la viscosité superficielle des lames de solution de saponine* (BULLET. DEL L'ACAD. DE BELGIQUE, 1870, 2nd series, vol. XXIX, p. 368).

6°. M. Lüdtge: *Ueber die Spannung flüssiger Lamellen* (ANN. DE M. POGGENDORFF, 1870, vol. CXXXIX, p. 620).

7° M. Luvini : *Alcune sperienze e consicerazioni intorno all' adesione tra solidi e liquidi* (ATTI DELLA REAL ACCADEMIA DELLE SCIENZE DI TORINO, vol. V, 1870).

8° M. Van der Mensbrugghe: *Sur un principe de statique moléculaire avancé par M. Lüdtge* (*Bullet. de l'Acad. de Belgique*, 1870, 2nd series, vol. XXX, p. 322).

9° Sir W. Thomson: On the equilibrium of vapour at a curved surface of liquid (PROCEEDINGS OF THE ROYAL SOCIETY OF EDINBURGH FOR 1869-70).

10º M. Duclaux: *Sur la tension superficielle des liquides* (*Ann. De Chim. et de Phys.* de Paris, 1870, 4th series, vol. XXI, p. 378).

11° M. Marangoni: *Sulla proprietà che hanno varj liquidi d'impedire o far cessare talune reazioni tra acidi e metalli* (*Nuovo Cimento*, series 2, vol. IV, Déc. 1870).

12° M. Mousson: *Bemerkungen über die Theorie der Capillarerscheinungen* (ANN. DE M. POGGENDORFF, 1871, vol. CXLII, p. 405).

13° M. Schwarz: *Bestimmung einer speciellen Minimalfläche* (MÉMOIR COURONNÉ PAR L'ACADÉMIE DE BERLIN en 1867, et publié en 1871).

C'est le Mémoire dont j'ai parlé au § 143.

14° M. l'abbé Laborde: *Caléfaction, faits nouveau* (LES MONDES, 1871, 2nd series, vol. XXV, p. 379).

15º M. Norris : *Soap-bubble experiments* (Journ. NATURE, 1871, vol. III, p. 395).

16° Sir W. Thomson: *Ripples and waves* (Ibid., 1871, vol. V, p. 1).

17º M. Beetz: *Ueber die Einwirkung der Elektricität auf Flussigkeitsstrahlen* (ANN. DE M. POGGENDORFF, 1871, vol. CXL1V, p. 443).

18° M. Bosscha: LEERBOEK DER NATUUREUNDE EN HARE VOORNAAMSTE TOEPASSINGEN (4th book. 1st part, Leyde, 1871).

19° M. Mach : *Eine akustische Mittheilung* (TAGEBLATT DER 44STE VERSAMMLUNG DEUTSCHER NATURFORSCHER UND AERZTE, 1871, p. 53).

This work relates to the ear. The author, in connection with a question concerning the vibrations of the tympanum, observed those of small films of glyceric liquid.

20° Mellberg: *Om utspänningen hos vätskor* etc. (*Sur la tension superficielle des liquides* etc.). Helsingfors, 1871.

21° M. Bosscha : (PROCÈS-VERBAUX DE LA SECTION DE PHYSIQUE DEL L'ACAD. D'AMSTERDAM, 1871-1872, nº 3 and 5).

22° M. Valson : *Sur une relation entre les actions capillaires et les densités dans les solutions salines* (COMPTES RENDUS, 1872, vol. LXXIV, p. 103).

23° M. Van der Mensbrugghe: *Note préliminaire sur une fait remarquable qu'on observe a contact de certains liquides de tensions superficielles très-différentes* (BULLET. DE L'ACAD. DE BELGIQUE, 2nd series, 1872, vol. XXXIII, p. 223).

24° M. Schwarz : *Fortgesetzte Untersuchungen ueber specielle Minimalfächen* (BULLET. DE L'ACAD DE BERLIN, 1872, p. 3).

25° M. Marangoni: *Sul principio della viscosità superficiale dei liquidi stabilito dal signor J Plateau* (NUOVO CIMENTO, 2nd series, vol. V-VI, Avril 1872).

26° M. Mach: *Die Gestalten der Flüssigkeit*. Prague; 1872 (Conférence donnée en 1868).

27° MM. Tomlinson et Van der Mensbrugghe: *On a relation between the surface tension of liquids and the supersaturation of saline solutions* (PROCEEDINGS OF THE ROYAL SOCIETY OF LONDON, n° 135, 1872).

28° M. Moutier. *Sur la tension superficielle des liquides* (JOURN. DE PHYS. DE M. D'ALEMÉIDA, 1872, vol. I, p 98).

29° M. Duclaux : *De l'influence de la tension superficielle des liquides sur les mesures aréométriques* (Ibid., p 197).

30° M. Mach : *Optisch-akustische Versuche. — Die spectrale und stroboskopische Untersuchung tönender Körper*. Prague, 1872[284], p 92-94.

31° M Van der Mensbrugghe : *Sur la tension superficielle des liquides* (JOURN. DE PHYS. DE M. D'ALEMÉIDA, t I, 1872, p 321).

32° M Gernez : *Sur les propriétés des lames minces élastiques* (Ibid. ibid., p 324).

33° M Duclaux : *Sur la capillarité* (Ibid. ibid., p. 350).

34° M Gernez : *Note relative à l'action pretendue des lames minces liquides sur les solutions sursaturées* (COMPTES RENDUS, 1872, vol. LXXV, p 1705).

35° J. Plateau : *Réponse aux objections de M. Marangoni contre le principe de la viscosité superficielle des liquides* (*Bullet. de l'Acad. de Belgique*, 1872, 2nd series, vol. XXXIV, p 404).

36° M. Duclaux: *Théorie élémentaire de la capillarité, fondée sur la connaissance expérimentale de la tension superficielle des liquides*, 1872, Paris.

37° M. Schwarz: *Beitrag zur Untersuchung der zweiten Variations des Flächeninhalts von Minimalflächen im Allgemeinen und von Theilen der Schraubenfläche im Besonderen* (BULLET. DE L'ACAD. DE BERLIN, 1872, p. 718).

38° MM. Marangoni et Stefanelli: *Monografia sulle bolle liquide* (NUOVEO CIMENTO, 1872, 2nd series, vol. VII-VIII, p. 301, et 1873, vol. IX, p. 236).

39° M. Van der Mensbrugghe: *Résponse à la communication précédente de M. Gernez* (COMPTES RENDUS, 1873, vol. LXXVI, p. 45).

40° M. Gernez: *Note relative à l'action prétendue des liquides à faible tension superficielle sur les gaz dissous dans les liquides à forte tension superficielle* (Ibid., p. 89).

41°. M. Moutier : *Sur la tension superficielle des liquides* ((JOURN. DE PHYS. DE M. D'ALEMÉIDA, vol. II, 1873, p. 27).

42° M. Gernez: *Sur un nouveau moyen de déterminer la position des surfaces nodales dans les masses gazeuses vibrantès* (COMPTES RENDUS, 1873, vol. LXXVI, p 771).

43° M. Lissajous: *Méthode pour étudier la propagation des ondes* (JOURN. DE PHYS. DE M. D'ALEMÉIDA Mars 1873, p. 99).

44° M. Van der Waals: *Over de continuiteit van den gas- en vloeistofloestand* (Dissertation inaugurale, Leyde, 1873).

45° M: Van der Meùsbr : *Sur la tension superficielle des liquides considérée a point de vue de certains mouvements observés à leurs surface* 2nd Mémoire (ACAD. DE BELGIQUE, vol. XXXVII des Mém. couronnés et Mém. des savants étrangers).

46° M. Gernez: *Expériences de capillarité* ((JOURN. DE PHYS. DE M. D'ALEMÉIDA, vol. II, l873, p. 326).

47° M Tomlinson : *On the motions of camphor and of certain liquids on the surface of water* (PHILOS. MAGAZ., 4th series, vol. XLVI, 1873, p 376).

48° M Lippmann : *Beziehungen zwischen, den capillaren und elektrischen, Erscheinungen* ANN. DE M. POGGENDORFF, 1873, vol. CXLIX, p. 546).

NOTE. The last three articles above were published since the impression of my Histories relative to the tension and the liquid films, and it is necessary, consequently, to add n° 47 and n° 48 to the enumeration contained in the third note of § 169, and n° 46 to that of the second note of § 354bis. In addition, I very recently found mention of an article concerning liquid streams; but, not having been able to consult it, I am unaware if it treated with the constitution of the stream; I register it here simply according to its title. It is indicated in a summary of work of the Academy of St. Petersburg for the year

[284] Le Mémoire porte par erreur la date de 1873.

1870 (see the newspaper *Les Mondes*, vol. XXXII, 1873, p. 378). The author of this summary expresses himself as follows: "We, moreover, inserted in our publications research of Mr. Popow *On the free surface of a constant current of a homogeneous liquid subjected to the action of gravity and emanating from a horizontal and circular opening.*"

THE END.

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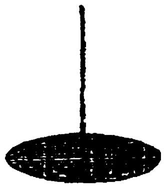
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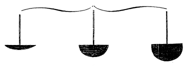
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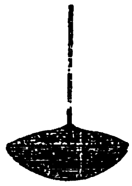
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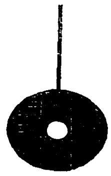
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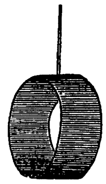
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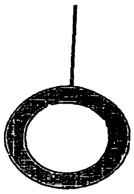
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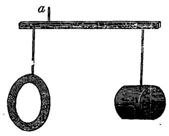

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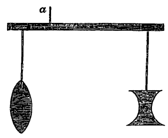

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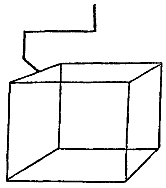
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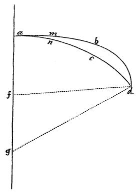
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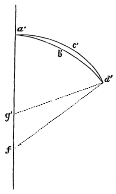
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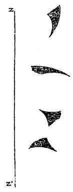
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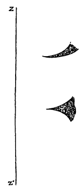
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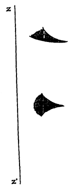
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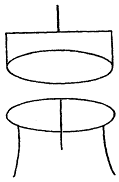
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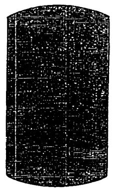
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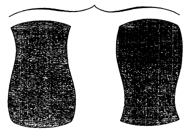
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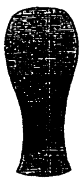
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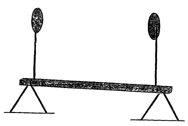
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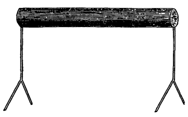
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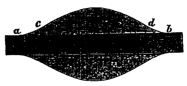

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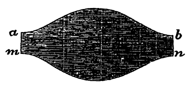

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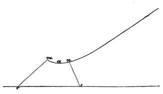
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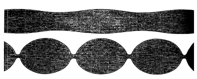
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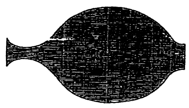
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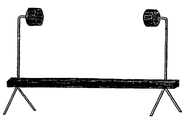
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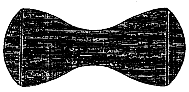
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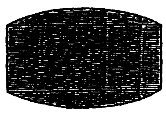
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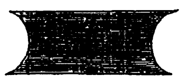
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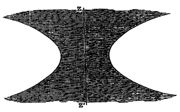
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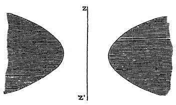
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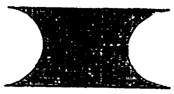
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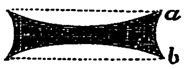
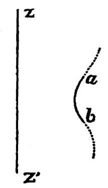


Fig. 34



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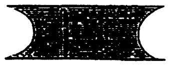
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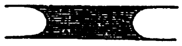
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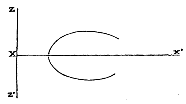
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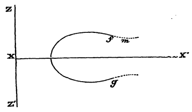
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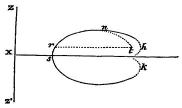
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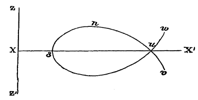
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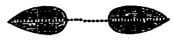
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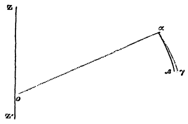
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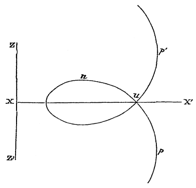
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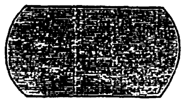
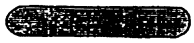


Fig. 45


Fig. 46


Fig. 47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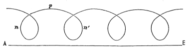
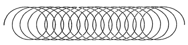


Fig. 49


Fig. 50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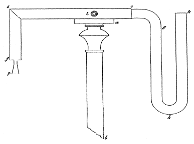
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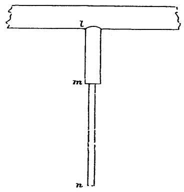
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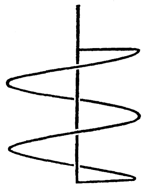
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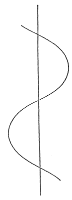
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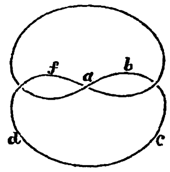

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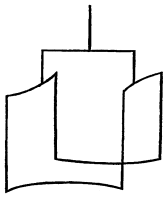
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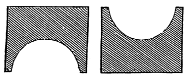
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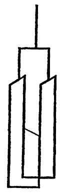
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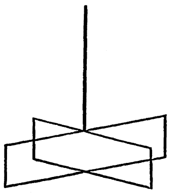
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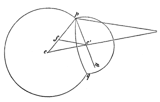
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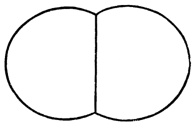
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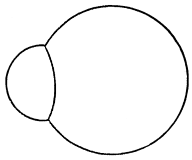
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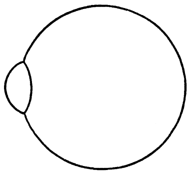
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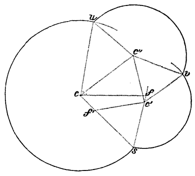
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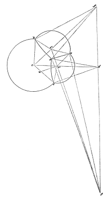
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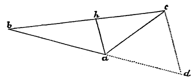
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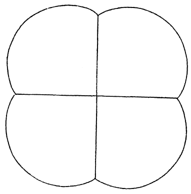
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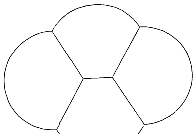
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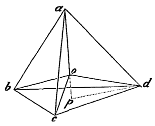
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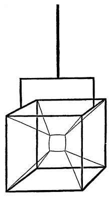
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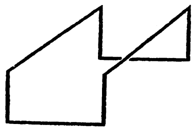
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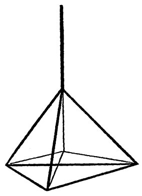
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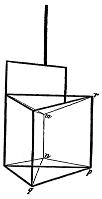
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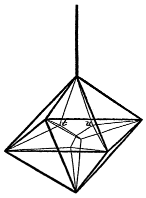
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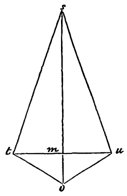
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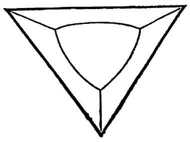
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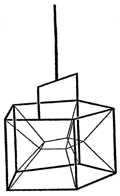
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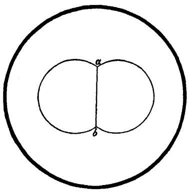
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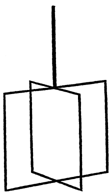
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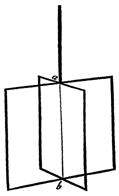
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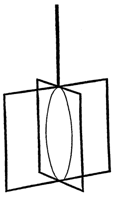
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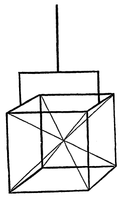
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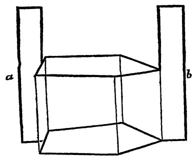
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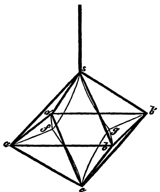
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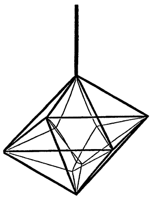
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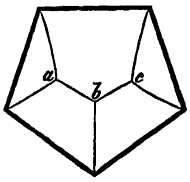
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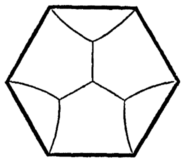
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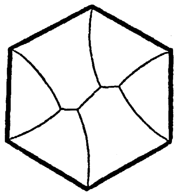
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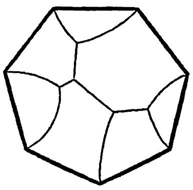
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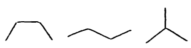
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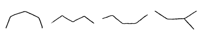
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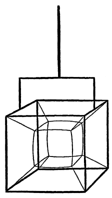
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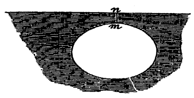
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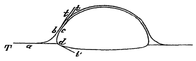
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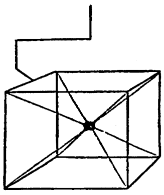
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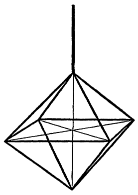
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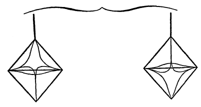
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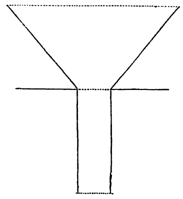
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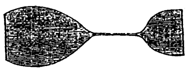
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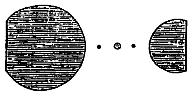
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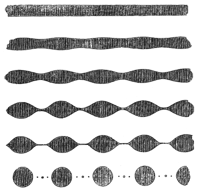
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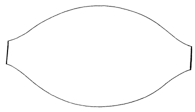
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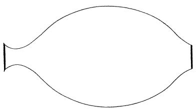
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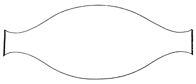
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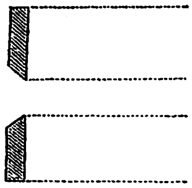
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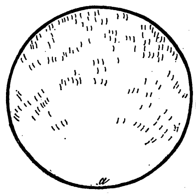
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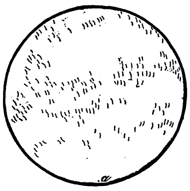
Fig. 105