1874

Report to the Lords Commissioners of the Admiralty on Experiments for the Determination of the Frictional Resistance of Water on a Surface, under various conditions, performed at Chelston Cross, under the Authority of their Lordships. By William Froude, F.R.S.

[A Communication ordered to be printed in extenso *by the General Committee of the British Association for the Advancement of Science, at Belfast in August,* 1874.]

EXPERIMENTS FOR THE DETERMINATION OF THE FRICTIONAL RESISTANCE OF WATER ON A SURFACE UNDER VARIOUS CONDITIONS

*Second Report**

Chelston Cross,
13 December, 1872.

As in the Report on the subject handed in in August last, the results of the investigation will be presented under three principal aspects:—

(1) The law of the variation of the resistance, in terms of the variation of the speed.

(2) The law of the variation of the resistance, in terms of the variation in the length of the surface.

(3) The nature of the variation of the resistance, in terms of the variation in the quality of the surface.

It will be seen, however, that, as exemplified by the results now presented, no less than by those presented in the former Report, the three laws are more or less interdependent.

In this concluding part of the series it was sought to give completeness to the determination of the effect of quality, in what may be termed its practical extremes of smoothness and of roughness. The experiments comprising the completion of the trials made with a tinfoiled surface on the one hand, and one coated with rough sand on the other, represent these extremes.

The list of materials used in forming the surface includes (1) tinfoil; (2) hard paraffine, laid on thin and scraped perfectly smooth (this was also used as a substratum on which to lay the foil, the medium of adhesion being a thin coat of tallow); (3) blacklead, polished on the paraffine; (4) unbleached calico; (5) three varieties of sand, differing from one another in the coarseness of grain. The sands, of graduated fineness, were in turn sifted on to a paraffined surface, having been previously sufficiently heated to melt their way into it and become fixed there.

There was, as might be expected, some difficulty in securing identity of quality (1) throughout the length of each individual surface, and (2) (*à fortiori*) in the planes of different length. Of the smooth surfaces, the scraped paraffine, naked, was perhaps the most uniform for all lengths; of the rough ones, the calico. But in each case pains were taken to secure uniformity, and no difference of perceptible amount was permitted.

A tolerably correct perception of the different degrees of roughness obtained with the roughened surfaces will be conveyed by the full-size photographic representations. (Fig. 1).

In forming all the surfaces care was taken to avoid abnormal roughness, and to eliminate the effect of thickness of cutwater and of stern-end or run, the ends of all planes being formed as shown in Fig. 6 of the previous Report. In the case of the calico, a fine entrance was obtained by placing a sharp tin cutwater, 1 inch long, over the seam at the front edge of the plane; the calico was also carefully closed round the tail, and a fairly fine run secured.

The results obtained are shown in full detail in the accompanying diagrams, four in number, which, as in the former Report, represent them *seriatim*, as finally reduced, in two separate forms. In one form (series 1, Figs. 2 and 3) the abscissæ or measurements along the base line represent speed; in the other (series 2, Figs. 4 and 5) they represent length of surface. The corresponding ordinates in each case represent resistance.

In the first-named series, each of the successive lengths of surface has a group of curves assigned to it, corresponding with the various qualities of surfaces, and exhibiting the law of resistance in terms of *speed* of surface.

In the second-named series, each of the successive speeds of surface has a group of curves assigned to it, corresponding with the various *qualities* of surface, and exhibiting the law of resistance in terms of *length* of surface. In each of the diagrams, curves showing the results given by a surface coated with shellac varnish are given as a standard of comparison, the former experiments having shown that this quality of surface might be regarded as in some sense a standard quality—it being easily laid on with invariable quality, and being practically identical in respect of resistance with Hay's or Peacock's composition, smooth paint, or tallow. These standard curves are copied from the diagrams which accompanied the former Report.

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* For Preliminary Report *vide* Report of Brighton Meeting, 1872. (See p. 138 in this volume.)

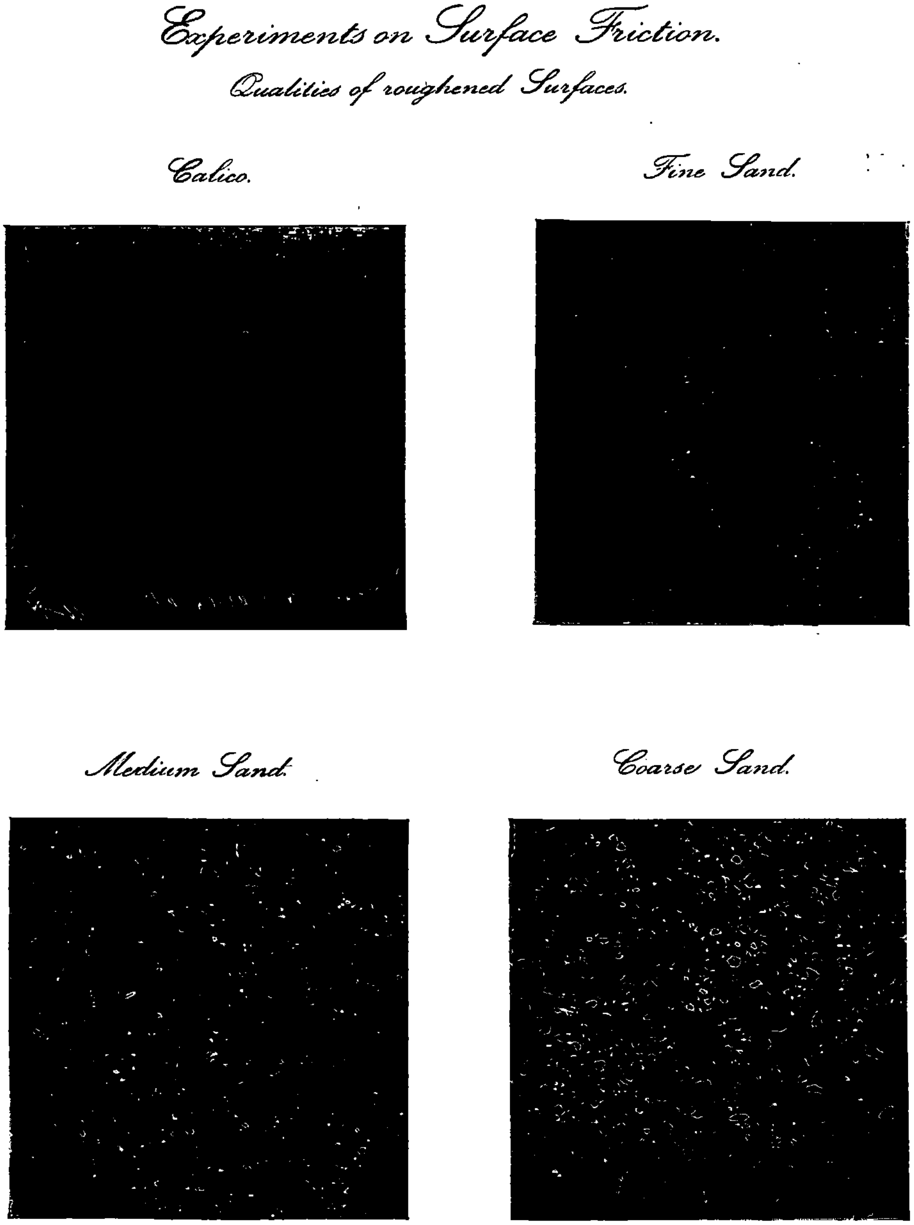

FIG. 1

The planes used in the experiments were, as before, about 19 inches wide; but the resistances shown for each length are those of the entire length of surface, assuming it to be of parallel width, and to expose to the frictional action one square foot of surface per foot of length.

It will be seen that the diagrams of each form are deducible from those of the other.

The results are shown in a more compendious but necessarily less complete form in the accompanying tabular statement.

This represents the resistances per square foot due to various lengths of surface, of various qualities, when moving with a standard speed of 600 feet per minute, accompanied by figures denoting the power of the speed to which the resistances, if calculated for other speeds, must be taken as approximately proportional.

Under the figure denoting the length of surface in each case, are three columns, A, B, C, which are referenced as follows:—

A. Power of speed to which resistance is approximately proportional.

B. Resistance in pounds per square foot of a surface the length of which is that specified in the heading—taken as the mean resistance for the whole length.

C. Resistance per square foot on unit of surface, at the distance sternward from the cutwater specified in the heading.

Looking at the subject in its practical aspect, the results exhibited in the diagrams and tabular statement may be regarded as *literal facts*, ascertained with great

Experiments on Surface Friction.

Total Resistances of surfaces of various lengths, reduced to one foot of area per foot run.

Series 1.

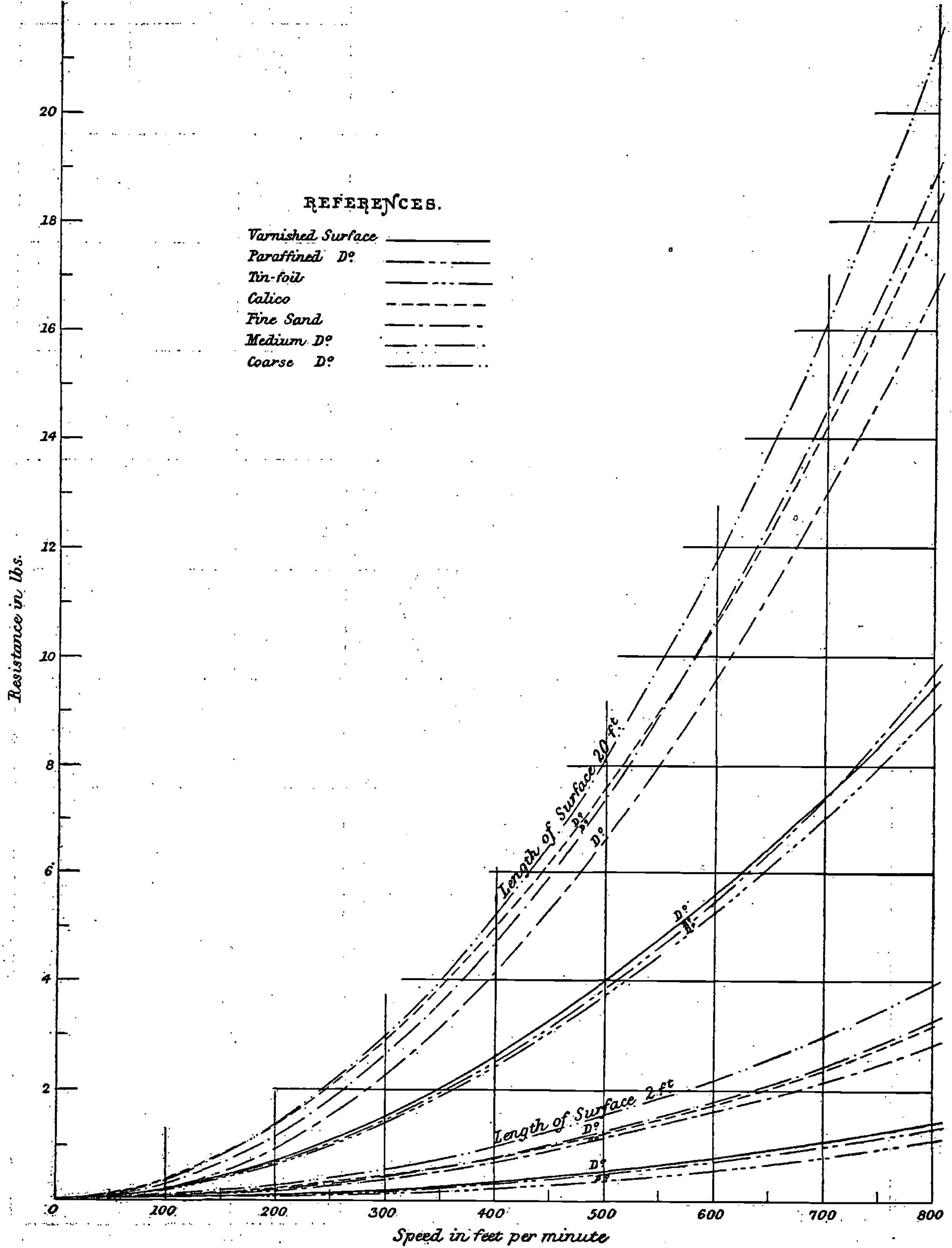

Fig. 2

Experiments on Surface Friction.

Total Resistances of surfaces of various lengths, reduced to one foot of area per foot run.

Series. 1.

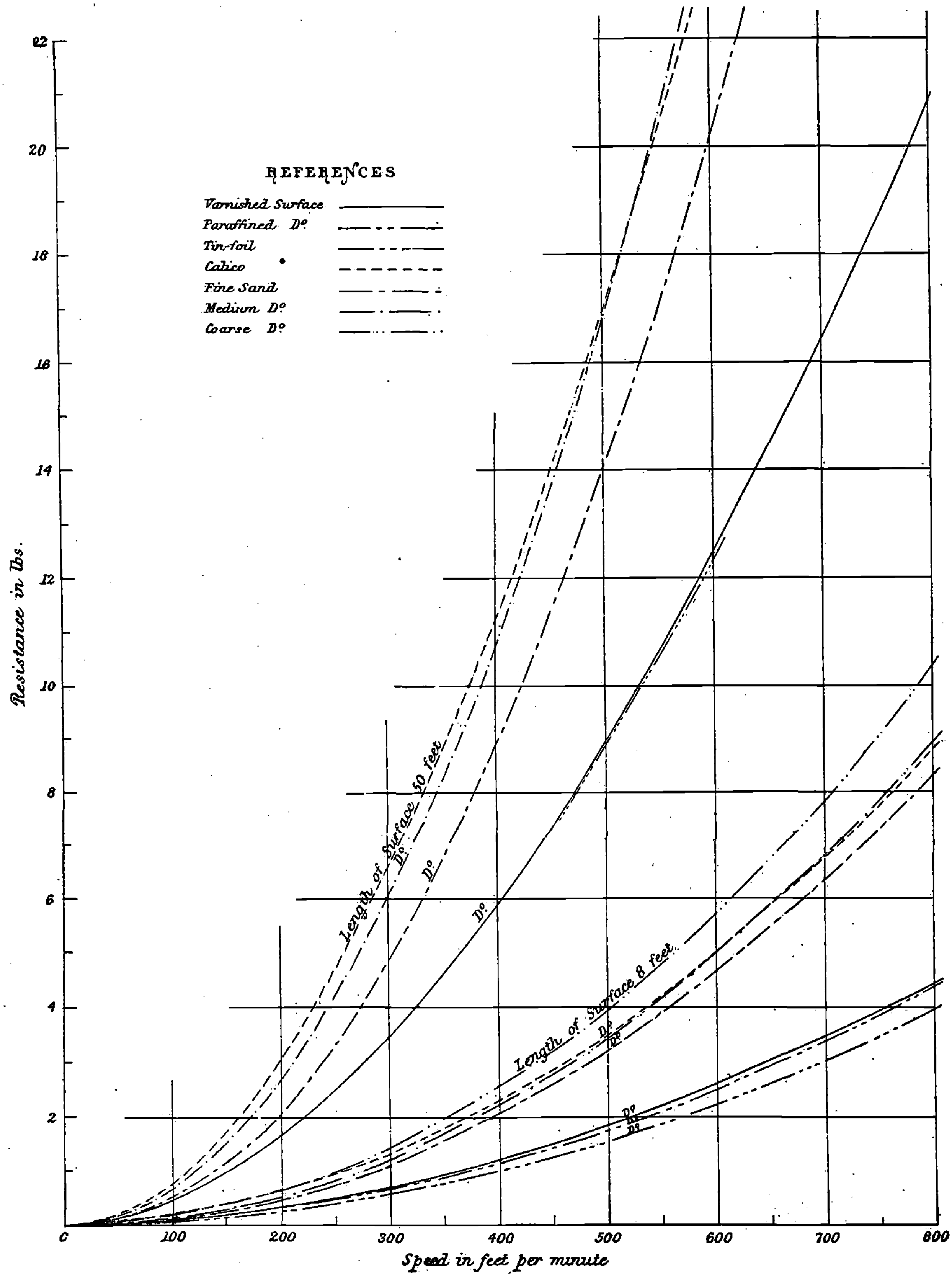

FIG. 3

Experiments on Surface Friction

Total Resistances of surfaces of various lengths, reduced to one foot of area per foot run

Series 2.

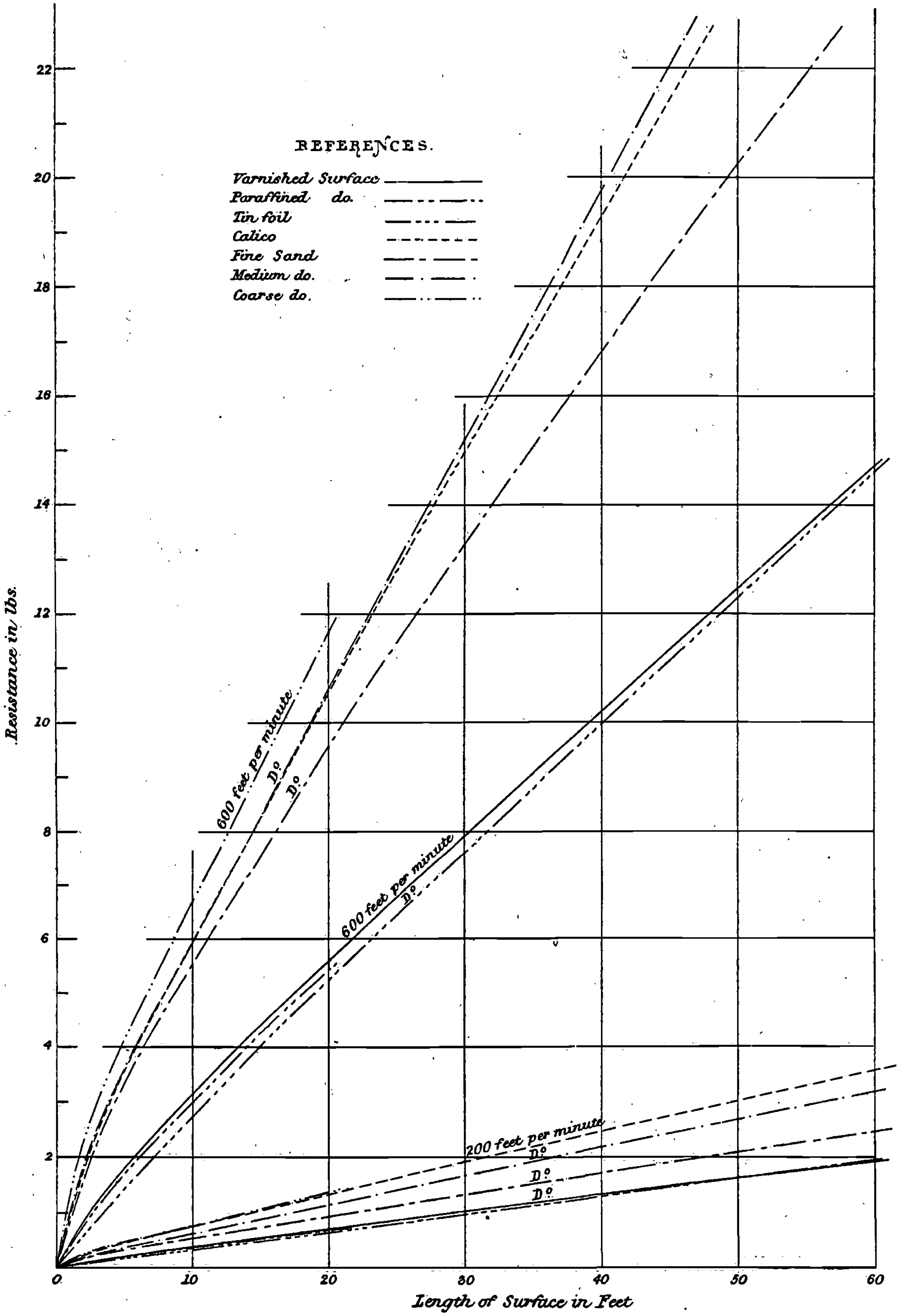

FIG. 4

Experiments on Surface Friction.

Total Resistances of surfaces of various lengths, reduced to one foot of area per foot run.

Series 2.

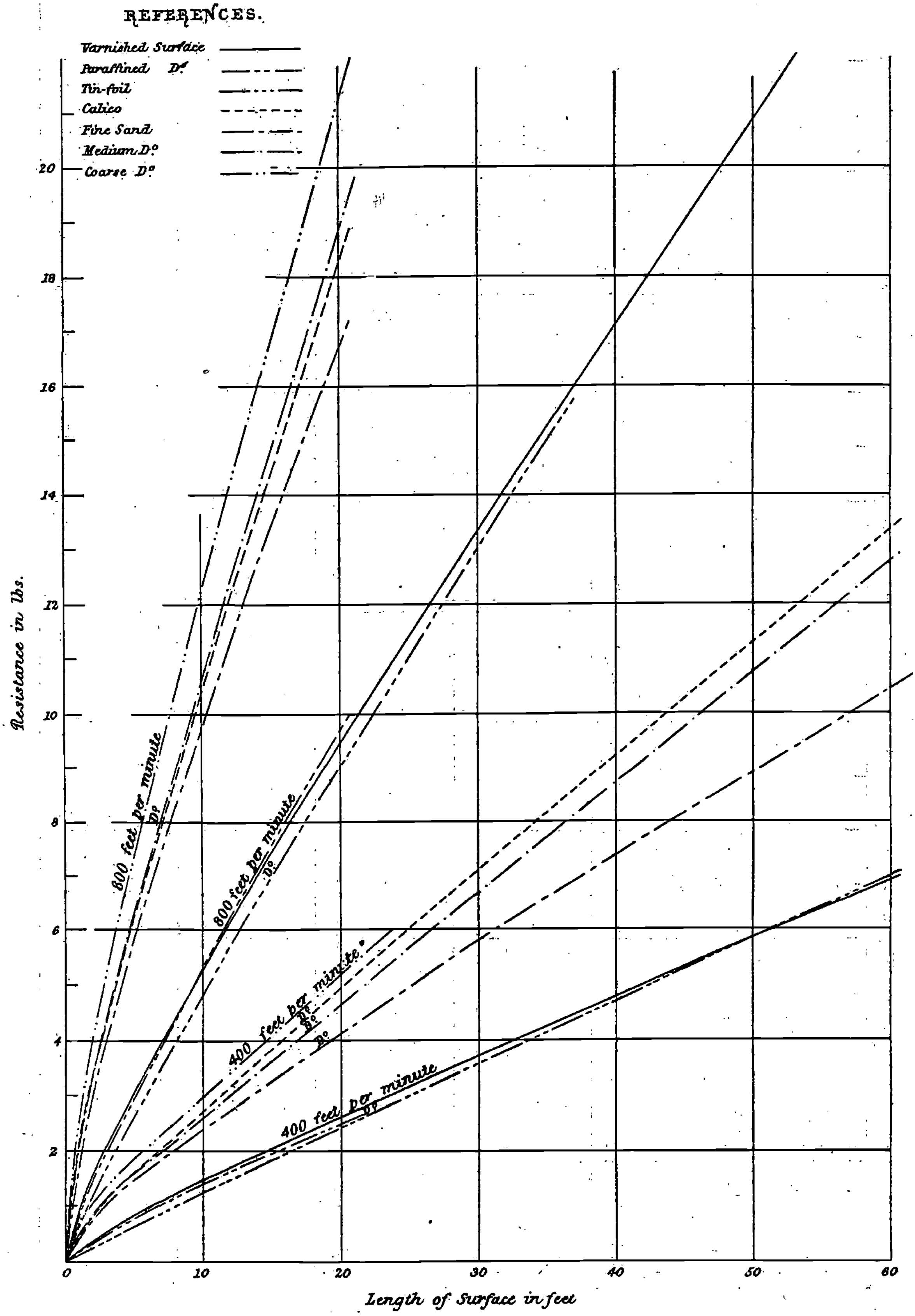

FIG. 5

	Length of surface, or distance from cutwater, in feet											
	2 feet			8 feet			20 feet			50 feet		
	A	B	C	A	B	C	A	B	C	A	B	C
Varnish	2·00	·41	·390	1·85	·325	·264	1·85	·278	·240	1·83	·250	·226
Paraffine	1·95	·38	·370	1·94	·314	·260	1·93	·271	·237	—	—	—
Tinfoil	2·16	·30	·295	1·99	·278	·263	1·90	·262	·244	1·83	·246	·232
Calico	1·93	·87	·725	1·92	·626	·504	1·89	·531	·447	1·87	·474	·423
Fine sand	2·00	·81	·690	2·00	·583	·450	2·00	·480	·384	2·06	·405	·337
Medium sand ..	2·00	·90	·730	2·00	·625	·488	2·00	·534	·465	2·00	·488	·456
Coarse sand ..	2·00	1·10	·880	2·00	·714	·520	2·00	·588	·490	—	—	—

care and exactness by reiterated experiments, the close mutual accordance of which was instanced and sufficiently attested by the diagrams in Fig. 4 in the series which accompanied the former Report, in which the points deduced immediately from the experiments are shown in connexion with the "fair lines" drawn through them; and no difficulty deserving of notice presents itself in reference to the practical employment of the results, except that, when the probable resistance of a more or less rough surface is to be estimated, discrimination must be exercised in selecting, among the qualities of surface used in the experiments, that which best serves the purpose of the intended comparison.

Looking at the subject in a speculative aspect, however, certain features of the results present perplexing anomalies.

It is true that the tabulated powers for each quality are, as may be seen, very nearly the same, whatever be the length of the surface, presenting only a slight tendency to a decrease in the "power" as the length is greater; and this difference is not unsuggestive. And again, if in each case, taking the resistance at 600 feet per minute as a basis, the resistances at other speeds be calculated from this according to the tabulated power, they will be found almost in every case to agree very closely, throughout the entire line, with those shown in the diagram; and this to a singular degree as regards what is treated as the surface of standard quality—namely, the varnished surface.

But the regularity here exhibited gives additional weight to the discrepancies which appear in other aspects of the effect of quality of surface, and some of these seem extremely anomalous; for whereas on comparing the surfaces of tinfoil and again that of scraped paraffine, both of them extremely smooth, with the slightly rougher and, consequently, more resisting varnished surface, we find that the *rougher* surface follows the *lower* power of the speed—the power being 2·0 for the tinfoil, 1·94 for the paraffine, and 1·85 for the varnish; we find, on the contrary, in the comparison between the comparatively smooth varnished surface and the far rougher and far more resisting surfaces of calico and sand, that the *rougher* surface follows the *higher* instead of the lower power of the speed, the power being 1·85 for the varnish, and 1·93 and 2·00 (in one case 2·06) for the calico and sand respectively.

The case of the tinfoil is very remarkable: with a very short plane its resistance is little more than half of that of the varnished surface; yet, possibly owing to the combined effect of the greater power of the speed to which the resistance is proportional, coupled with its less rapid declension in terms of length of surface, with a length of 50 feet the mean resistance of the tin-foiled surface is barely less than that of the varnished surface, and its resistance per square foot at the 50th foot is the greater of the two.

It is true that this apparent anomaly probably in part depends on the fact that the coating of the longer surfaces with the foil was not so easily effected as that of the shorter, and therefore perhaps their smoothness was less perfect and their resistance somewhat increased; yet, making every reasonable allowance for this, the anomaly is still remarkable.

Again, no rational explanation presents itself of the differences in the law of variation of resistance in terms of length, exhibited by the rougher and more highly resisting surfaces. The resistance, for instance, of the medium sand alters disproportionately little towards the end of the plane, nor do any of these resistances exhibit as marked an excess of decrease in that direction as might have been expected. Partly, no doubt, this is owing to the difficulty in securing uniformity of coating; but also, it must be admitted, that the law which really governs the decrease has yet to be discovered, though it can hardly be doubted that it depends somehow on the current created by the passage of the surfaces.

I shall conclude the Report with some remarks on what appears to me to be the rationale of the declension of resistance in terms of length of surface.

It is certain that any surface which, in passing through a fluid, experiences resistance, must, in doing so, impress on the particles which resist it a force in the line of motion equal to the resistance. Now, we cannot regard a fluid as anchored to the shore or bottom by lines of tension or of thrust which are snapped or crushed by the force which causes motion; but, on the contrary, we must assume the resistance offered by the particles of fluid to be purely dynamic, and to be dependent on and correlative to their weights and the velocities imparted to them.

This being so, it is quite certain that the operating force, which (whatever be its amount) must be precisely equal to the resistance when the speed is steady, will in each unit of time, say in each second, generate a given definite amount of new momentum, estimated in the line of motion, in the system of particles on which it operates. The force must, in fact, generate somewhere and somehow in the surrounding fluid the momentum which exactly corresponds dynamically to the universal law connecting force and momentum.

That law may be expressed as follows:—

If F be the force in pounds which operates in a given direction,
W the weight operated on in pounds,
V the velocity in feet per second,
t the time of action,
g the force of gravity $= \frac{32 \cdot 2 \text{ ft.}}{1''^2}$,

then $V = \frac{F g t}{W}$.

For the momentum, therefore, we have

$$W V = F g t; \quad . \quad . \quad . \quad . \quad (1)$$

and this is equally true, whether it be the result of a small force acting on a large mass, or *vice versâ*, or of a single force acting on a succession of masses.

The expression, therefore, quantifies the momentum which must be generated in each second in the surrounding fluid, by the transit of a surface the resisting force of which is F. In some shape or other, there must be left behind it, in each second, new momentum to that extent, existing either in the shape of a narrow and rapid current, or a broad and slow one, or one of graduated speed and corresponding volume.

This last supposition is clearly the most reasonable one, and it is approximately in visible accordance with fact; and, without speculating on the *modus operandi* by which the motion is communicated, it becomes easy by help of this supposition to put an approximate value on the breadth of the current produced under any given circumstances.

It will be seen presently that if the surface is long, the current thus estimated must be of considerable breadth; and if this be so, instead of finding it difficult to explain why the resistance per square foot grows less as the length is increased, the perplexing question is, how the rate of declension is so slow. For a little reflection obliges us to see that it is the motion of the surface relative to contiguous particles, and not relative to distant ones, that governs the resistance; and if these contiguous particles are already possessed of considerable velocity, concurrent with that of the surface, their resisting power must plainly be impaired.

When we proceed to trace the genesis of the momentum in detail, as it must exist in the completely generated current left behind by the surface, if we select at any point an element or strip of current parallel to the line of motion, and possessing the velocity v in feet per second in that line, we see that in that element the quantity of matter newly put in motion per second will, at that point, be a portion of the strip, $(V - v)$ feet in length (that being the length left behind by the surface), while the velocity impressed on it is v; and if all the dimensions be in feet, taking the depth of the current parallel to the surface as unity, and the thickness or breadth of the element as $d h$ (h being the distance from the plane of the surface), we shall have for the weight of the element, $d w = \acute{\omega} (V - v) d h$, $\acute{\omega}$ being the weight of a cubic foot.

Now if we assume that the current possesses a velocity $= V$ at the plane of the surface (that is to say, that the particles in contact with the surface have the same speed as the surface), and that where $h = H$, then also $v = 0$, the intermediate gradation of speed being uniform, we have

$$v = \frac{V (H - h)}{H};$$

hence

$$d w = \acute{\omega} V \frac{h}{H} d h;$$

and if M be the momentum,

$$d M = v d w = \frac{\acute{\omega} V^2}{H^2} (H - h) h d h;$$

$$\therefore M = \frac{\acute{\omega} V^2}{H^2} \left(\frac{H h^2}{2} - \frac{h^3}{3} \right);$$

and if $h = H$, we have, for the complete current,

$$M = \acute{\omega} V^2 \frac{H}{6}; \quad . \quad . \quad . \quad . \quad (2)$$

and this must equal F t, as given in equation (1); or, since $t = 1''$,

$$F g = \acute{\omega} V^2 \frac{H}{6};$$

or, since salt water weighs 64 lbs. per cubic foot, so that $\acute{\omega} = 64$, and $g = 32 \cdot 2$, we may write the equation with sufficient exactness

$$F = \frac{V^2 H}{3},$$

or, as the extreme breadth of the current, $H = \frac{3 F}{V^2}$.

If we apply this to the 50-ft. varnished surface, having a speed of 600 ft. per minute, or 10 ft. per second, which had the definite resistance of 12·5 lbs., we have

$$H = \cdot 375 \text{ ft., or about } 4\tfrac{1}{2} \text{ inches};$$

and this was not far from the truth, though, as it is not easy to obtain an exact measurement, the agreement must not be represented as more than approximate.

But if the surface had been 500 feet instead of only 50 feet in length, and if we could assume the same resistance per square foot to be retained throughout the length, the current would be 3·75 feet broad, and the velocity, to a sensible distance from the surface, would be not far short of that of the surface; and we should have to encounter the paradox that under these circumstances the surface when enveloped in a favouring current more than 3 ft. in breadth, and having, for a breadth of many inches, scarcely less speed than the surface itself, would be experiencing the same resistance as when entering undisturbed water.

If we suppose the law of distribution of velocity through the current to be different from that assumed in the above investigation, so as to allow particles having much less velocity to be near the surface, the breadth to be assigned to the current must be on the whole much greater, and the method by which the velocity could be thus distributed would be difficult to conceive.

However, we do in fact see that the current is greatly disturbed by eddies; and these, no doubt, furnish a machinery by which the distribution of velocity is modified—the modification being of such sort that relatively undisturbed particles are being perpetually *fed* inwards towards the surface from the outer margin of the current; and it is by this agency alone that the resistance throughout the length of surface is so little reduced as these experiments prove: though, on the other hand, it seems to me certain that *unlimited* elongation of surface must nevertheless be accompanied by an *all but unlimited* reduction of resistance. At least it appears impossible to conceive a system of eddies such as to bring undisturbed particles across a current of unlimited width into close proximity with the surface, and in such quick succession, as a sustained scale of resistance would imply.

Practically, however, although these experiments do not directly deal with surfaces of greater length than 50 feet, they afford data sufficient to enable us to predict with tolerable certainty the resistance of surfaces of such lengths as are commonly met with in ships. For it is at once seen that, at a length of 50 feet, the decrease (with increasing length) of the friction per square foot of every *additional* length is so small that it will make no very great difference in our estimate of the total resistance of a surface three hundred feet long, whether we assume such decrease to continue at the same rate throughout the last two hundred and fifty feet of the surface, or to cease entirely after fifty feet; while it is in effect certain that the truth must lie somewhere between these two assumptions.

1874

*Memorandum of Mr. Froude's Experiments in relation to the Pressure-Log, with a Description of the Apparatus employed. A Report to the British Association for the Advancement of Science, Belfast, August, 1874, by a Committee on Instruments for Measuring the Speed of Ships.**

EXPERIMENTS IN RELATION TO THE PRESSURE-LOG, WITH A DESCRIPTION OF THE APPARATUS EMPLOYED†

It seems best to begin by stating broadly the results which appear to have been established, reserving till afterwards the description of the apparatus and the details of the several experiments.

(1) If a plane be moving edgeways through the water, and the end of a pipe connected with a pressure-gauge be brought square through the plane and terminates flush with the surface (Fig. 1), the motion of the plane causes a small positive pressure within the pipe, amounting to about ·04 of the pressure due to the speed. If, however, the end of the pipe be not very exactly flush with the plane, this positive pressure is increased when the rearward edge is the projecting part (Fig. 2), and is diminished, or even becomes negative, when the position is reversed (Fig. 3). If the end of the pipe is flush with the plane, but has its internal edge slightly rounded off (Fig. 4), the positive pressure caused by motion of the plane very nearly disappears.

If the end of the pipe be closed by a disk forming a smooth flush end with a small aperture in it (Fig. 5), there is no appreciable positive pressure caused by the motion of the plane; nor is positive or negative pressure caused when this disk forms a slight angle with the line of motion, whether facing forward or facing sternward (Figs. 6 and 7), unless the angle is considerable (say some five degrees or so), a very much larger angle than produced considerable effect of this kind with the open-mouthed pipe.

The pipe with which these results were obtained was about ½ inch diameter, and the speeds used ranged from 280 to 600 feet per minute.

(2) In a cylindrical tube projecting into the fluid at right angles to the line of motion, with the end closed but with a hole in the side, the angle of position of the neutral point, referred (that is to say, measured circumferentially from the foremost side of the cylinder) to the point where the pressure is not affected by the motion, depends considerably upon the relative diameter of the tube and the hole in it. The greater the relative diameter of the hole, the greater is the angle of position of the neutral point. Thus the angle of position of the neutral

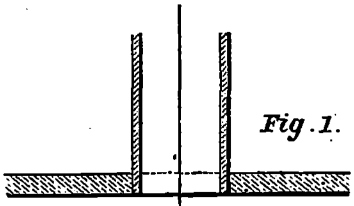

Fig. 1.

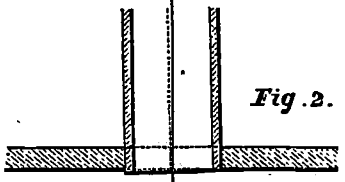

Fig. 2.

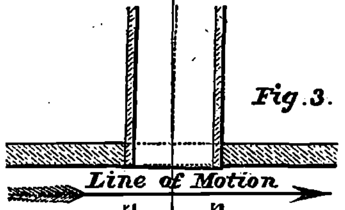

Fig. 3.

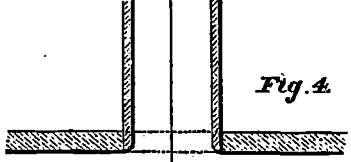

Fig. 4.

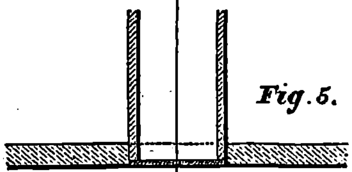

Fig. 5.

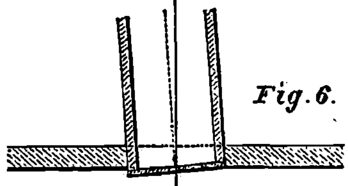

Fig. 6.

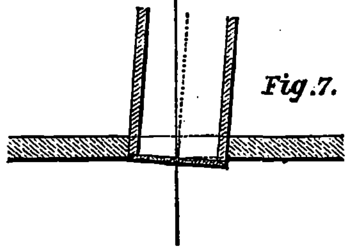

Fig. 7.

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* Editor's Note.—The members of the Committee were: W. Froude, F.R.S., F. J. Bramwell, F.R.S., A. E. Fletcher, Rev. E. L. Berthon, James R. Napier, F.R.S., C. W. Merrifield, F.R.S., Dr. C. W. Siemens, F.R.S., H. M. Brunel, W. Smith, Sir William Thomson, F.R.S., and J. N. Shoolbred.

† The experiments must be regarded as strictly elementary.

British Association Committee on Instruments for Measuring the Speed of Ships.

Drawing of Apparatus used in Mr. Froudes experiments with Pressure Log.

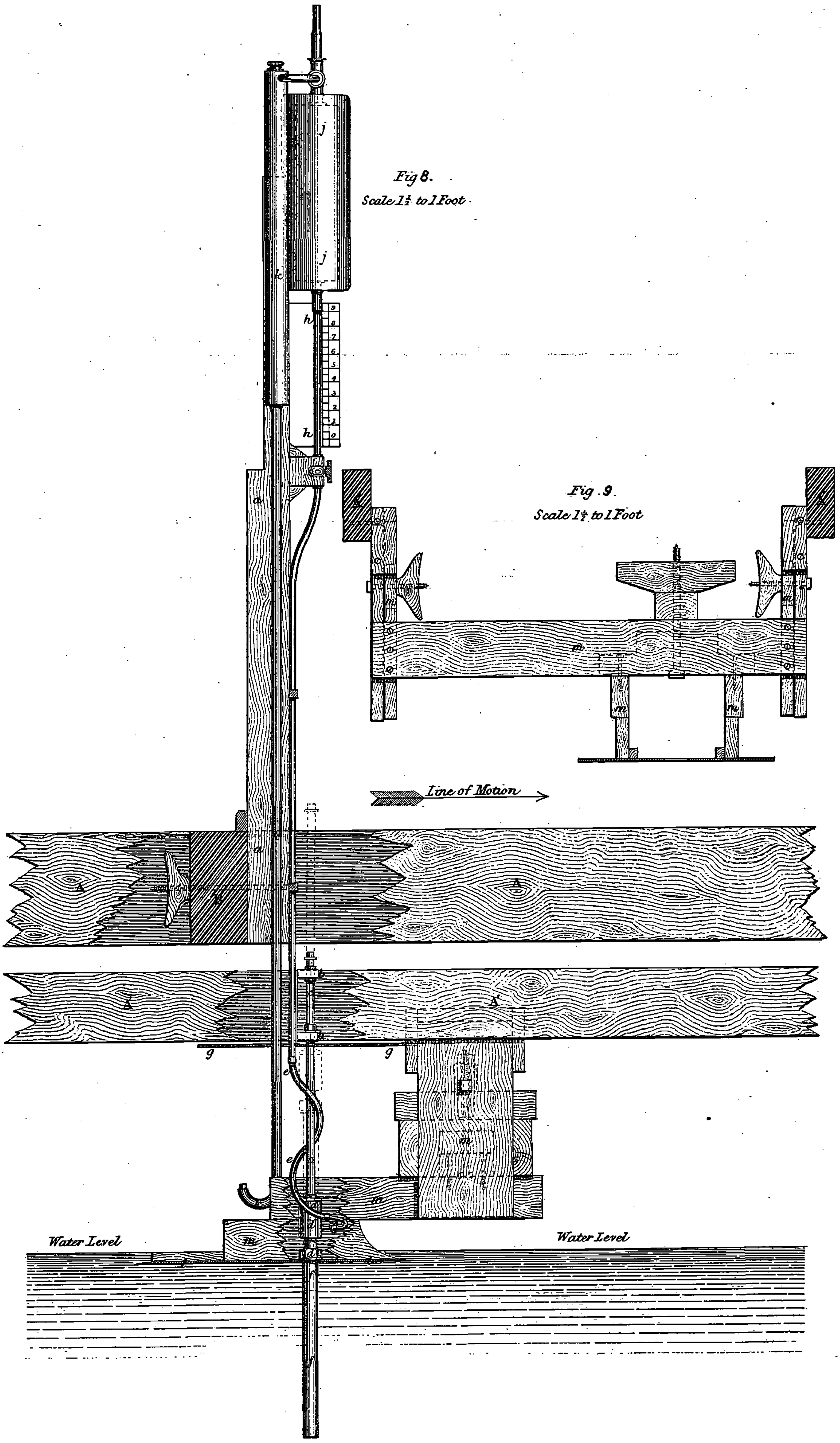
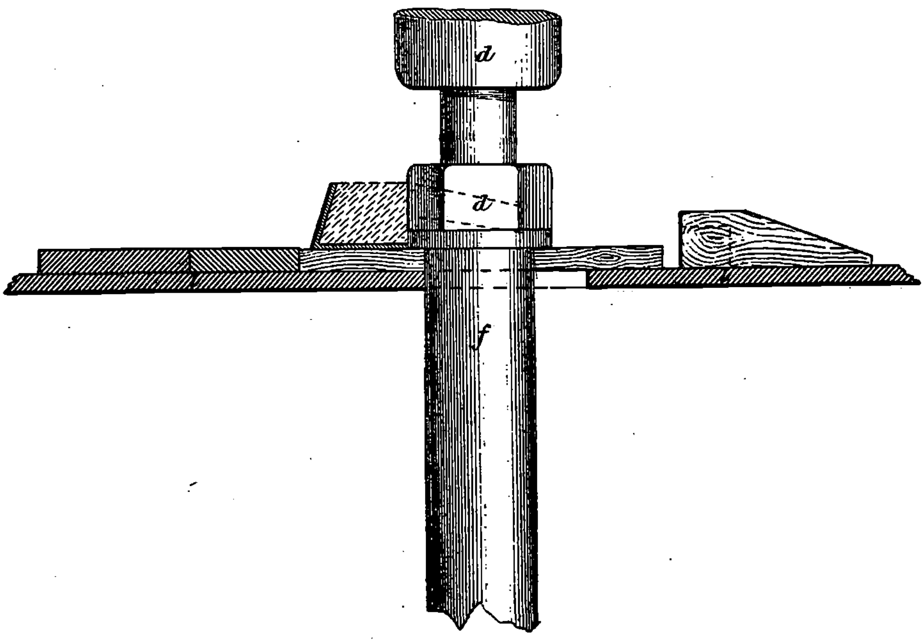


Fig 8.
Scale 1½ to 1 Foot.

Fig. 9.
Scale 1½ to 1 Foot

Fig 10.
Scale 6 to 1 Foot



British Association Committee on Instruments for Measuring the Speed of Ships.

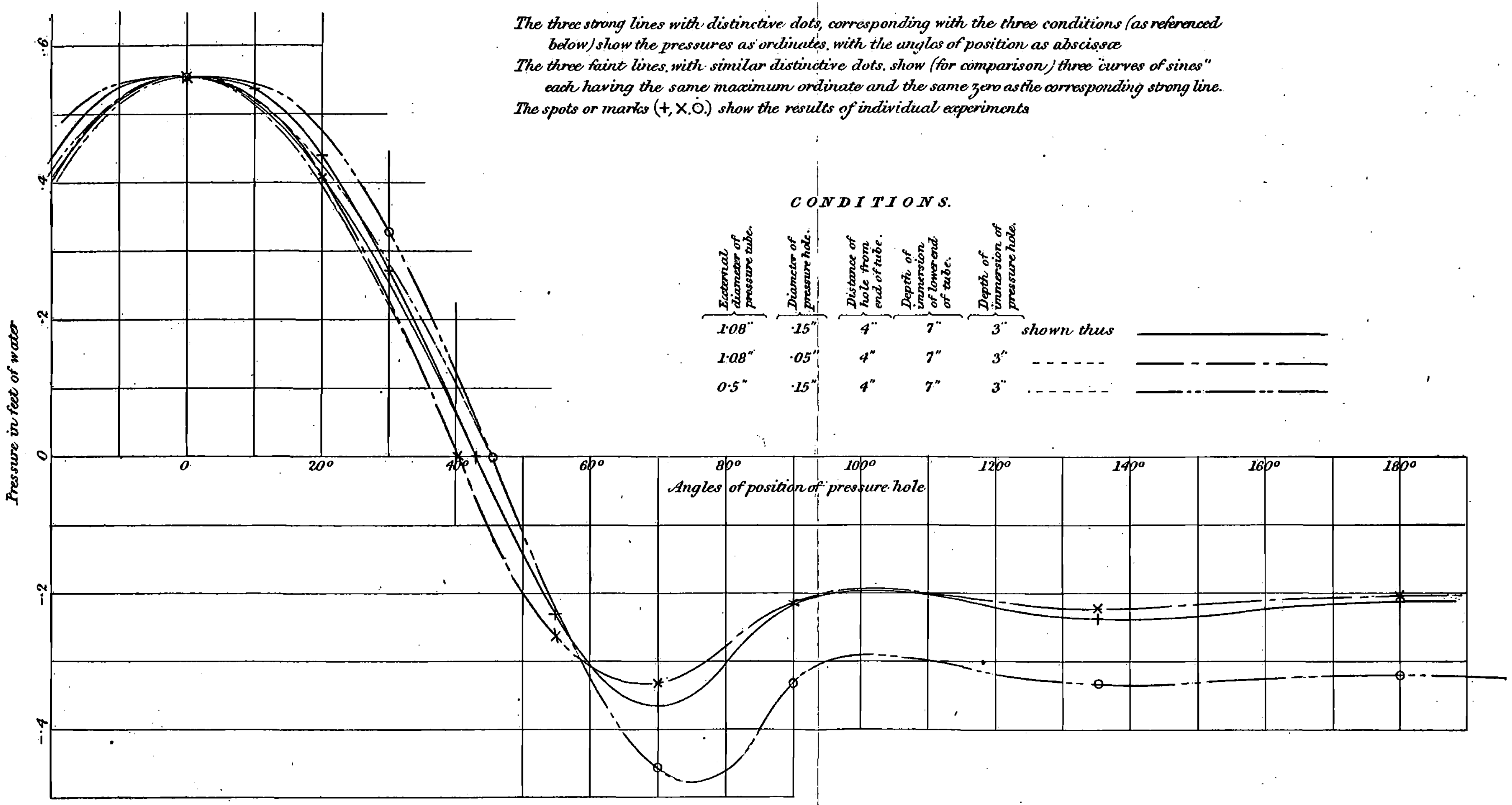
Mr. Froudes experiments with Pressure Log.

Graphic exposition, under three Conditions, of preliminary experiments to determine the pressure according to the "angle of position" of the pressure hole

The three strong lines with distinctive dots, corresponding with the three conditions (as referenced below) show the pressures as ordinates, with the angles of position as abscissæ

The three faint lines, with similar distinctive dots, show (for comparison) three "curves of sines" each having the same maximum ordinate and the same zero as the corresponding strong line.

The spots or marks (+, ×, ○) show the results of individual experiments



CONDITIONS.

External diameter of pressure tube.	Diameter of pressure hole.	Distance of hole from end of tube.	Depth of immersion of lower end of tube.	Depth of immersion of pressure hole.		
1·08"	·15"	4"	7"	3"	shown thus	————
1·08"	·05"	4"	7"	3"	- - - - - -	—— - —— - ——
0·5"	·15"	4"	7"	3"	. - - - - - -	——·-——·-——

FIG. 11

point in a tube 1·1 inch external diameter, having a $\frac{1}{20}$-inch diameter hole, seems about 40°·5; that of the same tube with a hole $\frac{3}{20}$ inch diameter is about 43°; and that of a tube $\frac{1}{2}$ inch diameter, with a hole $\frac{3}{20}$ inch diameter, is about 45°·5.

The position of the neutral points was also in these experiments sensibly affected by some unknown condition, dependent apparently upon the degree of projection of the tube into the fluid, and which I think may possibly have been of the nature of a vibration of the tube. Of this I will here merely say further that it prevented a precise determination of the degree to which the neutral angle is affected by close proximity of the hole to the end of the tube.

(3) The maximum positive pressure (which was obtained, of course, with the hole pointing directly in the line of motion) falls slightly short of that theoretically due to the speed, and is apparently unaffected either by the size of the tube or of the hole in it. It appears also to be unaffected by the above-mentioned unknown condition, being practically identical under all conditions, except when the hole approaches close to the end of the tube (within, say, a distance equal to the diameter of the tube), in which case the pressure is found to diminish.

(4) For some distance on either side of the neutral point the pressure decreases nearly uniformly, with uniform increments in the angular departure of the hole from the line of motion. The rate of decrease is about ·04 of the maximum positive pressure for every degree of angle. At angles of more than 50° the column was always unsteady, and it was impossible to obtain accurate measures of it; but the observations show consistently a maximum of negative pressure at somewhere about 70°, and then a decrease of between one third and one half of the maximum negative pressure between 70° and 90°. From 90° to 180° the negative pressure remains about uniform.*

The amounts of these negative pressures, besides being, as already mentioned, rather indefinite in consequence of the fluctuations of the column, are sensibly affected by the unknown condition already referred to, and therefore it is impossible to speak positively as to their absolute amount.

(5) A hole in the stopped end, instead of in the side, of the pressure-tube (the tube being set as in the experiment for side pressure) gives a considerable negative pressure, varying in amount according to the position of the hole in the disk which closes the end of the tube. In the case tried, the tube was 1·1 inch external diameter, the hole was $\frac{1}{20}$ diameter, and was eccentric in the disk by about half the radius of the tube. It was tried at a speed of 6 feet per second, corresponding with a direct pressure of ·56 foot; and the negative pressure recorded when the hole was nearest the forward edge was ·64 foot. When it was 180° from this position (*i.e.* nearest to the rearward edge) the negative pressure was ·29 foot; and this appeared to be the position of minimum negative pressure. The maximum negative pressure observed was ·67 foot, and was at 45° from the foremost position. At 90° it was ·64 foot, and at 135° was ·41 foot.

I proceed to describe the principal features of the apparatus, and the mode of trying the experiments.

The fundamental parts are as follows:—

(1) A covered tank or water-space, 278 feet long in all, about 228 feet of this being available for the run. The water is 36 feet wide at the surface and 10 feet deep.

(2) A railway suspended from the framed roof, dead straight and dead level, at a height of 19 inches above the water, the space between the rails being quite clear, and the rails being traversed by an endless wire rope.

(3) A small double-cylinder engine to drive the truck, fitted with a special governor, and capable of assigning to the truck a series of definite steady speeds (if required, indeed, *any* definite steady speed) between 100 feet per minute (about 1 knot) and 900 feet per minute (or about 9 knots).

The above-named elements are also the fundamental parts of the apparatus used in the experiments which I am carrying out for the Admiralty in the investigation of the resistances of ship-models of various forms at various speeds.

For the purpose of the present experiments, there was attached to the truck an additional apparatus, represented in Fig. 8.

It may be serviceable to observe at starting that, with a view to many (perhaps sufficiently obvious) points of convenience, the principle adopted in the arrangement of the pressure-gauge is one in virtue of which it might be termed a "sympiezometer"—the variations of pressure to be recorded being, however, not those of the atmosphere, but those of the pressure of the water on the open end of the instrument, that is to say, on the pressure-hole. It is true that were the pressure of the atmosphere to vary during any individual "run," that variation would enter into the result; but this is a condition which, because of its inevitably infinitesimal character, may be safely left out of the account.

The following references will assist in explaining the arrangement.

A A, A′ A′. Longitudinal timbers of the truck-frame.

B. Transverse timber of truck-frame.

a a. A stout standard, bolted to the main cross bar.

b b. A shallow headstock (as it may be called) like that of a lathe, securely screwed to the foot of *a a*.

c c. A vertical cylindrical steel arbor, which is capable of sliding vertically through a pair of collars which revolve (without endways-motion) in the bearings afforded by the headstock. The arbor can be clamped to the lower of these collars by a pinching-screw at any level which its length permits—that is to say, with a travel of 10 inches.

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* The diagram, Fig. 11, shows the pressure for all angles between 0° and 180° under three of the different conditions tried. The curves thus presented, between 0° and the neutral angle, somewhat resemble curves of sines. The degree of resemblance is indicated by the companion lines shown in fainter dots, and which are true curves of sines. It may be observed that the wider the neutral angle the greater is the departure from the companion curve.

d d. A sort of "chuck" or screwed hollow nozzle, to which the various pressure-pipes used in the experiments are fixed by a union collar, so as to be thus carried concentrically by the arbor. As the first step in filling the system with water, the air which this chuck contains is wholly exhausted by a mouthpipe which leads out of the highest part of the interior.

e e. An india-rubber pipe which conveys the water to the indicating part of the apparatus. This pipe is long enough to allow the arbor to be adjusted vertically (so as to vary the depth of immersion of the pressure-hole) and circumferentially (so as to allow the hole to be presented in any required direction relative to the line of motion). The pipe leads out of the lower part of the hollow or chamber in the nozzle, so that any bubbles of air which may enter the pressure-pipe become impounded in the upper part of the hollow, instead of rising in the pressure-pipe.

f f. The pressure-pipe. The pipe here shown is the largest of those used, and it is in the lowest possible position. The range of vertical adjustment is indicated by dotted lines.

g g. A disk 16 inches in diameter, divided to degrees, and, by a vernier, giving tenths of degrees, fixed to the lower of the two collars in which the arbor slides—the collar, namely, in which the arbor is clamped so as to define its level. The collar, with the divided disk attached to it, can be clamped in any required circumferential position, so as to secure the pressure-hole in the required position relatively to the line of motion.

h h. The glass index-tube, forming a connexion between the pressure-pipe and the vacuum-chamber, and provided with scale for reading the level at which the water stands.

j j. The vacuum-chamber. The required degree of exhaustion is produced in it by the descending leg of a siphon. It is connected at the top with the external air by a vertical india-rubber pipe, and with the siphon by a horizontal one, either of which can at pleasure be closed air-tight by a clamp.

k k. The siphon, consisting of a water-chamber and a descending pipe. The lower end of this pipe is turned upwards, and is closed by a cork while the siphon-chamber is being charged with water through an aperture with screwed stopper at the top. When the chamber is fully charged, the cork is removed and the water descends, raising the column on the other side above the top of the glass tube. The india-rubber connexion with the vacuum-chamber is then closed, and air is admitted to the latter through the india-rubber pipe at the top, until the water assumes a convenient zero-level. The vacuum-chamber is effectually "jacketed" with paraffine, so that changes of atmospheric temperature do not rapidly affect its interior.

l l. A plane surface or deck (of thin board, 14 × 19 inches) for restraining the surface of the water, so as to prevent the formation of waves and the consequent dissipation of pressure, and give additional stiffness to the pipe and the arbor which carries it. The deck is securely bracketed to a pair of transverse bars, carried by vertical slides which are attached to the side-frame of the truck, and which are firmly clamped when the deck is brought to the required level. The brackets which carry the deck can be adjusted on transverse bars, and are finally clamped to them (like the saddle of the rest on the bed of the lathe) when the deck has been duly adjusted to the pipe. The drawing shows the deck as fixed at its working immersion.

As the hole in the deck is necessarily large enough to admit the largest pipe, and as it is convenient that the fit should be easy while the adjustments are being made, each pipe is provided with a detached stout plate through which it slides with a close fit, and which by a suitable arrangement is firmly clamped to the deck and blocked by wedges on all sides so as to support the pipe effectually, and, moreover, prevent the admission of air behind the pipe, which at high speeds would affect the negative pressure in the rear. To exclude the air with still greater certainty, a "wall" of tin encloses the sides and rear of the tube above the plate (acting as a water-trap), so that the hole through which the pipe passes shall be always gorged with water when the apparatus is in motion. Thus the leakage, if any, which the suction in the rear of the pipe creates is satisfied by water instead of air.

m m m. The brackets, transverse bars, and vertical slides, forming an adjustable framework.

The details of these arrangements will be readily understood by inspecting the drawing, including Figs. 9 and 10.

In the tabulated statement of experimental results, the diameter of the tube used, the diameter of the pressure-hole, its level above the end of the tube, and the immersion of the end of the tube below the surface of the water are fully stated.

It is obvious that, under the arrangement described, the changes of pressure indicated by the rise and fall of the water in the glass tube include not only that due to the difference in the height of the column, but also that due to the small variation in the tension of the air within what has been called the "vacuum-chamber." This circumstance has to be taken account of in the interpretation of the observed results, and involves a calculation, which, however, is readily made, in terms of the ratio of the diameter of the glass tube to the capacity of the vacuum-chamber. Taking account of the dimensions of the parts, the correction is made by adding 15 per cent. to the observed change of column. This correction has been made throughout in framing the table, and the figures there given may be accepted as expressing the true pressures in terms of head of water at about the temperature of 60° Fahr.

The adaptation of what has been called the water-deck was found to be absolutely necessary after a few preliminary trials had been made without it. Indeed, as the

Table of Results of MR. FROUDE'S *Experiments with the Apparatus described in his Memorandum*

External diameter of pressure tube	Diameter of pressure hole	Distance of pressure hole from lower end of tube	Depth of immersion of lower end of tube	Depth of immersion of pressure hole	"Angle of position" of "neutral point"	Pressures recorded for various "angles of position" (that is, angular distance of hole from front side of tube) at a speed of 6 feet per second The several "angles of position" head the several columns									
						0°	10°	20°	30°	50°	55°	70°	90°	135°	180°
in.	in.	in.	in.	in.	°										
* 1·42	0·05	4	16	12	37·4	·549	·506	..	..	..	..	—·496†	—·345†	..	—·368
1·42	0·05	4	13	9	37·4	·548	..	..	..	..	..	—·502	—·368	..	—·379
1·42	0·15	4	16	12	39·3	·556	·528	..	..	..	..	—·552	—·379	..	—·379
1·42	0·05	0·5	12·5	12	35·4	·517	..	..	..	..	..	—·770	(‡)	—·787	—·528
1·42	0·05	0·5	12·5	12	35·1	·506	..	..	..	..	..	..	..	..	..
1·42	0·05	12	15	3	38·0	·549	..	..	..	..	..	—·476	—·368	..	—·370
1·42	0·05	12	21	9	36·4	·546	..	..	..	..	..	—·632	—·494	..	—·620
1·42	0·05	12	15	3	38·1	·551	..	..	..	..	..	—·488	..	..	..
1·42	0·05	12	18	6	37·3	·549	..	..	..	..	..	—·569	..	..	..
1·42	0·05	4	13	9	37·6	·545	..	..	..	..	..	—·505	—·345	..	—·368
1·42	0·05	4	19	15	36·9	·552	..	..	..	..	..	—·511	—·379	..	—·414
1·42	0·05	1	16	15	35·2	·538	..	..	..	..	..	—·804	—·747	..	—·575
1·08	0·05	12	15	3	37·3	·547	..	..	..	..	..	—·551	—·414	..	—·402
1·08	0·05	12	21	9	36·1	·542	..	..	..	..	..	—·666	—·534	..	—·575§
1·08	0·05	4	13	9	38·2	·551	..	..	..	—·269	..	—·494	—·356	..	—·379
1·08	0·15	4	13	9	40·6	·547	..	..	..	—·225	..	—·517	—·391	..	—·373
1·08	0·05	1	7	6	37·3	·545	..	..	..	—·292	..	—·572	—·414	..	—·402
1·08	0·05	0·5	7	6·5	37·3	·531	..	..	..	—·296	..	—·609	—·425	..	—·437
1·08	0·15	0·5	7	6·5	39·5	·531	..	..	..	—·255	..	..	..	..	..
1·08	0·15	4	7	3	42·9	·550	·535	·434	·271	..	—·230	—·368	—·218	—·241	—·218
1·08	0·05	4	7	3	40·5	·549	..	·402	..	..	—·264	—·333	—·218	—·224	—·207
0·5	0·15	4	7	3	45·5	·549	..	..	·326	..	—·225	—·460	—·333	—·333	—·322

N.B.—The pressures are throughout given in decimals of a foot, and give the true pressure, not that actually read off the instrument. The theoretical head or pressure due to 6 feet per second is ·556 feet.

* These results were obtained before the "water-deck" was fitted.

‡ Less than —·800, but could not be read off, being below the index-tube.

† Really taken at 67½° and 87½° respectively.

§ Really taken at 177½°.

depth to which the pressure-pipe could be immersed was of course limited, it had from the first been a question how far the pressures on the apertures would be affected by the proximity of the free surface of the water—since the natural stream-line forces, which would have existed in their completeness had the immersion been of unlimited depth, would inevitably tend to resolve themselves, to some extent, into some kind of wave-motion or surface-disturbance; and the first few preliminary trails led to the suspicion that this cause was producing effects of tangible magnitude, and to the belief that they might become very great at high speeds: a trial was therefore made at a speed of 900 feet per minute.

The effect of this speed was so remarkable as to deserve notice, if only as affording a striking exhibition of some of the forces inherent in stream-line action.

The end of the pipe was immersed 21 inches, the pipe being 1½ inch in diameter.

Immediately in front of the pipe, and embracing its anterior surface, the water rose in a thin sheet, which was shattered on the underside of the divided disk. In the immediate rear of the pipe the exact state of the water-surface could not be very clearly discerned, because the conoidal sheet of water which shot upwards from the sides of the pipe, and was broken up by the framing of the truck, fell in such a "heavy rain" as to obscure the view; probably, however, the water-surface was opened in a deep "gash" nearly to the full depth of the tube's immersion.

The most striking phenomenon was that which appeared at a small distance sternward "in the wake."

At about 3 feet astern of the tube the "gash" had become closed by the gradual meeting of the side streams which had bounded it: from this point to about 7 or 8 feet further sternwards there rose vertically a central wall of water, the crest of which, in its side elevation, had a parabolic form (as far as could be estimated by the eye), the highest part of the ridge being certainly over 2 feet above the natural water-level; its sectional form was tolerably discernible when it was looked at endways, and was not unlike that of an ordinary fountain issuing from a circular orifice; the thickness increased as the upward velocity lessened, till at the crest the water spread laterally in a kind of mushroom form, and fell in streams on either side. These streams in side view formed ragged sheets, through which the central wall of water could be seen at intervals.

The disarrangement of forces which at high speeds took so intensified a form would of course produce results of sensible magnitude at smaller speeds; but it seemed that a tolerably effective remedy would be supplied by the application of the water-deck which has been already described.

This was so arranged that the depth of its immersion could be varied within moderate limits. If too little immersed it would not sufficiently restrain the surface-disturbances, or might allow the intrusion of air. If too deeply immersed it might produce stream-line forces of its own, though its under surface was plane from end to end and truly horizontal. Eventually it was found to produce least disturbance when its underside was immersed about ⅞ of an inch, and at this level it was maintained during the subsequent experiments. The area of the deck was 19 inches in length and 14 inches in width.

One valuable purpose which the deck served was to give additional steadiness to the tube. Some collateral experiments showed distinctly that the pressure in a long tube of small diameter underwent most abnormal disturbances; and though it can hardly be said with confidence that tremor would account for these, it is the only condition which suggests itself as a possibly relevant "vera causa"; and even in the experiments which are reported, there are certain discordances which may possibly be attributable to the same cause, though the tubes used were stiff and were pretty rigidly held at the deck level: the discordances or unintelligible differences were felt, not in the maximum pressure delivered on an aperture exactly facing the line of motion, but in the pressure exhibited in the experiments relative to the position of the neutral point and to the negative pressures.

In performing each experiment the aperture was set in the required direction and the apparatus clamped. The zero of the pressure-scale was brought to a convenient level according as a negative or positive pressure was to be expected. The zero was recorded; and the mean height attained by the water in the tube was also recorded when the steady speed was attained.

Partly because time did not permit the extended variation of conditions which was desired, partly because, at higher speeds, increase of tremor (or of the unknown cause of irregularity whatever it may be) was to be apprehended, the speed adopted throughout the tabulated experiments was 360 feet per minute.

After these explanations, the details of the tabulated statement must be allowed to speak for themselves. It does not, however, contain the record of the experiments with the pipe-end flush with the underside of the deck, or of those made with the hole in the stopped end of the ordinary pressure-tube, because the particulars were not readily reducible to the form of the table. The results were therefore fully stated in the prefatory matter.

The series of experiments requires extension in many directions which are at once obvious: one of the most important of these is that which relates to the effect experienced by a pressure-tube when arranged as a log, from the stream-line disturbances which the passage of a ship's hull introduces into the relative speeds of the water surrounding the various parts of the hull.

It is hoped that this latter investigation, and perhaps all the others that are required, may be introduced as part of the series of experiments on the forms of ships which I am conducting here for the Admiralty, since the two subjects are inherently and closely related to each other. But the introduction of the experiments now reported has under present circumstances been, in effect, an interruption; and though the interruption was permitted, it has been carried to the full limits of the permission.

Incomplete as the experiments are, they tend, I fear, to confirm rather than to dissipate the difficulties which have to be overcome before the pressure-log can be accepted as supplying the greatly desired object, an independent and self-justifying measure of a ship's speed.

The inventors whose plans have been before the Committee have, I believe, felt the difficulties forcibly. Mr. Berthon* and Mr. Napier have indeed expressed their belief that it was unsurmounted, perhaps unsurmountable.

The foremost of the difficulties to be overcome is that of finding a self-justifying zero of the pressure-scale.

This, *primâ facie*, might have been supplied by either of three conditions:—

(1) The determination of the position of neutral pressure.
(2) The determination of the position of maximum negative pressure, and the ratio of this to the maximum positive pressure.
(3) The determination of the ratio of the negative pressure, in the region of tolerably uniform negative pressure in the rear of the tube, to the maximum positive pressure.

With regard to the former of these conditions, the present experiments show, I think, conclusively that the position of the neutral point is governed by conditions which it is difficult to count on with certainty; or if this difficulty be surmounted at all, it only can be by much laborious investigation: there remains the circumstance that the neutral point is placed exactly where the pressure is changing with maximum rapidity in terms of angle of position; so that any small error in taking account of the governing conditions will produce the greatest relative amount of error in the working zero from which the pressures are counted.

Thus the very elegant and instructive proposition as to the existence of this neutral point at a little over 40° from the line of motion, which Mr. Berthon discovered and determined with approximate exactness, and announced long before the promulgation of the doctrine of stream-lines had shown that such a point should exist nearly in that position, appears to involve special difficulty in its utilization as the basis of a pressure zero.

And difficulties hardly less serious in amount attach themselves to the determination of the two other conditions which have been referred to, though it is no doubt true that subsequent examination may determine with sufficient exactness the conditions which govern the relation of the negative pressure in the rear of the tube, to the positive pressure in front of it, in such a manner that the causes of uncertain variation may be excluded, and that the entire disturbance of pressure may be capable of definite interpretation.

If this can be accomplished so that in effect a working zero can be established, the only difficulty remaining to be encountered is the collateral one which arises from the motions impressed by the passage of the ship on the fluid which she displaces; this too, however, may prove not altogether intractable.

Apart from the unexpected variations in results the general character of which had been already known, the only new results which have been brought out by these experiments have been those which relate to the state of pressure at the end of the pressure-tube, whether (1) it project into the water in the usual manner, or (2) be cut off absolutely flush with the surface through which it issues.

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* Mr. Berthon has since informed me that I have rather overstated his opinion on this point.

The fact that in the former case the area of the pipe-end when stopped is covered (so to speak) with negative pressures which are of considerable amount, and which vary largely within a limited area, only serves to show that this part of the tube cannot be usefully applied to the purposes of the log.

But the fact that (contrary, I own, to my previous belief), in the latter case, the pressure seems to be almost absolutely neutral, whether the end of the tube be stopped with a perforated plane or be wholly open, suggests the hope that here also might be found a tolerably sound basis for a working zero of pressure. Doubtless the use of it would be exposed to one important objection—namely, that if a barnacle were to attach itself to the surface anywhere near the aperture, especially in front of it, the truthfulness of the zero would be destroyed; it is possible, too, that some causes of error might be found to exist in the "drag" of the eddies in the belt of water disturbed by the friction of the ship's surface. Nevertheless the idea that a trustworthy zero may be obtained on this basis, suggests itself as one deserving of consideration and inquiry.

Nothing in these experiments, however, tends at all to disparage the value of an instrument based on the principle which has been investigated, if the instrument be regarded as one the scale of which has to be interpreted by special experiment after it has been fitted to the ship in which its indications are to be made use of; and although in some respects its value would have been considerably greater if its scale could have been regarded as self-interpreting and self-justifying, yet, even under the practical limitation which has been referred to, the instrument, if well organized, must be regarded as possessing the highest practical usefulness.

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