MATHEMATICAL AND PHYSICAL PAPERS.

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ON THE EFFECT OF THE INTERNAL FRICTION OF FLUIDS ON THE MOTION OF PENDULUMS.

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THE great importance of the results obtained by means of the pendulum has induced philosophers to devote so much attention to the subject, and to perform the experiments with such a scrupulous regard to accuracy in every particular, that pendulum observations may justly be ranked among those most distinguished by modern exactness. It is unnecessary here to enumerate the different methods which have been employed, and the several corrections which must be made, in order to deduce from the actual observations the result which would correspond to the ideal case of a simple pendulum performing indefinitely small oscillations in vacuum. There is only one of these corrections which bears on the subject of the present paper, namely, the correction usually termed the *reduction to a vacuum*. On account of the inconvenience and expense attending experiments in a vacuum apparatus, the observations are usually made in air, and it then becomes necessary to apply a small correction, in order to reduce the observed result to what would have been observed had the pendulum been swung in a vacuum. The most obvious effect of the air consists in a diminution of the moving force, and consequent increase in the time of vibration, arising from the buoyancy of the fluid. The correction for buoyancy is easily calculated from the first principles of hydrostatics, and formed for a considerable time the only correction which it was thought necessary to make for reduction to a vacuum. But in the year 1828 Bessel, in a very important memoir in which he determined by a new method the length of the seconds' pendulum, pointed out from theoretical considerations the necessity of taking account of the inertia of the air as well as of its buoyancy. The numerical calculation of the effect of the inertia forms a problem of hydrodynamics which Bessel did not attack; but he concluded from general principles that a fluid, or at any rate a fluid of small density, has no other effect on the time of very small vibrations of a pendulum than that it diminishes its gravity and increases its moment of inertia. In the case of a body of which the dimensions are small compared with the length of the suspending wire, Bessel represented the increase of inertia by that of a mass equal to k times the mass of the fluid displaced, which must be supposed to be added to the inertia of the body itself. This factor k he determined experimentally for a sphere a little more than two inches in diameter, swung in air and in water. The result for air, obtained in a rather indirect way, was $k = 0{\cdot}9459$, which value Bessel in a subsequent paper increased to 0·956. A brass sphere of the above size having been swung in water with two different lengths of wire in succession gave two values of k, differing a little from each other, and equal to only about two-thirds of the value obtained for air.

The attention of the scientific world having been called to the subject by the publication of Bessel's memoir, fresh researches both theoretical and experimental soon appeared. In order to examine the effect of the air by a more direct method than that employed by Bessel, a large vacuum apparatus was erected at the expense of the Board of Longitude, and by means of this apparatus Captain (now Colonel) Sabine determined the effect of the air on the time of vibration of a particular invariable pendulum. The results of the experiments are contained in a memoir read before the Royal Society in March 1829, and printed in the *Philosophical Transactions* for that year. The mean of eight very consistent experiments gave 1 655 as the factor by which for that pendulum the old correction for buoyancy must be multiplied in order to give the whole correction on account of the air. A very remarkable fact was discovered in the course of these experiments. While the effects of air at the atmospheric pressure and under a pressure of about half an atmosphere were found to be as nearly as possible proportional to the densities, it was found that the effect of hydrogen at the atmospheric pressure was much greater, compared with the effect of air, than corresponded with its density. In fact, it appeared that the ratio of the effects of hydrogen and air on the times of vibration was about 1 to $5\frac{1}{4}$, while the ratio of the densities is only about 1 to 13. In speaking of this result Colonel Sabine remarks, "The difference of this ratio from that shewn by experiment is greater than can well be ascribed to accidental error in the experiment, particularly as repetition produced results almost identical. May it not indicate an inherent property in the elastic fluids, analogous to that of viscidity in liquids, of resistance to the motion of bodies passing through them, independently of their density? a property, in such case, possessed by air and hydrogen gas in very different degrees; since it would appear from the experiments that the ratio of the resistance of hydrogen gas to that of air is more than double the ratio following from their densities. Should the existence of such a distinct property of resistance, varying in the different elastic fluids, be confirmed by experiments now in progress with other gases, an apparatus more suitable than the present to investigate the ratio in which it is possessed by them, could scarcely be devised: and the pendulum, in addition to its many important and useful purposes in general physics, may find an application for its very delicate, but, with due precaution, not more delicate than certain, determinations, in the domain of chemistry." Colonel Sabine has informed me that the experiments here alluded to were interrupted by a cause which need not now be mentioned, but that as far as they went they confirmed the result of the experiments with hydrogen, and pointed out the existence of a specific action in different gases, quite distinct from mere variations of density.

Our knowledge on the subject of the effect of air on the time of vibration of pendulums has received a most valuable addition from the labours of the late Mr Baily, who erected a vacuum apparatus at his own house, with which he performed many hundreds of careful experiments on a great variety of pendulums. The experiments are described in a paper read before the Royal Society on the 31st of May 1832. The result for each pendulum is expressed by the value of n, the factor by which the old correction for buoyancy must be multiplied in order to give the whole effect of the air as deduced from observation. Four spheres, not quite $1\frac{1}{2}$ inch in diameter, gave as a mean $n=1{\cdot}864$, while three spheres, a little more than 2 inches in diameter, gave only 1·748. The latter were nearly of the same size as those with which Bessel, by a different method, had obtained $k=0{\cdot}946$ or 0·956, which corresponds to $n=1{\cdot}946$ or 1·956. Among the "Additional Experiments" in the latter part of Baily's paper, is a set in which the pendulums consisted of plain cylindrical rods. With these pendulums it was found that n regularly increased, though according to an unknown law, as the diameter of the rod decreased. While a brass tube $1\frac{1}{2}$ inch in diameter gave n equal to about 2·3, a thin rod or thick wire only 0·072 inch in diameter gave for n a value as great as 7·530.

Mathematicians in the meanwhile were not idle, and several memoirs appeared about this time, of which the object was to determine from hydrodynamics the effect of a fluid on the motion of a pendulum. The first of these came from the pen of the celebrated Poisson. It was read before the French Academy on the 22nd of August 1831, and is printed in the 11th Volume of the Memoirs. In this paper, Poisson considers the case of a sphere suspended by a fine wire, and oscillating in the air, or in any gas. He employs the ordinary equations of motion of an elastic fluid, simplified by neglecting the terms which involve the square of the velocity; but in the end, in adapting his solution to practice, he neglects, as insensible, the terms by which alone the action of an elastic differs from that of an incompressible fluid, so that the result thus simplified is equally applicable to fluids of both classes. He finds that when insensible quantities are neglected $n=1{\cdot}5$, so that the mass which we must suppose added to that of the pendulum is equal to half the mass of the fluid displaced. This result does not greatly differ from the results obtained experimentally by Bessel in the case of spheres oscillating in water, but differs materially from the result he had obtained for air. It agrees pretty closely with some experiments which had been performed about fifty years before by Dubuat, who had in fact anticipated Bessel in shewing that the time of vibration of a pendulum vibrating in a fluid would be affected by the inertia of the fluid as well as by its density. Dubuat's labours on this subject had been altogether overlooked by those who were engaged in pendulum experiments;

probably because such persons were not likely to seek in a treatise on hydraulics for information connected with the subject of their researches. Dubuat had, in fact, rather applied the pendulum to hydrodynamics than hydrodynamics to the pendulum.

In the *Philosophical Magazine* for September 1833, p. 185, is a short paper by Professor Challis, on the subject of the resistance to a ball pendulum. After referring to a former paper, in which he had shewn that no sensible error would be committed in a problem of this nature by neglecting the compressibility of the fluid even if it be elastic, Professor Challis, adopting a particular hypothesis respecting the motion, obtains 2 for the value of the factor n for such a pendulum. This mode of solution, which is adopted in several subsequent papers, has given rise to a controversy between Professor Challis and the Astronomer Royal, who maintains the justice of Poisson's result.

In a paper read before the Royal Society of Edinburgh on the 16th of December 1833, and printed in the 13th Volume of the Society's *Transactions*, Green has determined from the common equations of fluid motion the resistance to an ellipsoid performing small oscillations without rotation. The result is expressed by a definite integral; but when two of the principal axes of the ellipsoid become equal, the integral admits of expression in finite terms, by means of circular or logarithmic functions. When the ellipsoid becomes a sphere, Green's result reduces itself to Poisson's.

In a memoir read before the Royal Academy of Turin on the 18th of January 1835, and printed in the 37th Volume of the memoirs of the Academy, M. Plana has entered at great length into the theory of the resistance of fluids to pendulums. This memoir contains, however, rather a detailed examination of various points connected with the theory, than the determination of the resistance for any new form of pendulum. The author first treats the case of an incompressible fluid, and then shews that the result would be sensibly the same in the case of an elastic fluid. In the case of a ball pendulum, the only one in which a complete solution of the problem is effected, M. Plana's result agrees with Poisson's.

In a paper read before the Cambridge Philosophical Society on the 29th of May 1843, and printed in the 8th Volume of the *Transactions*, p. 105*, I have determined the resistance to a ball

* [*Ante*, Vol. I. p. 179.]

pendulum oscillating within a concentric spherical envelope, and have pointed out the source of an error into which Poisson had fallen, in concluding that such an envelope would have no effect. When the radius of the envelope becomes infinite, the solution agrees with that which Poisson had obtained for the case of an unlimited mass of fluid. I have also investigated the increase of resistance due to the confinement of the fluid by a distant rigid plane. The same paper contains likewise the calculation of the resistance to a long cylinder oscillating in a mass of fluid either unlimited, or confined by a cylindrical envelope, having the same axis as the cylinder in its position of equilibrium. In the case of an unconfined mass of fluid, it appeared that the effect of inertia was the same as if a mass equal to that of the fluid displaced were distributed along the axis of the cylinder, so that $n = 2$ in the case of a pendulum consisting of a long cylindrical rod. This nearly agrees with Baily's result for the long $1\frac{1}{2}$ inch tube; but, on comparing it with the results obtained with the cylindrical rods, we observe the same sort of discrepancy between theory and observation as was noticed in the case of spheres. The discrepancy is, however, far more striking in the present case, as might naturally have been expected, after what had been observed with spheres, on account of the far smaller diameter of the solids employed.

A few years ago Professor Thomson communicated to me a very beautiful and powerful method which he had applied to the theory of electricity, which depended on the consideration of what he called *electrical images*. The same method, I found, applied, with a certain modification, to some interesting problems relating to ball pendulums. It enabled me to calculate the resistance to a sphere oscillating in presence of a fixed sphere, or within a spherical envelope, or the resistance to a pair of spheres either in contact, or connected by a narrow rod, the direction of oscillation being, in all these cases, that of the line joining the centres of the spheres. The effect of a rigid plane perpendicular to the direction of motion is of course included as a particular case. The method even applies, as Professor Thomson pointed out to me, to the uncouth solid bounded by the exterior segments of two intersecting spheres, provided the exterior angle of intersection be a submultiple of two right angles. A set of corresponding problems, in which the spheres are replaced by long cylinders, may be solved in a similar manner. These results were mentioned at the meeting of the British Association at Oxford in 1847, and are noticed in the volume of reports for that year, but they have not yet been published in detail.

The preceding are all the investigations that have fallen under my notice, of which the object was to calculate from hydrodynamics the resistance to a body of given form oscillating as a pendulum. They all proceed on the ordinary equations of the motion of fluids. They all fail to account for one leading feature of the experimental results, namely, the increase of the factor n with a decrease in the dimensions of the body. They recognize no distinction between the action of different fluids, except what arises from their difference of density.

In a conversation with Dr Robinson about seven or eight years ago on the subject of the application of theory to pendulums, he noticed the discrepancy which existed between the results of theory and experiment relating to a ball pendulum, and expressed to me his conviction that the discrepancy in question arose from the adoption of the ordinary theory of fluid motion, in which the pressure is supposed to be equal in all directions. He also described to me a remarkable experiment of Sir James South's which he had witnessed. This experiment has not been published, but Sir James South has kindly allowed me to mention it. When a pendulum is in motion, one would naturally have supposed that the air near the moving body glided past the surface, or the surface past it, which comes to the same thing if the relative motion only be considered, with a velocity comparable with the absolute velocity of the surface itself. But on attaching a piece of gold leaf to the bottom of a pendulum, so as to stick out in a direction perpendicular to the surface, and then setting the pendulum in motion, Sir James South found that the gold leaf retained its perpendicular position just as if the pendulum had been at rest; and it was not till the gold leaf carried by the pendulum had been removed to some distance from the surface, that it began to lag behind. This experiment shews clearly the existence of a tangential action between the pendulum and the air, and between one layer of air and another. The existence of a similar action in water is clearly exhibited in some experiments of Coulomb's which will be mentioned in the second part of this paper, and indeed might be concluded from several very ordinary phenomena. Moreover Dubuat, in discussing the results of his experiments on the oscillations of spheres in water, notices a slight increase in the effect of the water corresponding to an increase in the time of vibration, and expressly attributes it to the *viscosity* of the fluid.

Having afterwards occupied myself with the theory of the friction of fluids, and arrived at general equations of motion, the same in essential points as those which had been previously obtained in a totally different manner by others, of which, however, I was not at the time aware, I was desirous of applying, if possible, these equations to the calculation of the motion of some kind of pendulum. The difficulty of the problem is of course materially increased by the introduction of internal friction, but as I felt great confidence in the essential parts of the theory, I thought that labour would not be ill-bestowed on the subject. I first tried a long cylinder, because the solution of the problem appeared likely to be simpler than in the case of a sphere. But after having proceeded a good way towards the result, I was stopped by a difficulty relating to the determination of the arbitrary constants, which appeared as the coefficients of certain infinite series by which the integral of a certain differential equation was expressed. Having failed in the case of a cylinder, I tried a sphere, and presently found that the corresponding differential equation admitted of integration in finite terms, so that the solution of the problem could be completely effected. The result, I found, agreed very well with Baily's experiments, when the numerical value of a certain constant was properly assumed; but the subject was laid aside for some time. Having afterwards attacked a definite integral to which Mr Airy had been led in considering the theory of the illumination in the neighbourhood of a caustic, I found that the method which I had employed in the case of this integral* would apply to the problem of the resistance to a cylinder, and it enabled me to get over the difficulty with which I had before been baffled. I immediately completed the numerical calculation, so far as was requisite to compare the formulæ with Baily's experiments on cylindrical rods, and found a remarkably close agreement between theory and observation. These results were mentioned at the meeting of the British Association at Swansea in 1848, and are briefly described in the volume of reports for that year.

The present paper is chiefly devoted to the solution of the problem in the two cases of a sphere and of a long cylinder, and to

* [*Ante*, Vol. II. p. 328.]

a comparison of the results with the experiments of Baily and others. Expressions are deduced for the effect of a fluid both on the time and on the arc of vibration of a pendulum consisting either of a sphere, or of a cylindrical rod, or of a combination of a sphere and a rod. These expressions contain only one disposable constant, which has a very simple physical meaning, and which I propose to call the *index of friction* of the fluid. This constant we may conceive determined by one observation, giving the effect of the fluid either on the time or on the arc of vibration of any one pendulum of one of the above forms, and then the theory ought to predict the effect both on the time and on the arc of vibration of all such pendulums. The agreement of theory with the experiments of Baily on the time of vibration is remarkably close. Even the rate of decrease of the arc of vibration, which it formed no part of Baily's object to observe, except so far as was necessary for making the small correction for reduction to indefinitely small vibrations, agrees with the result calculated from theory as nearly as could reasonably be expected under the circumstances.

It follows from theory that with a given sphere or cylindrical rod the factor n increases with the time of vibration. This accounts in a good measure for the circumstance that Bessel obtained so large a value of k for air, as is shewn at length in the present paper; though it unquestionably arose in a great degree from the increase of resistance due to the close proximity of a rigid plane to the swinging ball.

I have deduced the value of the index of friction of water from some experiments of Coulomb's on the decrement of the arc of oscillation of disks, oscillating in water in their own plane by the torsion of a wire. When the numerical value thus obtained is substituted in the expression for the time of vibration of a sphere, the result agrees almost exactly with Bessel's experiments with a sphere swung in water.

The present paper contains one or two applications of the theory of internal friction to problems which are of some interest, but which do not relate to pendulums. The resistance to a sphere moving uniformly in a fluid may be obtained as a limiting case of the resistance to a ball pendulum, provided the circumstances be such that the square of the velocity may be neglected. The resistance thus determined proves to be proportional, for a given fluid and a given velocity, not to the surface, but to the radius of the sphere; and therefore the accelerating force of the resistance increases much more rapidly, as the radius of the sphere decreases, than if the resistance varied as the surface, as would follow from the common theory. Accordingly, the resistance to a minute globule of water falling through the air with its terminal velocity depends almost wholly on the internal friction of air. Since the index of friction of air is known from pendulum experiments, we may easily calculate the terminal velocity of a globule of given size, neglecting the part of the resistance which depends upon the square of the velocity. The terminal velocity thus obtained is so small in the case of small globules such as those of which we may conceive a cloud to be composed, that the apparent suspension of the clouds does not seem to present any difficulty. Had the resistance been determined from the common theory, it would have been necessary to suppose the globules much more minute, in order to account in this way for the phenomenon. Since in the case of minute globules falling with their terminal velocity the part of the resistance depending upon the square of the velocity, as determined by the common theory, is quite insignificant compared with the part which depends on the internal friction of the air, it follows that were the pressure equal in all directions in air in the state of motion, the quantity of water which would remain suspended in the state of cloud would be enormously diminished. The pendulum thus, in addition to its other uses, affords us some interesting information relating to the department of meteorology.

The fifth section of the first part of the present paper contains an investigation of the effect of the internal friction of water in causing a series of oscillatory waves to subside. It appears from the result that in the case of the long swells of the ocean the effect of friction is insignificant, while in the case of the ripples raised by the wind on a small pool, the motion subsides very rapidly when the disturbing force ceases to act.