ON THE SUPERFICIAL VISCIDITY OF LIQUIDS.

Translated for "SCIENCE" from the French of J. Plateau,

By THE MARCHIONESS CLARA LANZA.

In the year 1638 Descartes affirmed that the surface of water presented a resisting tendency as though it were covered with a thin pelicle. Several other learned men have asserted the same fact and sought to verify it by various experiments. Some limited their researches to water alone; others maintained that the tests were applicable to all liquids. In my own observations I have described experiments and facts which, in my opinion, at once remove all doubt as to the reality of a peculiar resisting force manifested by the surface of water, solutions, etc., and I have attributed this resistance to a characteristic viscidity or glutinous matter pertaining exclusively to the outer coating of these liquids. Nevertheless, I have demonstrated that certain other fluids are totally exempt from this peculiarity, and I hope I have fully established the fact, heretofore, that in several among them, such as alcohol, spirits of turpentine, sulphuric ether, etc., the invisible particles of the outer layer offer, on the contrary, less resistance in regard to relative displacement than those within the mass.

Permit me to recall the facts of which my principal experiments consist. A magnetized needle is placed upon a pivot in the centre of a glass cylinder; the liquid to be tested is then poured into the vessel—just enough to come in contact with the needle; the latter is then turned to a meridian of about 90°, then in a few moments is left to itself, and the time which it takes to travel over a determined angle must be correctly ascertained. In my experiments the angle was 85°. More liquid must then be introduced into the cylinder so that the needle is completely covered, the liquid rising at least two-thirds of an inch above it. The needle is then again turned to a meridian of 90°, and one must remark the time taken to describe the preceding angle. Now, for example, when the liquid is distilled water, my needle took precisely twice as long when upon the surface as when beneath it, notwithstanding that in the first case the under surface of the needle alone came in contact with the water, while in the latter it was completely immersed. When the liquid employed was alcohol or turpentine the time required by the needle, when upon the surface, was less by half than when beneath it.

I must add that in those liquids on whose surface the magnet moves more slowly than when beneath it, the entire outer coating moves also, although somewhat less rapidly.

Two liquids, one a solution of albumen and the other of a saponaceous consistency, have exhibited superficial viscidity in an extremely forcible manner. After having moved with the utmost slowness, the needle stopped at an angle of 35° on the surface of the former. It did not move at all upon the latter liquid.

I omit purposely the various details relative to those experiments, as well as other facts belonging to the subject in question. I shall mention them further on as the reasons present themselves, and at this time merely confine myself to the special object of these remarks, that is to say, the cause and nature of these phenomena.

In a notice published in 1870, M. Luvini expressed doubt in regard to the superficial viscidity of liquid matter[1]. He presumes that the effects I myself have observed are due to some alteration in the outer surface caused by the contact of the liquid with the air, or else by particles of dust floating about.

In 1872 M. Marangoni published a paper,[2] in which he seeks to prove that the viscidity upon the outer portion of the liquid is identical with that which is beneath the surface. According to him, in such liquids as water, for instance, which does not produce bubbles, the resistance is increased by a capillary action exercised by the glass upon the needle; while, when liquids which bubble easily are used, the resistance springs from a thin cuticle in coating of a nature peculiar to the liquid itself.

I replied to both these articles[3]; but M. Marangoni attacked me again last year[4]. In his second work he substitutes, for some unknown reason, *particles of dirt* for the word cuticle. When speaking of the saponaceous solution, he states that the carbonic acid in the air decomposes the soap and produces an alkaline carbonate which removes the fatty acids and forms a kind of emulsion upon the surface. As to the solutions of albumen he thinks probably that the coating of dust is produced by the evaporation of the water.

He does not positively deny that the surface of liquids cannot possess a viscidity of its own apart from that which is in the mass; but he is persuaded that the influence of the viscidity peculiar to the surface is very small indeed when compared with that which effects the final results. The following is the substance of his theory:

We all know that if we place upon any liquid a drop of another possessing less external elasticity, the drop will spread itself in a thin coating upon the surface of the former. Consequently, when a liquid is covered with a layer of dirt, we may reasonably admit that this layer possesses an elastic force much inferior to that which belongs to the pure, fresh surface of the underlying liquid. Now proper experiments show us, first of all, that the tension of this coating is effectively much less forcible than that of the liquid beneath; secondly, that if the coating becomes sufficiently thick, the elasticity disappears entirely, or very nearly; thirdly, that in any saponaceous solution the film can be accumulated upon certain portions of the surface and removed upon others.

When a bubble is blown from one of these liquids the layer of dirt extends in both sides of it and thus prevents its breaking. Liquids such as alcohol, ether, turpentine, etc., cannot, owing to the slight elastic force they possess, be covered with a coating of dirt, and for this reason they are unable to produce bubbles.

The retarded movement of the needle upon the surface of the liquid does not arise from any viscidity of the outer layer, for, in the saponaceous solution at least, this coating is very movable, as the two following facts will show:

In the first place, when a large soap bubble is blown, reflecting various colors, the slightest breath of air will cause it to whirl rapidly backwards and forwards. In the second place, if a certain amount of soap-suds be put into a horizontal brass tube sufficiently large for the purpose, and a magnet be placed inside upon a pivot, directed toward the magnetic meridian, and then left to oscillate at will, you will perceive that the vibrations are very nearly as rapid as when the magnet moves freely in the air, notwithstanding the fact that it has to overcome the resistance offered by the two outer coatings of the liquid.

When a coating of dirt exists, the somewhat retarded motion of the magnet upon the surface, together with the rotating movement of the entire mass, can be explained in the following manner: The magnet itself tends to remove the dirt which is behind it and accumulate it all in front; this produces an excess of elasticity along the posterior contour, directly opposed to the natural motion, and at the same time a diminutive expansive force along the anterior contour. Furthermore, behind each half of the magnet, the superfluity of expansive force on the fresh surface draws together the edges of that portion which is already freed from dirt as though to close the rent, and, at the same time, as in each of the anterior parts, the portions far removed from the edge of the magnet possess a weaker expansive force than those nearer to it; the former attract the latter and thus determine the rotation of the entire mass.

[1] *Alcune sperienze considerazioni intorno all' adesione tra solidi e liquidi.* Turin, 1870.

[2] *Sul principio della viscosita superficiale dei liquidi stabilito dal Signor J. Plateau.*

[3] *Réponse aux objections de M. Marangoni contre le principe de la viscosité superficiale des liquides.*

[4] *Difesa della teorica dell' elasticita superficiale dei liquidi.* 1878.

If the outer layer of the liquid should resist the movement of the magnet from any viscosity of its own, it would pucker perceptibly; moreover, viscous bodies propagate motion with difficulty from any distance.

The coating of dirt imitates closely an elastic body, inasmuch as it tends to return to its primitive state when broken; however, it substitutes, in place of superficial vsicosity, *superficial elasticity*.

In regard to those liquids of strong expansive force which do not produce bubbles, such as water, the greater portion of briny solutions, etc., liquids upon whose surface a layer of dirt cannot easily be attested, the retarded movement of the magnet upon the exterior is hardly due to the changes which occur in the cavities of the meniscus, terminating the magnet at each end, partly, also, to the beginning of a layer of dirt, M. Hagen having discovered that the surface of water undergoes modifications by exposing the liquid to the air. But the principal cause may reasonably be said to be the fact which M. Van der Mengsburgghe has so well described, namely, that when the surface of any liquid is augmented, or, in other words, when any diffusion of the pure exterior takes place, a sudden cooling, followed by an increased tension, ensues, and, reciprocally, a warmth and decrease of tension correspond to any diminution or contraction of the surface.

This then is the main substance of the theory proposed by M. Marangoni in compensation for mine. Let us endeavor to examine it. First of all, it would seem, according to his doctrine, that it is merely necessary to add to any liquid of strong expansive force which does not bubble, a drop of another liquid of weak tension in order to produce large bubbles from the former. Now if a drop of olive oil or spirits of turpentine be placed upon distilled water, the liquid will rebel strongly against the formation of bubbles. Should the water be covered with a thin coating of either of the above mentioned liquids, you will find that it bursts in the bowl of the pipe before you have even commenced to blow the bubble. We must admit therefore, in the first place, that the supposed coating of dirt must have close connection with the liquid beneath it. The author also assigns an additional and indispensible cause for the production of bubbles which he describes as the *superficial elasticity*, or in other words, the facility with which the dirt spreads itself over the liquid, so that the latter is always covered. Nothing however, goes to show us that a thin coating of olive oil or turpentine does not possess the same elasticity.

The author, in fact, describes two circumstances in which foreign substances produce a coating upon distilled water which is more or less effectual. First of all, if the pollen of flowers is spread upon the surface and air blown from above within an hour or two, the little apertures formed will remain for a long time; but the liquid refuses to form bubbles when blown from a pipe or tube. In the second place, they can be produced, nevertheless, by means of pure distilled water, if the tube is partially filled with small particles of camphor. The diameter of these bubbles may reach an inch and more. But we can readily see that these facts are only the beginning of success. However, they are in no wise opposed to the theory of superficial viscidity, since in both cases the outer layer of the water undergoes modifications.

According to the author, the superficial elasticity is estimated by the difference which exists between the tension of the pure surface and that of the dirty surface, and he determines this by means of a small apparatus which he calls a *capillary balance*. In his opinion, as we have already seen, when a bubble is blown the coating of dirt prevents its being broken. In his statement he gives no reason for this but in a preceding work he explains himself clearly on this point. He says that if the coating of dirt should become disunited, the excess of tension upon the under layer, or in other words, the superficial elasticity, would instantly close the aperture. Hence the facility for the formation of bubbles, or as the author calls it, the pompholygenic power, should decrease with the superficial elasticity. Now M. Marangoni is led to the conclusion that all causes which tend to diminish this elasticity without removing the dirt, render the development of bubbles much easier. Further on, he returns to this proposition and says that "all those conditions which diminish this elasticity to the advantage of the plasticity increase the pompholygenic force." If we examine closely his ideas, we can understand that an increase of plasticity favors considerably the generation of bubbles; but how is it possible that a diminution of elasticity can lead to the same result?

Let us return to the first of the two facts quoted above. The author finds, by means of his capillary balance, that the superfine elasticity of the distilled water, covered with pollen, may become doubly as great as that found in the saponaceous solution. Now, inasmuch as the latter produces large bubbles while the former gives none at all, it is necessary, according to M. Marangoni's proposition that the plasticity of the saponaceous solution should be much superior to that of the distilled water, which is rather di cult to admit owing to the peculiar rigidity of the surface of the former; indeed there are two totally different liquids in question; nevertheless, the author's statement seems to apply equally to both in this case.

In order to show that the layer of dirt can be accumulated upon one portion of the surface of a liquid and diminished upon another, M. Marangoni describes the the following curious experiment.

He plunges, into a soapy solution, a ring made of iron wire about seven and a half inches in diameter and fastened to the end of a fork which serves as a handle; when the ring is immersed he draws it out again, holding it in a horizontal position; he then raises it until the catenoid wave, which unites it to the surface of the liquid, separates into two portions, one of which forms an even layer within the ring, while the other produces a spherical cavity upon the liquid; now, if the temperature is low enough (from 12 to 14 degrees), this cavity is very hollow, the radius of the base measuring 48 *millimetres*, while the height is only 27. M. Marangoni began this experiment four times, always breaking the cavity before again immersing the ring, and by this means he obtained the maximum of depression in which the depth was exactly half of the radius of the base. While the ring is being raised the circumference, in accordance with which the catenoid lamina unites with the surface of the liquid, contracts, and as M. Marangoni affirms, condenses the coating of dirt on the interior and dilates it on the exterior. Hence, when the cavity is once formed a diminution of tension takes place in the space limited by its base, and an increase of tension occurs on the outside; this excess of tension consequently aids the basis of the cavity to enlarge, and results in the depth being diminished.

According to my theory, the superficial layer of the liquid contracts, as above stated, on the interior of the opening, and dilates on the exterior; but its consistency does not undergo any modification. The portion which contracts forces a part of its molecules into the mass beneath, and the dilated portions attract these atoms. Now, according to M. Van der Mensbrugghe's theory which I have mentioned already, these effects cannot be produced unless a diminution of tension takes place within the contracted portion and an augmentation of the same in the dilated part. This phenomenon, however, can only occur in a very low temperature, and when, in consequence, the cavities manifest a certain viscidity. When the temperature is notably higher the cavities are smaller and their depression less. At 26 degrees hardly any effect is visible. The radius of the base at this temperature was 23 *millemetres*, and the height 20; but I have shown that all cavities formed upon the surface of liquids are never complete hemispheres. M. Marangoni thinks it probable, as I have said before, that the coating of dirt on the saponaceous solution is due to the action of carbonic acid contained in the air.

I have ascertained that carbonic acid actually decomposes the solution inasmuch as it removes all fatty acids; but does the formation of the layer really arise from this cause? In order to discover this the following experiment has been made:

A certain amount of a concentrated solution of caustic potash was placed within a bottle holding almost a quart, then, after tightly corking the latter it was violently shaken so that the liquid swept over every part of the interior. The greater portion of the liquid was then poured out and the bottle instantly re-corked. In the meanwhile a funnel provided with a plug was procured and the interior of its neck moistened with the solution of potash; it was then placed in the neck of the bottle and wax applied at the junction. This done, almost 300 *grammes* of a solution of Marseilles soap previously rendered clear by means of filtration was poured into the funnel and left there for one hour. At the end of that time the wax was removed and the funnel gradually lifted, the plug being opened simultaneously, and, as the liquid flowed into the bottle the funnel continued to be slowly raised until the extremity of the neck was about on a line with the top of the bottle; the latter was then rapidly corked, some of the liquid remaining in the funnel.

The potash necessarily absorbed the small quantity of carbonic acid contained in the bottle, and at the moment when the funnel was removed no exterior volume of air could possibly penetrate within the bottle, because the stream of liquid flowing in must have expelled much more air than could possibly have found its way in to replace the neck of the funnel. Finally, as merely a portion of the liquid escaped into the bottle, and that at a distance far above the free surface, it could absorb nothing from the superficial layer. Now, with this liquid merely united with air deprived of carbonic acid, transverse waves of a very persistent character were easily developed (the bottle measured three and a quarter inches in diameter), which could evidently not have occurred had the liquid been without an efficient coating. It is quite impossible, therefore, for me to accept M. Marangoni's explanation. Besides, the effectual coating upon the saponaceous solution does not arise from the evaporation of water; for a fatty liquid like soap-suds, for instance, which produces bubbles in consequence of this consistency, does not evaporate at all, but, on the contrary, attracts the dampness in the air. In order to assure myself that the effectual coating of the saponaceous solution does not proceed from the evaporation of water as M. Marangoni thinks it does, I added two parts of Price's glycerine to three parts of the solution, about the proportions generally used to produce a liquid glycerine, and the two substances were thoroughly mixed together. This compound, in consequence of the glycerine, should absorb moisture instead of losing it; now, by means of a pipe it produced bubbles at least two inches and a half in diameter. I then increased the quantity of glycerine, so that the two substances were about equally divided, and even then bubbles two inches in diameter were obtained. Thus, the effectual coating of the solution is not due to the loss of water by evaporation.

As to the solution of albumen, inasmuch as its properties are analagous with those of the soapy solution, although less pronounced, I considered it useless to make the same experiments in reference to it.

Now, if the cause which originates the formation of the effectual coating upon the saponaceous solution is due neither to the action of carbonic acid contained in the air, nor to the evaporation of water, whence does it arise? Must we have recourse to Dupré's somewhat unacceptable idea, which holds that in certain solutions the substance dissolved rises abundantly to the surface? Is it not much easier to admit, as I do, that the superficial coating of liquids forms itself spontaneously into a particular condition, which results in a greater or less difficulty in regard to the relative displacement of the molecules than could occur in the interior of the mass? Does not the fact that tension exists suffice to show that this coating possesses an especial character in reference to the action of molecules?

The experiment which originated Dupré's singular idea mentioned above, is based upon the fact that the height of a fine stream of liquid precipitated from a certain distance must be considerably diminished by the tension of its surface, and Dupré, therefore, concludes that in a little stream of soap-suds the tension is sensibly identical with that of pure water, while we all know that when a solution of soap is in a state of repose its tension does not approach that of water by two-thirds. Dupré concludes that in the stream of saponaceous solution, where the surface is constantly renewed, the soap itself has no opportunity of coming to the outside. But in my theory—a remarkable fact which I have myself confirmed by an entirely different process which it is useless to refer to here—proves that the superficial coating of liquids requires a certain amount of time, however short, to assume its proper atomical condition.

"But," says M. Marangoni, "the superficial coating of the saponaceous solution has no extraordinary viscidity; on the contrary, it is very susceptible of motion." I acknowledge that it does in fact possess great mobility, which proceeds from the extreme thinness of its consistency. Also, it is capable in itself of making but slight resistance towards the movements of the magnetized needle. Still, as it adheres in its fullest capacity to the underlying liquid, and should therefore attract a certain amount of the latter as it rotates, a greater part of the resistance must necessarily be due to this fact. Moreover, we observe, nothing goes to show us that the superficial layer, although very mobile, is less so than the underlying liquid if both are of an equal consistency. We can reasonably admit this after an experiment with the magnetized needle placed within the liquid. Indeed, as the number of oscillations performed by M. Marangoni's needle when in the liquid and when removed from it were respectively five to six, the governing powers of the needle in these two conditions are in proportion to the square of the above numbers, as, for instance, thirty-six to twenty-five, or about three to two. The resistance of the liquid robs the needle of nearly one-third of its governing force; only as we require which part the two superficial coatings play in this resistance, nothing prevents us from attributing it to the principal one of them.

Finally, the resistance in regard to the displacing of molecules cannot be denied as far as the superficial layer of saponaceous solutions is concerned, consequently we should admit this fact, although in a much less degree, in reference to solutions of soap itself. In one of my papers, and also in paragraph two hundred and seventy-eight of my book, I have described a certain number of facts which prove the rigidity existing in the effectual elevating of the saponaceous solution. I will confine myself to one of them as follows:

A bubble about an inch and a half in diameter is blown and placed upon the surface of the liquid; now, holding the mouth of the pipe in close contact with the hemisphere into which the bubble is transformed, you blow gently, increasing its dimensions until it bursts. The spray immediately spreads itself upon the liquid in several parts, each, however, being separated from the surface by a small quantity of air, and gradually disappears as though sinking into the mass, the contraction occupying several seconds.[1]

M. Marangoni, although maintaining perfect silence in

[1] In order to make this experiment successful, it is necessary to use a perfectly pure solution.

regard to this powerful viscidity, relates several experiments which make the fact of its existence very perceptible. Let us quote the following which is merely the continuation of one I have already drawn attention to :

A bubble is blown from a moderately wide tube which, however, has a broad mouth, and the other end is then left perfectly free. The bubble decreases gradually in size, but not in a perfectly systematic manner. On the contrary it elongates and at the same time contracts transversely, assuming a series of longitudinal folds or wrinkles. M. Marangoni explains this fact by stating that owing to the diminution of the surface, the coating of dirt becomes supersaturated and consequently the tension is annulled or reduced almost to nothing, inasmuch as the thin layer forming the bubble thus wrinkled and of a nearly conical shape does not show any tendency towards the minimum of the surface. But, he adds, if the unoccupied end of the tube should be corked so that the bubble would not decrease in size, the form of the latter would grow gradually round, and at the same time it would expel from the bottom certain drops of frosty moisture which forms in the little folds or wrinkles we have already mentioned ; then the coating of dirt would resume its normal condition, and the bubble assume, once more, a spherical shape.

M. Marangoni supposes that apart from the wrinkles on the bubble, the tension is utterly null or very nearly so: Now, the existence of any liquid utterly devoid of tension would be very extraordinary and we may say hardly probable. Moreover, the drops of moisture in the interior of the bubble, being the liquid which constitutes the outer coating of dirt, should possess little or no tension. I have collected these drops upon the crystal of my watch, and after repeating the experiment a number of times, I finally procured enough of the liquid to attempt the formation of bubbles by means of it. (I must state here that these drops were purely liquid and not at all frothy like those M. Marangoni describes.) Now, bubbles were formed from this liquid, some of them extending three inches in diameter, that is to say, they were similar in proportion to those obtained by means of the saponaceous solution ; only, with the liquid collected from the drops in the crystal, this maximum was much more difficult to reach. In a word, I modified M. Marangoni's experiment in a manner calculated to render his explanation of it still more improbable. A bubble about two inches in diameter was blown from the pipe and the drop suspended from the bottom removed ; then, inasmuch as the tube was expressly narrow, the wrinkled and cone shaped form was produced by inhaling through it, and before the drop produced at the extreme point of the cone could fall, the pipe was turned upside down in such a way that the liquid forming the drop ran along the surface of the bubble and separated itself as much as possible on the exterior. Now, although the superficial coating thus conserves very nearly its former consistency, and as consequently (according to M. Marangoni), the tension becomes, so to speak, annulled, the bubble instantly resumed its spherical shape while the pipe was being turned upside down, the time thus occupied not being more than one second. This experiment was repeated several times and always with the same result.

In my opinion these facts can be explained very simply. When you breathe through the pipe, should it be moderately wide or even narrow, the bubble necessarily contracts. It consequently becomes of a thicker consistency and a surplus amount of liquld flows towards the lower extremity; but the strong viscidity of the superficial coatings renders the general augmentation of density, and the equal contraction on all sides, very difficult during the short interval of reduction. The surface wrinkles in very much such a manner as a small bladder would should the air within it be inhaled, and at the same time it elongates into a conical form from the weight of the liquid which accumulates at the bottom. Nevertheless, this liquid arising from the increased density of the bubble does not notably diminish the tension, as is shown by the fact that when the pipe is held upside down and the liquid rests upon the bubble sitelf, the latter regains its spherical form immediately.

In regard to the superficial coating of the solution of soap, M. Marangoni observed that if this coating was viscous it should wrinkle when before the needle, which, however, does not occur at all. In order to discover what really takes place in reference to this circumstance, I began the experiment once more by sprinkling the surface of the solution with pollen[1] just before liberating the needle. If attention is then drawn to the tension of the needle, it will be seen that on the side toward which this half advances, and until a moderate distance is reached, the dispersion of the pollen is diminished, while on the opposite side—that is to say, behind this particular half—it is considerably increased. Thus, the superficial coating in front of the needle, instead of puckering, contracts, and dilates behind it. Now, if we reason in accordance with my theory, and consequently do not admit the existence of a coating of dirt, we should acknowledge that in the portion constructed the molecules pertaining to the superficial coating have left it and entered the interior of the mass, and also that in the dilated portion the molecules belonging to the interior have annexed themselves to the superficial coating in order to maintain the density ; these two effects could not be produced, moreover, unless a certain amount of resistance existed. They have necessarily developed also a difference of tension ; but, in the second of the two series of estimates which I effected in reference to the duration of the needle's movements on the surface and in the interior of the solution, the temperature was about 21°, and from M. Marangoni's observations upon the spherical cavities before mentioned, it follows that at this degree of temperature the differences of tension should possess but slight influence. However, the ratio concerning the duration of these movements upon the surface and beneath it have been found to about equal 1–78. Besides, these experiments seemed to result in showing that the effect arising from the difference of tension is not altogether to be overlooked, for in the first series in question when the temperature was but 18°, the ratio of duration was somewhat increased ; that is to say, about 1–87.

At the beginning of these remarks it was seen that M. Marangoni explains the retarded motion of the needle upon the solution of soap by the difference existing between the tension of the dirty coating and that of the liquid beneath. We have also seen that in regard to liquids such as water, saline solutions, etc., which also retard the needle's movements, he seemed somewhat embarrassed. At the commencement of his work, he insists upon the capillary action of the meniscus, then further on, he appears to attach but little importance to it, and invokes a small quantity of dirt ; further on still, he takes refuge in M. Van der Mensbrugghe's theory.

As far as the capillary action of the meniscus is concerned, I have endeavored to make it thoroughly understood that if we admit it at all, we should consider it as being probable the very reverse of what M. Marangoni supposes. He knows, moreover, that the action of a meniscus would not be sufficient in itself to satisfactorily explain the existence of any phenomena ; for example, it could not account for the rotation of the entire surface of the liquid. M. Marangoni, therefore, only ascribes a partial *rôle* to it, and at the same time seeks protection under a coating of dirt and the ideas expressed by M. Van der Mensbrugghe. But, you will ask, where then does this coating of dirt come from ? Does it arise from particles of dust floating about in the atmosphere ? In his first work M. Marangoni says that water which has

[1] In order to do this, the pollen must be spread upon the surface of the liquid by means of a small paper tube held at a certain distance above the solution. Care must be taken to do this as quickly as possible, as the soap moistens the particles and causes them to sink rapidly.

been distilled several times can remain exposed to the air for six or eight days without the slightest augmentation of resistance, in regard to the needle, being apparent. Besides, in the measures taken with distilled water, the entire preparation of the experiment from the moment when the liquid was poured into the capsule until the needle, was left to itself, occupied but five minutes; then during the ten partial measures afterward effected, no increase of resistance was observable. Could particles of dust floating about in the atmosphere produce an effect during those five minutes? Is it admissible? Indeed, M. Hagen has shown us conclusively that the superficial tension of distilled water decreases perceptibly when the liquid is exposed to the air; but this diminution is gradual and continued, and in order to produce any visible effect requires several hours. The peculiar fact M. Hagen describes, therefore, appears to me to bear no relation whatever to the resistance shown to the needle's movements; and inasmuch as air on the other hand, exercises no chemical action upon distilled water, and moreover as we are unable to invoke the influence of particles of atmospherical dust, we are led to attribute the fact established by M. Hagen to a cause arising from the interior of the liquid.

Now, in reference to the actual state of the case, I shall say again that it is useless to have recourse to a coating of dirt whose existence we cannot account for, and also that it is much more simple to admit the presence of an atomic organization peculiar to the superficial layer of the liquid.

As far as M. Van der Mensbrugghe's theory is concerned, M. Marangoni expresses himself in the following manner:

"The mass of the liquid effectually diminishes the variations of temperature produced upon the surface, which, in its turn, also decreases the variations of tension; in ordinary cases the latter are but trifling when compared with the variations attributed to dirt."

According to this remark, we should believe that the surface of the saponaceous solution, which, M. Marangoni states, possesses an undeniable coating of dirt, resists the movements of the needle more forcibly than the distilled water which could have hardly any dirt on its surface. In my experiments however, directly the opposite of this has occurred. The ratio of time required for the needle to describe an angle on the surface and beneath it when distilled water was used was, 1, 92, while when soap was used it was but 1, 82.

M. Van der Mengsbrugge's theory certainly deserves some attention in regard to the phenomena in question; but owing to the above remark of M. Marangoni, and the considerable dimension of the needle, relatively speaking, we may be permitted to doubt that any notable effect can result from it. Besides, if it did, we should find it again in those liquids of weak tension which do not produce bubbles, that is to say, alcohol, spirits of turpentine, olive oil, etc.; at least we should be able to observe a feeble rotation of the entire surface; now, this is by no means authenticated.

Finally, before attributing these phenomena to any other cause than that of a peculiar viscidity of the outer coating, it would be necessary to refute those arguments which have led me to the conclusion that the superficial coating of liquids possesses more atomic mobility than the interior portion. M. Marangoni is perfectly silent in regard to this part of my work.

After this examination of M. Marangoni's theory however, I consider myself justified in maintaining my opinion; but I forego all ulterior discussions referring to the subject, and leave all those physicists who may be interested in the question, to compare for themselves M. Marangoni's writings with mine, and to try to discover, if possible, which of us is right.

ON THE STRUCTURE OF THE ORANG OUTANG.

By Henry C. Chapman, M. D.

From the paper on this subject in the Proceedings of the Academy of Natural Sciences, of Philadelphia, we take the following facts:

The subject dissected was a young male Orang Outang (*Simia Satyrus*), about three years old. The first thing to strike Dr. Chapman was the length of the upper extremity, it being three inches longer than the lower one, agreeing nearly in this respect with the Gorilla, the difference in the extremities of that animal being 3½ inches, whereas in the Chimpanzee a difference of 1¾ inches only was found. The foot in the Orang, however, was ½ inch larger than the hand, whereas in the Gorilla the hand was ½ inch larger than the foot; in the Chimpanzee the difference in this respect was ⅜-in. in favor of the foot. Indeed, the distinctness of hand and foot superficially is more marked in the Gorilla than in the other anthropoids. The same facial muscles are found in man and the Orang Outang, with the exception that there is but one zygomaticus, possibly corresponding to the zygomaticus minor of man. The facial muscles, however, are not differentiated as in man, rather hanging together. The upper extremity of the Orang, in its muscles, differed essentially from that of man in the absence of the flexor longus pollicis, and extensor primi internodii pollicis and in the presence of the additional tendons to the ring and middle fingers

The Orang agreed with the Gorilla in not having a flexor longus pollicis, but disagreed with it in having the pronator radii teres, arising by two heads in the presence of a palmaris longus, in the additional tendons for ring and middle fingers, and in not having the extensor primi internodii pollicis.

As compared with the Chimpanzee, the Orang agreed in reference to the pronator radii teres and palmaris longus, but in the absence of the flexor longus pollicis as a slip from the profundus, and in the presence of the additional extensor tendons it differed.

Dr. Chapman differed from Bischoff, Owen, Huxley and others, in seeing no essential difference between the *scansorius*, of Traill, and the glutæus minimus in man, an opinion, it appears, which had been previously expressed by Prof. Barnard in 1876.

The leg and the foot of the Orang, as compared with man, differed in the absence of the peroneus tertius, plantaris, flexor longus hallucis and transversus pedis, in the fibular origin of the soleus, and in the presence of the external origin of the accessorius only, in the distribution of the perforating and perforated tendons of the toes, in the interossei, and in the presence of an opponens for the big toe. In this latter respect, the Orang differs not only from man, but from all the other monkeys and anthropoids, the foot having a very hand-like appearance, as compared with that of the Gorilla and Chimpanzee. The foot of the Orang differs further in the absence of a special flexor for the big toe. This is supplemented, to a certain extent, by the opponens, and in a partly developed accessorius.

If Professor Huxley's canon can be accepted that the distinction between a hand and a foot consists in the latter possessing tarsal bones, the peroneus longus and brevis, the short extensor and short flexor muscles, then the posterior extremity of the Orang terminates in a foot.

Dr. Chapman, however, appeared to think that the difference between the hand and the foot in Man, the Gorilla, and Chimpanzee, and the lower monkeys, is greater than that observed between the corresponding members of the Orang.

It is usually stated that the uvula is absent in the Orang, and on looking into the mouth, at first sight this appears to be the case, as it does not hang down as in man, between the pillars of the fauces. Nevertheless, Dr. Chap-