captain of the ship at between eleven and twelve feet, and the indicator on the camera showed these wings apparently at full stretch at the instant that I pressed the button. The result is certainly somewhat astonishing, and I shall be glad to know whether it is worth comment in your paper; to me it certainly seems to entirely upset the accepted theories as to the flight of this bird. A. KINGSMILL.

Stanmore, October 10.

ON THE DOCTRINE OF DISCONTINUITY OF FLUID MOTION, IN CONNECTION WITH THE RESISTANCE AGAINST A SOLID MOVING THROUGH A FLUID.[1]

III.

§ 11. THE accompanying diagram (Fig. 1) illustrates the application of the doctrine in question, to a disk kept moving through water or air with a constant velocity, V, perpendicular to its own plane. The assumption to which I object as being inconsistent with hydrodynamics, and very far from any

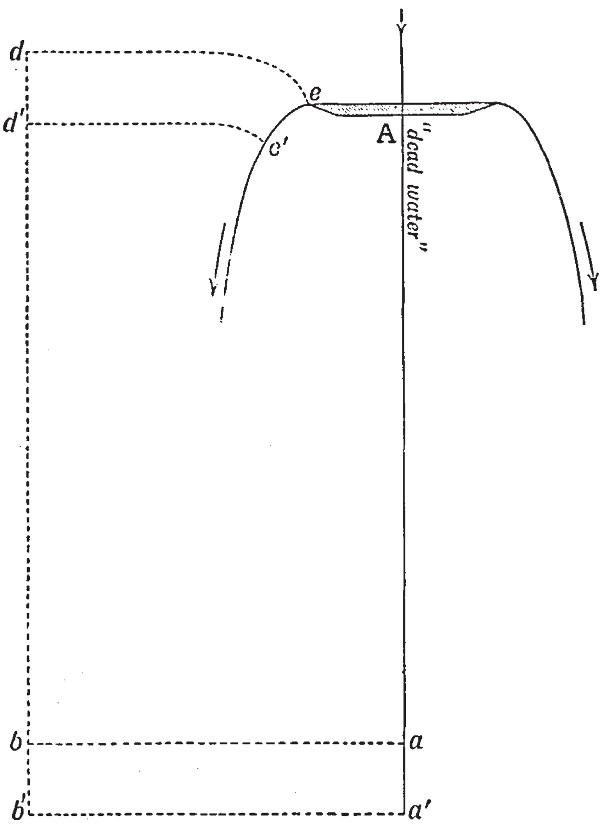

FIG. 1.

approximation to the truth for an inviscid incompressible fluid in any circumstances, and utterly at variance with observation of disks or blades (as oar blades) caused to move through water; is, that starting from the edge as represented by the two continuous curves in the diagram, and extending indefinitely rearwards, there is a "surface of discontinuity" on the outside of which the water flows, relatively to the disk, with velocity V, and on the inside there is a rear-less mass of "dead water"[2] following close after the disk.

§ 12. The supposed constancy of the velocity on the outside of the supposed surface of discontinuity entails for the inside a constant pressure, and therefore quiescence relatively to the disk, and rearlessness of the "dead water." How such a state of motion could be produced? and what it is in respect to rear? are questions which I may suggest to the teachers of the doctrine, but happily, not going in for an examination in hydrokinetics, I need not try to answer.

§ 13. But now, supposing the motion of the disk to have been started some finite time, t, ago, and considering the consequent necessity (§ 9) for finiteness of its wake, let ab, bd be lines sufficiently far behind the rear, and beyond one side, of the disturbed water, to pass only through water not sensibly disturbed. We thus have a real finite case of motion to deal with, instead of the inexplicably infinite one of § 11. Let us try if it is possible that for some finite distance from the edge, and from the disk on each side, the motion could be even approximately, if not rigorously, that described in § 11, and indicated by the diagram.

§ 14. Let v be the velocity at any point in the axis, Aa, at distance y from the disk, rearwards. Draw ed perpendicular to the stream lines of the fluid, relatively to the disk supposed at rest.

The "flow"[1] in the line ed is 0;
" " " db " $V \times db$;
" " " ba " 0;
" " " aA " $-\int_0^{Aa} v\,dy$;
" " " Ae " 0, by hypothesis.

Hence for the "circulation"[2] in the closed polygon $edba$Ae, we have

$$V \times db - \int_0^{Aa} v\,dy.$$

Similarly, for the circulation in the same circuit[3] at a time later by any interval, τ, when the line ba has moved to the position $b'a'$, and ed to $e'd'$, we have

$$V \times db - \int_0^{Aa} v'\,dy.$$

where v' denotes, for the later time, $t+\tau$, the velocity in Aa, at distance y from A. Hence the circulation in edaAe gains in time τ an amount equal to

$$-\int_0^{Aa} (v' - v)\,dy;$$

which is the same as

$$-\int_0^{\infty} (v' - v)\,dy,$$

This, by the general theorem of "circulation,"[4] must be equal to the gain of circulation in time τ, of all the vortex-sheet in its growth from the edge according to the statement of § 11. Hence, with the notation of § 10,

$$(\Sigma\kappa)' - \Sigma\kappa = -\int_0^{\infty} (v' - v)\,dy.$$

§ 15. Remarking now that the fluid has only continuous irrotational motion through a finite space all round each of the lines ed, db, ba, aA; and all round Ae except the space occupied by the disk and the fluid beyond its front side, we have, for the velocity-potential of this motion, relatively to the disk,

$$Vy + \phi(x, y, z, t)$$

where ϕ denotes the velocity-potential of the motion

[1] Continued from p. 547.

[2] This is a technical expression of practical hydraulics, adopted by the English teachers of the doctrine of finite slip between two parts of a homogeneous fluid, to designate water at rest relatively to the disk.

[1] "Vortex Motion" (Thomson), *Trans. R.S.E.*, 1869.

[2] *Ibid.*

[3] Remark that the circulation in $abb'a'$ is zero, and therefore the circulation in $edb'a'Ae$ is equal to that in $edbaAe$.

[4] "Vortex Motion," *Trans. R.S.E.*, 1869.

relative to the infinitely distant fluid all round: and we have

$$v = V + \frac{d}{dy}\phi(o, y, o, t).$$

With this the equation of § 14 becomes

$$(\Sigma\kappa)' - \Sigma\kappa = \{\phi(o, o, o, t+\tau)\} - \{\phi(o, o, o, t)\}.$$

Hence, by taking τ infinitely small,

$$\frac{d}{dt}\Sigma\kappa = \frac{d}{dt}\phi(o, o, o, t).$$

§ 16. Now in the time from t to $t + \tau$, there has been, according to the supposition stated in § 11, a growth of vortex sheet from e, at the rate $\frac{1}{2}$V, being the mean between the velocities of the fluid on its two sides,[1] and the circulation, per unit length, l, of the sheet thus growing is lV. Hence the vortex-circulation of the growing sheet augments, in time τ, by $\frac{1}{2}V\tau \times V$: and therefore, by § 15,

$$\frac{d}{dt}\phi(o, o, o, t) = \frac{1}{2}V^2.$$

§ 17. Now, if Π denotes the pressure of the fluid at great distances, where its velocity, relative to the disk is V, and p the pressure at any point of the rear side of the disk, being the same as the pressure at A, we have, by elementary hydrokinetics,

$$p = \Pi + \frac{1}{2}V^2 - \frac{d}{dt}\phi(o, o, o, t)$$

because the velocity of the fluid at every point of the rear side of the disk is zero according to the assumption of "dead water." Hence, by § 16,

$$p = \Pi,$$

which, being the same as the pressure on the rear side given by the unmitigated assumption of an endless ever broadening wake of "dead water," proves that our substitution (§ 13) of a finite configuration of motion conceivably possible as the consequence of setting the disk in motion at some finite time, t, ago, instead of the inconceivable configuration described in § 11, does not alter the pressure on the rear side of the disk.

§ 18. Hence were the motion of the fluid for some finite distance from the disk, on both its sides, the same, or very approximately the same, as that described in § 11, the force that must be applied to keep it moving uniformly would be the same, or very approximately the same, as that calculated by Lord Rayleigh from the motion of the fluid supposed to be wholly as described in § 11.

§ 19. But what reason have we for supposing the velocity of the fluid at the edge, on the front side of the disk, to be exactly or even approximately equal to the undisturbed velocity, V, of the fluid at great distances from the disk? None that I can see. It seems to me indeed probable that it is in reality much greater than V, when we consider that, with inviscid incompressible fluid in an unyielding outer boundary, the velocity, in the case considered in § 14, is equal to V at even so far from the edge as ·85 of an inch, and increases from V to 63·7 × V between that distance from the edge, and the edge with its 1/2000 of an inch radius of curvature.

§ 20. And what of the "dead water" in contact with the whole rear side of the disk which the doctrine of discontinuity assumes? Look at the reality and you will see the water in the rear exceedingly lively everywhere except at the very centre of the disk. You will see it eddying round from the edge and returning outwards very close along the rear surface, often I believe with much greater velocity than V, but with no steadiness; on the contrary, with a turbulent unsteadiness utterly unlike the steady regular motion generally assumed in the doctrine of discontinuity.

[1] Helmholtz; Wissenschaftliche Abhandlungen; vol. i., foot of p. 151.

§ 21. We may I think safely conclude that on the front side the opposing pressure is less than that calculated by Rayleigh. That this diminution of resistance is partially compensated or is over-compensated by diminution of pressure on the rear, is more than we are able to say from theory alone, in a problem of motion so complex and so far beyond our powers of calculation: but we are entitled to say so, I believe, by experiment. Rayleigh's investigation of the resistance experienced by an infinitely thin rigid plane blade bounded by two parallel straight edges, when caused to move through an inviscid incompressible fluid, with constant velocity, V, in a direction perpendicular to the edges and inclined at an angle i to the plane, gives a force cutting the plane perpendicularly at a distance from its middle equal to

$$\frac{3\cos i}{4(4 + \pi \sin i)}$$

of its breadth, and gives for the amount of this force in gravitation measure,

$$\frac{2\pi \sin i}{4 + \pi \sin i}\text{PA},$$

where A denotes the area of one side of the blade, and P the weight of a column of the fluid of unit cross-sectional area, and of height equal to the height from which a body must fall to acquire a velocity equal to V.

§ 22. The assumption (§ 11) on which this investigation is founded admits no velocity of fluid motion relatively to the disk greater anywhere than V. It gives velocity reaching this value only at the edges of the blade; and at the supposed surface of discontinuity; and in the fluid at infinite distances all round except in the infinitely broad wake of "dead water" where the velocity is zero. It makes the pressure equal to Π all through the "dead water," and makes it increase through the moving fluid, from Π at an infinite distance and at the "surface of discontinuity," to a maximum value Π+P attained at the water-shed line of the disk. If the fluid is air, and if V be even so great as 120 feet per second (1/10 of the velocity of sound)[1] P would be only 7/1000 of Π. The corresponding augmentation of density could cause no very serious change of the motion from that assumed: and therefore in Rayleigh's investigation air may be regarded as an incompressible fluid if the velocity of the disk is anything less than 120 feet per second.

We may therefore test his formula for the resistance, by comparison with results of careful experiments made by Dines[2] on the resistance of air to disks and blades moved through it at velocities of from 40 to 70 statute miles per hour (59 to 103 feet per second).

§ 23. Dines finds for normal incidence the resistance against a foot-square plate, moving through air at m British statute miles per hour to be equal to ·0029 m^2 of a pound weight.

This, if we take the specific gravity of the air as 1/800, gives according to our notation of §21,

$$1\ 116 \times \text{PA}$$

as the resistance to a square plate of area A. At the foot of p. 255 (*Proc. R.S.*, June 1890) Dines says that he finds the resistance to a long narrow blade to be more than 20 per cent. greater than to a square plate. For a blade we may there take

$$1{\cdot}34 \times \text{PA}$$

as the resistance according to Dines' experiments. This is 1·52 times the resistance calculated from Rayleigh's formula (§ 21 above), which is

$$\cdot 88 \times \text{PA},$$

for normal incidence.

§ 24. For incidences more and more oblique, the dis-

[1] Or $\frac{1}{10} \times \sqrt{1{\cdot}4 \times gH}$, where H is "the height of the homogeneous atmosphere."

[2] *Proc. R.S.E.*, June 1890.

crepancy is greater and greater. Thus, from curves given by Dines (p. 256) showing his own and Rayleigh's results, I find the normal resistance to a blade moved through air in a direction inclined 30° to its plane, to be 1·82 times that given by Rayleigh's formula. And by drawing a tangent to Dines' curve at the point in which it cuts the line of zero pressure, I find that, for very small values of i, it gives

$$3{\cdot}25 \times \sin i \times PA.$$

This is rather more than double the value of the force given by Rayleigh's formula for very small values of i, which is

$$\tfrac{1}{2}\pi \sin i . PA.$$

It is about three and a half per cent. greater than that given by my conjectural formula (NATURE, August 20, p. 426, and September 27, p. 525; and *Phil. Mag.*, October 1894) for very small values of i, which is

$$\pi \sin i \cos i . PA.$$

My formula is, however, merely conjectural; and I was inclined to think that it may considerably under-estimate the force. That it does so to some degree is perhaps made probable by its somewhat close agreement with Dines; because the blade in his experiments was 3i broad and ⅜ of an inch thick in the middle with edges "feathered off." An infinitely thin blade would probably have shown greater resistances, at all angles, and especially at those of small inclination to the wind.

(*To be continued.*)

OBSERVATIONS ON YOUNG PHEASANTS.

THE pheasants which formed the subjects of the following observations were hatched out in an incubator from eggs kindly given me by Sir Cecil Miles. The eggs were taken from the hen and transferred to the incubator a few days before the young birds were due to emerge.

The accuracy of pecking and seizing was found to be about the same as that of newly-hatched chicks. For example: two pheasants were hatched out at about 3 p.m.; that evening, at about 6.30, finely chopped egg was placed before them, but they showed no signs of pecking at it; nor did they peck at grain or sand next morning at 11 a.m. At 4 p.m. they began to peck, but seized very little. One struck repeatedly at a crumb of egg on the other's back, but failed to seize it, though the other bird was quite still. On the following morning they pecked at sand and grain (chiefly canary seed) with fair aim. One seized, at the first stroke, a grain of boiled rice at the end of a long steel pin. Another pheasant was hatched out in the night. At about 12 noon, I offered him some egg-bread on the end of a tooth-pick. He struck at it and missed, struck a second time and seized, swallowing some. He could not be induced to strike again. Later he picked up some ants' "eggs," striking with fair accuracy, but did not swallow any. At 4 p.m. he pecked some egg-bread off the end of the tooth-pick, and swallowed. He also pecked at an ant's "egg," but failed to swallow it; then at a second, and swallowed it. Further details would be merely wearisome. One may say that the co-ordination for pecking and swallowing is inherited in a condition such as to ensure fair but not complete accuracy; and that some individual experience is necessary to bring it to perfection.

The young pheasants took no notice whatever of water placed before them in a shallow vessel. When I gave them water on the tip of my finger, they seemed to enjoy it, and one in particular drank eagerly from the end of a tooth-pick, so that an association was established between the sight of the tooth-pick and the satisfaction of drinking. But when I lifted this bird and others, and placed them in the shallow vessel, they made no attempt to drink from it. They learnt to drink from the vessel through pecking at grains of food lying on the bottom. They drank, however, less freely than chicks.

The little birds showed no sign of fear of me. They liked to nestle in my warm hand. My fox-terrier was keen to get at them, much keener than with chicks, probably through scent-suggestion. I placed two of the young pheasants, about a day old, on the floor, and let him smell them (under strict orders not to touch them). He was trembling in every limb from excitement. But they showed no signs of fear, though his nose was within an inch of them. When the pheasants were a week old, I procured a large blind-worm and placed it in front of the incubator drawer in which the birds slept at night. On opening the drawer they jumped out as usual, and ran over the blind-worm without taking any notice of it. Presently first one, then another, pecked vigorously at the forked tongue as it played in and out of the blind-worm's mouth. Subsequently they pecked at its eye and the end of its tail. This observation naturally leads one to surmise that the constant tongue-play in snakes may act as a lure for young and inexperienced birds; and that some cases of so-called fascination may be simply the fluttering of birds round this tempting object. I distinctly remember when a boy seeing a grass-snake with head slightly elevated and quite motionless, and round it three or four young birds fluttering nearer and nearer. It looked like fascination; it may have been that each hoped to be the first to catch that tempting but elusive worm! Presently they would no doubt be invited to step inside.

Another incidental observation is worth recording here. I gave the young birds some wood-lice. These were frequently caught when they were moving, and eaten. But if one had time to roll up, and was thus seized, it was shot out to a distance by the pressure of the bill, just as a fresh cherry-stone is shot from between the finger and thumb of a school-boy. The protective value of the round and slippery form was thus a matter of observation.

I have not observed in the young pheasants the crouching down, which is seen in young chicks when an unusual sound startles them. They appear under such circumstances to stand motionless. For example, when two of them were walking about, picking up all the indigestible odds and ends they could find on my carpet, a high chord was sharply struck on the violin. Both stopped dead. The gentle piping noise they were making ceased. One of them was just lifting his leg, and remained in this position quite still, with neck stretched out, exactly as if he had been suddenly fixed in the attitude in which he chanced to be when the sharp sound fell on his ears. Thus he remained for half a minute. Then he took a few steps and again stopped, remaining quite still for about the same period. (Age 13 days.)

The method of tackling a worm appears to be a matter of inherited co-ordination. So soon as the worm is seized, it is shaken and battered about. There seems to be, also, an inherited tendency to run away with it to some distance before eating it. At all events, of two little pheasants, one of which was weakly, the stronger always bolted off with his worm, though his weakly brother or sister seldom or never chased him. He sometimes tried to bolt with one of his companion's toes by mistake, when one or both of the birds would topple over.

Two notes or sounds, one loud and distressful, the other soft and contentful, appear from the first to be clearly differentiated. A third sound, more gentle than the soft note and double, was occasionally heard when one caressed the birds in one's warm hand. It closely resembles a similar note uttered under similar circumstances by the chick. The note expressive of danger, alarm, or anger, was occasionally heard after about the