Mr. PHIPPS said, since the Paper was written, he had, through the courtesy of Mr. Marshall, of the Royal West India Mail Company, been permitted to have access to the drawings of that Company's ship the 'Atrato,' as well as to the particulars of the power of the engines, and speed attained on the trial trip of that vessel. He had considered it important to obtain reliable data concerning the 'Atrato' for two reasons; first, because this vessel had been taken as an example in support of a method of calculating resistance;[1] and secondly, because she was a vessel of totally different type from the 'Leinster,' on the particulars of which the calculations in the Paper were chiefly based. The proportions of the vessel and other data given on a previous occasion were widely different from the results he had obtained from the drawings, and particularly so in respect to the amount of wetted surface, which exceeded the estimate referred to by more than 3,000 square feet. The data relating to the 'Atrato' on the trial trip were:—

Length	315	feet
Breadth	42	"
Draught of water	16·4	"
Indicated horse power	3,046	
Area of immersed midship section	539	square feet.
Area of wetted surface	15,912	"
Speed	13·77 knots per hour = 13·77	feet per second.
Slip of paddle	25	per cent.

Taking from the above indicated H.P. $\frac{1}{10}$th for friction, working air-pumps, &c., and 25 per cent. of the remainder for the slip of the paddles, there remained a net amount of $2{,}053\frac{1}{2}$ H.P. as applicable to overcoming the resistance of the vessel. By his method of calculation, the net power required was 2,226 H.P., or $172\frac{1}{2}$ H.P. in excess of the above; whilst by the method previously given in the Minutes of Proceedings, the excess became $752\frac{1}{2}$ H.P., or more than four times as much. The difference was mainly attributable to the very high coefficient for friction, which, it would be remembered, had been derived from experiments upon the flow of water through pipes. An elaborate course of experiments, upon the frictional and other resistances of floating bodies, had been conducted by Mr. Colthurst, and the result of one of those experiments was confirmatory of the views expressed in the Paper, as to the much higher value of the pressure encountered by any unit of surface situated upon the foremost portion of an inclined plane, of limited extent, in its passage through water, than on

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xvi., p. 357.

a similar unit on the after part of the plane; a view which he believed was also fully confirmed by the known fact of the inutility of increasing, beyond a very limited extent, the acting surface of a screw propeller. In Mr. Colthurst's experiment, two bodies were drawn through the water at the same speed of 4·96 feet per second. They were furnished with angular prows of the same inclination, with proportion of base to slant length as 1 to 3·3, but one was constructed of exactly twice the width of the other, and being immersed to the same depth, 4½ inches, the latter presented double the surface in contact with water. By the experiment it was proved, that the comparative resistances were as 1·89 to 2·51, or nearly as 3 to 4, instead of 3 to 6, which would have been the result had the resistance been equal on each unit of surface throughout the extent of the prow.

Mr. J. M. Heppel said, whatever might be the opinion of the Members with regard to the theoretical views advanced in the Paper, all must agree that a very valuable collection of data had been contributed to the Minutes of Proceedings. It appeared from the statements in the Paper, that the value of frictional resistance, as deduced from the flow of water through pipes, if applied to the case of vessels moving in water, would in some instances bring the amount of this resistance to more than sufficient to absorb the whole indicated H.P. of the vessel. That would lead to the belief that, in the case of a vessel going through water, there was something different in the frictional resistance to that which obtained in pipes through which water was flowing. There was nothing surprising in such a result, when it was considered that the conditions of a vessel moving through a large expanse of water were not exactly analogous to the case of water flowing through pipes, or as it was called, 'in train.' There was one condition of the motion of water in pipes which was wanting altogether in that of vessels; if, for instance, the section of a pipe 'in train' in any one place were taken, and another of the same pipe at some distance from the first section, there was no reason why the condition of the water at one section should differ from that at the other. In the case of a vessel moving through water this did not obtain. In a long vessel, putting aside the stern and bow, and taking only that part which was pretty nearly equal to the midship section, the conditions of the water passing the section near the bow were not necessarily the same with those of the water passing near the stern. It was conceivable that they might be entirely different. He believed that, whereas the velocity of the water passing through pipes 'in train' had been well ascertained, the actual velocity of the water in contact with the surface of a vessel had not yet been determined. The velocity of the vessel, in reference to a fixed point, was known; but there was no means of deducing the actual velocity of the water in contact with the surface of the vessel. He thought the Author had logically and correctly deduced, that the whole frictional resistance of the vessel could not exceed a certain amount ascertainable by means of the indicated H.P.

Mr. ARMSTRONG said, the Author had adopted the only scientific method of investigating the problem of the resistance of floating bodies, by taking, as a basis, a vessel with a square head. The only path for the solution of the problem was to establish a general proposition, similar to the principle of the celebrated D'Alembert, for resolving the hitherto difficult dynamic problem which was under discussion. The question briefly was, whether the mechanical effect of forces, or a pressure of steam, was a constant or a variable quantity? In working out this problem, it was to be regretted that experiments, as to the towing of variously formed vessels at various velocities had not been made by the British Admiralty, for experiments of that character would have afforded abundant proof, that the resistance of bodies in a fluid did not vary as the square of the velocity. But, even without those experiments, it might be affirmed, that the data already given by the Admiralty contained all the elements necessary to the mathematician for the inductive solution of the theory of fluid resistance.

In the following table were given the data connected with the trials of the 'Flying Fish,' 'Victor,' and 'Pioneer.' They were selected to exhibit the variable amounts of indicated H.P., and the constant quantities of pressure in the cylinders for an equal speed of vessel.

NAME OF VESSEL.	No. of Experiment.	$\frac{V^3 \times \text{Mid. Sect.}}{\text{I. H. P.}}$	Velocity in Knots.	I. H. P.	Midship Section.	Velocity in Feet per Second.	Average Pressure on Cylinders.	$\frac{\text{A Vel.}^2}{\text{Feet per Second.}}$	$\frac{\text{A Vel.}^2}{\text{Average Pressure.}}$	Velocity of Piston in Feet per Second.
'Flying Fish' (July 3, 1856).	4	469	9·92	576	277	16·76	68,791	77,837	1·12	4·61
" "	5	443	11·20	877	277	18·92	94,305	99,792	1·04	5·12
" "	6	411	11·47	1,016	277	19·26	103,512	102,767	·98	5·40
" "	3	376	11·73	1,166	272	19·77	114,188	106,352	·96	5·02
" "	7	372	11·60	1,160	277	19·60	114,400	106,368	·96	5·58
" "	2	341	11·29	1,154	274	19·09	106,576	100,010	·94	5·96
" "	1	335	11·58	1,302	281	19·43	116,466	106,218	·94	6·15
'Victor'.	1	351	11·58	1,165	264	19·43	93,886	99,792	1·06	6·83
'Pioneer'	1	336	11·36	1,192	273	19·09	95,988	99,645	1·02	6·83

The vessels were identical in dimensions, form, amount of H.P., and in the description of propeller; and, moreover, four of the trials of the 'Flying Fish' were made on the same day. The coefficients for those four trials—4th, 5th, 6th, 7th, were respectively 469, 443, 411, and 372, or a variation of 25 per cent. On comparing the fifth with the seventh trial, it was found that in the former case 877 I.H.P. was required to obtain a speed of 11·2 knots, and in the latter case 1,160 I.H.P. was required to obtain a speed of 11·6 knots, or an increase of power somewhat about 50 per cent. He would ask whether it was possible that any formula could meet such an anomaly? It must be conceded, that in these tabulated trials the cause of the variation in the coefficients existed in the indicated H.P. If the first and last column were examined, it would be found that as the coefficient diminished, the velocity of the piston increased. The same fact was exhibited six times by the 'Flying Fish,' in 1861, and in a perusal of the whole of the published data given by the Admiralty, for instance, the

	Coefficient.	Velocity of Piston.
'Rattler' .	. 676	3·60 feet per second.
'Desperate'	. 697	2·51 ,,
'Cruiser' .	. 727	3·42 ,,
'Simoom'	. 688	4·37 ,,
'Miranda'	. 680	3·56 ,,
'Algiers' .	. 687	2·91 ,,
'Russell' .	. 394	8·12 ,,
'Cornwallis'	. 342	8·58 ,,

Although it might appear to be proceeding tentatively with those experiments, he would state what were the axioms of the conservation of the mechanical effects of force which he had adopted. First, with respect to the steam-engine, he asserted that the mechanical effect was only proportional to the pressure in the cylinders—not to the "force and space passed over," or, correctly speaking, that the pressure in the cylinders was properly the power of the engine. Applied to the theory of fluid resistance, he asserted that the work done was proportional to the motive weights expressed by Colonel Beaufoy's formula, viz. Force = AV^2, consequently, the resistance could only be as the simple speed; and with respect to diminishing that quantity, he had the authority of Sir Isaac Newton's Principia for stating, that it was impossible of diminution by any form, however pointed. He fully expected to hear that this theory only applied to bodies deeply immersed. If it was to be applied to bodies floating on the surface, according to Sir Isaac Newton's rules, it would be necessary to add a small amount to Beaufoy's quantities. To apply this theory to actual practice, the area of the midship section should be multiplied by the velocity in feet per second squared, or, in other words, $F = AV^2$

was the theoretical amount of pressure in pounds required in the cylinder of the steam-engine. That quantity could not be diminished by any alteration in the velocity of the piston. In the tabulation there was a variation of 50 per cent. in the velocity of the piston, with scarcely any variation in the actual pressure from the theoretical amount. What little variation did exist was in the wrong direction for the present theory of the steam-engine. He would remind the members that these experiments only practically established the amount of pressure required. The theoretical question remained, what was the actual amount of work performed, or the mechanical equivalent of a unit pressure of steam? To arrive at that quantity, he would again refer to Beaufoy's experiments, the results deduced from which might be expressed as follows :—

Motive Weights.	Area.	Velocity.
1	1 square foot.	1 foot per second.
4	1 „	2 „
9	1 „	3 „
16	1 „	4 „

The primary results were not denied; but he could not admit that the resistance was equal to the motive weight at rest. If the resistance were equal, then there was a weight of matter producing a variable amount of mechanical effect. Such a theory was repugnant to all inductive reasoning. It had always been conceded that the resistance of bodies in a fluid was one power below the propelling power. Now, if the pressure in the cylinders was the power of the engine, and varied as the square of the velocity, the resistance must be as the speed. But until experiments were tried to confirm that principle, he must be content to produce the only dynamometer experiments with which he was acquainted—namely, those made on board the 'Rattler.' Those experiments, so far as they could be relied upon, proved that the thrust only varied as the speed.

				lbs.
April 1st, 1845,	10 knots			9,463
„ 3rd „	8¼ „	no wind		6,243
„ „ „	4 „	„		4,751
„ 8th „	8½ „	4 points on starboard bow		7,997

In the first of those experiments, the actual power exerted by the engines was 369 indicated H.P., and that of the shaft 290, as taken by the dynamometer for a speed of 10 knots. Therefore assuming, as stated by the Author, that 170 H.P. was due to the friction on the immersed surface of that vessel, 120 actual H.P. would only be left for propelling the 'Rattler' 10 knots per hour.

Mr. J. SCOTT RUSSELL said, having been engaged in similar investigations, he had found that an enormous mass of experiments had been made on the subject, almost without method, no two sets being under similar circumstances, and very few under the circumstances to which it was endeavoured to apply the results deduced from them. One-half, perhaps he might say two-thirds, of the experiments upon the resistance of water to bodies passing through it, which were said to have been undertaken for the purposes of naval architecture and for throwing light upon the construction of ships, had actually been performed under water, as if it had been the business of the naval architect to make fishes instead of ships. There was practically little analogy between a vessel floating on the surface of the water and dragged along it, and the same vessel sunk in and moving through the water.

The first fixed principle which the Author thought might be adopted for ships was to assume, as was done in many other branches of engineering, as the measure of the head resistance to a square-ended body dragged through the water, the pressure of the head of water, or the pressure of a column of water raised to the height due to gravity by the velocity of the vessel. He approved of that measure; and his formula for direct head-pressure per square foot was almost similar to that of the Author. For convenience of computation he took 64 lbs. as the weight of a cubic foot of salt water. By adopting this degree of saturation he obtained the simple formula: one pound per square foot, per foot per second as a unit which could be easily remembered, and carrying through the calculation, he found that the resistance of any ship, or of any body dragged through the water was 1 lb. per square foot, per foot per second, in salt water of 64 lbs. to the cubic foot, increasing as the square of the velocity at all speeds.

If it were required from this formula to know what the resistance was, at 20 feet per second, it could be obtained thus—20 feet squared was $400 = 400$ lbs. In the same manner taking 9 feet per second as the velocity, $9 \times 9 = 81$, was the resistance in lbs. He was therefore in accord with the Author, that the measure of head resistance differed only by a minute fraction, and that that difference depended upon the density, or degree of saturation of the water.

As regarded the theory of the resistance of inclined planes, generally known as the theory of the sine squared, and which simply meant, when referred to ships, that with a constant width of vessel made with inclined plane bows, the resistance was as the square of the sine of the angle made by the inclined plane and centre line of the vessel, its truth depended upon the geometrical property involved in the above expression, namely, that the quantity of water removed from the vessel's track in any given time was the same, whatever the angle of the bow, so long as the width and draught of water remained constant, and that the velocity with which such amount of water was displaced corresponded with the sine of the angle of inclination, and hence the resistance should be as the square of that velocity.

On comparing the theory with the results of experiments made chiefly by Beaufoy, it was found to be true only in the case of the inclination being at an angle of about 45°, while at 6°, 12°, and 15°, the resistance was nearly four times that given by the theory, so that the law of the square of the sine was less and less applicable as the angle of the bow became sharper.

He could confirm these conclusions of the Author by his own examination of the experiments of the French Academicians, which clearly proved that, excepting at about an angle of 45°, the actual resistance always exceeded that of the theory, and all the more so in proportion to the acuteness of the angle.

SUMMARY of EXPERIMENTS on Angular Wedge-Bows by the Academy of Sciences.

Half-angle Bow.	Direct Resistance.	Column of Unity.	Sines of Angles.	Squares of Sines.	Half-angle of Escape.	Speed of Escape.	Resistance of Escape.	Numbers resulting from Formula by C = 75.
90°	$57^{k}\cdot04$	1·000	1·000	1·000	90°	$\frac{15}{15}$	$\frac{225}{225}$ 1·00	
84	$55^{k}\cdot83$	0·979	0·995	0·989	96	$\frac{15}{16}$	$\frac{225}{256}$ 0·87	
78	$54^{k}\cdot58$	0·957	0·978	0·957	102	$\frac{15}{17}$	$\frac{225}{289}$ 0·77	
72	$51^{k}\cdot55$	0·904	0·951	0·904	108	$\frac{15}{18}$	$\frac{225}{324}$ 0·68	52·1
66	$47^{k}\cdot77$	0·837	0·914	0·835	114	$\frac{15}{19}$	$\frac{225}{361}$ 0·62	46·7
60	$53^{k}\cdot99$	0·771	0·866	0·75	120	$\frac{15}{20}$	$\frac{225}{400}$ 0·56	42·2
54	$38^{k}\cdot55$	0·676	0·809	0·654	126	$\frac{15}{21}$	$\frac{225}{441}$ 0·51	38·3
48	$35^{k}\cdot78$	0·627	0·743	0·552	132	$\frac{15}{22}$	$\frac{225}{484}$ 0·46	34·6
42	$31^{k}\cdot53$	0·553	0·669	0·448	138	$\frac{15}{23}$	$\frac{225}{529}$ 0·42	31·9
36	$29^{k}\cdot70$	0·521	0·588	0·345	144	$\frac{15}{24}$	$\frac{225}{576}$ 0·39	29·3
30	$26^{k}\cdot67$	0·467	0·5	0·25	150	$\frac{15}{25}$	$\frac{225}{625}$ 0·36	27·0
24	$23^{k}\cdot77$	0·417	0·407	0·165	156	$\frac{15}{26}$	$\frac{225}{676}$ 0·33	25·0
18	$22^{k}\cdot10$	0·387	0·309	0·095	162	$\frac{15}{27}$	$\frac{225}{729}$ 0·30	23·1
12	$20^{k}\cdot29$	0·356	0·208	0·043	168	$\frac{15}{28}$	$\frac{225}{784}$ 0·28	21·5
6	$19^{k}.39$	0·340	0·105	0·011	174	$\frac{15}{29}$	$\frac{225}{841}$ 0·26	20·1

With reference to the amount of the resistance of square-ended vessels, he felt assured, both from the experiments of others, and from his own experiments made on various units of square-ended surfaces by means of the instrument known as the "tube of Pitot," up to a speed of 12 miles per hour, that his former statements were correct.

The diminution of resistance due to sharp inclinations of bow arose from the much larger space over which the water displaced by the ship at each instant of time could diffuse itself when the angle was sharp.

If a circle were described from the foremost point of the bow, the resistance of any angle of bow, from a square end, would be as the square of the number of degrees out of the whole 360° left unappropriated by the vessel's bow, and this he termed the angle of escape. This would give for a square end $360° - 180° = 180^2$, and for an angle of 10°, $360° - 10° = 350^2$, these two cases giving resistances as 180^2 to 350^2. The figures arrived at by the above method corresponded in ratio, through a considerable range of angles, with the experiments of the French Academy, although not exactly corresponding with the actual calculated amounts referred to the square end as unity. He agreed in the analogy of the screw propeller and of inclined planes at very acute angles drawn rapidly through the water, and that the enormous pressure developed against the surface of the screw would be equally felt by the bow of a vessel passing at the same angle and speed. He also agreed with the Author that the probability of the pressure only existing for a limited distance along any inclined plane was confirmed by the fact that as much pressure was realized against a screw propeller, as now generally made, where each blade was the $\frac{1}{6}$th part of a convolution only, as where, which was the case in some of the original screws, each blade occupied $\frac{1}{2}$ a convolution. He had no doubt that the power of the rudder in turning a vessel (which was always in excess of the ordinary calculation) was mainly due to the same cause as that adverted to in connection with the screw and much inclined bows. He also thought that the efficiency of the rudder was added to, by the head of water raised above the ordinary level by the resistance of the rudder, and that this head acting laterally against the considerable area of the dead wood of the stern tended very much to increase the effect in turning the vessel; an effect sometimes impaired, however, in screw vessels by the necessary cutting away of a considerable portion of the dead wood, in order to admit the screw propeller.

The properties of water-lines formed with curves of contrary curvature between the cutwater and the midship section—in other words, wave-lines—were very superior to plane surfaces in the facility with which they acted upon and removed the fluid lying in the track of the vessel; the best point of contrary curvature was at one half the distance of the cutwater from the midship section of the vessel. These curved water-lines, when properly formed, acted on the fluid in the same manner as the accelerating force of gravity, the time allowed for acting upon any particular particle of water so as to give it its maximum lateral velocity being the same as that occupied by the passage of the vessel through the distance from the cutwater to the point of contrary curvature. This form of bow therefore was superior to those formed with plane surfaces, the angle of entrance of which must necessarily be greater.

TABLE showing the Measures of Value of the Hollow Bow or Approximate Wave-line Entrance in diminished Head-Resistance.

Speed in Knots.	Length in Feet.	Beam in Feet.	Bow in Feet.	Values in Diminished Head-Resistance.
10	100	20	60	$(\frac{20}{60})^2 = \frac{1}{9}$
12	144	24	86	$(\frac{24}{86})^2 = \frac{1}{13}$
14	196	28	117	$(\frac{28}{117})^2 = \frac{1}{18}$
16	256	32	153	$(\frac{32}{153})^2 = \frac{1}{23}$
18	324	36	194	$(\frac{36}{194})^2 = \frac{1}{29}$
20	400	40	240	$(\frac{40}{240})^2 = \frac{1}{36}$

With regard to the minus resistance at the stern of a vessel, he could not agree with the Author as to the method adopted for calculating its amount, which method was, in fact, the same as taking the sine of the angle squared as the measure of the resistance. The form and length of a vessel's stern were regulated more by the nature of the wave rising up towards the stern than by the fineness and length of the run, and he had found, by experience, that no advantage accrued from elongating the run one third. He was disposed to think that with the special kind of wave generated by a vessel's bow constructed on the wave principle, the resistance at the stern would be less than that stated in the Paper. The chief question sought to be elucidated in the Paper was that of friction, and there was much pertinence in the remark that the ratio of length to breadth in a ship must not be increased to such an extent as to increase the surface resistance more rapidly than the head and stern resistances were diminished through the influence of form. This limit was clearly dependent upon the amount of friction to which a square foot of wetted surface was exposed, when of the degree of roughness usually met with in practice. According to Beaufoy, whose most valuable experiments were those directed to the friction of surfaces, and were made upon smooth painted planks, 287 feet of frictional surface equalled 1 square foot of direct head resistance. The Author's deductions from the hull of the 'Rattler,' which was copper-bottomed, gave 307 feet, whilst from the iron hull of the 'Leinster,' when on her trial trip, he had deduced 211 feet as the proper measure. Mr. Russell's calculation gave 285 feet for an iron bottom, and he had found that the direct head resistance of a vessel could be reduced from unity for a square end down to $\frac{1}{25}$ by using the wave form of bow, but if

VALUES OF SHAPE IN DIMINISHING RESISTANCE.

French Academy. Wedge Forms.	Beaufoy.*	Colthurst.†	The Wave Form.	Various.	Coefficients.
∠ 90°	90°	90°	.. .. ..	90°	1·00
		Cylindric ends . . .	.. .. ..	.. .. ..	0·99
72	.. .. ..	.. .. ..	.. .. ..	.. .. ..	0·90
63	.. .. ..	.. .. ..	.. .. ..	.. .. ..	0·80
		∠ 45° with convex arcs.	.. .. ..	.. .. ..	0·76
55	.. .. ..	.. .. ..	.. .. ..	Cube; edge in front . .	0·70
		∠ 35° convex arcs . .	.. .. ..	.. .. ..	0·64
44	.. .. ..	.. .. ..	.. .. ..	.. .. ..	0·57
35	.. .. ..	.. .. ..	.. .. ..	.. .. ..	0·51
18	.. .. ..	.. .. ..	.. .. ..	.. .. ..	0·38
	Cylindric bow. . . .	.. .. ..	.. .. ..	.. .. ..	0·37
	Sphere.	.. .. ..	.. .. ..	.. .. ..	0·33
	Cylindric ends . . .	∠ 25° convex arcs . .	.. .. ..	.. .. ..	0·31
6	∠ 30°.	.. .. ..	.. .. ..	.. .. ..	0·30
	∠ 20°.	.. .. ..	.. .. ..	.. .. ..	0·25
	∠ 15°.	.. .. ..	.. .. ..	.. .. ..	0·23
	Spherical ends . . .	.. .. ..	.. .. ..	.. .. ..	0·23
	Sharp cylindric arcs . .	.. .. ..	.. .. ..	.. .. ..	0·23
				Poncelet's (ship-form) .	0·22
	∠ 10°.	∠ 15° convex arcs . .	.. .. ..	Morin's (ship-form) . .	0·20
				Bourgois (whale-boat) .	0·20‡
				Navier's (ship-form) . .	0·18
	Sharpest cylindric arcs .	∠ 10° convex arcs . .	.. .. ..	.. .. ..	0·15
			Wave 5 to 1 form . .	.. .. ..	0·11
				Bourgois (whale-boat) .	0·11‡
			„ 6 to 1 „ . . .	.. .. ..	0·077
			„ 7 to 1 „ . . .	.. .. ..	0·056
			„ 8 to 1 „ . . .	.. .. ..	0·043
			„ 9 to 1 „ . . .	.. .. ..	0·034
			„ 10 to 1 „ . . .	.. .. ..	0·028

* These experiments were made under water, not on the surface.
† Both ends are alike throughout this column.—*Vide ante*, p. 339.
‡ These two results were obtained from the same finely-shaped boat; the less coefficient at a speed less than the speed due to the boat's length, the greater coefficient when that speed was exceeded.

from the effects of dirt, or roughness of the iron plates, the resistance of a vessel were increased in the high proportion indicated by the Author, the advantages of the vessel's fine entrance might very easily be more than counterbalanced by the additional friction thereby introduced.

Mr. ATHERTON said he wished to draw attention to the generally received law, by which present practice was regulated in determining the amount of power by which steamers of any given size might be expected to be propelled at any given speed. He referred to the general rule of arithmetical calculation which was enunciated by the formula $\frac{V^3 D^{\frac{2}{3}}}{P} = C$, C being the coefficient of dynamic performance already determined from the test trials of vessels of the same type of form and surface condition as the new vessel which might be under consideration. The coefficient (C) it would be observed, was deduced from the actual performance of a ship of the same type of form as the new ship to which it was to be applied. For example, supposing that a vessel of 1,000 tons displacement, of which the elementary dimensions of construction were known, were found on trial to be propelled at the speed of 10 knots per hour by 400 indicated H.P., then (by the rule above enunciated) $\frac{10^3 \times 1000^{\frac{2}{3}}}{400} = 250$; and this number would be assigned as the coefficient applicable to any vessel of which the immersed hull might be of the same type, and its condition as to the surface in contact with the water in all respects similar. Having thus experimentally ascertained the value of the coefficient for that particular type of form, the formula $\frac{V^3 D^{\frac{2}{3}}}{HP} = 250$, became applicable to the case in question, and the value of either one of the elements V, D, or P, could be ascertained when the other two were given. Having thus enunciated the rule to which he alluded, and shown its practical application, he would endeavour to explain the theoretical principles upon which it was based. Taking the immersed hull of a ship of any given type of form, and of which the two sides were similar and equal, it was in the first place assumed, that whatever might be the form of the vessel, the direct rectilinear resistance to motion was proportional to the number of molecular units, or particles of water to be removed by the vessel in a unit of time, and the collective force with which they individually acted. Next it was assumed, that the number of such particles to be removed by the vessel in a given time was proportional to the maximum sectional area (A) of the vessel, taken transversely to its line of motion, multiplied by the speed (v) at which the vessel moved, which might be denoted by Av. Also, that the force with which they individually resisted the motion of the vessel was proportional to the speed (v), and hence, the collective resistance (R) was proportional to $Av \times v$, or Av^2. And as it was an axiom in mechanics, that the power (P) required to overcome any resistance was proportional to the velocity with which it was to be overcome, or moved, it followed that the power (P) required to impel the vessel of which the midship section was A, and of which the resistance was (R), would be proportional to $A\, v^2 \times v$, or $A\, v^3$. Hence it was assumed that, for vessels of different sizes, but of similar types of form, P was proportional to $A\, v^3$, which was mathematically expressed by $P \propto A\, v^3$. But homologous sections of similar bodies were mathematically proportional to the square of the homologous rectilinear dimensions, and the relative cubical capacities (D) of similar bodies (such as the water displacement of similar ships) were proportional to the cubes of the homologous rectilinear dimensions, consequently $A \propto D^{\frac{2}{3}}$; hence, in the above formula $D^{\frac{2}{3}}$ might be substituted for A, and the formula became $P \propto V^3\, D^{\frac{2}{3}}$.

Up to this point he had made no direct mention of the surface (S) of the immersed hull in contact with the water. It was however to be observed that the particles of water acted against this immersed sloping surface of the hull, not in addition to, but in lieu of acting against the midship or maximum section; the effect of such action was modified by the type of form; but, as the surfaces of similar bodies were proportional to the homologous sectional areas, the formula $P \propto V^3\, D^{\frac{2}{3}}$ still held good; consequently $\frac{V^3\, D^{\frac{2}{3}}}{P}$ became a constant number (C) for vessels of the same type, whereof the actual value of C had to be experimentally determined, with reference to any particular type of form to which it might be applied; for, though C might be expected to be a constant number for all vessels of the same type of form, the rule was, that it would vary in different types, and that even in the same type it would be greatly dependent on the condition of the surface of the hull in contact with the water.

It was however to be observed that, as the forms of vessels both forward and aft admitted of infinite changes of type, a good form of forward body might be combined with a bad form of after-body, and vice versâ; and it might thus happen, that vessels of totally dissimilar types of form might possibly produce the same coefficient.

In the construction of the above formula, no notice had, however, been taken of the position of the centre of gravity of the maximum sectional area A, or of the immersed hull of the ship: and that was a point on which, in his opinion, the received formula was open to further investigation. Admitting therefore that in consequence of the formula taking no cognizance of the centre of gravity of the immersed midship section, or of the water displacement of the ship, it appeared desirable to inquire, how far the application of the formula practically answered the purpose of determining, approximately, the probable capabilities of a new ship of given type, with reference to the mutual relations of displacement, power, and speed. So far as his experience had extended, the practical application of the rule has been satisfactory, and he believed that by its careful application, under the essential conditions on which it was based, namely, similarity of type and surface condition, the capabilities of any new ship, with reference to the mutual relations of its displacement, power, and speed, might be predetermined with remarkable precision.

The performances of the 'Great Eastern,' though never yet fully tested as regarded the capabilities of the ship for the long passage service for which the vessel was specially designed, had on various favourable occasions confirmed the mutual relations of displacement, power, and speed, which were predicated as her probable performances; thereby establishing the general trustworthiness of the formula referred to, as applicable for determining the mutual relations of displacement, power, and speed, which might be expected to be realized by ships up to 25,000 tons of displacement. Although the 'Great Eastern' had thus far beneficially served the cause of theoretical science, it was to be regretted that circumstances had prevented her full capabilities, for making long voyages, as dependent on her surpassing magnitude, from being practically demonstrated.

While on this point he would make a few remarks, on the superior capabilities of large ships as compared with smaller vessels. He assumed the vessels to be all of the same type of form, giving a coefficient $C = 215{\cdot}5$; then, assuming the formula to be applicable, the following results would ensue. First, with reference to speed: the same proportion of power to displacement which drove a ship of 5,000 tons displacement at the speed of 12 knots per hour would drive a ship of 10,000 tons at 13 knots, and a ship of 20,000 tons at 14 knots. Secondly, with reference to power, the speed being given. To attain the speed of 15 knots per hour, a ship of 5,000 tons displacement, of the type under consideration, required 91 indicated horse-power for each 100 tons of displacement; a ship of 10,000 tons displacement required 72 indicated horse-power for each 100 tons of displacement; and a ship of 20,000 tons displacement required only 57 indicated horse-power for each 100 tons of displacement.

It was, however, to be observed, that these advantages of increased size became rapidly neutralized, if on the strength of size alone, obligations were undertaken which involved an increased rate of speed combined with an increased distance, without recoaling. These matters, however, being duly attended to, it appeared that nautical hazards, port accommodation, and mercantile considerations determined the limits by which the sizes of ships, with reference to their profitable employment, must necessarily be regulated. In a mechanical point of view, irrespectively of nautical, local, and mercantile considerations, the advantages of magnitude appeared to be unquestionable.

Mr. HAWKSLEY, V.P., said, he was perfectly sure that there was no difference whatever between the amount of resistance which water experienced when flowing within a pipe and when flowing without a pipe, and he was certain that the resistance on the external skin of a ship was precisely the same as that in the interior of a pipe when the two surfaces were in the same condition. In illustration, he would suppose, for the moment, that the radius of the pipe was gradually increased till it became infinite. When it became infinite, any small element of the surface was nothing more than a piece of plain plate, or a part of the side of a ship; and when in that condition, it was manifest to any one who had studied the subject under that form, that the friction of the water on the one side of that piece of pipe must be exactly the same as the friction of the water on the other side. But as to the measure of the friction itself, that was another question, and whatever might have been the views of the experimentors, Dubuat, Eytelwein, and Dr. Young, one of the most able of the English philosophers, with regard to the coefficient of the friction being constant and independent of the nature of the surface, and although at one time, believing entirely in their conclusions, he was quite able to admit that some of his own previous ideas on the subject, founded as they were in great measure upon their conclusions, had been erroneous. It had been found, in recent years, that the condition of the surface had very considerably to do with the coefficient of friction. It was perfectly well known, since asphalte had come into use for the prevention of corrosion on the internal surface of pipes, that the friction on an asphalte surface was considerably less than it was on a surface of raw iron. It was also well known, that when the interior surface of an iron pipe was covered with so-called carbuncles, which collected after long use, and especially under the action of soft water, the friction was considerably increased. And while entering upon this subject, he should be glad to be allowed to explain the most modern conclusions with regard to what had been called friction: it was known that if a particle of fluid was flowing down the axis of a pipe, it experienced no resistance whatever, except that which was occasioned by its proceeding faster than the adjoining particles. But by-and-by it became gradually diverted out of its course, and came in contact with the sides, against which it struck at an angle to its path, and thus lost a part of its preimpressed force: that was not friction at all. It entered into the consideration under the given name of friction, but it was simply force lost by impact, and so followed the ratio of the square of the velocity. But in addition to that, another resistance could also be measured, which was true friction, and that followed the ratio of the first power of the velocity. This latter resistance was very perceptible when the motion was not rapid, but became insensible, and was in a manner merged into the larger measure of resistance, when the motion was considerable.

Putting F for the statical head, which would be in equilibrium with the sum of these two resistances, the formula now generally concurred in by all theoretical writers and practical experimenters became

$$F = a v^2 + b v,$$

a and b being coefficients, to be determined by actual experiment on different materials and different qualities of surfaces.

From this equation, it might be observed, that when the velocity became considerable, the second term practically disappeared; while, on the contrary, when the velocity was fractionally small, the first term practically disappeared. That was a very important consideration, for it seemed to explain some of the many anomalies in the ordinarily accepted formulæ, derived from experiments made upon the resistance of water and gases at widely-varying velocities.

Hence, with respect to a vessel proceeding with a velocity of 10, 20, or 30 feet per second, the value of the first term became very large; whilst the value of the second term was perfectly insignificant. Therefore that term might be omitted, and the friction be considered simply as the square of the velocity, or $F = av^2$. The main question thus became reduced to this, "What was the value of the experimental coefficient (a)?" Upon that point, in which he differed in part from and in part agreed with the Author, the experiments of Colonel Beaufoy had been quoted. Now Mr. Hawksley was sorry to say that he, in common with many other persons, who had had a good deal to do with hydraulic subjects, did not entertain a very high opinion of the results obtained by Colonel Beaufoy. Those experiments were no doubt honestly and intelligently made; but Colonel Beaufoy did not conduct his experiments on friction so as to eliminate from his results the many sources of error by which they were affected. Colonel Beaufoy mixed up together a number of large resistances, of which friction formed only a small portion;

and, in consequence, it was impossible to establish an equation for friction only, by means of which any practical result could be deduced from his experiments. In point of fact, in all his great book there were only one or two pages which were of the smallest possible value for the purpose, and in these, the stated results were not only conflicting, but were derived from surfaces not at all analogous to that of an iron sea-going vessel. He was sorry to express himself in these terms with regard to experiments, the importance of which he thought had been estimated by the magnificence of the book, and the great liberality of its laborious author. He had the highest personal respect for Colonel Beaufoy. Every one who knew anything of the cost and difficulty of experimenting on hydraulics must have respect for him; but having respect for him, and having respect for his results, were two very different things. The Author had particularly quoted the portion of those experiments relating to the resistance to a piece of wood of a particular form, well painted, or varnished, made very smooth, and almost polished. Admitting the accuracy of Colonel Beaufoy's experiment, Mr. Hawksley disputed its applicability to the purposes for which the Author had appropriated it. The condition of the outside of a vessel in actual use, and not upon a trial trip, when she came out very nicely finished from the hands of the builder and painter, was very different from that under which Colonel Beaufoy's experiments were made. Colonel Beaufoy, he believed, under the circumstances mentioned, got a resistance so small that it amounted to less than one-half that which a ship actually sustained when working a voyage under ordinary circumstances. Now, so long ago as the year 1856, Mr. Hawksley was at a good deal of pains on this subject; because it so happened at that time the Institution was taking a strong interest in the subject which had now been again raised; and he found, from a number of observations, some of which he obtained from other parties, and some of which he made himself, in the course of voyages he had occasion to make across the Irish Channel, that the skin resistance of an iron-built ship, moving at the rate of 15 feet per second, was 25 ounces per square foot of surface of plate, and he compared that result with some large experiments which he had had an opportunity of making in the case of a new cast-iron, but somewhat rusty, pipe laid in Liverpool, which was 44 inches diameter and 22 miles long, and he found that within an ounce per square foot the friction in both cases was the same. He compared it also with a similar new cast-iron pipe 3 miles in length and 13 inches in diameter, which supplied the town of Whitehaven with water, and with the Versailles 18-inch (French) pipe recorded by M. Couplet, and he found in all these cases a very close agreement with the resistances observed in the case of the ships. He would give the data on which these conclusions were founded. In arriving at these facts he had taken a number of voyages in those vessels, and had ascertained the power that was exerted, the speed at which the ship ran, and the area of surface resistance which the vessel presented to the water. He had stated on previous occasions, and was still of the opinion, that the midship resistance was of no important value in the estimation of speed, when the vessel was of sharp build and going at a high velocity. There were considerable differences of friction, and smooth surfaces, so long as they could be kept smooth, would not offer half the resistance which a rough surface would present; and therefore between the worst case and the best, that of a very rough skinned ship, and of a very smooth skinned ship, there were many intermediate grades of resistance. The resistance of a ship loaded with barnacles or weeds would be much greater than 25 ounces to the foot, when moving at the rate of 15 feet per second, and therefore all those observations which were made with regard to what might be the coefficient in one case, and what might be the coefficient in another, were really of no value unless all the circumstances were distinctly given. With regard to iron ships, their surfaces were never in that smooth state for three months together.

There seemed to be some misapprehension with regard to what he had stated on a previous occasion.[1] He had then only devised a formula which represented, as he supposed, the different elements of resistance of steam ships, and a surplus of 25 per cent. for losses, and then he had said, after going into a discussion on the subject:—"Suppose it were wished to predetermine the speed of a vessel of nearly the magnitude and power of the 'Atrato,' and laid down on the following data, &c." These data were not the data of the 'Atrato,' although they were tolerably approximate. Nevertheless, they were indeed the data, as far as was known at the time, of a well-built, fast ship, of about the same tonnage as the 'Atrato.' He had said, according to that formula, the speed of the supposed ship would come out at $15\frac{2}{3}$ miles an hour. The 'Atrato' happened to be mentioned in the Paper which was read, and Mr. Hawksley had then said, "the 'Atrato' was stated by the Author of the Paper to have attained in fresh water a velocity of 16 statute miles per hour on the measured mile." He had only given that as a fact to show how closely, as he conceived, the results given by the formula approximated to the actual result stated with regard to the 'Atrato.' That result was obtained on the supposition that a ship of the supposed size, sailing in open sea, would, under ordinary circumstances, experience a resistance of

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xvi., page 357.

25 ounces per square foot; but if it had even been considerably less, the difference of speed would have been comparatively insignificant; because, supposing the estimate of the friction was reduced from 25 ounces to 16 ounces, then, with the same power, the speed would be increased only in the proportion of the cube root of 25 to the cube root of 16. He particularly wished to draw attention to the fact, that after a ship had been some time at sea, especially in a warm latitude, the condition of the surface was never the same as when on her trial trips, or the condition on which, he apprehended, the Author had based his assumption. Therefore Mr. Hawksley intended to support as a fact, till it was disproved by actual experiment, that an iron vessel had usually a surface resistance of about 25 ounces per square foot of wetted surface, when moving at the speed mentioned. He was, indeed, perfectly satisfied, that it would not be safe for the constructor of a sea-going vessel to calculate upon a lower resistance. The resistance of the midship section was of little importance in estimating the speed of fast vessels; but, of course, it had more effect on power than on speed. He saw no reason to alter the opinions he had expressed on a former occasion, when after giving the more elaborate formula, which included the midship resistance, he had made the following remark[1] :—

"It is worthy of observation, that, as respects sharp vessels, the speed may be approximately determined, without any direct reference to the midship section, by the very simple formula $V = 27\left(\frac{H}{s}\right)^{\frac{1}{3}}$; and as ($s$) frequently approaches the rectangle of the breadth (b) and the length (l), this becomes (approximately) $V = 27\left(\frac{H}{b\,l}\right)^{\frac{1}{3}}$, and therefore roughly $H = \frac{V^3\,b\,l}{20{,}000}$."

in which s = area in square feet of the wetted surface.
V = velocity in statute miles per hour.
H = indicated H.P.
b = breadth of vessel in feet.
l = length of vessel in feet.

That very simple formula $V = 27\left(\frac{H}{b\,l}\right)^{\frac{1}{3}}$ would be sufficient to express very approximately the velocity of any sharp-built iron steamship. He did not mean to say when the vessel was quite smooth and clean and well painted up, or if it were copper bottomed; but in the case of an ordinary iron steam-ship, in the ordinary good

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xvi., p. 358.

condition in which such vessels as the 'Persia' were usually kept, it was certainly within 10 per cent.; and, as he believed, generally within 5 per cent. of the actual speed attained in regular work.

To prevent any misapprehension in regard to the coefficient of 25 ounces to the foot, as applicable to an ordinarily corroded surface of iron, he wished to point out, that if the surface of a vessel were coated with paint and made smooth, then so long as the paint lasted there would be less surface friction; and with copper, also, as long as it lasted in a clean state there would be a still less surface friction. His previous remarks and assumptions had reference to a roughened and oxidized surface of iron, in the state in which it usually existed on the bottom of a vessel.

Mr. BIDDER agreed, to a considerable extent, with the conclusions arrived at by the Author, but in other respects, his views were more in accordance with the objections advanced against certain portions of the Paper. Before entering upon the discussion of the points at issue, he would protest against the employment of the term 'line of least resistance.' Such lines must necessarily vary with the class and purpose of the vessel, whether intended for the carriage of cargo, for making long voyages, or for short and rapid passages.

With regard to the question of surface resistance, it had been urged that the estimated value of 25 ounces at a speed of 15 feet per second was not the resistance due to a vessel in perfect condition, but it was admitted that it was the resistance to be practically calculated upon for a vessel on her passage; therefore he considered that the whole question raised by the Author on this point was conceded. In the course of the discussion it had been asserted, that the resistance due to midship displacement was so small, that it might be omitted from the calculation; and on the other hand it had been maintained, that this displacement exceeded the sine of the square; upon the latter hypothesis had been founded a theory which was so distinct from all the notions of ordinary hydraulics that he was compelled to dissent from it: he meant the theory with regard to the 'wave line.' He considered that, whether produced quickly or slowly, the resistance was due to the fact of there being a greater head of water at the fore part than at the remaining part of the bow. This was due simply to the circumstance, that however fine the cutwater of a vessel might be made, some interval of time must elapse before it accommodated itself to the ordinary condition of the surface of the water. What was called the wave-line had been introduced under the idea of communicating velocity to the water in a gradual manner. As regarded a sea way, the theory might be correct, but it was not so for smooth water, for as the velocity, inside the straight line and parallel to it, was then due to the extreme angle and not to the average angle, the result would be in favour of the straight line; at sea there were other considerations which might be urged in favour of the wave-line. It would not be contended that the velocity given to the water was more than sufficient to clear a path for the vessel. In observations of vessels going through smooth water at their highest velocity, particles were observed skimming along by the side, but that could not occur, unless the condition was, as he had stated—that the velocity imparted was that which was required to enable the water to clear a path for the vessel. He would, however, take the instance of the 'Great Eastern:' her ordinary displacement at midship section was about 1,800 square feet; she had attained the velocity of 15 miles per hour, which was 22 feet per second, and represented a head of water of nearly 8 feet, which was about 500 lbs. per foot, or 900,000 lbs. over the whole superficial area of displacement. He did not think that the back pressure was as much as one-half, but the two pressures combined were nearly $1\frac{1}{2}$; that made a pressure of 1,350,000 lbs. upon the resistance due to the entrance of the vessels, and at 22 feet per second, it would require about 54,000 H.P. for itself alone, while the actual H.P. was not more than 10,000, from which was to be deducted about 3,000 H.P. for slip and friction, leaving for available H.P. 7,000. The skin resistance of that ship could not be much less than 50,000 feet; taking 25 ounces, one of the proposed coefficients, for 15 feet per second, it gave 54 ounces for 22 feet per second, which applied to 50,000 feet, gave 170,000 lbs. as the skin resistance; that sum divided by 150 gave 1,100, which multiplied by 6, gave 6,600 H.P. for skin resistance alone, and left only 400 H.P. for displacement. Such a result could not be reconciled, and necessitated the introduction of the sine square of the resistance of displacement and the adoption of a lower coefficient. On the other hand, for commercial resistance, this coefficient was too small: in fact, every vessel that left these ports for a voyage to the East Indies required a higher coefficient than 25 ounces. It was important to agree upon some datum for the discussion of this question. If that were done, it would be possible to measure the resistance of a vessel in perfect condition. A great step had been gained by the establishment of the fact, that the interior surface of a pipe did affect the flow of water. He, in common with others, had previously held a contrary opinion. As a matter of theory, there could be no doubt that, by the use of asphalte or other surface bearing a fine polish, an increase of flow might be obtained in pipes of small areas.

Mr. Ditchburn observed, through the Secretary, that the theory of resistance was very imperfect, and that conclusions drawn from it could not be depended upon. When a vessel was at rest, the mutual and equal pressures would neutralize each other; but when a vessel was in motion, the water in her rear would follow and overtake her before those mutual pressures could be exerted. Hence it was evident that the pressure upon the after-part would be lessened, and the pressure upon the fore-part increased, and that the augmented resistance must be in proportion to the velocity of the motion of the vessel, and the form of her after-part. The action of the hull upon the fluid, as far as her progressive motion was concerned, ceased at the midship section; but there was an action of the fluid upon the hull produced by the gravitation of the water, causing particles to flow into the hollow formed by her continued advance. On the fore-body, however, of the ship, the effort of the water was increased in the ratio of the square of the velocity, so long as the velocity was maintained at a moderate degree; but beyond this degree, the resistances augmented at a still higher ratio, and even exceeded the cubes of the velocities, at some of the higher rates of speed, the effect of the fluid upon the after-body being diminished in the same proportion. Therefore, if a ship were going at the rate of 11 miles per hour, or 16 feet per second (which was about the velocity of a falling body at the earth's surface, and the utmost force of gravity upon water), that would be the velocity with which the fluid would fill up the hollow made by the ship; and as it was well known that a fluid could rush into a vacuum with a determinate rapidity only, it was important to discover what form of solid would give, while advancing at any assignable speed, the greatest facility to the divided fluid to reunite at the after-body. About forty-five years ago, he had conducted a series of experiments on the resistance of fluids for Sir R. Seppings, then Surveyor of the Navy, with the object of ascertaining the ratio which the resistance bore to the extent of surface under various given forms and angles. The models were of fir-wood, smoothly planed and painted. The experiments were made in a trough of water, about 4 feet square by 60 feet long; at an early stage of the trials it was observed that when perfectly dry models were used, the result was that the resistance offered by the water was much less than when water-soaked models were experimented upon, and to this fact was partly due the falling-off in speed of wood-built steamers. A wooden ship of 500 tons had been known to sink, from soakage, six inches in three years. There were twenty models, the bow ends varying from an angle of 186° to an angle of 12°. The result of these experiments was neither satisfactory, nor in accordance with the theory of mathematicians. According to theory, the resistance of a fluid decreased by the obliquity of its action, in the proportion of the square of the angle of incidence. Although these experiments were imperfect from the confined water-space and the small size of the models, yet it was apparent that the resistance to full bows was less, and to sharp bows and curved surface, greater than that predicated by theory. This discrepancy might perhaps be attributed to the compound nature of the resistance of water to bodies dividing and passing through it; part of this resistance was due to the action of the water against itself, and consequently other elements of calculation required to be adopted besides the form and angle of the bow. He was not acquainted with any reliable law for ascertaining the precise difference of the resistance of water to plane and to curved surfaces, and to surfaces at different degrees of obliquity. The experiments he had referred to also strengthened his views as to the importance of the form of the after-part of vessels. They had also proved that resistance was increased more by the addition of length than of depth. He would mention that for the purpose of reducing what was called the 'plus pressure,' in other words, the water forced up by the stem and bows of a full-built vessel, he had introduced a slight hollow close to the stem. This modification of form had been applied to river steamers, and had had the effect of reducing the resistance and drawing down the head of water, and thus removing the chief cause of the unsightly and destructive bow wave.

Mr. E. A. Cowper could not agree with those who maintained that the shape or form of a vessel had very little to do with the total amount of resistance. Notwithstanding the numerous models which had been adopted by shipbuilders, he believed that as yet they had not arrived at the most perfect form for any given speed. It was possible that modifications of the curve, called the 'conoid,' which was very similar to that of a wave, might be found useful in forming the lines of a vessel. The most convenient way of drawing a 'conoid' was to fix a pencil on a rod, and fix a pin in the rod at the end, letting this pin run along a straight edge, whilst the side of the rod was kept in contact with another fixed pin at a distance from the straight edge. A variety of curves could be drawn by varying the distances, and by having a rod bent at an angle where the pin was fixed in it. Some of the curves would look like waves about to break, their crests being curved over very much, or the curve could be arranged to form a loop, one part crossing the other, as though the wave had actually broken. Fig. 1 (page 362), represented a 'conoid' drawn with a rod bent at an angle of 60°; Fig. 2, that at an angle of 40°; Fig. 3, that at angle of 20°; Fig. 4, that at an angle of 10°; and Fig. 5, that with a straight rod. It was worthy of consideration whether the forward motion of a particle of water due to the passing of the wave was not equal to the forward motion of the pencil.

He had made an experiment some time since, with a view of ascertaining what curve a water-line should be, to exert an equal pressure against the water throughout every square inch of surface

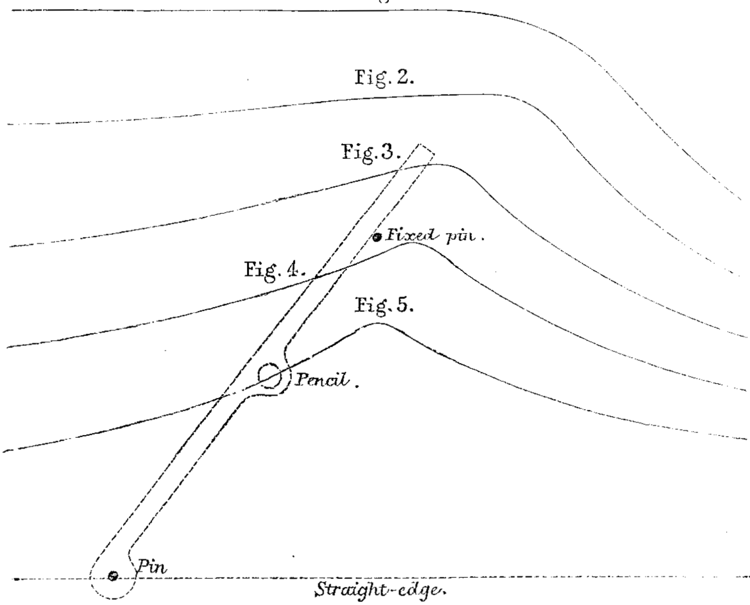

Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5.

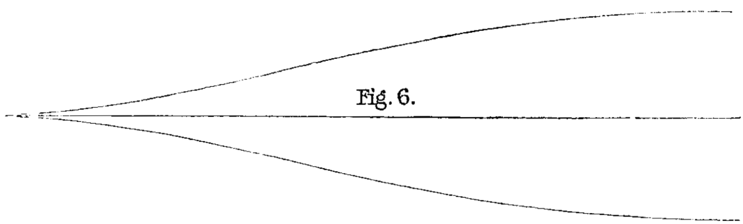

Fig. 6.

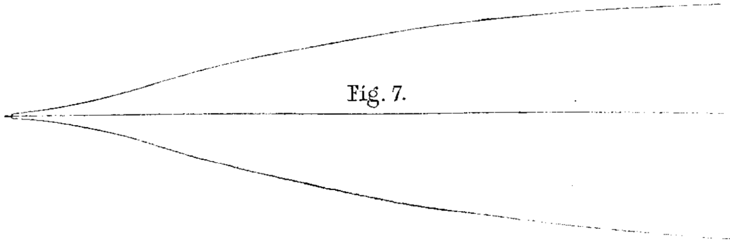

Fig. 7.

in its length; and in order to try this, for the fore-body of a ship, he made a rough wooden model which was so formed that the inside could be filled with water, the sides being quite flexible and only fastened to the flat wooden bottom, to the stem and stern posts, and to a bulkhead amidships; thus he had a rough boat with flexible sides, carrying an entire water cargo. This was towed through the water, in order to see what form the sides would take. When in motion the sides assumed a hollow and rounded curve, which varied in form according to the speed. If towed fast, the sides made a long hollow bow, with a gradual fulness towards the midship portion of the model (Fig. 6); whilst if towed slowly, they would take a smaller hollow curve at the bow, with a great fulness all the rest of the way to the midships. (Fig. 7.) This was to him a practical proof of the Author's proposition, that the resistance at the point of the wedge was greater than at any other part of the incline. It was evident that, with the water inside, the pressure must be equal throughout the length, and that the inside pressure increased in the depth exactly as the outside water increased in pressure. His experiments had been confined to the fore portion of the model, and although he did not assert that such a process gave the best form for a ship's fore-body, he considered it was a way of getting an answer from the water itself, as to what it would do under conditions of equal pressure.

There was one other point to which he would direct attention. It was important to find out, in what path the water passed round or under the bows of a ship, and came up or round under the stern of a ship. It had been stated that the water divided horizontally to allow the fore-body to pass, and rose up vertically to fill up the vacuity left by the after-body; but he believed that no one could tell in what path the water really did move round a ship, and he proposed to ascertain it by putting a number of small tin or copper weathercocks outside the ship, having their spindles passing through the sides at right angles, and having index fingers on the ends of the spindles inside, so that whilst the ship was running at any given speed, a note could be taken of the direction in which each finger and weathercock stood; then on going into dry dock, they could all be set exactly, and the path of the water could be traced on the outside of the ship. It was probable that different speeds would give different curves; but at all events, by such a device better opportunities would arise of making the true paths of the water into good lines. He had long been persuaded, that many more well-directed practical experiments, on which positive conclusions might be based, were necessary for the perfection of the art of ship-building.

Mr. W. Gravatt said that without entering into any mathematical calculations, he would briefly illustrate his views upon the subject of the best form for a vessel's bows or line of least resistance. If two thin laths were bound together at both ends for a finite part of their length, so that a portion at each end was in absolute contact, then, if the remaining portion were spread open by the insertion of a strut somewhat nearer the bow than the middle; the figure resulting therefrom, would approximately represent the form of least resistance, taking into consideration the sides of the figure only. The curve could not be arrived at by considering the total resistance as that of the sum of the resistances of any number of inclined surfaces, since, as was evident, the resistance due to each portion was materially affected by the whole action of the portions which had preceded it.

Mr. RICKMAN drew attention to a model of the sailing-yacht 'America.' He explained that it was composed of a series of sections in the plane of the surface of the water, and the novel feature was, that these were sections of the vessel, taken not when in the upright position of rest, but when heeling over at full speed; consequently it would show how far the water-line of the lee side of the vessel presented a wedge of a fuller form, and that of the weather side of a finer form, than when she was, as usually draughted, upright and at rest. The superiority of the line of counter-flexure, commonly called the wave-line, and its claim to be considered the line of least resistance, had been questioned, and the preference assigned to the straight line. He had produced the model of the 'America,' which in 1851 had been distinguished by her swift sailing 'on a wind,' to show that she possessed a lee bow water-line of a very hollow though regular counter-flexure.

Mr. F. J. BRAMWELL said there appeared to be a want of concord in the opinions which had been expressed on this subject. With regard to the observations made by the Author of the Paper, as to the resistance of plane surfaces drawn through the water at right angles to themselves being only equal to once the height of the column due to the speed, instead of twice the column, as the theory required, he thought the following illustration might possibly clear up any doubt on that point:—If a jet of water of a given size issued against a large plane, the force developed by the jet upon the plane would be double that which was due to produce the motion; but if the jet were indefinitely large, a force equal to the head which produced the motion was all that was obtained. With this exception, his opinions were generally in accordance with those expressed in the Paper.

There could be no doubt as to the fact, that inclined planes had a resistance greater at the apex than at any other point; therefore if that circumstance were not borne in mind, an erroneous impression might be arrived at as to the operation of the inclination of a bow. An opinion had been expressed, that it was almost impossible that the law of resistance should be directly as the square of the sine of the angle. But it appeared to him, that the law of resistance was in that direction, and under these circumstances he should have been glad if some experiments in support of that view had been adduced. He believed that if there were a flat surface in water, together with other surfaces of certain wedge forms, and that if those various surfaces and the water were quiescent, and then the power necessary to give them short but rapid motion against the quiescent water were ascertained, it would be found that in truth the resistance was directly in the proportion as the square of the sine of the angle. But if there were a flat surface in moving water, or the flat surface were moving through the water, he believed there was then a natural bow formed by the water itself. That was to say, the water did not strike the whole flat surface, as it would do had both the water and the surface been quiescent, until the rapid experiment referred to by him was made; but the water was gradually set in motion by the water-bow in front of the flat surface, and therefore the true effect of a flat surface for purposes of comparison with wedge bodies could not be obtained unless during very short trials, and in this way there arose an apparent discrepancy between the relation of the resistance to a flat surface and the relation of the resistance to a bow at an angle. It had been argued, by Mr. Scott Russell, that a nearer approximation to the truth of the rate of diminished resistance would be to calculate the increase of the area of escape, and to take the resistance as being diminished in proportion to the square of that increase. He thought that a formula based on such an assumption would be found almost as wrong as the other, and he would test this opinion by the following cases: suppose a perfectly flat disc, and that the area of escape was 180°, and next suppose a bow with infinitely fine lines, the area of escape being then 360°; as the area of escape was only doubled, the resistance should still be a fourth part of that required to propel a flat disc through the water. He thought that could not be, and that it was clear the resistance would have diminished in a far higher rate. In the consideration of this question no notice had been taken of the circumstance, that increasing the fineness of the wedge not only increased the area of escape, but diminished the amount of the water which had to escape. Mr. Bramwell concurred in the view, that the water removed by the action of the bow of the ship should be so displaced that at the termination of the action of the bow no momentum remained. Mr. Scott Russell proposed to accomplish this by adopting for the bow-lines double, or **S** curves, of such form as to accord, the one with a gradually accelerated, the other with a gradually retarded motion. This was the problem mechanics had to solve when it was desired to obtain a reciprocating motion, and it was accomplished by setting out the cams which were to produce this reciprocating motion on the same double-curve principle. Mr. Bramwell did not say that the circumstances of the fluid water were absolutely the same as those of the solid material with which mechanics had to deal; but he thought there was sufficient analogy between the two problems, to enable any mechanical engineer to understand and appreciate the form of bow which had been recommended. He could not agree in the theory of the resistance diminishing in the ratio of the square of what had been called by Mr. Scott Russell the area of escape; but the Institution had to thank Mr. Russell for calling attention to two things. One was, pointing out the reason for the great power put forth by the rudder, over and above the apparent effect it should have, which undoubtedly showed why the balanced or "Throttled valve rudder" did not succeed; and the other was, for stating why in screw ships with large openings in the dead wood astern, the power of the rudder was diminished. Those two points he regarded as amongst the most important which had been elicited by the discussion.

With respect to the vexed question of surface resistance, it appeared to him that further experimental inquiry was wanted. No doubt it was difficult to believe, that the friction outside of any surface was different from that inside a pipe; because if it held good in a pipe of a given size it must hold good in a pipe of an infinite size. Nevertheless it was clear if the proper friction of an ordinary surface of iron inside a pipe was 25 ounces per foot at the rate of 15 feet per second, it could not, judging by the case of the 'Leinster,' be the proper friction of the outside of a ship in ordinary condition. No doubt there was difficulty in ascertaining the real value of the midship section resistance, as reduced by the effect of the form of the bow and the run; and until these were known it was difficult to find out the value of the skin resistance, which with that of the midship section made up the total resistance, which total resistance was ascertainable as being a percentage of the total indicated H.P. But although it was difficult to ascertain what really was the amount of resistance due to the surface out of the total resistance, it was quite clear the surface resistance could not equal the whole, still less could it equal the whole indicated H.P. But it would be seen that if the resistance inside a pipe were to be taken as the measure of the surface resistance of the ship, that of the 'Leinster' would considerably exceed the whole indicated H.P., as she was stated to make 30 feet per second, which was double the pace, and therefore gave four times the resistance of the 15 feet per second for which the 25 ounces were taken. The result of that upon 15,000 feet of skin would be to consume 5,100 effective H.P. in driving the vessel, whereas the total H.P. was only 3,420. Thus it was clear that whatever the skin resistance might be, it could not be that which would be found inside a pipe. That being so, the question arose how the satisfactory data Mr. Hawksley had obtained, as to the friction of water through pipes, could be reconciled with the diminished resistance which it was clear must have existed in the case of the 'Leinster,' except upon the supposition that there was a difference between external and internal resistance? Might not the difference be due to the fact of the water being displaced by the bow, so that it was not in a state to give the full friction on the skin? Another matter bearing out that suggestion was, that in the table which had been exhibited, figures were given, for which Beaufoy and Mr. Hawksley were quoted as the authorities. Mr. Hawksley gave 1·56 for the surface of an iron ship; Beaufoy gave 0·763 for planed painted fir; while the Author gave 0·71 for a coppered ship; so that if a new iron ship and a coppered ship were taken for comparison, there was, according to these figures in round numbers, a skin resistance of only one half in a coppered as compared with an iron ship. He thought common sense taught, that the difference in speed between a new iron ship and the exactly same form of ship coppered was nothing like that due to the relative resistances of 2 and 1. If Mr. Hawskley's measure of friction for iron plates were correct, as well as that given by the Author for copper, every owner would have coppered ships, as they would get through the water at the same speed with half the power required for iron ships. As regarded the apparent slight differences between the friction of a rough and a polished surface moving through the water, he might mention that some years ago Griffiths' screw was applied to the 'Fairy' screw yacht. Indicator diagrams were taken at every mile, and the power carefully computed. Having obtained a very good result from the first screw, which was made of cast iron and put to work as it came from the sand, Mr. Bramwell had a similar screw made of gun-metal, and having had it well polished he looked for improved results. He found, however, there was no appreciable difference in the speed, nor any reduction in the indicated H.P., nor in the disturbance of the water.

He had only one other point to touch upon—that was the allusion in the Paper, confirmed by Mr. Scott Russell, as to the whole of the work being done by the early part of the screw-blade. In the Griffiths' screw of the 'Fairy,' the width of the point of the blade was 6½ inches, which replaced a screw of a width of 33 inches at that part. It was true the lower part of the Griffiths' blade was widened out, but the surface as a whole was nevertheless much diminished; notwithstanding that, the boat went a great deal better with the Griffiths' screw, showing that as far as screws went, unless they were of an increasing pitch, the real work was done by the leading edge of the blade.

PROFESSOR W. J. MACQUORN RANKINE observed, through the Secretary, that he had successfully applied to steam-vessels a method of estimating their probable speed with a given I.H.P., or conversely, namely, their probable I.H.P. at a given speed. That method was based upon the work performed by the production of frictional eddies in the layer of water adjoining the skin of the vessel, and by its means he had correctly predicted the speed of the 'Great Eastern' at various amounts of I.H.P. From the results of his own experiments, and from the experiments of others, he could corroborate the opinion advanced in the Paper, that the frictional resistance depended much upon the nature and condition of the surface; and he had found that the coefficient of friction for clean and smooth iron ships agreed with the constant part of a coefficient deduced by Professor Wiesbach of Frieburg. That formula was deduced from experiments upon the flow of water in clean and smooth iron pipes; viz., 1 lb. per square foot for a speed of 10 knots. For copper sheathing and very smooth pitch, the amount due to resistance was decidedly less, but owing to the scarcity of exact experimental data, he could not state precisely in what proportion. Upon two points his method differed from that of the Author, and from another recommended during the course of the discussion; the first point was, that when the lines of a ship were truly proportioned, and when the length of her fore-body and after-body, but especially the after-body, bore certain relations to the length of the waves travelling naturally with the greatest speed of the vessel, he considered that there was no sensible resistance, except that caused by frictional eddies. The second point of difference was, that instead of simply multiplying the immersed surface of a ship by a quantity bearing a certain proportion to the square of her speed he took into account, by an approximate process, the various velocities of the particles, and the different resisting effect of a given extent of the vessel's surface according to its position. That process consisted in computing a quantity called the 'augmented surface of the vessel,' which was greater than her real surface: being the product of her length on the water line, her mean immersed girth, and a multiplier depending on her figure, called the coefficient of augmentation. In ordinary cases, that coefficient ranged from one and a small fraction, up to 2, according to the vessel's lines; in some cases, of extremely full form, it might be as much as 5.

The cube of the probable speed in knots was to be found by multiplying the indicated H.P. by a certain factor, and dividing by the 'augmented surface.' The value of the factor was subject to variations depending upon the state of the vessel's surface, the slip of the propeller, and the efficiency of the engines; but when all those parts were in good order, 20,000 was a safe average value for iron ships. He was of opinion that as yet there was no exact method of calculating the resistance of a vessel, the length and form of which were unsuited to her speed. Deficient length of the after-body had been found in practice to be especially fatal to economy of power. Greater latitude might be allowed as to the length of the fore-body. As an example of his method, he gave an abstract of the probable speed of H.M.S. 'Warrior,' based on a computation of the exact measurements from her plans:—

H. M. S. 'Warrior.'

Displacement	8,997	tons.
Length on water line	380	feet.
× Mean immersed girth	76·3	„
× Coefficient of augmentation . .	1·275	
Augmented surface	36,967	square feet.
Indicated horse power on trial . .	5,471	
× Multiplier for clean iron ships . .	20,000	
Divide by augmented surface . .	36,967)109,420,000	product.
Cube of probable speed	2,959	
Probable speed by calculation . .	14·356	knots.
Actual speed on trial	14·354	„
Difference . . .	·002	

Mr. Phipps, in reply, said, as the correctness of the law, that the resistance was as the square of the velocity, had been impugned, he would wish to make some remarks in support of that doctrine before he offered any further observations upon the general question. The objections seemed mainly to be founded upon a series of comparative experiments made with H. M. S. 'Rattler,' a screw ship, and with the 'Alecto,' a vessel of similar lines but propelled by paddle-wheels. In the case of the 'Rattler,' the pressure of the screw shaft against the end of the vessel had been measured by the dynamometer, but he could not consider that the measures thus recorded were a true representation of the vessel's resistance, because he thought in screw vessels the pressure against the shaft was always greater than the real resistance of the ship. Referring to a drawing of the screw and stern of the 'Rattler,' Mr. Phipps pointed out the effect produced by the 'slip' of the screw in abstracting water from the stern, and thereby increasing the minus resistance. The quantity of water thus removed would, he said, probably bear a direct proportion to the 'slip,' and hence he concluded generally, that in order to give the true resistance of a vessel, it was necessary to deduct from the thrust on the screw shaft a certain amount of force bearing a proportion to the 'slip' of the screw. When the 'Rattler' made a speed of 4 knots per hour, the slip was nearly three times greater than with a speed of $8\frac{1}{4}$ knots.

The pressure of the screw shaft at $8\frac{1}{4}$ knots was . . 6,213 lbs.
" " at 4 knots . . 4,780 lbs.

If from the 6,213 lbs. were deducted 1,200 lbs., being as the slip $\times$ 1, there would remain 5,013 lbs., for the true resistance of the vessel at $8\frac{1}{4}$ knots, and if from 4,780 lbs. were deducted 3,600 lbs., being as the slip = 1,200 lbs. into 3, there would remain 1,180 lbs. for the resistance at 4 knots, the two amounts of 1,180 lbs. and 5,013 lbs. being nearly in the proportion of 4^2 to 8.25^2. It was quite possible that the above figures might be only an approximation to the true resistances of the vessel, but he thought that they were sufficient to explain the general results of his reasoning. Mr. Phipps agreed with some remarks which had been made upon the excess of pressure upon the rudders of vessels, over and above that recognized by the ordinary theory; and whilst on the subject of steering vessels, he would direct attention to the difficulties attending the steering of large and long ships, and of turning them round rapidly on a short radius. He had computed the requisite force at the rudder of a ship 300 feet long and drawing 25 feet, in order to bring her round in five minutes, and he had found it would amount to at least 20 tons. When it was considered, that in order to save one minute out of the five minutes, the pressure against the rudder must be increased as 4^2 to 5^2, or as 16 to 25, or to more than 31 tons, the difficulty of all such attempts would be understood, though, it must be remembered, that in screw vessels, the impact upon the rudder, of the water set in motion by the screw, was a source of greatly-increased steering power. He would next notice that method of computing resistance which might be described, in general terms, as being based upon the displacement of water caused by vessels of similar type, their resistances being, as the squares of the cube root of their displacement. The objection to this method was, that when the type varied it was necessary to introduce into the computation some particular coefficient appropriate to each class of vessel. He thought that the method he had proposed was more general in its application. With regard to the accuracy, or otherwise, of Colonel Beaufoy's work on the resistances of solid bodies passing through water, the experiments referred to in that work had been carefully made, and, in his opinion, were trustworthy, especially those at from 5 knots per hour to 8 knots per hour, in which of course the friction was of less moment than at higher velocities, though in all the experiments, this friction had been duly ascertained and deducted. The experiments upon plus and minus resistance, and the resistances of inclined planes, were also very instructive. It was true, that in the latter case no general law had been arrived at; but with respect to the resistance of any particular body, Colonel Beaufoy had shown that it was very nearly as the squares of the velocities, and it must be remembered, that where he had failed to deduce a general law for the resistance of inclined prows, no other Author had succeeded. In fact, it was quite impossible to approximate to clearness of ideas upon this point, until due value was given to the effect of impact upon the leading portion of the plane, as Mr. Phipps had endeavoured to illustrate in the Paper. It had been shown, in the course of the discussion, that if there was considerably more pressure at one end of a plane than at the other, it became impossible to lay down a general law, unless the planes were always of exactly the same length. This truth, once understood, there was no longer any difficulty in accounting for the large excess over the ordinary theory, in the case of very acute angled prows, or planes. Objections had also been made to the experiments of Beaufoy, because the bodies were entirely submerged; but it might be said, in favour of that method, that it avoided the great difficulty of valuing the effect of the wave raised at the bows, when the body was floating upon the surface of the water.

As to the effect of roughness, in diminishing the flow of water through pipes and open water-courses, it had now been admitted, that the resistance within a pipe was really affected by the roughness of its surface; and it therefore followed, that the fact stated, of the resistance within a pipe through which water flowed at a velocity of 15 feet per second being equal to 25 ounces per square foot of surface, could only be applicable under some given condition of the surface of the pipe. He had found, from an investigation of the experiments of M. Darcy, that this condition corresponded with a roughness a little greater than that of cast iron. But while the above admission had been made, it had still been contended, that the resistance within a pipe was a proper measure for that of a ship, when the average velocity of the water in the one was equal to the velocity of the other through still water, and that the true frictional resistance due to a velocity of 15 feet per second was 25 ounces per square foot, either for pipes, or for the surfaces of iron ships, on the ground that this formula was intended, not for the surfaces of new iron ships, but to be applied to mercantile vessels in their usual working condition. By referring to a previous discussion, when this formula was first given, it would, however, be seen, that after applying the formula to a supposititious vessel of nearly the power and magnitude of the 'Atrato,' a speed of $15\frac{2}{3}$ miles per hour was brought out; and then, it was added, "The 'Atrato' was stated by the Author of the Paper to have attained in fresh water a velocity of 16 statute miles per hour over the measured mile."[1] Comparing, therefore, as had been done, the result of that calculation with the 'Atrato' when tried over the measured mile, the question naturally arose, what was probably the condition of the 'Atrato's' hull on such a trial over the measured mile? To which, as it appeared to him, the answer must be, that the hull was new and clean; and it was in this sense that he had always understood the matter. Next, as a matter of fact, he had found, from a reference to the plans of the vessel, that the amount of wetted surface of the 'Atrato' differed considerably from the vessel assumed as nearly similar; the amount of wetted surface in the 'Atrato' on her trial trip exceeded by 3,112 square feet that of the supposititious vessel. The indicated horse power of the 'Atrato' was 3,046 H.P., to which that of the supposititious vessel was nearly equal, namely, 3,000 H.P. The actual speed attained by the 'Atrato' on her trial trip, with the above power, was 13·77 knots, or 23·27 feet per second. Deducting 25 per cent. for observed slip of the feathering paddles = 761·5, and $\frac{1}{16}$th of the remainder for friction, working air-pumps, &c. = 228·4, there was left to propel the ship, a net power of 2,056 H.P. Now, according to the formula in question, a vessel of the dimensions of the 'Atrato' would have required, to attain the above speed, a net power of 2,806 H.P., which was probably sufficient to show that the coefficient of 25 ounces per square foot, at a velocity of 15 feet per second, was considerably too high.

As to the frictional resistance on the internal surfaces of pipes and open water-courses, as compared with that on the exterior of floating bodies, it had been argued that, when these two kinds of surfaces were equally smooth, the resistances were equal. His opinion, that the resistance on the internal surface was the greater, was founded upon the hypothesis of a greater velocity of flow of water in the latter, when, in general phraseology, the speeds were said to be the same. He had not in any degree founded his arguments upon the notion of circularity. Indeed no one acquainted with the usual method of calculating the resistance within pipes could for a moment suppose, that the difference of form in a circular pipe, or in a rectangular water-course, could cause any difference of resistance. All the rules were based upon the relation which the weight or volume of water in any given length of the pipe, or open water-course, bore to the wetted surface in the same length, the quotient, found by dividing the volume by the wetted surface, being the quantity usually denominated the 'hydraulic mean depth.' In a circular pipe, this hydraulic mean depth was always $\frac{1}{4}$th of the diameter, so that in a pipe of 4 feet diameter, the

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xvi. p. 357.

hydraulic mean depth was' 1 foot, being the same depth as that in an open rectangular water-course, of such great width as to neutralize the effect of the sides. If, therefore, a pipe 4 feet in diameter were laid at the same inclination as an open water-course of infinite width, and 1 foot in depth, the water would flow at the same average velocity in both. In explanation of his views of there being a greater resistance in the interior of a pipe or water-course than upon an external surface, he repeated that his arguments had nothing whatever to do with the form of the pipe through which the water flowed. But they were based upon the idea of a greater velocity obtaining against the frictional surface of the former, when the velocities of the fluid through the pipe, and of the exterior surface through still water were nominally the same. This might be shown generally, by stating that when a fluid passed along an inclined open water-course, the velocity of its motion was that of the average of all the different filaments, those at the surface moving more rapidly and those at the bottom more slowly. Therefore the relation of the bottom and nominal velocities of the fluid was the same as that of the average and bottom filaments. But when a vessel passed through still water, the case was different; for then the relation between the velocity of the vessel through the water in immediate contact with it and its nominal velocity, was as the velocity of a surface filament in a water-course compared with the bottom velocity of the water-course, when its velocity was the same as the nominal velocity of the vessel.

With respect to prows of various shapes, he was compelled to differ from the opinion, that a prow formed of perfectly straight planes would cause the least possible resistance in its passage through the water. The following calculation would illustrate his views on this subject. (Plate I., Fig. 7.) Resistances, taking the square end A K as unity :—

$$\begin{aligned} A\,K &= 1{\cdot}00 \\ A\,D\,H &= 0{\cdot}044 \\ A\,H \text{ or } L\,H &= 0{\cdot}027 \\ H\,I\,L &= 0{\cdot}041 \end{aligned}$$

The usual method of calculating the resistances was by taking the ratio of the slant length $A\,H^2$ to the base $A\,K^2$, or by dividing the curved figure A B D E H into a polygon, taking each portion separately, as $H\,F^2$ to $G\,F^2$, &c., and then taking the sum of all the parts. According to that method, his own calculations bore out the suggested superiority of straight prows, for in the Table of Resistances, the resistance of the straight plane A H was to that of the curved surface A D H, as 0·027 to 0·044. But the above method was obviousl defective, for when applied to the straight plane A H, no cognizance had been taken of the increased head resistance pointed out in the Paper, while, with respect to the curved surfaces A D H, each portion H F, F D, D B, and B A, had been dealt with as a separate plane, acting upon the surrounding fluid. Beaufoy's experiments invariably showed a smaller resistance for curved surfaces than for straight ones, when standing upon equal bases; the reason of this, there could be little doubt, arose from the circumstance, that each portion of a curved surface, excepting the entering angle, had only to act upon a fluid already in motion, and consequently suffered a less resistance than if it had been detached, as assumed in the ordinary calculation. Then, with respect to lines of contrary curvature, there was another circumstance which tended much to a diminished resistance. When the vessel advanced, a particle of water pushed outwards laterally by the sharp end of the prow would have its motion continually accelerated in the same direction until overtaken by the point of contrary curvature; its motion would be then retarded during the advance of the convex portion of the curve, and when reached by the fullest part of the vessel, it would be at rest. If this were so, the source of the advantage over the straight plane was evident; for although in the curved form the outward velocity was greater, at the point of contrary curvature, than it was on the straight plane which corresponded with the point of fullest convexity, namely, its base, yet all the work in the former, between the point of contrary curvature and the fullest part of that curve, was done without the application of additional force; whilst with the straight plane, the power must be exerted all the way from the point to the base of the triangle of which it was a part. It had been stated that the reason why a plane drawn square to itself through water did not realize the theoretical resistance of twice the altitude of column due to the speed, was that as a stationary plane, placed in such a way as to receive the full impact of a jet of water, did not realise the above theoretical resistance unless the area of the plane was considerably greater than that of the jet, so there was no reason to expect that a plane drawn square to itself through water would experience the full effect of a column of double the altitude; but Mr. Phipps did not think the analogy of the two cases complete. Newton and Young had attributed the diminished resistance to the action of a supposititious prow, formed of the fluid itself travelling in advance of the square plane; but, after mature deliberation Mr. Phipps had deemed the propagation of pressure in advance of the plane a more tenable theory, because under such propagated pressure all the fluid which required to be removed in a given time would be acted upon gradually, and pass away laterally, at a smaller mean velocity than that of the moving plane. In conclusion, he observed, that while admitting that the recorded performances of many of the ships referred to in the Paper upon High Speed Steam Navigation[1] did not agree with the principles laid down in his Paper, he was disposed to attribute that discrepancy to the fact of the vessels quoted being screw-vessels. He believed that, under certain circumstances, the screw caused great increase of resistance, and therefore in all experiments on resistance, he thought it better to exclude screw ships, because of their liability to be acted upon by unknown influences. His reason for the selection of the 'Leinster' as an example was, that all the particulars relating to that vessel were accurately known, including the slip of the paddle-wheels, the engine-power, and all other minor conditions.

March 22, 1864.

JOHN ROBINSON M'CLEAN, President, in the Chair.

The discussion upon the Paper No. 1,105, "On the Resistance to Bodies passing through Water," was continued throughout the Meeting, to the exclusion of any other subject.

In pursuance of the notice on the card of the Meetings, it was proposed, and resolved unanimously,—

"That in order to insure a fuller attendance of Members than could be obtained on Easter Tuesday, the Meeting be adjourned until Tuesday evening, the 5th of April."

April 5 and 12, 1864.

JOHN ROBINSON M'CLEAN, President, in the Chair.

The following Candidates were balloted for and duly elected on April 5th:—Robert Bruce Bell, John M'Connochie, Charles Markham, Daniel Miller, George Brice Pennell, Charles Staveley Rooke, and James Cobby Street, as Members; John George Knight, Frank M'Clean, Samuel Garlick Martin, and Charles Harcourt White, as Associates.

The discussion upon the Paper No. 1,105, "On the Resistance to Bodies passing through Water," was continued throughout both evenings, to the exclusion of any other subject.

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[1] *Vide* Minutes of Proceedings Inst. C. E., vol. xvi., p. 327.