SCIENCE

FRIDAY, NOVEMBER 11, 1898.

CONTENTS:

MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

ON RECENT PROGRESS TOWARD THE SOLUTION OF PROBLEMS IN HYDRODYNAMICS.

IN this paper I shall not attempt to give an exhaustive account of the progress which has been made in hydrodynamics of recent years. Such an account, though possibly useful for purposes of reference, would be tedious and unsuitable for reading before an audience. I shall, therefore, try to give some idea of the general lines on which research has been carried on, laying stress on the more important discoveries and avoiding, as far as possible, mere technical details.

The choice of the period to be selected is not difficult. In 1846 Professor Stokes presented a report on the condition of hydrodynamics at that time, and this was continued by Professor Hicks in 1881–1882. Both these papers are printed in the reports of the British Association for the Advancement of Science. In the *Mathematische Annalen* for 1887 Mr. A. E. H. Love gave a summary of our knowledge of Vortex Motion, and Hicks practically carried this to the present day in his presidential address before Section A of the British Association in 1896. Professor Darwin's article on Tides in the Encyclopædia Britannica carries our knowledge of that subject to 1888. Hence I shall take the progress made in the general subject since 1882; the work on Tides will be taken from 1888 only, while Vortex Motion will be omitted altogether.

The application of mathematics to the solution of many natural problems in fluid motion possesses one difficulty which is not common to most of the problems which confront physicists. It arises from the fact that we are frequently unable to apply the method of approximation. It usually happens that when a problem arises from some natural phenomenon it is not capable of direct solution. But the mathematician is generally able to consider a simpler problem which more or less closely corresponds to the given conditions. Having solved it, he is able to take into account the conditions of the actual problem, and so to obtain a solution to any degree of accuracy which may be desired. This is frequently *not* the case with problems in fluid motion. The differential equations of motion may, perhaps, be written down, but the limitations which have to be imposed before a solution can be discovered are so numerous that the solution, when found, often gives no approximation at all to the real circumstances. To illustrate this, we need only mention the case of a sphere moving through water. If we neglect the friction between the water and the surface of the sphere, the viscosity and compressibility of the water, it is easy to find all the circumstances of the motion. But when the velocity of the sphere is not small, and we take these neglected circumstances into account, the motion, as any one will agree, quite changes its character. Instead of the stream lines —that is, the lines followed by the molecules of the fluid—being regular curves, eddies are formed along the surface of the sphere, and the motion in the rear of the sphere becomes turbulent and seems to defy all attempts at calculation. Further, the resistance to motion caused by the fluid, instead of being zero, as in the simplified problem, actually becomes very large. And here, it is to be remembered, we are dealing with a simple case of a class of problems which has a high practical interest—the resistance experienced by a ship moving at a speed which we are accustomed to expect, say, from ten to twenty miles an hour. The engineer now knows fairly well the resistance by experiment. But neither he nor the mathematician can calculate the resistance when the speed is forty miles an hour, and this speed has already become an accomplished fact.

I shall first deal with problems in which the motion is irrotational, that is, where the separate molecules of the fluid are not supposed to possess any rotation of their own independently of the rotation of the whole mass. In a fluid which is non-viscous such a molecular rotation can never be set up by any conservative system of forces. If we neglect the viscosity, and the skin friction of the solid with which it is in contact, all motions considered will be of the irrotational class. This class can be again divided into two others: first, that known as *continuous*, in which the pressure never comes out to be negative; and secondly, that known as *dscontinuous*, in which we may have a negative pressure or a surface across which there may be a finite change of velocity. In the first case the fluid completely occupies the spaces around the solid with which it is in contact. In the second case hollows may be formed and there may be a free surface, or the fluid in motion may be in contact with other fluid at rest. After treating these two classes of problems I shall go on to mention the advances made in various kinds of wave motion, including the tides. Then will follow the motions and forms of masses of fluid rotating about an axis under their own gravitation only. Finally, the influence of viscosity will be considered.

We shall first consider those problems in which the fluid is incompressible and without viscosity and its motion is irrotational, steady and takes place in two dimensions. If we take the plane of motion to be that of $(x, y,)$ this class, of course, includes the case of motion in three dimensions where the motion of every particle in a straight line perpendicular to the plane of (x, y) is the same. The theory depends on the velocity-potential φ and the steam-function ψ. If u, v be the velocities of a particle of the fluid at the point (x, y) the following equations hold:

$$u = \frac{\partial \varphi}{\partial x} \quad , \quad v = \frac{\partial \varphi}{\partial y},$$

$$u = -\frac{\partial \psi}{\partial y} \quad , \quad v = +\frac{\partial \psi}{\partial x},$$

$$\frac{\partial^2 \varphi}{\partial x^2} + \frac{\partial^2 \varphi}{\partial y^2} = 0 = \frac{\partial^2 \psi}{\partial x^2} + \frac{\partial^2 \psi}{\partial y^2}.$$

Putting $z = x + iy \quad , \quad w = \varphi + i\psi,$

these equations show that the determination of the motion reduces to the discovery of a function

$$w = f(z),$$

which will satisfy the given boundary conditions.

When w has been determined as a function of z, the curves,

$$w = \text{pure imaginary},$$

give the stream-lines, that is, the lines followed by the molecules of the fluid in their motion. The velocities are determined by partial differentiation with respect to x, y.

It is unnecessary to say more about the general problem. It is that of Dirichlet in the Theory of Functions. Solutions can, in general, only be obtained by the inverse process of taking known solutions of

$$\frac{\partial^2 \varphi}{\partial x^2} + \frac{\partial^2 \varphi}{\partial y^2} = 0 \text{ or } \frac{\partial^2 \psi}{\partial x^2} + \frac{\partial^2 \psi}{\partial y^2} = 0,$$

and inquiring what boundary conditions they can be made to satisfy. When the motion is discontinuous the boundaries, instead of being fixed walls, are free surfaces along which the velocity is constant.

As we are searching for the stream-lines

$$w = \text{pure imaginary},$$

that is, the lines parallel to the real axis in the w-plane, all that is necessary is to find the curves in the z-plane which correspond to lines parallel to the real axis in the w-plane. The problem is, therefore, reduced to one of conform representation. For walls consisting of straight lines a few cases have been long known, being solved by Schwarz's method. Some new ones, with more complicated rectilinear boundaries, were given by Réthy in 1895.

In 1890 Mitchell gave a new form to Schwarz's method which enabled him to solve some further problems in discontinuous motion. These are mainly the cases of jets issuing from apertures into fluid which may be at rest or in motion. This was shortly afterwards modified and extended by Love. Still later B. Hopkinson added the case where several sources or sinks might be present. (These are points where the fluid is supposed to enter or leave in finite quantities.) The transformation does not in general contain infinities, but Hopkinson includes the case where $\frac{dw}{dz}$ becomes infinite like

$$\frac{A_1}{z - z_1} + \cdots + \frac{A_n}{(z - z_1)^n}.$$

Several hydrodynamical examples are given, but the most interesting are the electrical applications.

These problems are of little practical value. They frequently demand the existence of dead water behind the moving solid. For example, if a rectangular board be drawn through a fluid it is directly seen that the fluid behind the obstacle is far from being at rest, which the theory would indicate. Again, if water is entering into a large tank which is full, by means of a projecting tube in the side of the tank, the fluid, instead of spreading in all directions (by the theory), actually moves like a fluid cylinder for a short distance. In fact, the fluid, instead of moving back along the outside of the projecting tube, actually moves the other way. The difference is, of course, caused by the viscosity of the fluid, which, even when small, produces vortex or eddy motion of a complicated kind. The difficulties have been stated with some detail and with several illustrations by Lord Kelvin in *Nature*, 1894.

Just lately Professor Hele-Shaw has succeeded in photographing the movements of an actual fluid under similar circumstances. It is striking to observe that when the motion of the water takes place between two parallel plates not far apart, and when its velocity is not very great, the stream-lines follow almost exactly the theoretical positions which they would have under the assumptions made above. In the case of a rectangular plate held at an angle of 45° to a steady stream the stream-lines agree almost exactly with the hyperbolas given by theory. When the parallel plates are not close, however, or the motion takes place in a tube of not too small section, any variation in the diameter of the tube soon produces turbulent motion.

When the motion takes place in three dimensions I divide it again into continuous and discontinuous motion. On neither is there much to say. When the motion is continuous, and there are no forces acting on the solid which is moving through the fluid, the system of differential equations admits of three integrals, and the integration is practically finished if we can find a fourth. Several Continental writers have been considering, during the past two or three years, under what circumstances a fourth integral of specified form may exist. Miss Fawcett has examined the case of the motion of a solid of the form known as an isotropic helicoid when gravity is the only force acting. The most interesting case is that where the solid starts from rest. The path followed by the center of gravity is traced. The motion of an anchor ring when there is circulation through the aperture has been discussed by Greenhill, Basset and Dyson. The first-named has applied the σ-functions to the solution of the problem. Basset has also discussed the motion of a spherical bowl. No problem of discontinuous motion in three dimensions has been yet solved, notwithstanding many unpublished attempts. I shall return to this subject later on.

Coming next to the general theory of wave-motion, including the problem of the tides, two main classes may be distinguished. Waves of expansion belong primarily to the theory of sound and they will not be touched upon here. The second class concerns the different kinds of waves which our every-day experience of water in motion brings before us. It includes wave-motion on the large scale as exhibited by the tides, and on a smaller scale in the long waves which are known under the name of a 'ground-swell' in the Atlantic; next the waves whose effects have the greatest destructive power—the short waves whose height is not very small compared with their length—the waves raised by the wind in a river, those which follow in the wake of a ship, the solitary wave travelling up a canal or in some rivers, known in England as a 'bore;' finally, the waves which are very small and which are mainly propagated by the surface tension of the fluid. These last are known as capillary waves and will only be mentioned incidentally. A general distinction of all these waves on the surface of water is made by mathematicians—the long waves, where it appears permissible to neglect the vertical acceleration of the particles of the fluid without materially affecting the results, and the short waves, where this vertical acceleration may not be neglected in the equations of motion. I shall, as far as possible, deal with them in this order.

The neglect of the vertical acceleration, which implies that the dynamical pressure is equal to the statical pressure, greatly simplifies the problem of wave-motion. Neglecting compressibility and viscosity, the ordinary problems of long wave-motion are not very difficult. The motion of long waves in canals is an old problem and was used by Airy for explaining the tides. A paper by McCowan in 1892 on these long waves when the section of the canal is uniform must be mentioned. Usually the canal has been taken to be of rectangular section. McCowan treats one which has sloping sides. Incidentally he obtains a canal of such shape that these long free waves are propagated along it without change of form. But a more important result is the detection of a serious error in Airy's explanation of the double high-tide, sometimes observed in estuaries and rivers. The method adopted was, as usual, one of continued approximation, and Airy fell into the error—not unknown to mathematical physicists—that of carrying his approximations further than the initial assumptions warranted. It appeared that the wave might divide into two or even three waves. McCowan shows that this division of the wave is without foundation when the equations are correctly treated and that, therefore, Airy's explanation of the double high-tide fails. The double high-tide is still unexplained..

The general theory of tides is fully dealt with and brought up to date by Professor Darwin in his article in the Encyclopædia Britannica of date 1888. Two important papers have appeared since then. To report how these two papers have advanced our knowledge of tidal theory, a few remarks must be made on the general problem.

The theory of the tides is mainly one of forced oscillations. The sun and moon, moving in orbits round the earth whose essential nature is periodic, their motions are expressible by means of sums of sines or cosines of angles which vary with the time, and the periods of these terms differ. The difference of the attractions on the earth and the water which covers it produces oscillations with periods which correspond to those of the sun and moon. Hence the periods of the principal tides will be known in advance. Notwithstanding this fact, the general equations which express the motion of the water have hitherto only been integrated on the following assumptions: First, that the particles of water never move far from their mean position in comparison with the radius of the earth—an assumption which is easily justified and need not be discussed. Secondly, that the ocean covers the whole earth supposed spherical. Thirdly, that the depth of the ocean is uniform or a function of the latitude only. Fourthly, that the attraction of the ocean on itself is to be neglected. The last phrase simply means that we neglect the difference of the attraction of the water on itself in its actual form from its attraction in the nearly spherical form it would assume if there were no disturbance.

The irregular shapes of the bed and shores of the ocean make calculation of the effects due to them almost impossible. But the attraction of the ocean on itself should be susceptible to calculation. Hitherto this has always been neglected, owing to mathematical difficulties. Poincaré and Hough, in two papers which have lately been published quite independently and nearly at the same time, have taken it into account. Of these two papers Hough's is the most important in view of the applications.

Previous writers, and particularly Darwin, have used simple sums of harmonic terms. Hough uses Zonal Harmonics and finds that this enables him to include the attraction of the ocean on itself. In particular the fortnightly tide has to be altered from 5 to 8 per cent. owing to this cause. Poincaré makes a rough estimate only, which is double this amount. At the same time he points out that the irregular shape of the continents may alter the coefficients of certain terms to a large extent. In consequence of this he criticises the arguments of Thomson and Tait, who attempt to explain the difference between theory and observation by the solid tides which the moon and sun must produce in a solid earth possessing slight elasticity. He leaves the question open, however. Poincaré's paper contains much more in the way of theoretical researches, which must be omitted here. Hough also determines the free oscillations by means of an indefinite determinant.

When we do not neglect the vertical acceleration of the particles of fluid the mathematical treatment alters and becomes more difficult. It is in general supposed that the height of the wave is small in comparison with the distance between the crests. In 1883 Lord Rayleigh discussed the short waves which are seen in front of an object trailed along the water, and also the longer waves that are left behind. His method is to find the effect produced by a line of disturbance inclined at an angle to the direction of steady motion of the fluid. The effects observed by Froude in his famous experiments on ship-waves are fairly well accounted for. This year a paper by Mitchell has just appeared in which he calculates the wave-resistance due to a body shaped approximately like a ship. Again the results are not unsatisfactory. Lord Kelvin has solved a similar problem in several articles in the *Philosophical Magazine* for 1886–7. He considers the effect of small inequalities in the bed of a stream. Of special interest are the standing waves produced by a stone, or by a hole in the stream-bed, or by a wavy bottom, such as we sometimes see left on the sea shore by the retreating tide.

When the wave-height, wave-length and the depth of the fluid are comparable in magnitude the problem becomes very difficult, as the waves are not in general propagated without change of shape. They were first treated by Stokes. In 1889 von Helmholtz took up the general problem and considered the effect of the wind in making permanent waves. Mitchell has traced the free-wave in the case of infinite depth and its changes as it proceeds. Incidentally he finds again the case where the crest becomes a cusp with an angle of 120°, a result predicted by Stokes from simple considerations. A paper by W. Wien should be mentioned; in it the forms of the waves at the surface of separation of the two media are discussed, and forms for the waves produced by the winds, more or less closely approximating to actual phenomena, are found. The wave-fronts are figured by means of Schwarz's method of representing conformably a lemniscate on a circle.

The solitary wave has received a fair share of attention. In this wave the height is not necessarily small compared with the depth of the fluid, and it may travel for a long distance along a uniform canal with little or no change of form. Experiments have been made and the results published by Scott Russell. Boussinesq and Lord Rayleigh have lately investigated his figures from the mathematical standpoint, and have given a simple formula for the relation between the wave-length and the height which agrees with that deduced by Scott Russell from experiment. Korteweg and de Vries, in 1895, extended the theory of the solitary wave and showed that a new type—named cnoidal—can be propagated. They also proved that a depression without a corresponding elevation can move along, but such depressions are generally unstable. McCowan and Stokes have also treated these waves. The existence of free oscillations in canals has been investigated by Greenhill, Lamb and MacDonald by Fourier's methods. There is some doubt about Lamb's results. MacDonald has shown that it is necessary to take into account the possibility of satisfying the surface conditions and that simple formulæ are not sufficient; this point will be readily appreciated. The criticism appears to be valid, but the subject needs elucidation.

An interesting problem is obtained by considering the tiny waves formed in a glass of water when the glass is made to vibrate by means of a violin bow drawn across its edge. Rayleigh has shown that they are due to capillary action. It is curious that the number of waves found on the water in a given time is double the number of vibrations made by the glass in the same time.

A good deal of interest has been taken during the last two decades in the forms assumed by masses of fluid rotating about a fixed axis under their own attraction only. Darwin and Poincaré are mainly responsible for the developments which the subject has received, although much has been done by Madame Kowalewsky, Basset, Dyson, Bryan and Love. To give an account of all this work would take me far outside the limits of this paper, and I shall, therefore, simply mention some of the more interesting results obtained by the first two writers.

In 1886 Darwin worked out fully the various possible forms of the Jacobian ellipsoids, showing where the limits of stability came. In 1887 he took up the more general problem of two nearly spherical masses of fluid rotating like rigid bodies round a fixed axis under their own attractions only. This problem is of great importance in theories of cosmogony. Especial attention is paid to the cases where the two bodies get very near to one another. When quite close they may coalesce and form dumb-bell shaped figures. Or the smaller mass may have a tendency to break up into two parts, as shown by a furrowing in its contour.

Poincaré took the subject up from a different point of view. He starts with the rotating ellipsoidal form and investigates what forms of relative equilibrium are possible when small deviations from the ellipsoid occur consistently with the conditions. He figures a pear-shaped form of possible equilibrium, and also discusses, with much detail, the stability of the various figures.

I am indebted to Professor Darwin for a reference to a paper by Schwarzchild, which has just appeared in the Annals of the Munich Observatory. In this memoir certain portions of Poincaré's work, with respect to the exchange of stabilities between two classes of possible figures of equilibrium at the place where they meet, are criticised. He also examines the stability of Roche's Ellipsoids by means of Lamé's functions, and shows that there is no figure of bifurcation in this series.

A considerable amount of attention has been devoted to our last subject, the viscosity of fluids, partly owing to the mathematical interest of the subject and partly to the difficulty of obtaining any close approximation to the results afforded by natural phenomena. It must be stated at the outset that in all the work hitherto attempted the motion is supposed to be sufficiently slow to enable us to neglect the squares and higher powers of the velocities. Practically this entails a serious limitation on the usefulness of the results. Even the most casual observation shows that when the velocity exceeds a limit which is very easily reached, eddies are formed and the motion entirely changes its character. Korteweg has made some suggestive remarks on this point in a paper on the stability of the motion of viscous fluids. He remarks that the existence and formation of eddies was generally supposed to be due to unstable solutions of the equations of motion. von Helmholtz, however, had found that when a solution with given boundary conditions in a simply-connected region is obtained, that solution is unique. Hence, we cannot attribute the formation of eddies to other solutions which in fact do not exist. Korteweg further proves that this unique solution is stable. Hence, neglecting squares and products of the velocities, it is evident that having got a unique stable solution, eddies cannot be formed. Concerning these eddies, he says: "When, on the contrary, squares and higher powers of the velocities are taken into account I have my reasons for supposing that even in the case of a sphere moving with uniform velocity—if such a state of steady motion can be reached—the motion must finally become unstable." Lord Rayleigh has also attempted to examine how viscosity affects the motion, and how eddies, when formed, are maintained.

"Lord Kelvin concludes that the linear flow of a fluid through a pipe or of a stream over a plane bed is stable for very small disturbances, but that for disturbances of more than a certain amplitude the motion becomes unstable, the limits of stability being smaller the smaller the viscosity." (Lamb's Hydrodynamics.) It is possible that a remark made by Klein in his lectures on the top may have some bearing in this case. He points out that near the limit of stability, obtained as usual by neglecting the second and higher powers of small quantities, instability really takes place when we include them. It is, indeed, possible that all viscous fluid motions as at present investigated are really unstable and that eddies are always formed. It is instructive to read Reynolds' experiments of 1883 as to the point at which, with increasing velocity, stream lines appear to break up into eddies.

A warning must be given against laying too great a stress on the equations of motion. They are formed under certain suppositions as to the character of the internal friction of the fluid, but we have no security that these suppositions represent the facts. At the same time most of the different assumptions made lead to the same equations, so that only a very fundamental alteration would affect the equations of motion.

Another difficulty arises with the skin friction at the surface of a body moving in the fluid. The difficulty arises from the fact that the action along the wall in contact with the fluid is treated quite differently in the cases of no friction and very small friction. The mathematical character of the motion may be completely altered. The difference is this. With non-viscous fluids we assume a finite slip of the fluid along the wall. If even the smallest coefficient of friction be introduced, a finite slip cannot be consistently allowed. The whole subject is very obscure. It is now believed by some prominent physicists that no such finite slip actually takes place at all.

A somewhat easier problem is the decay of waves proceeding along water, owing to the influence of viscosity. In several short papers during the last three years in the *Comptes Rendus*, Boussinesq has treated the various kinds of waves, in many cases reducing his results to numbers. A practical application is a determination of the time necessary for the sea raised by a storm to subside. Hough has treated a similar problem, namely, the effects of viscosity on ocean currents and on tides of long period.

He concludes that for big slow currents—such as the Gulf Stream—the friction of the ocean bed is by far the most important factor in the dissipation of the energy of motion, while for the short waves in deep water viscosity becomes paramount. The continued existence of ocean currents is a problem not satisfactorily explained. They are usually attributed principally to the tendency of the winds to blow on the average mainly in one direction. Against this is urged the dissipation of the energy thus acquired, by the viscosity of the fluid. Hough concludes that too much effect has been attributed to viscosity. Such currents will doubtless take a long time to start, but when once set in motion the modulus of decay is so large that energy is dissipated very slowly and the winds are sufficient to supply the energy lost by viscosity. The long-period tides, again, are supposed to be greatly affected by viscosity. If, however, Hough's conclusion that the modulus of decay is comparable more nearly with 20 years than with a few months those tides whose periods are as great as one or even six months will be but little affected. Hence the differences between observation and the results of the equilibrium theory of the tides, originally attributed to viscosity, cannot be explained in this way.

A few special problems of motions of solids have been solved, the squares of the velocities being neglected. The motion of a sphere and the linear motion of an ellipsoid in an infinite fluid had been solved. Edwardes, in 1892, added the rotational motion of an ellipsoid and the motion of fluid through a channel bounded by a hyperboloid of revolution. As before stated, the results have little more than a mathematical interest.

To attempt to give any idea of the possible directions in which future progress is likely to be made is a dangerous task. One can, however, do something by mentioning the problems in which a little progress has been made and also those which have been before the scientific world for some time and remain yet unsolved. For some of the indications given below I am indebted to friends who have themselves contributed to recent progress.

Problems in discontinuous motion in two dimensions in an infinite, frictionless, incompressible fluid are now without fundamental difficulties. In fact, they are mainly exercises in conform representation. The problem is reduced to that of finding a function to satisfy Laplace's equation for two dimensions with given boundary conditions. No special service will be gained by hydrodynamics, by solving for new forms of boundaries, unless the cases arise in experiment. But I have mentioned the fact that no problem of discontinuous motion in three dimensions has yet been solved. The difficulty is one which can easily be appreciated. The theory of functions deals with a complex of the form $x + iy$, and this suits all problems in two dimensions. But little has been done with a vector in three dimensions, and certainly nothing has been built up concerning it which corresponds to the results obtained for the two dimensional vector. The subject of discontinuous motion was set for the Adams prize in 1895; this is the prize which has produced Maxwell's essay on Saturn's Rings and J. J. Thomson's on Vortex Motion. A solution for a solid of revolution was asked for, and it was generally supposed that the circular disc would be the easiest to attempt. No essay was sent in. One prominent mathematician, who has aided considerably in the development of hydrodynamics, mentioned that he had worked for six months and had obtained absolutely nothing. A magnificent reception, therefore, awaits the first solution!

Some mention has been already made of difficulties awaiting solution in tide and wave problems. Whether much more can be done by the present analytical methods is not certain. We may frequently meet in this department with series which formally satisfy the equations, but which either converge very slowly or not at all. The theory of free and forced waves, depending to a large extent upon methods of approximation, when numerical results are required for comparison with observation, is in many respects fairly satisfactory if the height of the wave crest above the mean level is not very great and if the motion is slow. But there are comparatively few problems solved for shorter waves in confined bodies of water. The oscillations of water across a canal of circular section is an instance of a problem which should yield to analysis. But little is known of the causes which produce double and other curious kinds of tides in enclosed areas.

Another class of problem is the wave-resistance experienced by various shaped bodies moving over water, especially when the velocity is not small. That this class is becoming extremely important, in view of the high speeds attained by torpedo boats, there can be no doubt. The lines of a ship are at present a matter mainly of experience. One of the most puzzling things is the difference in the speed qualities of two ships built on the same plans. A very slight difference in the lines will frequently enormously alter the speed qualities, and one can scarcely doubt that this is due to the differences in the waves which are formed as the ship moves. The kind of problem thus presented to the mathematician is always a difficult one. Briefly stated, it is a problem where a slight difference in the boundary conditions makes a large difference in the effects produced. A similar difficulty occurs when one approaches the limits of stability of steady motion.

The treatment of viscosity and skin friction seems to be a subject lying nearer to one's grasp. The difference between no viscosity and small viscosity—namely, the mathematical assumptions of finite slip—and no finite slip—has already been pointed out; this requires further investigation. Again, the fact that in all problems hitherto solved, only the first powers of the velocities are taken into consideration seems to point to an opening for research. We cannot hope that, even should this single difficulty be overcome to a certain extent, the mechanical difficulties would be made much easier. But every step taken ought to lead to some further insight into the most intractable subject that mathematical physics presents. It must be admitted that the practical engineer has almost no use for any of the results obtained by the theory as it at present stands. It may be answered that pure science does not look to practical ends. This is perfectly true in general. But it can hardly be an argument in the case of hydrodynamics, the very bases of which are assumptions which are supposed to approximate more or less closely to actual conditions.

In conclusion, I cannot resist making, or rather repeating, an appeal to our pure mathematicians to devote more attention to this subject. The literature is easily accessible. The English treatises of Lamb, Basset, Thomson and Tait will supply most of the needs of a student, while the works of Voight and Kirchhoff and the Handbuch der Physik of Winkelmann may be used to supplement them. Again, Stokes' report of 1846 and that of Hicks in 1881–2 will furnish the main points in the history of the subject. On Vortex Motion we have the later paper of Love in 1887 and the presidential address of Hicks to the British Association in 1896. On Tides, Darwin's article in the Encyclopædia Britannica brings our knowledge up to 1888. Most of the work done since 1882 is to be found in the columns of such easily accessible periodicals as the Philosophical Magazine, the Proceedings of the London Mathematical Society and the Proceedings and Transactions of the Royal Society. A full index of titles of papers up to date, with short abstracts, is obtainable from the Jahrbuch über die Fortschritte der Mathematik and the *Revue Semestrielle.*

It is, of course, unfair to ask anyone successfully engaged on his or her own special line of research to leave it for doubtful profit in another. But much may be done by those who have the direction of the studies of the future generation by interesting and suggestive courses of lectures. Pure mathematicians will not find their knowledge useless here, and students will not be backward in following the footsteps of such men as Laplace, Stokes, Kelvin, von Helmholtz and Rayleigh. The tendency towards the separation of pure mathematics from their applications to physical problems has already been arrested. The future progress of Hydrodynamics appears to demand a closer union of these two branches of science.

ERNEST W. BROWN.
HAVERFORD COLLEGE.

BOTANY AT THE ANNIVERSARY MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

I.

SECTION G was organized Monday noon, August 22d; Dr. W. G. Farlow, President. Regular sessions were held Tuesday morning, afternoon and evening and Thursday morning and afternoon; Wednesday and Friday being given up to excursions. Fifty-six papers were listed and forty-seven were read.

Thursday morning Mr. A. B. Seymour, on behalf of the Committee on Bibliography, appointed at the Madison meeting, made a report of progress, which dealt principally with the question of subject arrangement. On motion of the Secretary, the Section directed the Committee to include Bacteriology in the list of subjects covered by this Bibliography.

The large number of papers and the limited time prevented full discussion in many instances. Numerous excursions also interfered more or less with the regular work of the Section, but these afforded much pleasure to all who could take part in them and were not least of the Boston attractions.

Visiting botanists were very hospitably entertained, and altogether the Boston meeting was exceedingly pleasant and profitable.

The following abstracts have been prepared with much care, in most cases from the authors' MSS. or abstracts, and it is to be hoped that they are reasonably free from errors, and ample enough to give the many who could not be present a clear idea of what was said and done.

The Carposporic Type of Reproduction of the Rhodophyceæ. BRADLEY M. DAVIS.

RECENT investigations in this field of research show a tendency to depart from the teachings of Fr. Schmitz. These were characterized by the assumption of a second act of fertilization in the Rhodophyceæ exhibited in the phenomenon of fusion between auxilliary cells and filaments or processes put out by the carpogonium. The speaker described studies of his own upon *Champia*, showing their divergence from the doctrines of Schmitz, and followed with a more general discussion of the peculiar conditions found here, expressing himself as in sympathy, in the main, with the recently published views of Oltmann. All evidence at present points to the probability that the cell-fusion phenomenon following the development of the carpogonium is associated with and the result of nutritive functions. The entire group of Rhodophyceæ is so pe-