strains had been pre-eminent, has probably been the initiative cause of an acute epoch of crust-torsion and folding along oblique and transverse lines.

The new movements affected all European areas, dovetailing new folds into the midst of, and across, old folds, and determining new centres of virgation. In the Alps new arches and troughs were formed obliquely and transversely across the older series; the first-formed basins in the new movement were themselves over-arched or blocked up as the fan-shaped mountain-massives gradually became more and more compactly pressed together, and the great torsion-basins of southern Europe became confirmed in their new shape and position acquired in accordance with the altered conditions of crust equilibrium.

As might be expected, there is frequent indication that eruptive activity in Tertiary time broke out afresh in the same areas where eruptive activity had marked the Upper Carboniferous and Permo-Triassic period of movements. But the chief groups of eruptive rock round the inner *curves* of the Alps, Apennines and Carpathians, as well as the injections along oblique directions of shearing, may be clearly identified with the Tertiary torsion movements, for the most part, with the acute Mid-Tertiary epoch of torsion. The larger masses of igneous rocks in the central massives may belong in part to the ancient Palæozoic or Permo-Carboniferous epochs of upheaval, in part to the late-Mesozoic and Tertiary epochs.

A general conclusion may be made from the above that there are serpentines, diorites, granites, felsites, basalts in Alpine folds and faults which can be identified more especially with the "evolute" phenomena of Tertiary torsional movements. And these intrusions, injections, and eruptions involved in the last acute epoch of upheaval in Southern Europe are clearly correlated with similar eruptive phenomena throughout the same period in other parts of Europe, *e.g.* Auvergne, Scotland, Iceland.

MARIA M. OGILVIE.

THE MOTION OF A PERFECT LIQUID.[1]

IF we look across the surface of a river, we cannot fail to observe the difference of the movement at various points. Near one bank the velocity may be much less than near the other, and generally, though not always, it is greater in the middle than near either bank. If we could look beneath the surface and see what was going on there, we should find that the velocity was not so great near the bottom as at the top, and was scarcely the same at any two points of the depth. The more we study the matter, the more complex the motion appears to be; small floating bodies are not only carried down at different speeds and across each other's paths, but are whirled round and round in small whirlpools, sometimes even disappearing for a time beneath the surface. By watching floating bodies we can sometimes realise these complex movements, but they may take place without giving the slightest evidence of their existence.

You are now looking at water flowing through a channel of varying cross section, but there is very little evidence of any disturbance taking place. By admitting colour, although its effect is at once visible on the water, it does not help us much to understand the character of the flow. If, however, fine bubbles of air are admitted, we at once perceive (Fig. 1) the tumultuous conditions under which the water is moving and that there is a strong whirlpool action. This may be intensified by closing in two sides (Fig. 2), so as to imitate the action of a sluice gate, through the narrow opening of which the water has all to pass, the presence of air making the disturbed behaviour of the water very evident.

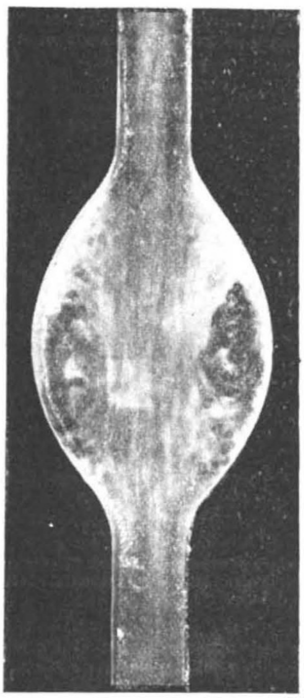

FIG. 1.

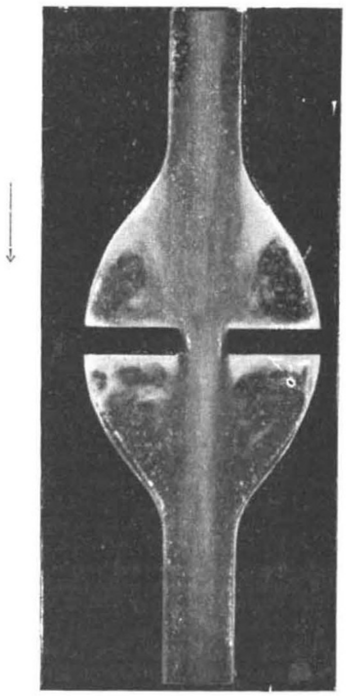

FIG. 2.

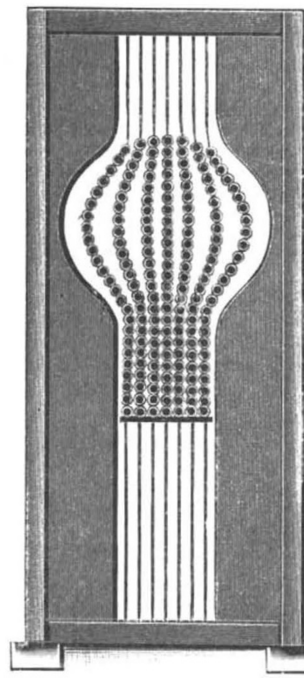

FIG. 3.

Now you will readily admit that it is hopeless to begin to study the flow of the water under such conditions, and we naturally ask, are there not cases in which the action is more simple? Such would be the case if the water flowed very slowly in a perfectly smooth and parallel river bed, when the particles would follow one another in lines called "stream-lines," and the flow would be like the march of a disciplined army, instead of like the movement of a disorderly crowd, in which free fights taking place at various points may be supposed to resemble the local disturbances of whirlpools or vortices.

The model (Fig. 3) represents on a large scale a section of the channel already shown, in which groups of particles of the water are indicated by round balls, lines in the direction of flow of these groups (which for convenience we may call particles) being coloured alternately. When I move these so that the lines are maintained, we imitate "stream-line" motion, and when, at any given point of the pipe, the succeeding particles always move at exactly the same velocity, we have what is understood as "steady motion."

[1] A discourse delivered at the Royal Institution on Friday, February 10, by Prof. H. S. Hele-Shaw.

As long as all the particles move in the straight portion of the channel, their behaviour is easy enough to understand. But as the channel widens out, it is clear that this model does not give us the proper distribution. In the model the wider portions are not filled up, as they would be with the natural fluid; for it must be clearly understood that the stream-lines do not flow on as the balls along these wires, passing through a mass of dead water, but redistribute themselves so that every particle of water takes part in the flow. Perhaps you may think that if these wires were removed, and the wooden balls allowed to find their own positions, they would group themselves as with an actual liquid. This is not the case; and, for reasons that you will see presently, no model of this kind would give us the real conditions of actual flow. By means of a model, however, we may be able to understand why it is so absolutely essential we should realise the correct nature of the grouping which occurs.

First look at the two diagrams (Figs. 4 and 5), which you will see represent channels of similar form to the experimental one. The same number of particles enter and leave in each under apparently the same conditions, so that the idea may naturally arise in your minds, that if the particles ultimately flow with the same speed whatever their grouping in the larger portion of the channel, it cannot much matter in what particular kind of formation they actually pass through that wider portion. To understand that is really very important. Let us consider a model (Fig. 6) specially made for the purpose. You will see that we have two lines of particles which we may consider stream-lines, those on the left coloured white, and those on the right coloured red. The first and last are now exactly 18 inches apart, there being eighteen balls of 1 inch diameter in the row. If I move the red ones upward, I cause them to enter a wider portion of the channel, where they will have to arrange themselves so as to be three abreast (Fig. 7). It is quite clear to you, that as I do this their speed in the wider portion of the channel is only one-third of that in the narrow portion, as you will see from the relative positions of the marked particles. Now, directly the first particle entered the wider channel, it commenced to move at a reduced speed, with the result that the particles immediately behind it must have run up against it, exactly in the same way that you have often heard the trucks in a goods train run in succession upon the ones in front, when the speed of the engine is reduced; and you will doubtless have noticed that it was not necessary for the engine actually to stop in order that this might take place. Moreover, the force of the impact depended largely upon the suddenness with which the speed of those in front was reduced. Applying this illustration to the model, you will see that the impact of these particles in the wider portion would necessarily involve a greater pressure in that part. Turning next to the white balls, I imitate, by means of the left-hand portion, the flow which will occur in a channel six times as large as the original one, and you now see (Fig. 7) that as the particles have placed themselves six abreast, and the first and last row are 3 inches apart instead of 18 inches, the speed in the wider portion of the channel must have been one-sixth of that in the narrow portion. Evidently, therefore, the velocity of the particles has been reduced more rapidly than in the previous case, and the pressure must consequently be correspondingly greater.

We may now take it as perfectly clear and evident, that the pressure is greater in the wider portion and less in the narrower portion of the channel. Turning now to the two diagrams, we see that the pressure is in each case greater in every row of particles as in the wider portions of the channel, but that instead of being suddenly increased, as in the model, it is gradually increased. The width of the coloured bands, that is, rows of particles, or width apart of stream-lines, is a measure of the increased pressure. Thus you will now regard the width of the bands, or what is the same thing, the distance apart of the stream-lines, as a direct indication of pressure, and the narrowness or closeness of the stream-lines as a direct indication of velocity.

Next notice the great difference between the two diagrams. In one diagram (Fig. 4) the change of width is uniform across the entire section. In diagram (Fig. 5), however, this is not the case. In the narrowest portion of the channel in each diagram there are seven colour bands of little balls each containing three abreast, but we find that in one diagram (Fig. 4) they are equally spaced in the wider part six abreast throughout. In the other diagram (Fig. 5) the outer row is spaced eight abreast, the second row rather more than six, and the inner rows rather more than four abreast, and the middle row less than four abreast, making in all forty-two in a row, as in the previous case. One diagram (Fig. 5) therefore will represent an entirely different condition to the state represented by the other diagram (Fig. 4), the pressure in the wide part of the latter varying from a maximum at the outside to a minimum in the middle, while the corresponding velocity is greatest in the middle and least at the outside or borders.

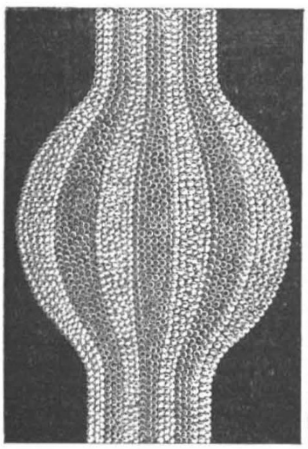

FIG. 4.

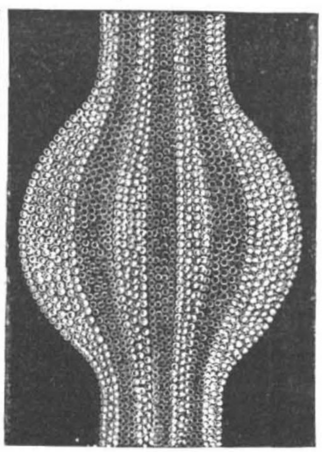

FIG. 5.

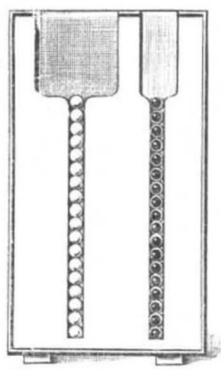

FIG. 6.

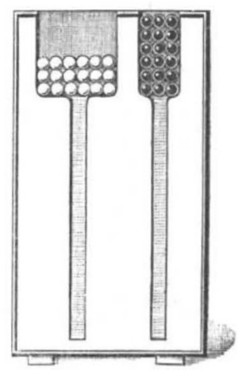

FIG. 7.

Now, when we know the pressure at every point of a liquid, and also the direction in which the particles are moving, together with their velocity at every point, we really know all about its motion, and you will see how important the question of grouping is, and that, in fact, it really constitutes the whole point of my lecture to-night. How then shall we ascertain which of the two groupings (Fig. 4 or 5) is correct, or whether possibly some grouping totally different from either does not represent the real conditions of flow?

Now, the model does not help us very far, because there seems to be no means of making the grouping follow any regular law which might agree with fluid motion. In whatever way we improve such a model, we can scarcely hope to imitate by merely mechanical means the motion of an actual liquid, for reasons which I will now try to explain.

In the first place, apart from the particles having no distinguishing characteristics, either when the liquid is opaque or transparent, they are so small and their number is so great as to be almost beyond our powers of comprehension. Let me try, by means of a simple illustration, to give some idea of their number, as arrived at by perfectly well recognised methods of physical computation. Lord Kelvin has used the illustration that, supposing a drop of water were magnified to the size of the earth, the ultimate particles would appear to us between the size of cricket-balls and foot-balls. I venture to put the same fact in another way, that may perhaps strike you more forcibly. This tumbler contains half a pint of water. I now close the top. Suppose that, by means of a fine hole, I allow one and a half billion particles to flow out per second—that is to say, an exodus equal to about one thousand times the population of the world in each second,—the time required to empty the glass would be *between* (for of course we can only give certain limits) seven million and forty-seven million years.

In the next place, we have the particles interfering with each other's movements by what we call "viscosity."

Of course, the general idea of what is meant by a "viscous" fluid is familiar to everybody, as that quality which treacle and tar possess in a marked degree, glycerine to a less extent, water to a less extent than glycerine, and alcohol and spirits least of all. In liquids, the property of viscosity resembles a certain positive "stickiness" of the particles to themselves and to other bodies; and would be well represented in our model by coating over the various balls with some viscous material, or by the clinging together, which might take place by the individuals of a crowd, as contrasted with the absence of this in the case of no viscosity as represented by the evolutions of a body of soldiers. It may be accounted for, to a certain extent, by supposing the particles to possess an irregular shape, or to constantly move across each other's paths, causing groups of particles to be whirled round together.

Whatever the real nature of viscosity is, it results in producing in water the eddying motion which would be perfectly impossible if viscosity were absent, and which makes the problem of the motion of an imperfect liquid so difficult and perplexing.

Now, all scientific advance in discovering the laws of nature has been made by first simplifying the problem and reducing it to certain ideal conditions, and this is what mathematicians have done in studying the motion of a liquid.

We have already seen what almost countless millions of particles must exist in a very small space, and it does require a much greater stretch of the imagination to consider their number altogether without limit. If we then assume that a liquid has no viscosity, and that it is incompressible, and that the number of particles is infinite, we arrive at a state of things which would be represented in the case of the model or the diagram on the wall, when the little globes were perfectly smooth, perfectly round and perfectly hard, all of them in contact with each other, and with an unlimited number occupying the smallest part of one of the coloured or clear bands. This agrees with the mathematical conception of a perfect liquid, although the mathematician has in his mind the idea of something of the nature of a jelly consisting of such small particles, rather than of the separate particles themselves. The solution of the problem of the grouping of the little particles, upon which so much depends, and which may have at first seemed so simple a matter, really represents, though as yet applied to only a few simple cases, one of the most remarkable instances of the power of higher mathematics, and one of the greatest achievements of mathematical genius.

You will be as glad as I am that it is not my business to-night to explain the mathematical processes by which the behaviour of a perfect liquid has been to a certain extent investigated. You will also understand why such models as we could actually make, or any analogy with the things with which we are familiar, would not help us very much in obtaining a mental picture of the behaviour of a perfect liquid. If, for instance, we try to make use of the idea of drilled soldiers, and move the lines with that object in view, we see that instead of the ordinary methods of drill, the middle rank soon gains on the others, and enters again the parallel portion of the channel in a very different relative position to the opposite lines, although the stream-lines would all have the same actual velocity when once again in the parallel portion. Since, then, we cannot use models or any simple analogy with familiar things, or follow—at any rate this evening—the mathematical methods of dealing with the problem, what way of understanding the subject is left to us?

If we take two sheets of glass, and bring them nearly close together, leaving only a space the thickness of a thin card or piece of paper, and then by suitable means cause liquid to flow under pressure between them, the very property of viscosity, which, as before noted, is the cause of the eddying motion in large bodies of water, in the present case greatly limits the freedom of motion of the fluid between the two sheets of glass, and thus prevents, not only eddying or whirling motion, but also counteracts the effect of inertia. Every particle is then compelled by the pressure behind and around it to move onwards without whirling motion, following the path which corresponds exactly with the stream-lines in a perfect liquid.

If we now, by a suitable means, allow distinguishing bands of coloured liquid to take part in the general flow, we are able to imitate exactly the conditions we are seeking to understand.

[Prof. Hele-Shaw here gave demonstrations of the stream-lines in liquids flowing under the conditions of a gradually enlarging and contracting channel. He proved that the condition of flow corresponded closely with that shown in Fig. 5 and *not* with that given in Fig. 4. The method of the experiments has already been described in NATURE (vol. lviii. p. 34), though by using glycerine instead of water much more perfect results were obtained than in those then described.]

But at this stage you may reasonably inquire how it is that we are able to state, with so much certainty, that the artificial conditions of flow with a viscous liquid are really giving us the stream-line motion of a perfect one; and this brings me to the results which mathematicians have obtained.

The view now shown represents a body of circular cross-section, past which a fluid of infinite extent is moving, and the lines are plotted from mathematical investigation and represents the flow of particles. This particular case gives us the means

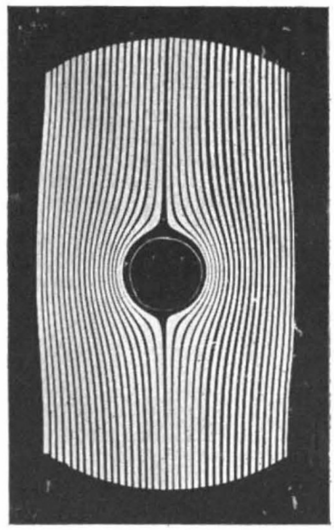

FIG. 8.

of most elaborate comparison; although we cannot employ a fluid of infinite extent, we can prepare the border of the channel to correspond with any one of the particular stream-lines, and measure the exact positions of the lines inside.

By means of a second lantern, the real flow of a viscous liquid for this case is shown upon the second screen, and you will see that it agrees with the calculated flow round a similar obstacle of a perfect liquid. The diagram shown on the wall is the actual figure employed for comparison, and upon which the experimental case was projected. By this means, it was proved that the two were in absolute agreement. If we start the impulses, as before, in a row, we at once see how the middle particles lag behind the outer ones, as indicated by the width of the bands, showing that it is not necessarily the side stream-lines that move more slowly. It may be more interesting to you to see, in addition to the foregoing case—in which for convenience, and as quite sufficient for measurement only, a semi-cylinder was employed—the case of a complete cylinder (Fig. 8). In this case two different colours are used in alternate bands, and these bands are sent in, not steadily, but impulsively, in order to illustrate what I have just pointed out. You will see how the greater width of the colour bands before and behind the cylinder indicates an increase of pressure in those regions. This in a ship-shape form accounts for the standing bow and stern waves, whereas the narrowing of the bands at the sides indicates an increase of velocity and reduction of pressure, and accounts for the depression of water level, with which you are doubtless familiar, at the corresponding part of a ship.

I will now take a more striking case. If, instead of a circular body, we had a flat plate, the turbulent nature of the flow is evidently very great, as you will see from the view (Fig. 9), which is a photograph of the actual flow under these conditions, made visible by very fine air bubbles, and showing water at rest in the clear space behind the obstacle.

We can, however, take steps to reduce this turbulence, and you now see on the second screen the flow by means of apparatus which time does not permit me to describe, but which gives a slow and steady motion that it would be impossible to improve upon in actual conditions of practice, or even, I am inclined to think, by any experimental method. Instead of using air to

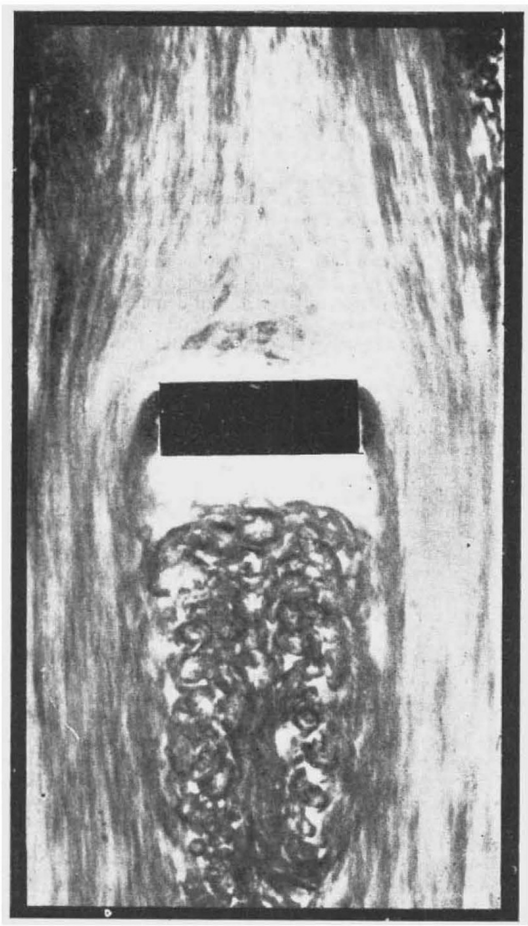

FIG. 9.

make this flow clear, we now allow colour to stream behind the plate, and you will see that the water still refuses to flow round to the back, and spreads on either side. We have so slow a velocity as not to induce vortex motion, but the inertia of the particles which strike the flat plate causes them to be deflected to either side, exactly as tennis-balls in striking against a wall obliquely. The sheet of water is so thick, that is to say, the parallel glass plates are so far apart, that they do not enable the viscosity of the water to act as a sufficient drag to prevent this taking place.

Mathematicians, however, predicted with absolute certainty that with stream-line motion, the water should flow round and meet at the back, a state of things that, however slow we make the motion in the present case, does not occur owing to the effect of inertia. They have drawn with equal confidence the lines along which this should take place. We could either effect this result with the experiment you have just seen, by using a much more viscous liquid, such as treacle, or, what comes to the same thing, bringing the two sheets of glass nearly close together; and the flow which you are now witnessing (Fig. 10) shows the result of doing this. The colour bands in front of the plate no longer mix at all with the general body of flow, or are unsteady, as was the case in the last experiment, but flow round the plate, and flow so steadily, that unless we jerk the flow of the colour bands, it is impossible to tell in which direction they are actually moving. It is interesting to note that where the divided central colour band re-unites is clearly shown in the illustration.

Whilst I have been dealing with the stream-lines of a perfect liquid, your minds will doubtless have turned to the lines along which magnetic and electrical forces appear to act. We are possibly further from realising the actual nature of these forces, than from a correct conception of the real nature of a liquid. We have long agreed to abandon the old ideas of the electrical and magnetic fluids flowing along these lines, and to substitute instead the idea that these lines represent merely the directions in which the forces act. Now we can easily see that this conception is quite a reasonable one, for in the case of the model it is not necessary to have the row of balls actually moving in order that the effect may be transmitted along the different lines they occupy. If I attempt to raise the plate upon which they rest, the pressure is instantly transmitted through

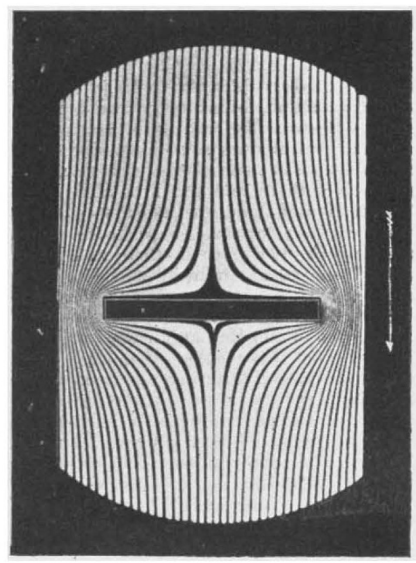

FIG. 10.

the whole row to the top ball along each line, whatever curve the line may take. In the same way, you will remember that it was not necessary to have the colour bands actually in motion, for, though apparently free to move in any direction, they retain their form for a considerable time, and the path along which they would influence each other as soon as the tap is opened would be along those lines in which the liquid was flowing before it was brought to rest. Hence it is possible, with some suitable means, to cause a viscous liquid to reproduce exactly the lines of magnetic and electrical induction. In the case of magnetism and electricity, it is of course possible, by means of a small magnetic needle or a galvanometer, by exploring the whole surface through which magnetic induction or electrical flow is acting, to plot the lines of force for innumerable cases, where we can work in air or on the surface of the solid conductor.

But in this building it seems natural to take as an example the case first used by the great man to whom the conception of lines of magnetic force is due, for the first reference I have been able to find to such lines is in one of Faraday's earliest papers on the indication of electric currents ("Experimental Researches in Electricity," vol. i. p. 32), in which he says, "By magnetic curves I mean the lines of magnetic forces, however modified by the juxtaposition of poles, which would be depicted by iron filings, or those to which a very small magnetic needle would form a tangent."

You are all familiar with the way in which iron filings set themselves when shaken over the north and south poles of a magnet. The magnetic lines are then nearly, but not quite, circular curves between the two poles. Now, the mathematics of the subject tells us that if the poles could be regarded as points, the lines of force between them would be perfect circles.

You are now looking at the colour bands, the edges—or indeed any portion—of which represent lines obtained by admitting coloured liquid from a series of small holes round a central small orifice, which admits clear liquid, and allows them to escape through another small orifice (called respectively in hydromechanics a *source* and *sink*), and I leave it to you to judge how far these curves deviate from the ideal form.

My assistant is now allowing the colour to flow, first steadily, and then in a series of impulses, and the latter gives us the conception of waves or impulses of magnetic force, though of course the magnetic transmission force would be instantaneous. Regarded as a liquid, it is here again clear how absolutely the truth of our views concerning the slower movement in the wider portion is verified by this experiment.

A last experiment shows the streams admitted, not from a source, but from a row of orifices in what corresponds to the slowest moving portion of the flow. The result is that the colour bands are much narrower, and although the circular forms of the curves are, as in the previous experiment, preserved, the lines are so fine at the point of exit, which, as before, corresponds to the South Pole, as to really approximate to ideal stream-lines.

The same method enables us to trace the lines of force through solid conductors, for, as long as we confine ourselves to two dimensions of space we may have *flat* conductors of any shape whatever. But it does something more, for by making the film rather deeper in some places than others, more particles arrange themselves there, and the lines of flow will naturally tend in the direction of the deeper portion. This will give the stream-lines identically the same shape as the magnetic or electrical curves which encounter in their paths a body of less resistance, for instance, a para-magnetic body.

If, on the other hand, at these points the film is made rather thinner, less particles will be able to dispose of themselves in the shallow portion of the film, and hence the lines of flow will be pushed away from this portion, giving us exactly the same forms as magnetic lines of force in a magnetic field in proximity to a diamagnetic body.

Here, again, mathematical methods have enabled lines of actual flow to be predicted, and you may compare the actual flow for the case of a cylindrical para-magnetic body, which was worked out some years ago.

You will doubtless not be inclined to question the practical value of stream-lines in the subject which we have just been considering, because, unlike the flow of an actual liquid, magnetic lines of force can never be themselves seen, and because there is no doubt as to the correspondence of the directions to the lines of a perfect liquid. It was the conception of these lines in the mind of Faraday, and more particularly their being cut by a moving wire, that enabled him to realise the nature of the subject more clearly than any other man at the time, and to do much towards the rapid development of electrical science and its practical applications.

When we come to consider the relation of the study of the motion of a perfect liquid with hydromechanics and naval architecture, it must be admitted that the matter is a difficult one. Probably one of the most perplexing things in engineering science is the absence of all apparent connection between higher treatises on hydrodynamics and the vast array of works on practical hydraulics. The natural connection between the treatises of mathematicians and experimental researches of engineers would appear to be obvious, but very little, if any, such connection exists in reality, and while at every step electrical applications owe much to the theories which are common to electricity and hydromechanics, we look in vain for such applications in connection with the actual flow of water.

Now the reason for this appears to be the immense difference between the flow of an actual liquid and that of a perfect one owing to the property of viscosity. A comparison of the various experiments which you have seen to some extent indicates this.

In the first place, let us consider for a moment some of the things which would happen if water were a perfect liquid. In such a case, a ship would experience a very different amount of resistance, because, although waves would be raised, owing to the reasons which we have already seen, the chief causes of resistance, viz. skin friction and eddying motion, would be entirely absent, and of course a submarine boat at a certain depth would experience no resistance at all, since the pressures fore and aft would be equal. On the other hand, there would be no waves raised by the action of the wind, and there would be no tidal flow, but to make up for this rivers would flow with incredible velocity, since there would be no retarding forces owing to the friction of the banks. But the rivers themselves would soon cease to flow because there would be no rainfall such as exists at present, since it is due to viscosity that the rain is distributed, instead of falling upon the earth in a solid mass when condensed. In a word, it may be said that the absence of viscosity in water would result in changes which it is impossible to realise.

We may now briefly try to consider the difference between practical hydraulics and the mathematical treatment of a perfect liquid. The earliest attempts to investigate in a scientific way the flow of water appears to have been made by a Roman engineer about 1800 years ago, an effort being made to find the law for the flow of water from an orifice. For more than 1500 years, however, even the simple principle of flow according to which the velocity of efflux varies as the square of the head, or what is the same thing, the height of surface above the orifice varies as the square of the velocity, remained unknown. Torricelli, who discovered this, did so as the result of observing that a jet of water rose nearly to the height of the surface of the body of water from which it issued, and concluded therefore that it obeyed the then recently discovered law of all falling bodies.

Though it was obvious that this law did not exactly hold, it was a long time before it was realised that it was the friction or viscosity of liquids that caused so marked a deviation from the simple theory. Since then problems in practical hydraulics, whether in connection with the flow in rivers or pipes, or the resistance of ships, have largely consisted in the determination of the amount of deviation from the foregoing simple law.

About one hundred years ago it was discovered that the resistance of friction varies nearly in accordance with the simple law of Torricelli, and also—although for a totally different reason—the resistances due to a sudden contraction or enlargement of cross-section of channel or to any sudden obstructions appear to follow nearly the same law. Now it is extremely convenient for reasons which will be understood by students of hydraulics to treat all kinds of resistance as following the same law, viz. square of velocity which the variation of head or height of surface has shown to do. But this is far from being exact, and an enormous amount of labour has consequently been expended in finding for all conceivable conditions in actual work tables of coefficients or empirical expressions which are required for calculations of various practical questions. Such data are continually being accumulated in connection with the flow of water in rivers and pipes for hydraulic motors and naval architecture. This is the practical side of the question.

On the other hand, eminent mathematicians, since the days of Newton and the discovery of the method of the calculus, have been pursuing the investigation of the behaviour of a perfect liquid. The mathematical methods, which I have already alluded to as being so wonderful, have, however, scarcely been brought to bear with any apparent result upon the behaviour of a viscous fluid. Indeed, the mathematician has not been really able to adopt the method of the practical investigator, and deal with useful forms of bodies such as those of actual ships, or of liquid moving through ordinary channels of varying section, even for the case of a perfect liquid, but he has had to take those cases, and they are very few indeed, that he has been able to discover which fit in with his mathematical powers of treatment.

This brief summary may possibly serve to indicate the nature of the difficulties which I have pointed out, and will show you the vast field there yet lies open for research in connection with the subject of hydromechanics, and the great reception which awaits the discovery of a theoretical method of completely dealing with viscous liquids, instead of having recourse as at present principally to empirical formula based on the simple law already alluded to.

We may, however, console ourselves with the thought, that in the application of the laws of motion themselves to *any* terrestrial matters, the friction of bodies must always be taken into account, and renders it necessary, that we should commence by studying the ideal conditions. In this as in other matters, the naval architect and engineer must always endeavour as far as possible to base their considerations and work upon the secure foundation of scientific knowledge, making allowances for disturbing causes, which then cease to be the source of perplexity and confusion. From this point of view, the study of the behaviour of a perfect liquid, even when no such form of matter appears to exist, has an interest for the practical man in spite of the deviation of actual liquids from such ideal conditions. If the truth must be told, it is such a deviation from the simple and ideal conditions that really constitute the work of a professional man, and it is only practical experience which, based upon sound technical knowledge, enables 50,000 tons of steel to be made to span the Firth of Forth, Niagara to be harnessed to do the work of 100,000 horses, or an *Oceanic* to be slid into the sea with as little misgiving as the launch of a fishing-boat.

I have, I am afraid, brought you only to the threshold of a vast subject, and in doing so have possibly employed reasoning of too elementary a kind. After all, I may plead that I have followed the dictum of Faraday, who said, "If assumptions must be made, it is better to assume as little as possible." If I have assumed too little knowledge on your part, it is because of the difficulties I have found in the subject myself. If I have left more obscure than I have been able to make clear, it is consoling to think how many centuries were required to discover even what is known at the present time, and we may well be forgiven if we cannot grasp at once results which represent the life-work of some of the greatest men.

A PROBLEM IN AMERICAN ANTHROPOLOGY.[1]

WHILE engaged in writing the address that I am to read to you this evening, the sad news reached me of the death, on July 31, of our President of five years ago, Dr. D. G. Brinton. Although not unexpected, as his health had been failing since he was with us at the Boston meeting, where he took his always active part in the proceedings of Section H, and gave his wise advice in our general council, yet his death affects me deeply. I was writing on a subject we had often discussed in an earnest but friendly manner. He believed in an all-pervading psychological influence upon man's development, and claimed that American art and culture were autochthonous, and that all resemblances to other parts of the world were the results of corresponding stages in the development of man; while I claimed that there were too many root coincidences with variant branches to be fully accounted for without also admitting the contact of peoples. Feeling his influence while writing, I had hoped that he would be present to-night, for I am certain that no one would have more readily joined with me in urging a suspension of judgment, while giving free expression to opinions, until the facts have been worked over anew, and more knowledge attained.

Now that his eloquent tongue is silent and his gifted pen is still, I urge upon all who hear me to-night to read his two addresses before this Association—one as Vice-President of the Anthropological Section in 1887, published in our thirty-sixth volume of *Proceedings*, the other as retiring President in 1895, published in our forty-fourth volume. In these addresses he had in his usual forcible and comprehensive manner presented his views of American anthropological research and of the aims of anthropology.

Dr. Brinton was a man of great mental power and erudition. He was an extensive reader in many languages, and his retentive memory enabled him to quote readily from the works of others. He was a prolific writer, and an able critic of anthropological literature the world over. Doing little as a field archæologist himself, he kept informed of what was done by others through extensive travels and visits to museums. By his death American anthropology has suffered a serious loss, and a great scholar and earnest worker has been taken from our Association.

In the year 1857 this Association met for the first time beyond the borders of the United States, thus establishing its claim to the name American in the broadest sense. Already a member of a year's standing, it was with feelings of youthful pride that I recorded my name and entered the meeting in the hospitable city of Montreal, and it was on this occasion that my mind was awakened to new interests which in after years led me from the study of animals to that of man.

On Sunday, August 16, while strolling along the side of Mount Royal, I noticed the point of a bivalve shell protruding from roots of grass. Wondering why such a shell should be there, and reaching to pick it up, I noticed on detaching the grass roots about it that there were many other whole and broken valves in close proximity—too many, I thought, and too near together, to have been brought by birds, and too far away from water to be the remnants of a musk-rat's dinner. Scratching away the grass and poking among the shells, I found a few bones of birds and fishes and small fragments of Indian pottery. Then it dawned upon me that there had been an Indian home in ancient times, and that these odds and ends were the refuse of the people—my first shell-heap or kitchen-midden, as I was to learn later. At the time this was to me simply the evidence of Indian occupation of the place in former times, as convincing as was the palisaded town of old Hochelaga to Cartier when he stood upon this same mountain side more than three centuries before.

At that meeting of the Association several papers were read, which, had there been a section of anthropology, would have led to discussions similar to those that have occurred during our recent meetings. Forty-two years later we are still disputing the evidence, furnished by craniology, by social institutions and by language, in relation to the unity or diversity of the existing American tribes and their predecessors on this continent.

Those were the days when the theory of the unity of all American peoples, except the Eskimo, as set forth by Morton in his "Crania Americana" (1839), was discussed by naturalists. The volumes by Nott and Gliddon, "Types of Mankind" (1854) and "Indigenous Races of the Earth" (1857), which contains Meigs' learned and instructive dissertation, "The Cranial Characteristics of the Races of Men," were the works that stirred equally the minds of naturalists and of theologians regarding the unity or diversity of man—a question that could not then be discussed with the equanimity with which it is now approached. The storm caused by Darwin's "Origin of Species" had not yet come to wash away old prejudices and clear the air for the calm discussion of theories and facts now permitted to all earnest investigators. Well do I remember, when, during those stormy years, a most worthy Bishop made a fervent appeal to his people to refrain from attending a meeting of the Association then being held in his city, on account of what he claimed to be the atheistic teachings of science. Yet ten years later this same venerable Bishop stood before us, in that very city, and invoked God's blessing upon the noble work of the searchers for truth.

At the meeting of 1857 one of our early presidents, the honoured Dana, read his paper entitled "Thoughts on Species," in which he described a species as "a specific amount or condition of concentrated force defined in the act or law of creation," and, applying this principle, determined the unity of man in the following words:—

"We have therefore reason to believe, from man's fertile intermixture, that he is one in species; and that all organic species are divine appointments which cannot be obliterated unless by annihilating the individuals representing the species."

Another paper was by Daniel Wilson, recently from Scotland, where six years before he had coined that most useful word "prehistoric," using the term in the title of his volume, "Prehistoric Annals of Scotland." In his paper Prof. (afterwards Sir Daniel) Wilson controverted the statement of Morton that there was a single form of skull for all American peoples, north and south, always excepting the Eskimo. After referring to the views of Agassiz, as set forth in the volumes of Nott and Gliddon, he said, "Since the idea of the homogeneous physical characteristics of the whole aboriginal population of America, extending from Terra del Fuego to the Arctic circle, was first propounded by Dr. Morton, it has been accepted without question, and has more recently been made the basis of many widely comprehensive deductions. Philology and archæology have also been called in to sustain this doctrine of a special unity of the American race; and to prove that, notwithstanding

[1] Address delivered before the American Association for the Advancement of Science, at Columbus, Ohio, on August 21, by Prof. Frederic Ward Putnam, the retiring President of the Association.