Mr. W. Froude remarked, through the Secretary, that he felt great hesitation in offering what would be regarded as an adverse criticism of an elaborate theoretical discussion embodied in technical mathematics, presented by a writer whose command of deductive mathematical machinery was, he doubted not, superior to his own. Nevertheless the subject was one to which he had long devoted much thought and research, and in the investigation of which he had enjoyed opportunities of testing his views both by varied observation and direct experiment, so that the remarks he had to make were by no means the expression of hasty or immature conclusions. On the other hand, it was to the fundamental principles from which the discussion proceeded, and not to the discussion itself, that his remarks would apply. He must also frankly add, that those principles, which he should point to as erroneous, used to be almost universally promulgated in mathematical treatises of reputed authority; and were very commonly even now relied on as the basis of mathematical reasoning, by those whose independent investigations and experimental researches had not obliged them to detect their unsatisfactory character.

He referred, in the first place, to what might be called the very foundation of the Author's deductive process,—the proposition, that when a plane moved obliquely through a fluid at a given velocity, the normal pressure on its surface was as the square of the sine of the angle of obliquity.

He referred, in the second place, to the hypothesis that when the true law of pressure on a plane thus moving was in any way determined, it was legitimate to determine the local pressure on each unit of area in a curved surface moving through a fluid, by applying that law to the unit in virtue of the angle presented by its tangent plane to the plane of rotation.

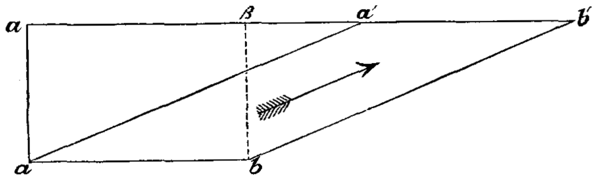
Thirdly, he referred to what appeared to be a misconception of the dynamical relations, or inherent conditions of the slip of the propeller.

First, as regarded the proposition that the normal pressure on an obliquely moving plane was as the square of the sine of the angle of obliquity. The basis on which the assertion of this proposition usually rested would be best explained by reference to the accompanying diagram (Fig. 2):

Let the line $a\ b$ represent the sectional or edgeways view of a given plane immersed in a fluid, and let the plane be moving through the fluid, and traversing successively the positions $a\ b$ and $a'\ b'$, so that the space $a\ a'$, or $b\ b'$ was traversed in an unit of time; then

$a\ a'$, or $b\ b'$ would be the velocity of the plane in the line of motion through the fluid, which might be called (v).

Fig. 2.



It was common to regard this velocity as resolved into two components: one edgeways in the direction of $a\ b$, the other normal in the direction of $a\ \alpha$, and in the diagram $a\ \alpha$ was the measure of the component.

It was then held that the edgeways component was inoperative as a condition of resistance, in the same sense in which it might be fairly said that if the motion were purely edgeways the motion would be unresisted (for in this phase of the discussion, surface friction might be not improperly disregarded), while as regarded the normal component of the velocity, it was held that this alone was operative as creating resistance; and since the distance $a\ \alpha$, or $b\ \beta$, expressed the normal component of the velocity, and since it was held that when the motion of a plane was purely normal the resistance experienced was as the square of its velocity, it was inferred that the pressure on the plane would be as ($a\ \alpha^2$). Now if θ was the angle between the plane and the actual line of motion, ($a\ \alpha$), or ($\beta\ \beta'$) = v sin. θ, and the result was equivalent to the expression, Resistance = $k \times$ area $\times\ v^2$ sin. $^2\ \theta$; and it must be admitted that there was, at first sight, much plausibility in this mode of viewing the question.

Yet a very little reflection on the proposition in the aspect in which the diagram presented it, showed at once that the quasi reasoning by which it was arrived at was open to grave question, since it was obvious that the fluid to the right hand of the plane, into which the plane was continually intruding itself edgeways, was in a very different condition from that immediately in front of it. For it was certain, both as a matter of observation and of inference, that when the motion of the plane was purely normal, the mass or volume of fluid immediately in front of and behind it, must be, and was, moving forward with it, and almost with exactly the same velocity; whereas the fluid on either side of it was comparatively undisturbed, so that when the edge of the plane intruded itself into this undisturbed region, it must experience an excess of pressure, due to the normal motion which it at once imposed on the fresh particles which it pushed forward in front of it, and dragged forward behind it.

This operation indeed was a matter of common experience, and was unhesitatingly remarked on by those who witnessed it, as a natural and obvious consequence, when the circumstances presented themselves under a suitable aspect, and when their inherent contravention of the accepted law of the square of the sine was not perceived.

Thus, when a vessel was working to windward, immediately after she had tacked and before she had gathered headway, it was plainly visible, and it was known to every sailor, that her leeway was much more rapid than after she had begun to gather headway. The more rapid her headway became the slower became the lee drift; not merely relatively slower, but absolutely slower.

Again, any one might obtain conclusive proof of the existence of this increase of pressure, occasioned by the introduction of the edgeways component of motion, who would try the following simple experiment. Let him stand in a boat moving through the water, and, taking an oar in his hand, let him dip the blade vertically into the water alongside the boat, presenting its face normally to the line of the boat's motion, holding the plane steadily in that position, and let him estimate the pressure of the water on the blade by the muscular effort required to overcome it. When he has consciously appreciated this, let him begin to sway the blade edgeways like a pendulum, and he will at once experience a very sensible increase of pressure. And if the edgeways' sweep thus assigned to the blade is considerable and is performed rapidly, the greatness of the increase in the pressure will be astonishing, until its true meaning has been realised. Utilizing this proposition, many boatmen, when rowing a heavy boat with narrow-bladed oars, were in the habit of alternately raising and lowering the hand with a reciprocating motion, so as to give an oscillatory dip to the blade during each stroke, and thus obtained an equally vigorous reaction from the water, with a greatly reduced slip or sternward motion of the blade.

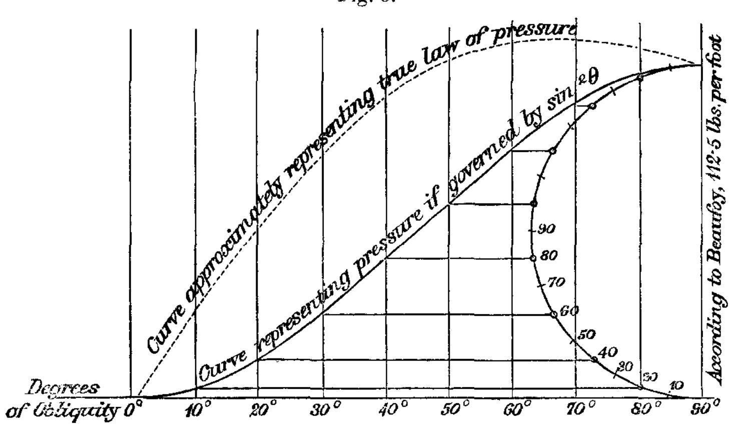
The attempt to quantify this increase of pressure, or in other words to assign a true law of resistance to an obliquely moving plane in virtue of the obliquity, was, he believed, an extremely difficult problem, and one which had not yet been regularly solved. He had himself attempted and obtained an approximate solution, the steps of which it would take too long to discuss here; and it was the less necessary to do this, as he hoped some of the advanced mathematicians of the day would shortly direct their attention to the question.

He believed, also, that no really satisfactory series of experiments had ever been tried by which an empirical law could be laid down. Hutton's experiments on the resistances of surfaces of various shapes moving obliquely through air gave some suggestions towards the formation of such a law; and something might be gleaned from some experiments of Vince's, performed with small discs moving obliquely through water, though the discs were too small to give decisive results, and the mode in which the pressures were determined was far from satisfactory.

Beaufoy, indeed, tried a series of experiments in water with planes of sufficient area; but his method of experimentalising was by no means satisfactory, and the results, when analysed, presented extraordinary anomalies (explicable, perhaps, by the faults of the method), which rendered a large part of them certainly unacceptable.

Relying, however, on such results as could be gleaned from the several sets of experiments referred to, and on the support which these furnished to the approximate solution which he had himself deduced, he ventured to indicate what he believed to be the character of the true law, in comparison with that depending on the squares of the sines of the obliquity.

Fig. 3.



He could best do this by the annexed diagram, Fig. 3, in which the spaces on the abscissa represented the various angles of obliquity, with which the plane—say a square plane of 1 foot area—might be supposed to be moving, and the ordinates represented the corresponding normal pressures in lbs. experienced by the plane; the motion being supposed to take place in water, and the velocity of motion to be in every case the same, namely, 10 feet per second.

The continuous line represented, thus, the accredited law of resistance as regulated by the squares of the sines, and the dotted line represented in character the true value of the resistance.

The curve representing (k sin. $^2\theta$) was readily drawn, if it was borne in mind that sin. $^2\theta = \dfrac{\text{vers } 2\,\theta}{2}$.

It must be observed, in reference to the dotted line, which represented the true law of resistance, that it must vary somewhat, in terms of the variation, in the form of the plane and the character of the movement of the plane in relation to that form. Thus, the curve would not be the same for an oblong rectangular plane as for a square plane of the same area. And with an oblong plane, the curve would be different according as the plane was supposed to be moving obliquely edgeways or obliquely lengthways.

The figure 112·5 lbs. per square foot, as the resistance of a plane when its motion was purely normal through water and its speed 10 feet per second, was quoted from Beaufoy's results.

As regarded the general aspect of the comparison between the suggested approximately true law of pressure, and the erroneous law which represented it as governed by (sin. $^2\,\theta$) several instructive features might be confidently accepted.

(1.) Indubitably, according to the true law, the pressure was far more nearly proportionate to (sin. θ) than to (sin. $^2\,\theta$); but there was good reason to think that the magnitude of the pressure at oblique angles was understated, even when it was held to be proportional to the simple power instead of to the square of the sine. For angles of very high obliquity the latter law understated it almost infinitely, and this difference was indicated by the diagram.

(2.) According to the suggested law, an apparently paradoxical result arose with angles of small obliquity; the result, namely, that by slightly inclining a plane to the line of motion the normal pressure which it experienced was slightly increased instead of being diminished; and even its resistance in the line of motion was thus increased if the obliquity was very small. The theoretical aspect of the conditions inherent in the intrusion of the plane into undisturbed fluid, in virtue of the superadded edgeways component of motion, pointed to this quasi-paradoxical conclusion;

and Hutton's experiments distinctly, and Vince's and Beaufoy's, though less clearly, presented indications of its truth.

(3.) The excess of resistance experienced by a plane moving with a high degree of obliquity explained the otherwise inexplicable phenomenon of the support offered by the air as an inclined plane to the rotating Australian weapon 'boomerang,' which, in virtue of its rapid rotation, virtually or approximately realised a normal resistance to normal motion, as great as that due to the whole area covered by it while *rotating*, even when *rotating* locally and without progression edgeways through the air; and probably a far greater normal resistance when the edgeways motion was superadded. Whereas, according to the popular law which referred the normal pressure to ($\sin.^2 \theta$), the normal motion should be as rapid when the weapon was spinning and travelling edgeways as when at rest.

It was in virtue of this same condition of rotation, combined with progression, that a screw propeller, consisting of a pair of comparatively narrow sectorial blades, realised the reaction (available for propulsive action) due to the whole 'disc of rotation;' or perhaps it would be more correct to say, due to the dynamic resistance of the whole cylindrical column of fluid traversed by it in its rotative progression. In point of fact, a two-bladed screw of ordinary proportions rotating locally without progression, at a speed due to a given velocity of progression, experienced about as much propulsive pressure as if it consisted of blades wide enough to occupy the complete circle, and, instead of rotating, were being pushed sternward with that given velocity; and when, by the ship's advance, it was constantly being carried forward into undisturbed fluid, its efficiency of resistance was still farther increased.

The second point to which he objected in the Paper was the hypothesis that when the true law of pressure on a plane moving obliquely through the water had in any way been determined, this law could be applied separately to each separate unit of a curved surface moving through the water, taking, as the basis of the application, the angle between the line of motion and the tangent plane of the unit. The mathematical discussion of the pressure on each point in the surface ought to take account, not merely of the tangential direction of the surface at that point, but also of its position in the surface as a whole, and of its relation to all other points in the surface; for the operation of each point in the surface on the whole volume of fluid through which the surface was moving modified the pressure which the fluid exerted on all other points. The discussion, to be real and complete, must take account of the surface as a whole, and of the universe of fluid which surrounded it; and though, no doubt, the volumes of fluid nearest to the surface were those which were most operative in governing the pressure, it was quite impossible to proceed in the investigation by the simple differential method which the Author had adopted.

He had, himself, long since seen this to be the case, but to effect the solutions he desired, by regular mathematical methods, applied to the principles which he saw and appreciated, was beyond his powers; and it was only recently that he had become acquainted with some of the results which the higher mathematicians of the period had achieved in this direction; and he was neither fully informed of these, nor was he able here to give even a limited account of them.

He could, however, refer to facts within the range of common experience, and well known to most engineers who were conversant with questions of fluid dynamics or of naval architecture, which showed sufficiently how inadequate was the mode of investigation adopted in the Paper.

Foremost of these was the fact, that when a plane area moved obliquely through a fluid, the normal pressure was most unequally distributed over the surface of the plane, existing in great excess towards the anterior edge; yet, according to the method relied on in the Paper, the pressure on every unit throughout the surface would be assumed to be the same.

The fact here referred to was the direct consequence and the counterpart of the excess of surface pressure, the genesis of which he described in the earlier part of these remarks. Its existence, as a fact, was at once perceived by any one who, holding a tolerably large plane in his hands, moved it obliquely through water or other fluid, as he would at once find that it exerted a considerable torsional force, indicative of the alleged excess of pressure on its anterior edge. Another simple indication of the same action was seen when a sheet of paper was launched edgeways through the air, and resting on the air as on an inclined plane: it was at once seen that the advancing edge was canted upward, and that the direction of motion was altered by the relative excess of upward pressure under the anterior edge; and a surface would require to be of specialised curvature in relation to its other proportions, in order that it should have an equal supporting pressure throughout, and should thus continue to descend on the air as an inclined plane. The 'boomerang,' indeed, continued to do this approximately, but this was in virtue of its rapid rotation, which gave quasi-fixity to its axis; but the peculiar twist which the axis received under the gyroscopic principles which the circumstances called into play here also witnessed to the unequal distribution of the supporting pressure.

Another practical illustration of the same condition was the well-established fact that a rudder, when mounted, as it was in some screw-ships, without the intervention of a stern-post and dead wood between it and the screw, was less powerful in its 'turning' action than if hung to a stern-post in the usual way. In the latter case, when the helm was 'put over,' a considerable lateral pressure was piled up, so to say, on the flat surface of the stern-post, and contributed materially to the whole turning power exerted.

Perhaps a more striking indication of a distribution of fluid pressure on a curved surface, irreconcilable with the Author's hypothesis, was supplied by the *primâ facie* paradoxical curvatures into which sails often arranged themselves under the effect of wind, as was specially noticeable in jibs.

In these sails especially, many sailmakers, for reasons which it would be out of place to enter on here, cut the canvas with an extravagant roundness or convexity of outline on the anterior edge of the triangle (or 'luff of the sail,' as it was called) before roping it; and as the rope was made somewhat shorter than the rectilinear dimension of the side of the triangle, the prominent edge of the convexity became gathered in, so as to form, immediately behind the rope, a narrow tapered belt of slack canvas, which became conspicuously bagged out by the pressure of the wind.

Now it was a most noticeable fact, familiar, doubtless, to all who had studied the 'sit' of sails, that when the vessel which carried such a sail was 'close-hauled,' that was to say, when the wind struck the sail obliquely from ahead, say at an angle of 45° with the line of the keel, the general wind pressure which the reaction of the rest of the sail produced swelled out the 'baggy' belt of canvas, not simply to leeward, but also so much forward that an observer viewing it in a direction at right angles to the vessel's course could see the convexity protruding itself ahead of the bolt-rope, although, from the direction of the wind current as a whole, that part of the sail, when thus protruded by the internal pressure, must experience externally also a considerable direct pressure on its convex or (so to call it) leeward side.

As the vessel was pressed closer to the wind, it was this part of the sail which would first begin to flap or 'lift;' but this would not happen until the greater part of the windward surface of the sail was brought so nearly edgeways to the wind, that the flatter or less 'baggy' portions of its surface, were nearly relieved of pressure.

It was but the same phenomenon in another form, when a bag, having its mouth partially opened and presented to the wind, became distended, in a sausage-like form, to a diameter much larger than that of the aperture of its mouth, its shoulders retaining a fully developed convexity, though subject to an external wind pressure, which, though by no means inconsiderable, was yet less forcible than, and was overcome by, the general internal force of distension which the reaction of the bottom and sides maintained within it.

In both these cases the Author's method would not merely fail to appreciate the correct balance of local pressure, but would absolutely ignore, or rather pronounce to be non-existent and impossible, what was, in fact, the conspicuously preponderating pressure.

He need not farther multiply illustrations of the proposition which he had asserted, in opposition to the view of the distribution of surface pressure relied on in the Paper, and he would now turn to the last of the three points to which he proposed to call attention—he meant the misconception which appeared in the Paper as regarded the dynamical relations, or inherent conditions, of the 'slip' of the propeller.

The Author defined 'the slip' as merely "the difference between the speed of the ship through the water, and the speed which she would have in a non-resisting medium under the same power." And he complained of the view expressed by Dr. Main, who, he said, "expressly attributes it to 'the yielding nature of the medium,' as if the law of the equality of action and reaction did not apply to fluid media." He thought that the words quoted did not necessarily convey the interpretation put on them; on the contrary, to say that a yielding medium yielded when force was applied to it did not in any way imply a failure of the relation between action and reaction, any more than it would to say that a gun yielded under the pressure of the exploded powder or elastic gas which propelled the shot, a proposition which precisely illustrated instead of negativing the due relation between action and reaction.

On the other hand, it might be thought that the very paradox of which the Author complained was suggested by his own words, when he spoke of "the speed which the ship would have in a non-resisting medium under the same power;" for the exertion of a "power" to keep the ship moving in a non-resisting medium, could such a conjuncture exist, would be precisely "action" without "reaction," though it was certain that the Author had understood his own words in a sense different from that impossible sense which they seemed *primâ facie* to convey. He thought, however, the Author had, at all events, not fully taken account of the dynamical condition which, in fluid propulsion, made "slip," or the equivalent of "slip," an inherent necessity.

The condition he referred to was that when dealing with matter which was free to move, as were the particles of a fluid, if it were wished to exert a forward force by rendering available their reaction, the particles among which the force was exerted must themselves yield, and receive motion from the operation of the force; and not only so, but the amount of motion generated among them in the direction of the backward force had a precise simple and defined relation to the magnitude of the force and the time during which it operated. And that relation was as follows: whatever lateral or transverse derivative motions the particles might receive under the operation of the force, if the momentum generated in the direction in which the force operated during a given interval of time was summed up, that was to say, the components of motion in that direction impressed during the interval on all the particles acted on, that integral momentum would be the exact dynamic equivalent of the operating force.

By dynamic equivalent, he meant as follows: the force of, say a ton, acting during a given interval of time on matter free to move, would invariably generate one and the same amount of momentum in the direction in which it acted, whether the matter it acted on consisted of a single ton of coherent matter, or of a mass greater or less than that in any assignable ratio, or of a multitude of particles acted on either simultaneously or in succession. The momentum thus generated was the dynamic equivalent of the force.

The relation might be expressed as follows: calling the force which acted F, M and V respectively the integrals of the units of mass acted on and of the velocities imparted to them in the direction of the force, during the second, so taken that $MV =$ the momentum thus generated during the second, or (μ); then making all the terms duly commensurate, the expression $F = \frac{MV}{g}$ or $= \frac{\mu}{g}$, where (g) was the force of gravity. Here MV, or μ, was the dynamic equivalent of F.

Or, if it were desired to adapt the expression specially to the circumstances of most common occurrence, bearing in mind that (g) might be written = 32, when feet and seconds were the denominations used in representing V, and that a cubic foot of salt water weighed 64 lbs., M being counted in cubic feet, it might be said F (in lbs.) $= 2\,M\,V$ or $= 2\,\mu$. Thus to maintain (say) 1000 lbs. of propulsive force, the propeller must in each second generate in the surrounding fluid a sternward momentum $= \frac{1000}{2}$, that was to say a momentum equal to that of 500 cubic feet moving with a velocity of 1 foot per second. And in proportion as the mass operated on was larger or smaller, the velocity imparted to the mass would be smaller or larger.

Now assuming that the water acted on by the propeller was stationary when the propeller began to act on it, then since *primâ facie* the propeller must virtually move sternward at least as fast as the water which it pushed, it would seem that the minimum possible slip must have exactly that velocity which it was necessary to impart to the mass of fluid operated on, in order to develop in it the dynamic equivalent of the force; and this proposition, which in fact expressed the highest limitation of slip attainable by the best form of ordinary propeller, pointed instructively to the necessity of always so proportioning the propeller as to make it operate on the largest possible mass of water, so that the sternward velocity which must be imparted to it should be a minimum.

But a theoretically perfect propeller might be imagined, such as would pervadingly operate on each particle of the reacting mass by a direct sternward force, and would impart the motion by a gradual acceleration, as gravity did, rather than by an impulsive blow; and such a propeller would impart the necessary final velocity by operating with only half the mean velocity which it finally attained, as a falling body acquired a velocity of 32 feet per second, though the force which impelled the body, that was to say its own weight, only travelled $\frac{32}{2}$ or 16 feet while imparting that velocity; and since 'the slip' was the mean virtual sternward motion of the propeller, measured as regarded the stationary fluid, it followed that with the theoretically perfect propeller acting on undisturbed water, the mean velocity of the slip was limitable to half the velocity which must be imparted to the mass of fluid operated on.

The result here stated pointed in an interesting and instructive manner to the theoretical possibility of apparent negative slip in a screw-propeller. For the water in which a single central screw operated was not stationary, but consisted of a forward current, called the 'wake,' which the transit of the ship had generated.

Part of the forward motion of the particles which surrounded and followed the ship's stern constituted, indeed, what might be termed the "conditions of established motion," belonging to the ship, as, for instance, that of the deadwater behind the stern-post and the 'square tuck' or other heavy lines of the run; or as, again, the converging lines of inward and forward movement, by which the surrounding water refilled the hollow which the transit of the ship had created. These conditions differed in the principle of their constitution from the 'wake' proper, inasmuch as, when once established, they no longer expressed any elements of which the ship's resistance consisted, since they had been established once for all, and either accompanied the ship inertly, as motion resident in unchanged particles, or as motions transmitted from particle to particle along the line of the ship's progress, on principles analogous to those of wave motion, without loss of energy.[1]

Setting aside these passive accompaniments of the progress of the ship, and referring only to the 'wake' proper, there would be seen in this the 'dynamic equivalent' of the ship's resistance, or, what was the same thing, of the propulsive force employed in keeping her in motion; for it represented wholly the constantly renewed forward motion which the friction of her 'skin' against the particles that rubbed against her, and the induced frictional movements of the particles of water "*inter se*" were ever creating in matter previously undisturbed; and it was now well known, that if the divergent waves generated by the ship *in transitu* were excluded (which with a well-formed ship moved at a moderate speed were trivial) these frictionally created movements were responsible for the entire resistance which the ship experienced.

The integral forward momentum, therefore, of the wake generated second by second, which was the exponent of the ship's resistance, must be identical with the integral sternward momentum, simultaneously generated in the 'slip,' which was the exponent of the propulsive force; and since two equal and opposite *momenta*, when brought into opposition, were mutually destructive, it followed that a screw propeller which operated so as to pervade the wake, would precisely neutralise the forward velocity of the wake; as in point of fact visibly happened.

[1] *Vide* "On the Mathematical Theory of Stream Lines, especially those with four Foci and upwards." By W. J. M. Rankine. Proceedings of the Royal Society of London, vol. xviii., p. 207.

And if the propeller were made to operate on the wake so far astern of the ship as not to displace the dead water, or interfere with the orderly replacement of the water in the track of the ship (in either of which cases a factitious excess of resistance was developed), and if, moreover, it was assumed to be of the theoretically perfect character already described, then since, as was explained, its mean sternward velocity, or slip through undisturbed water, would be only half of that which it finally imparted to the water, it followed that in neutralising the forward current of the wake and thus maintaining the propulsive force, it would in reality be receding before that current with a mean velocity of retreat equal to half the velocity of the current. That was to say, a theoretically perfect propeller, operating on the wake, quite clear of the ship's stern, might realise an apparent negative slip equal to half the forward mean velocity of the wake at the point where the propeller operated.

Mr. A. MURRAY, C.B., said that it was evident that a plane surface revolving upon an axis at right angles to it had no propulsive power in it; the action was similar to that of a circular saw, and there would be no propulsion in a plane surface placed in a line with the axis. It was also manifest that between these there must be one angle better than another, which it was the province of mathematicians to determine. The Author referred in the Paper to Mr. Griffiths' propeller, in which the blades were placed upon the circumference of a sphere as a boss, to avoid the nearly flat portion of surface towards the axis. There was no doubt that that, for which the Paper proposed another remedy by altering the angle, was good; but instead of the sphere he preferred, wherever the length of the screw would permit, an egg-shaped boss, the forward part of the egg being made to fit the ship, so that there should be no hollow between the stern-post, for the current of water to go in and come out again over the surface of the sphere, and the after-part of the egg should be equal to the breadth of the stern-post. He thought it better to lessen the diameter of the sphere than to depart from the view he wished to express.

The Author had spoken very clearly on the subject of slip; but he had not made himself sufficiently acquainted with the actual facts of the case, when he said that negative slip was impossible. It really did exist, and the explanation was found in the current of water which followed the ship, produced by her progress through the water. That there should be such a current following the ship was no doubt detrimental, and he looked upon that as evidence of malformation in the after-body of the vessel. If a ship were so formed that there was no after-current, and some were so formed, then less work would be required from the engine.

Mr. R. Griffiths exhibited the model of a propeller, and said that what he was going to explain was gained from experience. He objected to most of the theories advanced on the subject of the screw-propeller. In the first place, the ball offered no resistance in going through the water. The water for 3 feet or 4 feet on each side of the screw aperture travelled with the ship, and not only so, but much faster, because it flowed in to fill the space which the ship left. If there were no ball, but the blades were continued through to the centre, they would only churn the water. The model represented a screw 14 feet in diameter, the diameter of the ball being 3 feet 9 inches. Now, if the ball were removed, and the water flowed through the vacant space, the screw would make a greater cone of disturbance, but the smaller the cone the greater was the gain; in fact, it was an object of importance to keep the cone of disturbed water as nearly as possible of the diameter of the screw, which in this case was 14 feet.

In the next place, in that screw as it was there would be slip more or less from 10 to 20 per cent.; but if the same blades were laid to an angle of 20° to 30° towards the ship, there would be negative slip, but the propeller would not give better results. In the 'Flying Fish' the blades were put first at right angles to the screw shaft at a pitch of 20 feet, and then at 16 feet pitch, with the blades inclined towards the ship; the engines made the same number of revolutions and power, but the speed of the ship was no better, although there was apparent negative slip. Slip was a wrong term to use. Suppose a ship were fastened so that it could not move, and the screw set going, a column of water would be driven backwards equal in diameter to that of the screw; so when the ship was going the screw would still drive the same column of water. The same amount of water must be driven through the screw, whether the ship was under way or standing still; consequently it might be called all slip; it was only the resistance got by driving that column through the screw that gave the thrust to the screw shaft for propelling the ship.

If the screw were working in still water, he did not think any form could be better than the true screw when the blades were vertical; but when they were horizontal they were in a very different current. Therefore, unless it was possible to have blades which changed the propelling surface, so as to suit the different currents into which they entered at each revolution, the object aimed at by the Author of the Paper could not be arrived at. It was astonishing what a little difference there was between a perfectly flat blade and a true screw. A flat blade would be at its best when in the horizontal position, and a true screw would be at its best in the vertical. He found that screw-propellers with the point of the blade made of a coarser pitch than the root did much better than if made with the pitch the same throughout.

Mr. BRAMWELL said that Mr. Griffiths had made a statement of the results of his actual experience; and those statements ought to be treated with great respect, looking at the undoubted boldness and originality of Mr. Griffiths' views in screw propelling. Twenty-five years ago Engineers made the bosses of screw-propellers as small as possible, and to allow this they went to the length of forging the boss with the blades; at that time, also, the width of the blade at its circumference was commonly equal to the radius. But at that time came Mr. Griffiths, who reversed the whole theory and practice, making the boss one-third the diameter, and the point of the blades narrow, so as not to subtend more than one-sixth of the width they formerly subtended. That was a bold thing to do on reasoning unaided by mathematics.

One proposition of the Author of the Paper was borne out by Mr. Griffiths' practical experience. He understood from the Paper that, instead of making a true screw, the Author would make a screw, whatever the pitch at the circumference was, of less pitch as it approached the shaft; so that in no case would there be more than an angle of 45°, even if the blade were continued to the centre line of the shaft of the propeller. This was what the Author called the semi-helix, and was a screw in which the pitch was gradually lessened as the centre of the shaft was approached. Mr. Griffiths had contended that it was advisable to make a true screw, say of 12 feet pitch, and then so to set the blades (which in Mr. Griffiths' screw were generally adjustable) that they should have 16 feet or 18 feet at the circumference, and 3 feet or 4 feet less where they joined the large boss. In that respect it would be satisfactory to the Author to find that his suggestions agreed with Mr. Griffiths' practical experience; but the Author put it as though the screw of diminishing pitch to the centre was a new thing. Mr. Griffiths and himself had known of it for the last fifteen years, and it was not new then: therefore the Author was wrong in supposing he was the first to point out that modification of the screw.

The Author had spoken of speed in a non-resisting medium; but it appeared to him that if there could be a non-resisting medium, any power, if applied long enough, would develop an infinite speed. With respect to slip, that it did exist in a yielding material was obvious; and he thought Mr. Griffiths put it very clearly. The screw was propelling a column of water; whether the ship would move, and how much it would move, would depend upon the resistance offered by the ship. The vessel might be fast to a wharf, and still the screw would propel the column of water. If the ship were unmoored she still propelled the column of water, and the ship made motion. That the matter of slip was not an unimportant one was evident from the circumstance that, when the vessel was moored, the whole of the power of the engine would be absorbed to work the screw and to make slip; and that if it were unmoored, but were attached to something which offered great resistance to motion, then a very large part of the power would be absorbed in mere slip. In other words, it was quite possible to proportion the parts, so well or so ill, that a great or a small proportion of the power should be utilized. After all, this was the true test: what was the proportion of power utilized? Therefore he had contended many years ago, when conducting experiments on Mr. Griffiths' screw, that persons using a screw ought not to complain of the rate of slip; because, supposing with a given consumption of coal one kind of screw would drive a ship faster than another, then, whatever the relative slip might be, the former screw would get over more space with the same consumption of coal, and that was the end to be aimed at.

Slip did and must exist, and good performance might coexist with considerable slip; but, nevertheless, the question of slip was important because its consideration was necessary when proportioning the parts.

The Author further said whenever negative slip occurred it must be due to the action of sails combined with steam, and that seemed to be regarded by him as a sufficient explanation. Supposing in every case of negative slip canvas had been used, that was not an explanation, because, with the decrease in the resistance of the ship, due to the use of canvas, the engines would increase the speed; but it was the fact, as he had himself seen, that negative slip without canvas was a perfectly possible thing, and could be obtained whenever anybody wished to obtain it. Nobody would wish to obtain it, however, because it was occasioned by a larger following current of the ship than other ships had, and that was only got by a greater expenditure of power. But if a ship had a full run, and a screw of large dimensions and of fine pitch, negative slip would exist; and that there might be no mistake, he meant a case where, if the screw was working say 6 knots, the ship was going say 7 knots an hour, and that without canvas. The Author, however, said that was an impossibility, and that wherever negative slip happened it was due to the use of canvas. From his own observation he could say that was not so. Mr. Froude had very properly said it would not do to take one part of a propeller blade only and consider that part by itself; it must be taken as a part of the whole. This fact the Author had overlooked; and as a consequence the Paper afforded no instruction whatever as to what should be the number or area or form of the blades.

Mr. W. Airy said the Author had not fixed upon the precise helical surface best adapted for screw-propellers, but he had determined the family of surfaces which answered the conditions. It was stated in the Paper that, "This surface has a property in common with the ordinary helix. If it be intersected by a cylinder concentric with the shaft, the spiral thread or trace on the surface of this cylinder will make a constant angle with all lines on the cylinder drawn parallel to its axis; but the essential difference was that a plane through the axis cuts this surface in a curve, instead of in a straight line." He would observe that both these conditions were fulfilled by the family of developable surfaces, such as he had found best adapted for Archimedean screws for lifting water.[1] He had not himself investigated the general equation to these surfaces, but he had laid a model on the table to explain his meaning; and he would like to ask the Author whether that family of surfaces was comprised in the general equation which he had obtained for the screw-propeller. With regard to the curve in which the developable spiral surfaces were cut by a plane through the axis of the core, it would be something of the nature of a hyperbola; for it had an asymptote, to which the curve constantly tended, and which it never reached, and it sprang from the surface of the core at right angles. The shape of the spiral surface which the Author had arrived at as best adapted for screw propellers appeared to be due to the same principles as those which governed the weathering of windmill sails. These were spiral surfaces very nearly parallel to the plane of rotation at the extremities of the sails, and considerably inclined to the plane of rotation at the parts towards the axis. As the conditions were theoretically very similar, it would seem likely that the spiral surface which was found to be best for windmill sails would give a clue to the spiral surface which would be best for screw-propellers.

[1] *Ante*, p. 1.

Mr. C. G. Gumpel said the Author had overlooked the physical phenomena involved in the screw propeller. He would give an illustration. No mathematics would ever prove that it was the pressure of the atmosphere that drove the water up the pump. This was the result of a known physical phenomenon, but mathematics would determine how high the water would rise in the pump, according to the pressure of the atmosphere; when the practical man would step in, and show that the physical impossibility of producing an airtight piston would make it doubtful whether the water could be drawn up the pump to the theoretical height. In a similar way he looked upon screw propulsion as a physical phenomenon, and the question was how far water would offer resistance in giving a fulcrum for propulsion. It seemed extraordinary that the Author ignored that one point which had always been the stumbling-block in the question of propelling ships, viz., the yielding nature of the water; he, in fact, declared that the water was of too unyielding a nature. Now, the yielding nature of the water was the only guide in designing a screw-propeller of maximum efficiency, so far as influenced by form of blade, as other elements of the propeller could only be determined by the conditions of special cases. He would illustrate his meaning by the following experiments:—Take a board of about 10 square feet at the end of a stick, dip it into water, and apply a constant pressure against the water in a direction normal to the surface of the board. The water would be found to yield upon the continuous pressure. Upon taking a board of only 1 square foot, and applying the same amount of pressure in a similar manner against the water, but for a short space of time only, the water would be found less yielding. This evidently proved that time was an important factor in estimating the yielding of the water; that the longer each particle of water was pressed upon the readier it would be to yield. If this test was applied to a paddle-wheel, it would be found that the paddle-float, acting, while immersed, constantly on the same quantity of water, caused it to yield readily, so that the entering float was the really only efficient one, by its concussion against the undisturbed water. The screw-propeller, owing to its oblique action, encountered constantly fresh particles of water, comparatively at rest, and acted on them for a more or less short time only. This would explain why a long screw was inefficient. The screw-blade, by acting in a constantly changing circular direction, had the natural tendency of driving the water before it in a tangential direction, away from the axis. The particles of water which were taken up by the leading edge of the blade near the axis would leave the blade of a long screw near the circumference, hence there would be formed a conical hollow and a tendency to a vacuum behind it, which would act as a drag on the vessel instead of propelling her ahead. A short screw, for the same reason, would show better results, since it constantly took up fresh particles of water, which left the blade again after their inertia had been used as a fulcrum, and before they had commenced yielding to a more or less considerable extent. He would call attention to the experiments made in the 'Dwarf,' in 1844, in which the length of the screw was reduced from 3 feet 6 inches to 1 foot 3 inches, when a maximum effect was gained.

With regard to the question: What was the best form of screw-blade to obtain the best fulcrum in the water? not only must the blade be narrow, but it must be such a blade as would counteract the centrifugal action, and would direct the particles of water towards the axis instead of tangentially; in fact, it must be a curved blade, curved in the direction of the screw's rotation, instead of being placed radially; it would then be found that the water would be driven aft in a cylindrical column.

A screw of such a form had lately been tried in one of the Government vessels, and was proved to possess an advantage of 10 per cent. as compared with Griffiths' screw; as 400 H.P. less out of 4,000 H.P. were required for the same speed; and he had no doubt that it was the only screw that would be found to meet all expectations of efficiency.

He was tempted to refer to some remarks of Mr. Murray's and Mr. Griffiths' to show with what different views the phenomena of screw propulsion were looked upon. When speaking of negative slip, Mr. Murray showed that the rational way of explaining this phenomenon was, by taking into account, in the progress of the screw, the current of water which followed the ship with a certain velocity, in which current the screw-propeller found its fulcrum. And he very rightly observed that, by dragging such a following current behind the ship, great loss of power was experienced. Mr. Griffiths, when explaining the efficiency of his screw-propeller, used this same phenomenon to show that power was gained by the special form of the boss. The vacuum which this boss left behind it, he said, produced a tendency in the surrounding water to close in upon the centre, and thereby prevented centrifugal action. This surely could not but be a loss of power. Besides which, the better plan to prevent centrifugal action of the propeller was by forming the blades in the manner already pointed out. It was a question, however, whether the large boss was necessary to prevent the churning near the axis; since the method alluded to by Mr. Griffiths, of making the blade of a correspondingly less pitch near the axis than at the circumference, would prevent this evil. Take, for example, a screw of 20 feet pitch with an allowance of 10 per cent. slip, then the pitch of the blade near the axis would be made 18 feet, so that, when the vessel was going at a fair speed, no churning of the water would occur. He considered that experimental enquiry into the physical phenomena of the question at issue would be more serviceable than mathematical investigations to determine a screw of maximum efficiency.

Mr. W. Airy said that the spiral exhibited was one of the models with which he illustrated his Paper on the Archimedean screw for lifting water. The spiral threads of this model were visibly of the nature of developable spiral surfaces. The Author had noticed two properties of the surface of maximum work which he had taken as particularizing that surface. The first was that it resembled an ordinary helix in making a constant angle with lines on an intersecting cylinder drawn parallel to its axis; the second was that a plane containing the axis cut the surface in a curve and not in a straight line. The combination of these properties was undoubtedly a peculiar feature, and they were both satisfied by the developable spiral surface of the model.

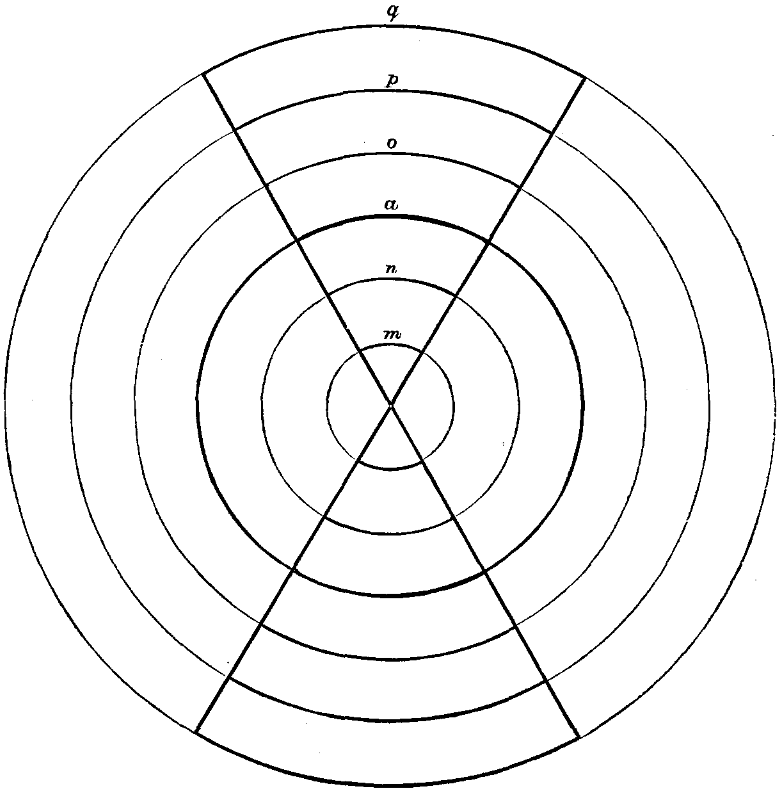
Mr. Bramwell remarked that the pitch of the screw near to the wooden axis of the model exhibited by Mr. Airy was the same as at the circumference; but the Author of the Paper recommended that the pitch of the screw towards the axis should be less than the pitch towards the circumference.

He did not understand Mr. Airy to say that the developable screw would do that which the Author contended, viz., make a screw of less pitch at the centre than at the circumference. The Author said, "This it is which enables a moderate angle to be obtained close to the shaft, where the crank exerts the greatest power, and the blade, not clearing the water, does not cause the vibration observable in the common helix, while its hold upon the dead water is a clear gain in power." From this it was clear that the Author considered the best screw of all was where the pitch was less at the centre than at the outside.

The mode in which the Author proposed to proportion the angles at different circumferences of the propeller to the angle determined to be employed at some one circumference in that propeller might be thus stated in popular language.

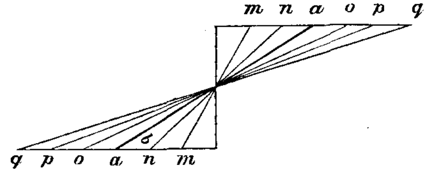
Let "*a*" be one of the circumferences in the propeller, and by preference let this circumference, "*a*," be half that of the propeller (Fig. 4).

Fig. 4.



Face view of an ordinary true Screw two-bladed Propeller, having an angle of 30° at the circumference *a*, with a uniform pitch of 0·90726, the diameter of the propeller being unity.

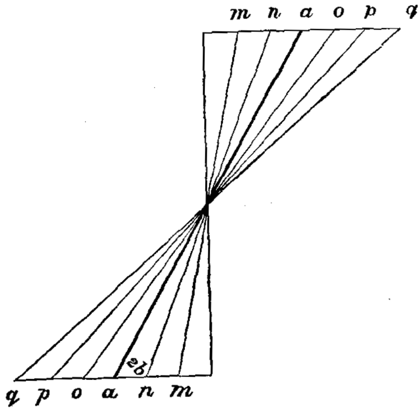
Fig. 5.



End view of one of the blades of Fig. 4 (but with the segments laid out into straight lines), showing the angle *b* of 30° for the circumference *a*, and the varying angles for the other circumferences.

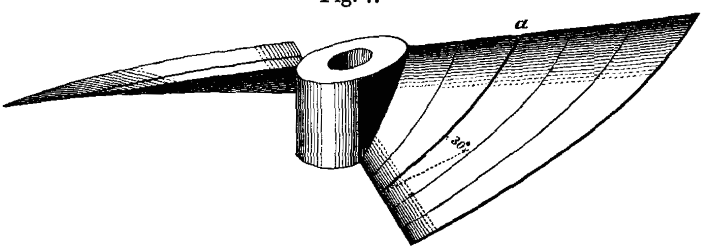
Let "*b*," Fig. 5, be the determined angle to be made at that circumference by the face of the propeller with a plane at right angles to the axis, say for the sake of illustration 30°. Then double the angle "*b*," and with that double angle, 60°, set out a true screw, Fig. 6, that was to say, a screw of equal pitch for all circumferences of the propeller.

Fig. 6.



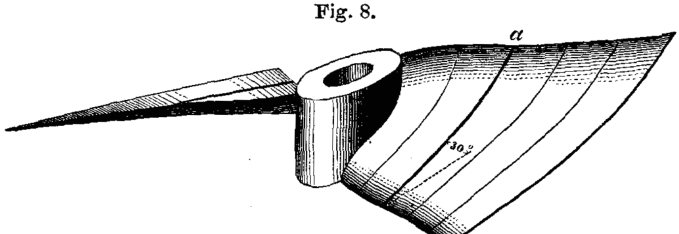
End view of one of the blades of a true Screw-Propeller, as in Figs. 4 and 5, but with the angle *b* doubled (= 60°)—showing also the varying angles for the other circumferences, with a uniform pitch of 2·72199, the diameter of the propeller being unity. In Figs. 4, 5, and 6 the angle at the centre is 90°.

Fig. 7.



Ordinary two-bladed true Screw-Propeller.

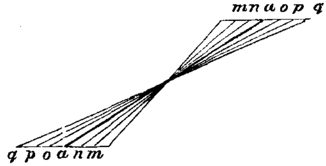
Fig. 8.



Two-bladed Screw-Propeller proposed by the Author.

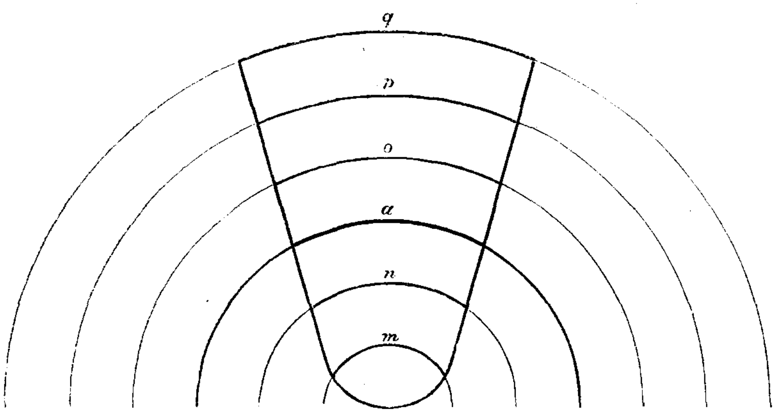
Then to make the actual propeller recommended by the Author, his 'semi-helix,' bisect the angle made at any circumference, *m*, *n*, *o*, *p*, *q*, between the face of this true screw and a line at right angles to the axis, and a propeller surface corresponding with these bisections would give the semi-helix, Fig. 9.

Fig. 9.



End view of one of the blades of the Author's proposed Propeller, formed by bisecting the different angles of Fig. 6, and using these new angles to give the twist of the blades, which is no longer a true screw, but one of a pitch at the circumference of a = the pitch of Figs. 4 and 5, and of a pitch increasing from the circumference a towards the outer circumference, and decreasing from the circumference a towards the axis. See also Fig. 10.

Fig. 10.



Showing the curved form of the edges of the blades when they are not true screws, but are of a twist in accordance with Fig. 9.

In Figs. 9 and 10 the angle at the centre is 45°.

In Fig. 10 the pitch at	q is	1·168
" "	p is	1·11
" "	o is	1·028
" the determined pitch at	a is	0·90726
" the pitch at	n is	0·7194
" "	m is	0·4319

Such a propeller would fulfil the Author's conditions.

1st. At any circumference, *m, n, o, p, q*, its face would be at an unvarying angle.

2nd. Whatever the angle determined on at the circumference "*a*," that at the axis would be 45°.

3rd. If the propeller were cut by a pair of planes at right angles to the axis, the boundary lines of the blades of the propeller would not be straight radial lines as in the true screw, Fig. 4, but would be curved as in Fig. 10.

Admiral E. P. Halsted said that nothing had yet succeeded in producing better effects in absolute propulsion than the original true screw, which was identical in mechanical principle and formation, whether worked for driving ships through the water, or used as a fastening to the hinge of a door. That true screw, he believed, had never yet been surpassed in its effect of maximum work; and he had never known an authenticated instance in which it had been excelled. The difference of form, at the extreme edge, between the true screw, and that of Woodcroft's, of increasing pitch, was so small that either screw could be cast in the same mould, the difference amounting to only the fractional part of an inch. In relative work however there was this remarkable difference—that when tried in the same ship under similar circumstances, the increasing pitch, used with its fine edge foremost, gave nearly similar results, with similar power, to the true screw; but when used with the increasing pitch foremost there was a material increase of power developed by a greater number of revolutions, with a considerable decrease of effect. The result of turning the increasing pitch end for end was that ships could not 'back astern' so effectively with the Woodcroft's screw as with the true screw; but in 'going ahead,' throughout all the trials of those days, all modifications of the true screw gave favourable results between the one and the other, precisely as the variation in pitch and other conditions came nearer and nearer to those of the screw with a uniform pitch. This matter was closely experimented on and discussed at the time, and he did not think much more was known about it now. At that time, too, the practical effect of negative slip was known, and in some remarkable cases no repetition of trials could alter it. Notice was then also taken, and stress laid on the fact, that with ships of any form, or of any size, going through the water at any speed, there was, and still must be, what came to be called in screw propulsion 'a following current,' which filled up the hole made by the ship's motion, precisely as fast as it was made, and without which there must be a hole, or furrow, or reduced level of the water, astern of her. In that 'following current' was recognised the solution of what the screw did in contrast with the paddle, and why the ship, when under sail, could not over-run the screw; because, whether driven by sails, or by steam, or even pulled by horse power, there always was this current, in which the screw in its true position in the 'dead wood' found its ever-following fulcrum. It was not determined then, any more than now, what were the details of action by which the water following the ship was operated upon by the screw; nor the various directions and degrees whence were derived the power of the current as regarded its following the varying motion of the ship. In those days, however, there were apertures, up which the screw was raised out of water, that the propeller should cause no obstruction to the ship while sailing; and on thrusting an oar down these apertures at the foremost corners when the screw was at work, it was often found very difficult to hold it, not from its being dragged behind or astern, but from its being powerfully dragged forward by the strength of the stream rushing under the bottom from aft. There were still these grave mysteries about this matter, to solve which, he thought, there must be a great deal more practical observation, before the final facts could be arrived at on which to found a formula.

Admiral G. Evans remarked that in 1837, having carefully examined Mr. Smith's screw-propeller, then fitted to a boat lying in the West India Docks, he was much surprised at its simplicity, and saw that it was far superior to the paddle; and only required, for the protection of the screw from ropes and weeds, a prolongion of the keel and a false, or outer, stern-post, on which the rudder could be hung, and the screw to be placed in the space between the two stern-posts.

Lord Sligo and Lord Western got up a company, and built the Archimedes,' screw-steamer, and in 1839 he made a report to the Admiralty on the performance of that vessel.[1] Having been employed since then in other departments, he had no further experience of the working of the screw, or he would have endeavoured to get a trial of Mr. John Brett's plan of using a four-bladed screw when steaming against heavy head-winds, and altering it to a two-bladed screw when under sail or during fine weather. By that plan, the steam now blown away would be used on the four blades, to great advantage, during the prevalence of strong head-winds, while the speed of the vessel was necessarily slow.

[1] *Vide* The Nautical Magazine and Naval Chronicle for 1852, p. 676.

Mr. G. H. PHIPPS remarked, through the Secretary, that he did not consider mathematics usefully applicable to any subject abounding to such an extent with unknown laws and quantities as the action of the screw-propeller. Nearly all the attempts made for the improvement of the screw-propeller, from its first useful application, by Mr. F. P. Smith, Assoc. Inst. C.E., up to the present time, were of an empirical character. For instance, at first a large portion of the spiral equal to, at least, one entire revolution was considered necessary. Then the increasing pitch suggested by Mr. Woodcroft and others began to be a favourite idea, until, as time went on, $\frac{1}{6}$ of a revolution was admitted to be a sufficient length of the spiral, and was generally adopted in the Royal Navy.

The French found, indeed, that the screw-blades might be still further diminished with advantage; the only limit being apparently to leave sufficient substance of metal for the necessary strength. The increasing pitch was generally admitted to be of little or no advantage, as indeed must be apparent when the blades were so much reduced in depth.

When the screw-propeller superseded the paddle-wheel, it was naturally reasoned, that as to obtain more and more power, with the latter, it was necessary to use larger and larger paddle-blades, so, in substituting the screw, an analogous increase of surface acting upon the water must be obtained. The discovery that no such increase was required led him to an entirely new set of ideas, and, in his opinion, gave the key to an explanation of the great propelling power given out by that instrument.

Every small unit of the contact surface of a screw propeller in motion might be considered as a plane surface, moved through the water in a direction at a very acute angle with its own plane; and when this was the case, it was well known that the pressure against the side of such plane vastly exceeded that arrived at by the law of the square of the sine exceeding it all the more as the angle became more acute. He had particularly pressed this fact in a Paper read before the Institution in 1864,[1] as explanatory of the impossibility of comparing the resistance of bodies in water when differing considerably in magnitude, and which were not similar bodies; as, if the draught of water were identical in two cases, and the angles of entrance also the same, both these bodies would encounter equal resistances of the kind above referred to, *i.e.*, above the law of the square of the sine, while all the other resistances might differ largely.[2] Another point as to the resistance of plane surfaces

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxiii., p. 221.

[2] *Vide* Transactions of the Society of Engineers for 1868, p. 220.

consisted in the fact, that whilst moving either at right angles, or at some smaller, but still considerable angle, to themselves, the law of the resistance being as the square of the sine was nearly correct, and the depth of immersion caused no appreciable difference; but with plane surfaces, very little inclined to their direction of motion, *the amount of immersion constituted an important element* of the resistance.

An explanation of the cause of the great comparative resistance to the motion of very slightly inclined planes might in his opinion be found in the circumstance, that all the water displaced by them was so displaced, laterally, to their line of motion; and so all the particles of water in advance of the plane were left nearly in a state of absolute rest. The plane, consequently, when coming into contact with particle after particle, could only displace them by raising up the whole of the column of water above them. Suppose a plane 1 foot square, immersed to a mean depth of 9 feet, inclined at such an angle to the direction of its motion as to give a sine of 3 inches, and moved at a speed of 18 feet per second, it would have to put into motion, in one second of time, $18 \times 9 \times 62{\cdot}5 = 10{,}125$ lbs. of water, at a speed of $\frac{18}{4} = 4{\cdot}5$ feet per second, equal to $\frac{\text{gravity}}{7{\cdot}1} = \frac{10125}{7{\cdot}1} = 1{,}426$ lbs. pressure against the side of the plane. By the law of the square of the sine, the pressure against the side would be,

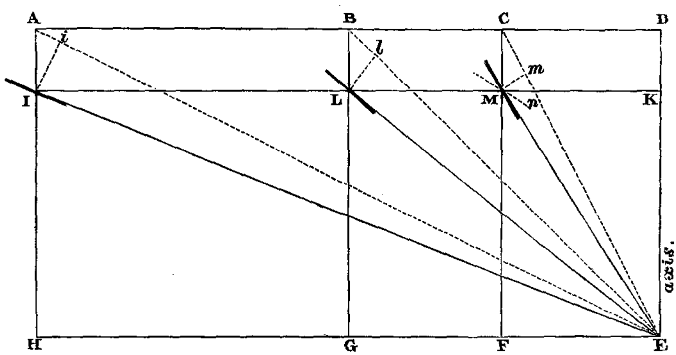
$$\frac{\frac{18^2}{8} \times 62{\cdot}5}{4} = \frac{2{\cdot}25^2 \times 62{\cdot}5}{4} = 79 \text{ lbs.}$$

It would thus be seen that on this law the resistance was $\frac{10125}{79} = 128{\cdot}16$th part only of the former. Great as this difference was, however, it would in effect be doubled if a smaller plane, 12 inches by 6 inches only, had been taken, as on the first mode of calculation, the resistance would not be diminished by the reduction in area; whereas, on the second, it would be reduced one-half.

Going now to the Paper under discussion, he contended that as so important a matter as the above increase in the law of resistance of inclined planes was left untouched, involving as it did the important fact of the equal efficiency of short planes with long ones, all the elaborate logic of mathematical reasoning used by the Author must needs fail in arriving at the truth.

The paragraph he would first touch upon was where, speaking of the properties of the peculiar helix recommended, the Author said, "This it is which enables a moderate angle to be obtained close to the shaft, where the crank exerts the greatest power; and the blade, not churning the water, does not cause the vibration observable in the common helix; while its hold upon the dead water is a clear gain in power." Now he considered that a considerable fallacy was involved in these observations. In the first place, he was far from admitting that the acuteness of the angle of the ordinary helix near the shaft was a disadvantage. He believed that, theoretically, every portion of the ordinary screw-blade gave out a thrust commensurate with the power employed in working that portion; that if the term 'churning' meant water through which any portion of the screw actually passed, at right angles to itself, which he called slip, this would be greater with the helix described than with the ordinary one; and, lastly, that 'vibration' had nothing to do with the 'churning,' but arose from the shocks experienced by the entrance of the screw-blades into water, either at rest, or in various angles and directions of motion, in the course of each revolution of the shaft. In Fig. 11 was represented the development of a screw with ordinary

Fig. 11.



helix, in which A H and D E represented the pitch, A D its circumference at the periphery, B D = $\frac{1}{2}$, and C D = $\frac{1}{4}$ of A D. If there were no slip, the ship and screw travelling at the same rate, the course through the water of the different units of the blade, I, L, and M, would be on the lines E A, E B, and E C; but allowing $\frac{1}{5}$th of A H for slip, the actual courses became E I, E L, and E M. Now, first, as regarded the relation of the power absorbed by each unit of contact surface to the work done by it in propelling.

s 2

It was a recognised fact, in estimating the power of any metallic screw, working in a nut, that provided the length of the lever applied were the same, no difference of effect would arise, except friction, which was here disregarded, from the largeness or smallness of the diameter of the screw; and, consequently, any power applied to the lever would respectively raise corresponding weights through the distances F C, G B, and H A; but, in consequence of the slip, in all cases equal, and corresponding to A I, $\frac{1}{5}$th of A H, $\frac{4}{5}$ths only of the power applied on each unit was realized. There was, therefore, no greater proportion of power lost on a unit M, near the axis, than upon I, remote from it; and, as the distance M *m* was less than either L *l*, or I *i*, there was less churning of the water at the interior than at the exterior circles. It also appeared that, if the angle of the unit of surface at M could be altered according to the Author's views to, say M *n*, such alteration would increase, rather than diminish, the churning of that unit, and that if altered more than to bring it in line with M E, instead of propelling it would actually obstruct.

As to the Author's objection to the term slip, he was quite at a loss to conceive how any word could better convey the meaning intended. The blade of an oar was let fall into the water, a strain was put upon the oar, the blade shifted backward a certain distance, and it surely slipped through the water that distance. Again, the use of the hypothesis of a vessel moving in a "non-resisting medium" was, to him, perfectly unintelligible, since no power could be required to propel such a vessel. And, finally, as regarded negative slip, he could not admit the correctness of the Author's assertion that "it can only arise from the joint action of steam and sails," as it was well known often to exist under steam alone, and could well be traced to the effect of water in motion, carried along by the vessel itself, and that, all the more in proportion as the after lines of the vessel were deficient in fineness.

Sir F. C. Knowles, in reply, observed that there was very little room for discussion between him and Mr. Froude, when Mr. Froude objected *in toto* to the hitherto accepted principle of resistance to motion in fluid media, on which the whole of his investigation was founded. He could only, as to this principle, give a direct demonstration of the principle itself, which demonstration, as far as he could judge, assumed nothing beyond the well-established laws of force and motion, and the ordinary mechanical constitution of incompressible fluids. He, however, would point out that the apparent discrepancies, from the principle of the resistance being "as the square of the velocity multiplied by the square of the sine of the angle of inclination of a plane to its line of motion," might more probably be attributable to the inaccuracy of the constant of resistance, and more frequently, in the cases of bodies moved with very high velocities, in elastic and compressible media, which might greatly modify the phenomena and the apparent law of resistance. It had been contended, in the course of the discussion, that the application of mathematical reasoning in the treatment of such subjects was wholly inadequate and out of place; and perhaps it was not agreeable to be so tied down to measure and number, as it was apt to check the imagination. But it was rather too late in the day to object to the treatment of questions touching material force and motion by mathematical analysis, when it had been so long and so successfully applied to phenomena infinitely more varied, complicated, and delicate than those presented by any possible question in the mechanics and hydrostatics of engineering. Mr. W. Airy, however, had fairly entered into the spirit of his method, and appreciated its results, and the illustration from the 'weathering' of windmill sails was as appropriate as it was conclusive. He was disposed to think these surfaces were comprised in his group. Mr. Andrew Murray also seized the very essence of the problem and its importance. He might have put this in a popular form as follows: "Take, for every point within the limits of the blade's dimensions, an indefinitely small plane surface, or facet, at the end of an arm or radius from the axis, and place each such facet in the position to give the greatest possible propulsion in the direction of the keel, by rotation round the axis; then unite from point to point all these minute facets, supposing them to cohere and to become one polyhedral surface. Lastly, pass from the discontinuous to its limit, the continuous arrangement of the system, by supposing the facets infinitely small and infinite in number, and there would result a surface comprised in his solution of the problem. The surface so obtained was of course only one type of the family which answered the conditions." He did not here mean his method of solution, but the kind of result obtained by it; the method itself, by means of the abstruse relations of quantities in or about the state bordering upon or attaining a maximum or a minimum value, determining the form required. Mr. Froude overlooked one important consideration, that, even admitting the law of resistance to be defective, as he asserted it to be, if he should be the fortunate person to discover the true law, the method employed in the Paper was the only one which could be effectually applied by his law to the determination of the best form of propeller. There was a choice between a certain mode of selection, and endless trials, with endless disappointment and expense. He saw no obstacle to following out, by a correct mathematical analysis of the case, the motion of every particle of water acted upon either directly by the screw, or indirectly by means of the ship's form. The great difficulty was to take account of the secondary disturbances, due to the waves of water acted upon by statical pressure, which waves rushed in laterally to fill up the void left from moment to moment by the ship in her course. He could not omit to remark that Mr. Froude proposed no definite principle to replace the present one. He suggested only one ($v \cdot \sin \theta$) to throw doubts upon its application at oblique angles, in favour of some other which he had 'in petto.' Of this latter he did not give even an approximate expression, or it might at once be put to the test of some simple practical application susceptible of a conclusive trial. It would occupy too much time to analyse all the cases which Mr. Froude put in illustration of his adverse views of the law, or rule, of the square, but he saw in them quite enough to be convinced that all the views enunciated might be reconciled with the law in question, or the apparent discrepancies from it assigned to independent disturbing forces inseparable, or at least not yet separated, from the conditions of the experiments upon resistance. He might remark that in gunnery, where accuracy in calculating the trajectory of a projectile was of the greatest importance, no artillerist had ever proposed to abandon the principle of the square of the velocity for that of the simple velocity; for example, though the trajectory in the latter case could be readily calculated, and in the former (unfortunately) could not be calculated at all except by a laborious approximation.

He had already referred to the constant of resistance, as k in $k\,v^2$, and he would have it borne in mind, in dealing with these somewhat vague objections, that in many experiments upon resistance rough surfaces were used; so that it would be difficult to say whether the simple sensible perpendiculars to the apparently plane surface were the real normals. For example, in a wavy surface the real normals would be only those on the crests and on the hollows of the waves in direct motion; and, in oblique motion, only those between the crest and the hollow. Thus only a portion of the particles would, in either case, be in the conditions of motion which were the supposed data of the problem. There was an analogous case in light reflected very obliquely to an ordinary table, where a bright image was obtained: none but the faintest, if any, being perceptible at higher angles. And the cause of this difference was correctly assigned to the perfect reflection of light from the prominences, unmixed with the scattered rays from the hollows. Now, it was only required to suppose these ridges and hollows of the planes, which were used in the experiments on resistance, to be large in proportion to the diameter of the molecules of water, to be sure that such a physical conformation must largely interfere with the uniformity of the results. This, then, would be no reason for abandoning the law of the square, *à priori* the most probable, but simply a motive for employing none but the most highly-polished substances, and in every case of change of material to modify the constant of the formula. The perfect conformity of the law, or principle of the square, to the known laws of force and motion, raised a high, *à priori*, probability in favour of its correctness, and a no less probability was created by the simple explanations of all apparent deviations from the law which were afforded by the above given physical imperfections of the planes employed to test the truth of the hypothesis.

No one seemed to have understood the result at which he had arrived on the subject of 'slip' of the screw, although the views upon this matter of Mr. Griffiths and Sir Francis Knowles were virtually identical. It was asked whether an oar did not 'slip' back in the water. Without inquiring whether the term was appropriate, he answered to the meaning, certainly it did, but in doing so it communicated motion to matter; to the water in one direction, and to the boat in the other; so that, if the mass of water was m, and its velocity v, the mass of the boat being M and its velocity V, while the mass of the water pushed ahead was m' and its velocity v', then $m v = \mathrm{M} \mathrm{V} + m' v'$. But the slip in question was 'slip;' not a source, but a supposed loss of power; for, what other could it be, when the advance of the whole system, ship and screw together, was less than the advance of the screw in a nut with the same number of turns per minute? *Pro tanto* the screw might as well revolve in the air! This was not his view of the matter, for he said there was no loss of power in any way, or direction; nor was there any 'slip'—a word used only to mask an erroneous theory. Consider, irrespectively of any theoretical views upon the subject, how 'slip' was computed. The number of turns of the screw working alone, and in a nut per minute, gave the potential speed in advance in direction of the keel. From this was deducted the actual speed of the screw and ship together, in the same direction; and this difference, divided by the former speed, was the ratio called 'slip.' Now, what was this potential speed of a screw working in a nut under the same power of the engine? Was there any deduction from it for either friction or resistance of any kind? None! Then, if not, could the speed of the point be other than the unresisted speed *in vacuo?* Take the formulæ of Professor Main, viz.:—

w = angular velocity of the screw in rotation;
b = $a \tan \alpha$ (a = the radius, α the angle of the screw);
then $w\, b$ = the potential speed in advance of a point in the edge of the blade;

and $w\, b$ put = u, let v = speed of ship and screw;

then $\frac{u - v}{u}$ = the 'slip' of the screw.

Now, in these elements there was no symbol which took into account friction, or resistance, of any kind. And it followed that all such sources of retardation were supposed to be non-existent, and that this speed of the screw in a nut was its unresisted speed *in vacuo*. Now, in this condition put before it a ship on a perfectly smooth plane. The ship under the power of the engine would attain the same speed as that of the screw connected with it, and the two would go on together. But, what was this but the motion of the ship in a non-resisting medium? And could the ratio of the above difference, $\frac{u - v}{u}$, be any other than the measure of the degree in which the medium resisted the motion, or, as he had properly called it, was unyielding to it? It was a mere affair of convertible terms! Let the question be viewed in another light, which seemed to afford a decisive criterion. If there was any inherent defect in the medium which led to loss in the form of what was called 'slip,' it ought to be constant for the same screw, and to follow it as its shadow when transferred to another ship with a smaller or a larger midship section; but the algebraical expression for 'slip' contained H, the area of the effective midship section, and the 'slip' must therefore vary from one vessel to another, in all cases save where it was in a constant ratio to β A—an exceptional case. His interpretation of the formulæ showed that this was so, because the larger or smaller the effective midship section, the greater or smaller must be the resistance to motion, respectively, and the difference between the speed of the ship at its best, and the potential speed in a non-resisting medium, must be greater or less accordingly. Again, it could not be by accident that the algebraical expression was the same in both cases. As to 'negative' slip, it meant, according to the explanation of his critics, that in the last resort the screw made a greater number of turns per minute and advanced with the ship more than if it were working in a nut; in fact, that it was positively accelerated by motion derived from its own power —in other words, perpetual motion. It might fairly be asked whether there was any but an insignificant "following of water" due to friction between the ship's hull and the water, a motion speedily subdued by the mass of inert water into which she was momentarily intruded? The real movement of water abaft, due to statical pressure, was transverse from opposite sides, and these lateral waves rushing to fill up the void astern, were compounded with the motion of the column driven through the screw (their limiting lines depending on the form of the stern) in such a manner that they must either wholly destroy each other, or partially so, leaving a part to conspire with the sternward column. The former condition might so far deaden the water astern as to increase the reaction, or thrust, of the screw, and thus, by increasing its efficiency, appear to diminish slip, the speed of ship and screw increasing accordingly. He should call it diminishing the relative resistance by an increase of speed in the ship; but, for the rest, as 'slip' could never be $= 0$, *à fortiori*, it could never be negative, by any motion derived from its own power, whether direct or indirect. He wished it to be understood that in his mention of Mr. Griffiths' moveable screw blades he had no intention to depreciate the merits of his invention; on the contrary, he thought it highly ingenious and important; and whatever might be the latent advantages of a screw of his own type, he was ready to admit that experience alone could show which was the most advantageous as a propelling machine, some well-selected form of his own or that of Mr. Griffiths.

He had tried the simple power ($v \cdot \sin \theta$) in his method, but it gave no result beyond the ordinary family of helicoids.

It would be well to engage the British Association to institute a course of experiments upon resistance with highly-polished surfaces, and to compare the results obtained with those given by ordinary planes.