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INSTITUTION OF ENGINEERS & SHIPBUILDERS
IN SCOTLAND.
(INCORPORATED.)

TWENTY-EIGHTH SESSION—1884-85.

Introductory Address.

By Professor JAMES THOMSON, C.E., LL.D., F.R.S., President.

Read 28th October, 1884.

GENTLEMEN,

In taking the Presidential chair at this first meeting of our new Session, I have to thank you for the honour you have done me in selecting me to be the President of this important Institution. I am fully sensible that honour of this kind involves also duty and responsibility; and I shall aim at doing my best to fulfil the trust you have placed in me.

I think we are entitled to notice with satisfaction that our Institution continues to prosper. We have had, in recent years, a goodly array of valuable papers read at our meetings and published in our Transactions; and the number of Members on our roll continues to increase.

I hope that, for the Session now opening, you will keep your minds duly impressed with the importance of providing good papers for our meetings. If each member will question himself as to what ideas, or results of experience, he has attained to, that may be new, and true, and useful for communication to his fellow-members; and will, whether through generosity towards others, or for advancement of his own interests, take the pains of bringing them before this Institution, much that is deserving of development will be advanced in growth and fructification.

It is customarily expected that a Presidential Address should be given at the commencement of each new Session. Accordingly, I propose now to offer a short address, having chosen not a wide range of subjects in engineering in general, but just two subjects, both of which I deem to be of real importance to engineers, and which commend themselves to me at present because they have engaged my own attention very much, and on them I think I have something useful to tell, or have useful considerations to adduce. The first of these will relate to considerations on the fundamental principles of the kinetic branch of dynamics—a subject which forms an essential part in the science of mechanics. The second will relate to questions as to suitable means for attainment of safety, or for abatement of danger, in various kinds of engineering structures; and my special purpose will be to show that the method by application of force-tests is deserving of more frequent and more consistent application than is customarily accorded to it.

On the former of these subjects I have long felt the need of improvement in our modes of thought. We want more thoroughly clear fundamental ideas, and we want clear expressions in which to set them forth. Lately, I have been able, I think, to clear up some parts of this subject a little; and I have, within the present year, submitted papers upon it to the Royal Society of Edinburgh, and my sayings to you this evening will include some passages from those papers.

We are all accustomed to speak readily of the *inertia* of matter, though generally we would find it very difficult to explain exactly what we mean by the term. No doubt we can understand that manifestations of inertia are strikingly exhibited in the blows of a steam hammer, in the collisions of railway trains, and in those of ships at sea, and in the impacts of projectiles; and we meet with it forcibly in the regulating effects of fly-wheels and governor balls. We are accustomed to overlook the deficiency of our knowledge of any explicitly clear principles on which the discrimination ought to depend of what shall constitute portions of time in the future equal to portions of time in the past; while we cannot bring them together to compare their lengths, as we might do with yard wands if we wanted to test *their* agreement. The past time has vanished already, and the future time has not yet come, and we cannot make the two be present together for comparison.

One of our fundamental difficulties, then, relates to attainment of any principle for true chronometry, either in idea or in fact.

The manifestations of inertia of matter are certainly connected with what is called *changing motion*; but then we want clear ideas as to what may be any real distinction between changing motion, and either rest or changeless motion.

In the universe of boundless and unmarked space we men have no means for knowing any condition to be called absolute rest. We do not know even how to imagine a distinction between rest and changeless motion. We have, however, through properties of matter, perfect means of distinguishing in principle between changeless motion and changing motion; and, likewise through properties of matter, we have very good means of distinguishing practically among different degrees of rapidity of change of motion.

The rocking of a cradle, the tossing of a ship on a stormy sea, the vibration of a pendulum or that of the balance wheel of a watch, and the continuous regular motion of a fly-wheel revolving uniformly, are all instances of changing motion, and are perfectly distinguishable from the condition of changeless existence which, so far as we have any means of knowing, may be regarded as either rest or changeless motion. The last of the instances just named—that of uniform revolutional motion of a fly-wheel—might over hastily be mistaken for changeless motion; but the perpetual *changing of direction* of the motion of each small part of the rim of the fly-wheel, moving as it does in a circular path relatively to the ground, constitutes a perpetual changing of the motion. The statements just now made as to changing motion, such as may be felt by an infant in a cradle, or by any person in the cabin of a ship tossed by waves, or such as belongs to any part of the rim of a fly-wheel revolving uniformly in a circular path relatively to the ground, may serve to introduce some preliminary notions as to there being truly in nature modes of existence of matter which may be designated intelligibly as *changing motion.*

At first sight many of us might fancy that we understand quite readily the reciprocating motion of the bob of a pendulum going forward and backward in its circular arc within the clock case. Let us, however, introduce further the thought of the earth shooting forward in its annual orbit round the sun, with a velocity of about 18 miles per second, and carrying the clock case and clock with it, while the pendulum may be moving relatively to the case at quickest perhaps only with a velocity of a few inches per second; and the nature of the changes of motion in progress, whether as to velocity or direction, will not be very evident, and may indeed become rather perplexing to the mind.

After the announcement which has been made of our utter inability to know of any condition to be called absolute rest in a boundless and unmarked universe, and the suggestion put forward of perplexities or more than perplexities as to direction of motion in the universe, and the concomitant assertion on the other hand that there is truly in nature a condition to be called change of motion which can be practically appreciated by men; it may be some comfort to be told of a most important truth discovered in the nature of things—the truth that there is a real distinction, appreciable with extremely great exactitude to men, between rotational rest and rotational motion in the unmarked universe.

During by far the greater part of the period within which men have given any attention to astronomical and other physical questions, the earth has been very generally supposed to be at rest, and the sun, moon, and stars have been supposed to revolve round the earth, most of the stars appearing as if fixed together in the heavens, and as if revolving together in a period slightly shorter than that of the solar day. It was thus supposed that straight lines directed from us towards the stars—such, for instance, as the straight lines along which we look in viewing the stars—are not directionally at rest in the universe; but that each of them participates in the supposed revolution of the star towards which it is directed, and has directional or rotational motion in sweeping out angular space round our own station on the earth as a vertex. Directional fixity was assumed to belong to straight lines attached in unchanging configuration to the earth imagined itself as being at rest.

Belief to the effect so described was prevalent, and was very dominant in past times, down to the period of Galileo, about 300 years ago. In those former times there was no physical principle, nor any valid reason known to men, which could afford a criterion for deciding on any particular condition as being one of absolute rotational rest in the universe. It is now, however, discovered and fully established that there is a real and true principle in nature determining a condition of absolute rotational rest. It is also fully established that lines of direction almost perfectly unchanging are available to us in the straight lines from the stars designated as "*fixed stars*" to any observer's station on the earth.

Out of multitudinous considerations founded mainly on astronomical observations—the nature of some of which may be suggested by the remarks just made—there have emerged to the notice, and to the more or less clear cognisance of men, a few profoundly important dynamic laws which have come to form the basis for further dynamic reasonings, and, to us engineers, the basis for most of our investigations in mechanics.

Sir Isaac Newton sets forth, under the designation of the FIRST LAW OF MOTION, the statement that—*Every body continues in its state of resting or of moving uniformly in a straight course, except in so much as by applied forces it is compelled to change that state.*

A most important truth in the nature of things, perceived with more or less clearness, is at the root of this enunciation; but the words, whether taken by themselves, or in connection with Newton's prefatory and accompanying definitions and illustrations, are inadequate to give expression to that great natural truth. In attempting to draw from the statement a perfectly intelligible conception, we find ourselves confronted with the preliminary difficulty or impossibility as to forming any perfectly distinct notion of a meaning in respect to a single body, for the phrase "*state of resting or of moving uniformly in a straight course.*" Newton's previous assertion *that there exists absolute space which, in its own nature, without reference to anything else, always remains alike and immovable,* does not clear away the difficulty. It does not do so, because it involves in itself the whole difficulty of our inability to form a distinct notion of identical points or places in unmarked space at successive times, or of our inability to conceive any means whatever of recognizing afterwards in any one point of space, rather than in any other, the point of space which, at a particular moment of past time, was occupied by a specified point of a known body. We have besides, as I have already mentioned, no preliminary knowledge of any principle of chronometry; and, for this additional reason, we are under an essential preliminary difficulty as to attaching any clear meaning to the phrase *state of moving uniformly in a straight course*, the uniformity being that of equality of spaces passed over in equal times.

The only motion of a point that men can know of, or can deal with, is motion relative to one, two, three, or more other points. Three points marked or indicated on one, two, or three bodies, the centres, for instance, of three balls, whether preserving their distances apart, unchanging or not, are sufficient for enabling us to construct or to imagine a reference frame of any changeless configuration desired—three rectangular co-ordinate axes, for instance, or three rectangular co-ordinate planes—to which the situations, instantaneous or successive, of points may be referred. We may have a firm persuasion, even without perfect understanding, that, in the nature of things, there must be a reality corresponding to our glimmering idea of motion of a body along a straight course with changeless velocity; and that there must be an essential distinction between such motion and motion along a curved course, or motion with varying velocity. We cannot, however, specify such motions relatively to unmarked space and unmeasured passage of time. Briefly, we can deal only with relative motions or relative rest; not with absolute motions nor absolute rest.

Sir Isaac Newton sets forth as the SECOND LAW OF MOTION in his arrangement, a statement to the effect:—

That, *Change of motion is proportional to the magnitude and duration of the applied force, and takes place in the direction in which that force is applied.*

His THIRD LAW is to the effect:—

That, *Action is always accompanied by equal and opposite re-action; or the mutual actions of two bodies, each on the other, are equal and opposite.*

It may now readily be noticed, that Newton's enunciation, set forth as the Second Law, involves elements of obscurity alike with that which has been shown already as rendering the enunciation of the first law inadequate for expressing the great natural truth to which it relates.

I will now proceed briefly and without trespassing on your patience by attempting to enter into any elaborate illustrations, to mention an amended mode of enunciating what men really can know in respect to the natural law or laws here referred to.

Let us introduce the conception of a reference frame to which the situations and motions of any moving points are to be referred, and let us introduce also the conception of what may be called a dial traveller, consisting of a hand, like that of a clock, travelling round a dial; the motion being produced in any way so as to accomplish conditions to be afterwards specified.

Now, we are to accept as an established law of nature, established through multitudinous observations and speculations, together with theories confirmed by multitudinous agreements, the following, which may be called the LAW OF INERTIA.

THE LAW OF INERTIA.

For any set of bodies, acted on each by any force, a REFERENCE FRAME and REFERENCE DIAL-TRAVELLER are kinematically possible, such that relatively to them conjointly, the motion of the mass-centre of each body, undergoes change simultaneously with any infinitely short element of the dial traveller progress, or with any element during which the force on the body does not alter in direction nor in magnitude, which change is proportional to the intensity of the force acting on that body, and to the simultaneous progress of the dial-traveller, and is made in the direction of the force.

From this Law of Inertia the *Principle of Chronometry* is readily deducible, as a corollary, by elementary mathematical considerations, and it may be enunciated thus :—

Any dial-traveller, which would accomplish the conditions stated, would make progress proportionally with any other dial-traveller, obtained likewise from the same set of bodies, or any other set of bodies with the same or any other reference frame. Then, in view of this remarkable agreement, we define as being equal intervals of time, or we assume as being somehow in their own nature intrinsically and necessarily equal intervals of time, the intervals during which any such dial-traveller passes over equal spaces on its dial. Thus, any dial-traveller which would accomplish the conditions stated, would constitute a perfect chronometer.

This gives us the ideal of a perfect chronometer. It remains for men to aim at approaching as near as they can towards that ideal in the practical realization of good chronometry.

For good and long-enduring raelizations of chronometry, astronomical methods are alone available. None of these present any simple method of procedure. They require hypothetical assumptions of supposed forces acting on the bodies considered, and, above all, there is involved in them the assumption, and after multitudinous tests, accompanied by multitudinous confirmations, the discovery of the Law of Universal Gravitational Attraction—the grandest of the discoveries of Sir Isaac Newton.

Further the principle of Absolute Directional or Rotational Rest, and of Absolute Rotation is also readily deducible, and may be stated thus :—

Any straight line fixed relatively to any reference frame which accomplishes the conditions specified in the statement of the Law of Inertia, has absolute rotational or directional rest. If another straight line fixed in any other such reference frame be parallel to that former line, the two lines will continue parallel, so that by either of them the one same absolute direction is permanently preserved.

The principle here called that of absolute directional rest is clearly enunciated in Thomson and Tait's *Natural Philosophy*, § 249, under the designation of "Directional Fixedness." It is there exhibited by a very simple device, from which, however, that just now stated is different.

Any body which has no rotation relative to a framing, which accomplishes the conditions stated, is devoid of absolute rotation.

The Law of Inertia, here enunciated, sets forth all the truth which is either explicitly stated or is suggested by the first and second laws in Sir Isaac Newton's arrangement; and by a slight extension of collateral explanations, it can be made to include also the truth that is in the third law.

By applying the Law of Inertia to the case in which the forces acting on the bodies vanish, the law becomes a remodelled substitute for the statement set forth by Sir Isaac Newton as the First Law of Motion in his arrangement.

Now, gentlemen, I have to say, and I think you will agree with me in the opinion, that some of the considerations I have been bringing before you have been rather abstruse, and have been not quite easy for complete comprehension on their first presentation. I will try, however, to make amends now by offering to you one or two cases in illustration, which, I think, are very easily understood and easily carried in the mind.

It is told of the great Syracusan philosopher of old, that he said "Give me a fulcrum and I will move the world." But another great philosopher—one who has been contemporary with ourselves, the late Professor Clerk Maxwell—has taught us how to perform the more wonderful feat of changing the earth's motion and leaving it permanently altered, without our having to seek for aid from any external fulcrum or anything external to the earth itself. To do this, all that is required will be accomplished if we look towards the pole-star, and wave the hand with a motion of revolution round the line of vision so assumed. In other words, the deed will be done if we make any wheel or any mass of matter revolve in a plane parallel to the earth's equatorial plane. While the mass is revolving the speed of the earth's diurnal revolution is different from what it would be if the mass were left standing still. The effect of the operation thus performed would be, I must admit, very small indeed; but engineers, when once they know of something that can be done, even in a very small way, are generally eager to carry it out on a larger scale. Great operations on the earth do not deter them, if only a company can be formed and the money be amply provided. Let them then construct a Grand Equatorial Belt Railway round the earth, and set heavily loaded trains on it; and, if they want to lengthen our days, let them run the trains forward from west to east. The earth will now be revolving slower than before; and if, at any time after several months or years have passed, the trains be all stopped, the earth will revert to its old speed of revolution; but it will then, at any instant of time (indicated, it may be, astronomically, by an eclipse of a satellite of Jupiter, for instance), be behind the position of diurnal revolution, which it would have had if the trains on that great railway line had not been in motion.

To proceed now to another case:—Let us suppose that a cannon is placed at the North Pole, and is fired towards a vertical flagstaff standing at a distance of 4 miles from the Pole. Let us suppose that, at the instant when the ball is leaving the gun, a certain star is just behind the flagstaff so as to be hidden from the gunner when taking aim. Let the ball be supposed to move at 1960 feet per second as the horizontal component of its velocity; and, for simplicity, let it be supposed that this velocity will be maintained throughout the flight. On the supposition of the motion being unaffected by resistance or disturbance from the air, let us entertain the question: —*How will the ball fly as to the horizontal projection of its path over the earth?* A little consideration and calculation will bring out the answer to be this:—The ball will fly in a vertical plane passing, at the first instant of the flight, through the earth's axis, and the flagstaff, and the distant star. This vertical plane of the ball's motion will continue to be directed from the earth's axis out towards the star, but the flagstaff will be moving away from that plane to the eastward with the rotation of the earth; and the flagstaff, at the moment of the ball's passing it, will have escaped to the east by a distance of 16½ feet from the course of the ball.

If, further, we raise the question as to how the projectile would proceed out through space, as shot from the gun towards the distant star; and if, for simplicity, we imagine it to be unaffected by the earth's attraction, or by any force whatever; and if we suppose the gunner's aim to have been perfect, so that the ball in departing continues to eclipse the star from his eye; we shall have to conclude that the ball, in its straight course with changeless velocity through space, may be going along any straight line whatever, quite as well as going along that one which would take it ultimately into collision with the star. This statement looks puzzling; but to bring out the truth more clearly, let us imagine a reference frame founded on two or more straight lines extending out from the distant star, and both or all of them maintaining absolute directional rest, or non-rotation. We may now understand that the earth, and with it the gun, will very surely be moving, relatively to that frame, in some direction utterly unknown to us, and with some velocity quite as unknown as the direction, while the velocity may even be vastly great in comparison to the velocity of departure of the ball from the gun. Thus it may even be the fact that the ball, in proceeding through space, may, so far as we can tell, almost as likely be increasing its distance from the star, as getting nearer to it; and, relatively to the reference frame attached to the star as already explained, the ball's straight course may be in any direction whatever.

I have now finished all that I would propose on this occasion to lay before you on the Laws of Motion, and the Law of Inertia and Principle of Chronometry. I aimed at being very brief, but in spite of my efforts, the subject compelled me to expand; and there is left now very little time reasonably available for the second of my intended subjects for this address.

I will now proceed, however, to make a few remarks in advocacy of more frequent and more consistent applications of force-tests than is customarily accorded to them, for the attainment of safety or abatement of danger in various kinds of engineering structures. I have already included this subject in an introductory lecture delivered in the University of Glasgow on the occasion of my first entry on my professorship there, and that lecture was soon after published. There may therefore not be much reason for my renewing at any considerable length on this occasion the arguments there put forward; and I think it best at present to do little more than to offer a very few recommendations and suggestions.

In many of the materials and structures, on the sufficiency of which in respect to strength, human life is staked, there may exist, through various modes of origin, unseen flaws or imperfections. There may also be faults of design, in some cases, as to which the best available science applied through methods of calculation may be practically inadequate for their avoidance. In many such cases where force-tests are not applied at all, or if applied, are not severe enough or not often enough renewed, faults or imperfections may pass unnoticed which might be brought to light by such a test as would do no harm to the structure if free from the defect or flaw in the material or in the design.

In respect to boilers as commonly used on land, I do not think there is any established system, or prevalent usage, for ascertaining their sufficiency, in respect to strength, better than the system practised under the Board of Trade for the boilers of steam ships carrying passengers. Now in the Board of Trade's instructions to their surveyors, while it is made a rule that surveyors should see all new boilers, and boilers that have been taken out of the ship for a thorough repair, tested by hydraulic pressure up to at least double the working pressure that will be allowed, which allowance for working pressure is intended to be arrived at from inspections and measurements through calculations under prescribed rules and formulas, and not by the hydraulic test. It is also stated that in the case of old boilers care is to be taken, in testing, not to overstrain them, but that *the test must always exceed the working pressure.*

Thus, while for new or thoroughly repaired boilers, there is provision for a proved or ascertained strength, double of that intentionally allowed to be brought into play in ordinary work, yet in the case of old boilers, which may have undergone much corrosion and damaging usage in many ways, there appears to be no provision for any definite amount of proved excess of strength beyond that called into exertion in ordinary work under the allowed working pressure. But instead of the provision of any, even small, specified margin for safety, there is a caution given that *care must be taken not to overstrain the boiler,* coupled with the instruction that *the test pressure must always exceed the working pressure.* The tendency of this instruction on the mind of a careful and submissive inspector—submissive to the instructions of the higher authorities—must naturally be to make him, when a boiler appears rather critical and of dubious safety, tend to subject it to scarcely any excess of pressure beyond the sanctioned working allowance. Now, I do not think this is a desirable state of things. It appears to me that the best formulas and other means for calculating, estimating, and guessing the capabilities of endurance of a boiler, can bear no comparison with the hydraulic test as to trustworthiness. Strong confirmation of this opinion, it appears to me, can be drawn from an instruction given by the Board themselves, in their paragraph 73, viz., "*Strength of Boilers to be Ascertained, and Working Pressure fixed by Calculation.*—Before testing a boiler, the Surveyor should examine it, take the necessary measurements, and calculate what the working pressure should be, in accordance with the Board of Trade Regulations, and only test to double the working pressure; if the test is not satisfactory, the defects must be made good, and the boiler re-tested."

It seems to me that the idea put forward in the title of this instruction is rather different from that which is virtually brought out in the instruction itself. The title says that the strength of the boiler is to be ascertained, and the working pressure to be fixed, by calculation; but the instruction ordains that when the formulas, rules, and calculations have all been found in a particular case to have been in vain, and to have been fallacious guides for ascertainment of the actual strength of the boiler, that boiler is to be somehow altered, or amended, so as to increase its strength, until it can exhibit by the hydraulic test, not a vainly calculated strength, but a proved strength coming up to a previously calculated but practically unattained standard. Here it is certainly the force-test, not the formulated calculation or estimate, that is the final arbiter for deciding how the boiler may be worked.

The subject now touched upon would, if it were to be properly treated, open out to an indefinitely wide extent. Time would not now permit, and the present occasion would not be suitable, for entering into it with anything like the fulness that it deserves. I will only say further that I hope that in some of our future meetings in the Session now opening, the discussion of the subject through a much wider range of practical cases may be taken up by various members of our Institution.

On Approximation to Curves of Stability, from Data for Known Ships.

By Messrs F. P. PURVIS and B. KINDERMANN.

The discussion of this paper, which was held as read on the 22nd April, 1884, took place on the 28th October, 1884.

On the suggestion of the President,

Mr PURVIS gave a short resumé of the paper. In speaking of the curves appended, he described them as forming the chief value of the paper, and not simply illustrations. In starting on the consideration of the matter of approximation, they had set before them the determining in an approximate manner of some means by which to arrive at a knowledge of the stability of a vessel without having the labour of making the calculations involved in any known method—they wanted to discover some basis on which a satisfactory approximation could be founded, and in doing so they kept before their minds three primary considerations as affecting stability—viz., the dimensions of the vessel, its form, and the height of its centre of gravity. The latter was outside the scope of any rule; but they were able to take up the question of the dimensions, reserving that of form to be treated of afterwards, either by themselves or others. For a vessel of the type to which the curves had reference, the means of determining the stability was provided, whatever the dimensions. They claimed that these curves gave, not an approximation only to the stability, but the absolute stability itself, and any one looking into them would see that this was the case, if only the vessel dealt with were of the type form. The original ship might be considered as an elastic body, which could be drawn out or contracted in any one of the three directions—length, breadth, and depth—and for any ship so produced, the curves gave the righting arms absolutely for angles of 15, 30, 45, 60, 75, and 90 degrees, and for a position of centre of gravity coincident with the metacentre. What they had thus done, was to cover the ground for one type of ship only; they wanted next to see results that would be applicable to a ship of any type. In the practical use of the curves given for approximating purposes, two difficulties would be sure to arise. First of all, the sheer of the new ship would probably not be the same as that of the type ship, and as the sheer has considerable effect at large angles of heel, absolute trustworthiness could not be obtained. They had attempted to lay down guiding principles to overcome this difficulty. A more important difference would arise if, having the dimensions of a certain vessel, and making use of the curves on Plate XVII. (see Vol. XXVII.) to obtain the height of the metacentre, the user were to find that the height of the metacentre thus obtained was much different from that of the metacentre of the actual vessel. This point Mr Purvis illustrated on the blackboard, and showed that the assumption which would probably give the best result was that the righting arms given by the curves were for a position of centre of gravity identical with the metacentre of the actual ship, and not with the metacentre of the type ship.

Mr Robert Mansel said that the problem to the solution of which Messrs Purvis and Kindermann had addressed themselves, was one of great difficulty, which had been attempted to be solved in many ways; and, doubtless, each of them, to its inventor, appeared the simplest and best. For his own part, he should not like to place confidence in any generalized approximation, unless it were checked by direct calculations on the actual vessel in question. It was praiseworthy in Messrs Purvis and Kindermann to investigate the question, so as to see how far it was possible to approximate with type ships of the same geometrical form but different dimensions; if guarded against the error of assuming that the centre of gravity of each respective vessel was in the same relative position. Many years ago,* he had gone some way into the system of applying the calculated abstract ratios of type ships to others of different

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* See "Scottish Shipbuilders' Transactions," October, 1861.

dimensions, but he had not attempted to carry it so far as now proposed, to the comparison of stabilities. The real difficulty lay in the determination of the exact position of the centre of gravity of the hull and its lading; the above, and other recently proposed systems, dealt with the variations of the centre of buoyancy, to the neglect of the equally involved and necessary variations of the height of the centre of gravity, as stated in the following quotation from the above-noted paper: "The complete investigation of the stability of floating bodies, upon strict mechanical principles, has been published a few years ago by Canon Moseley, who properly named the object of his investigation the dynamical stability of those bodies. Thus, instead of considering the moment of restoration when the vessel is deflected through the given angle, Canon Moseley proposes to calculate the work which must be done during the motion from the upright to the deflected position. Obviously, the more work done in deflecting the vessel through a given angle, the greater is her stability. Now, to do work upon the vessel, we must raise her centre of gravity, or depress her centre of buoyancy; but, in the deflection, we may find that the centre of gravity sinks, or the centre of buoyancy rises. In either case, the corresponding work is antagonistic to the stability, and must be subtracted from the work to be done. Hence, considering the variation of the height of the centres positive when they go to increase the stability, and negative when they tend to diminish it, then the algebraic sum of the variations of height of the centres of gravity and buoyancy, in moving from the upright to the deflected position, multiplied by the weight of the vessel, is the measure of the dynamical stability of that vessel. In addition to this, in all ordinary cases, in the act of heeling we will have water displaced and moved aside. The work involved in this motion goes to increase the dynamical stability, and ought to be included in a strict investigation, but would be much more difficult and uncertain to calculate than even the position of the centre of gravity. . . . One deduction of theory, confirmed by experiment, however, is worthy of notice. It is this: a pressure, such as a sudden and constant gust of wind, will deflect a vessel through twice the angle at which the same pressure would keep the vessel permanently deflected."

Mr J. **MACFARLANE GRAY**, of the Board of Trade, London, as a stranger asked permission to say a few words. He was afraid, in reference to what Mr Mansel had said, that that gentleman's remarks might convey an erroneous impression. There could be no question that as regards absolute trustworthiness, that the tables given being accurate for the type ship, must also be accurate for similar vessels, which could be derived from the type ship by either lengthening or shortening, or crushing or expanding it sideways, so that any one using those tables to get the stability of a vessel of the same type was in reality going to the actual vessel itself—the very thing Mr Mansel said he preferred to do—without instituting special calculations, or doing the work himself. He was of opinion that what was wanted was to get the stability of a vessel with the same ease and accuracy as at present they could work a problem in trigonometry, by merely turning up a table, get the values of sines and tangents. He thought they all must be very much indebted to Messrs Denny's staff for having brought that paper before the Institution.

Mr **PURVIS** said he had to thank Mr Gray for his remarks, which quite answered Mr Mansel's objection. The question of the variation of type Mr Biles had touched upon in his paper last session, giving curves of stability for different vessels of the same dimensions, and at one particular draught; one vessel especially a very fine and another a very full one. What Mr Biles had done for one draught he would like to see carried through all variations of draught. Of course, that would involve a great deal of work. He looked upon the present paper, laborious as it had been, as merely introductory to the subject, and he trusted they would yet make further investigations on the effect of a variation of the types.

Mr J. **MACFARLANE GRAY** remarked that he might have said when on his feet that the method of types is not absolutely novel, for he had found recently the germ of the same treatment of this subject in an ancient book. Archimedes carried out the same investigation 2100 years ago, in what is substantially the very same way as Messrs Purvis and Kindermann had done now. It appeared that he had worked out with perfectly mathematical accuracy the problem for parabolic conoids of all proportions, and that he had formed rules for comparing the stability of those, and that the ships at that period, so far as the question of stability, might be validly treated as of this parabolic form. He (Mr Gray) explained that if these conoids were extended lengthwise, made say twelve times longer, retaining each section as before, they would get a figure which would not be unlike the vessels of the ancients; so that Archimedes might be said to have investigated the problem of the stability of ships of his time substantially as the authors of this paper were doing now—and there is nothing new under the sun. The merit of origination was still due to the authors of the paper, and when he pointed out that in what they had done they had been working alongside of Archimedes, Messrs Purvis and Kindermann would not object to the companionship.

The PRESIDENT said that he was sure the efforts of Messrs Purvis and Kindermann to bring out new methods intended to save labour, and to render information more readily available, deserved their praise and thanks. He trusted they were ready to give a warm vote of thanks to these gentlemen.

The vote was unanimously agreed to.

On Manipulating the Material, and Building, and Drilling the Great Tubes of the Forth Bridge.

By Mr ANDREW S. BIGGART.

(SEE PLATES I., II., III., IV., AND V.)

Received 22nd; Read 25th November, 1884.

THE Forth Bridge has on various occasions formed a theme of deservedly widespread interest, and the general character of the undertaking, is more or less familiar to engineers. A comprehensive view of the subject, and of the numerous engineering questions involved, has also been lately so ably given by Mr Baker, that the writer purposes in this paper to at once pass on to examine some of the later details of the manufacture of the superstructure; such as, that of the work in connection with the great tubes.

One of the well-known features in the design of this undertaking demands that struts of hitherto unequalled length, and capabilities for resisting thrust, be employed. The form which best fulfils these conditions is the tubular.

As well nigh six miles of tubes are required in the completed Bridge, it at once becomes evident that the construction of them could only be effected within a reasonable time, by the adoption of special plant. Owing also to their novelty of form and great size, no machinery was in existence capable of dealing with such work. On account of this, and for various other reasons, Mr Arrol determined to design special plant for the whole work, the description of a part of which, and the mode of working the same, can be but scantily treated of in this paper.

The struts required are of various dimensions, ranging from that of the largest, which is 12 feet diameter, to that of the smallest, which is only 3 feet. The description of the former will be considered in this paper, although all are very much alike in design.

Fig. 1, Plate II., shows the cross section of one of the 12 feet horizontal tubes between the piers. It consists of 10 plates and 10 longitudinal H beams, stiffened at intervals of 8 feet by means of the circular girders shown in elevation. The girders, again, are made up of diaphragm plates, connected to inner and outer angles, the former being riveted to the H beams, while the latter are similarly fixed to the tube plates.

The work to be performed is somewhat as follows :—

The first, and, for a time, the most difficult operation (owing to causes to be hereafter referred to) was the curving of the heavy plates. These, it may be mentioned, are 16 feet × 4 feet 4 inches, × 1⅛ and 1¼ inch thick, and weigh from about 28 to 32 cwt each. The method now adopted is to bend them while hot in a large hydraulic press, from which they are removed, and allowed to cool slowly. When cold they are again placed in the press, and straightened finally. The edges and ends are then planed, and each plate is weighed, marked, and laid aside, ready to be placed on the tube when required.

The longitudinal H beams are made up of a deep webbed tee and two angles, being partly drilled through these before erection. The circular girders are also partly drilled before being placed on the mandrel. These different parts form the main tube proper, leaving out the connections to skewbacks, the girder fixtures, tees, and other minor details, with which it is not at present intended to deal.

The tubes are built round about a mandrel, being supported therefrom by temporary connections, and drilled through the various parts, while in the exact form they are intended to be when finally erected. (See Fig. 2, Plate II.)

This hasty sketch of the different steps of the work, required to be executed, will enable the details to be more clearly followed.

The plates are heated in gas furnaces, of the style shown in Fig. 2, Plate III. The producer is close at hand, and from it the gas is led along the tube T to the box B, and thence distributed to the different furnaces by means of other tubes.

The gas is admitted at the side, as well as the back of each furnace. This, by the way, was an afterthought, to enable the plate to be more evenly heated, than when the gas was admitted at the back only, and it turned out a decided improvement.

The escaping gases pass off through the flues G, which are highly heated thereby, and these in turn give a part of their heat to the incoming air, which is then passing along the flues A, on its way to the open furnace.

The plates to be curved are heated to a dull red, after which they are withdrawn from the furnace by means of a hydraulic ram. To the end of the chain from the ram is attached a pair of tongs, made so that the greater the pull required, the grip is the firmer. The plate is withdrawn from the furnace on rollers, and run over a table into the hydraulic bending press, shown by Figs. 4 and 5, Plate III. A pressure of 800 tons is now applied while the plate is between a set of convex and concave blocks of the form necessary to bring it to the proper curvature.

Almost immediately, on the blocks being separated, the plate is seen to be undergoing a change of form, and this so quickly, that it is quite perceptible to the eye. Sometimes the convexity becomes greater, while at other times it is the reverse. In all cases the plate warps longitudinally, this taking place principally at the ends. The distortions are most irregular and inexplicable, a plate seemingly under exactly the same conditions assuming a totally different form, nay perhaps the very reverse of that taken by the immediately preceding one. On being removed and allowed to cool, the plates gradually become in almost every case somewhat better, but scarcely ever sufficiently so as to be suitable for the purpose for which they are intended.

Many methods were suggested, and tried, to overcome this warping of the plates: thus, for instance, the edges were covered up, thereby allowing them to cool more from the centre; another mode was to reheat and give them a second squeeze; yet another was to allow them to cool partly, lying on a series of iron rollers, set to the true form the plate should take. These and others gave only very varying success. The plan finally adopted was to curve a quantity at a time, laying each plate, as it left the press, on the top of the immediately preceding one, with a layer of ashes between, and allow them to cool in piles of convenient size. When cold, each plate is again placed in the press, and straightened by means of repeated squeezes, strips of thin iron being placed above and under the points necessary to be brought to the true form. This answers the purpose admirably, and is the only method now in vogue.

A somewhat striking incident happened during these preliminary trials. It arose out of an attempt to bend one of the 1⅛ inch thick plates while cold. During this process the plate cracked in several places, although the curve was only equal to that of a circle with a six feet radius. Samples of bending and tensile tests were cut off, and showed the plate to be of remarkably good material, and quite up to the specified quality. (See Fig. 3, Plate IV.)

Mr Arrol attributed the failure to unequal cooling at the steel works, and this is borne out by the fact that different parts of the same plate are not uniformly easy or difficult to cut, but both these experiences are often found in a single plate.

Mr Baker thought the failure of the plate to stand the bending was due to the fact that its edges and ends were not planed, but in the state they were in when they left the shears at the works. He had made a series of experiments with sheared and planed plates, and from the results obtained arrived at this conclusion

Annealing removes satisfactorily both these objections, and in this lies the great benefit of bending the plates while hot, and allowing them to cool as described.

The hydraulic press (see Figs. 4 and 5, Plate III.), for bending the plates, consists of a set of four 24-in. cylinders C, resting on two cast-iron girders G, and supporting by means of two 7-inch wrought-iron columns, from each cylinder, a fixed table T overhead. On the top of the rams is placed another table T^2, which is raised and lowered in conjunction with the rams. Between these two tables are placed the blocks B required to bend the plates to their particular form, equal in this case to a curve, the radius of which is six feet. The pressure brought to bear on the plates while being stamped, is about 800 tons, provision being made for doubling this if necessary. The lower pressure has thus far been found sufficient for all purposes.

After bending one of the first plates, it was kept between the upper and lower blocks for a few minutes, while it was yet hot. The consequence was that the side of the block next the plate heated much more rapidly than the other, or remote side. This induced a very heavy strain on the metal, so much so that it broke the upper one completely through, at the same time giving a report somewhat resembling that of the discharge of a pistol.

The plates after having been brought to their true form, as already described, are passed on to a large planing machine, as shown (see Fig. 1, Plate IV.), to be there planed on both sides.

This is an ordinary planer, having the table driven by a six-inch screw, but supplied with double cheeks, between which, on each side, is fixed the special tool boxes A A. The plate being operated upon, is placed on cast-iron curved blocks B, and held down to the table by means of bolts and draw washers, arranged so that they can be either quickly tightened up or loosened at will. Both tools cut simultaneously, and as the plate is being travelled backwards, as well as forwards. They are fed into their work, and reversed, by hand.

When the sides have been planed, the plates are placed in another machine (see Fig. 2, Plate IV.), to be there planed on both ends.

They are here supported from and held down to the table at one end in a manner similar to that adopted for the former machine, while at the other they are fixed by means of screws through the beam overhead. Wedges are also driven in lightly, at the sides, to prevent the plate being shifted sideways by the action of the tool while cutting. The tool box, and tool T are carried in a pendulum P, receiving its motion from the travelling saddle S, by means of the connecting rod R. Here also the tool is fed, and reversed, by hand.

The pendulum P, and plates Q, have a series of holes H, each of which in its turn will be made a new swinging centre, to enable the tool to sweep the ends of the plates for the various sizes of tubes· After one end of a plate has been planed, it is reversed, and the finished end butted against a cast-iron face plate, set parallel to the plane of motion, of the cutting edge of the tool, thus making the two ends of each plate truly parallel, and thereby securing accuracy at the tube joints.

Being now finished, each plate is weighed, marked, and placed on the pile with the nearest corresponding weight, ready to be removed, and built on the tubes, when required.

In the shops the handling of these plates, as well as the rest of the heavier parts of the structure, is done almost wholly by hydraulic cranes of the style shown in Fig. 1, Plate III. In the ground there is placed a cast-iron box B, which carries the cylinder A. This again carries a mast consisting of two channels, having a hollow turned casting H at the bottom, and a solid one S at the top. In the centre of the latter is fixed a pin, which allows the mast to turn, carrying with it the jib, as in an ordinary crane, but with this great difference, that it can make a complete circle. The jib is fixed to the piston rod P by means of cross-heads, which are at the same time made to carry the supporting wheels, running inside the channels of the mast, and bearing against the flanges, By this means the bending moment of the jib, and the weight thereon, are taken up, while the downward thrust is passed into the piston rod P. When loaded, the cranes are capable of handling fully two tons, with ease.

The H beams, formerly mentioned, are built up of tee and angle bars. These have first of all to be straightened, which is accomplished, in the case of the former, by means of a 15-inch hydraulic ram, and, in that of the latter, by an ordinary bending machine.

All are then cut to the exact length of 32 feet, by a cold steel saw, moving with a velocity at the periphery of about 70 feet per minute.

They are now ready to be built into cast-iron blocks on the drilling tables, and in building it is carefully attended to, that there is a distance of 16 inches between the different joints of the angles, while those of the tees are placed midway between those of the angles.

To secure good butting, the end of each new beam about to be built is placed hard against the one already set up, and to which it will be joined when placed in the tube; that is, the end of the new one, is brought hard up against that of the old one, tee to tee, and angle to angle.

Everything being now in order, drilling, and that through angles and tees at one and the same time, proceeds, by means of radials of the style shown (see Fig. 2, Plate V.). When this part is finished, the beams are marked and laid aside till required to be placed in position, on the mandrels, where, as is evident, the joints will meet as fitted in the shop. Thus this section proceeds, beam following beam till the total length required is completed.

It may be mentioned that, as regards the arrangement of the tables, they are placed on each side of the line of radials, which allows building up to go on at the one side while drilling is being proceeded with at the other.

To ensure accuracy in the form of the tubes, and also correctness in workmanship, the stiffening circular girders required (see Fig. 1, Plate II.) are built within a wrought-iron ring, the inside diameter of which is 12 feet, this being the mean size of the tubes at present under consideration. Within this ring, and at equal distances apart, are placed ten castings, each of which occupies the same relative position to the different parts of the girder as the longitudinal beams in the completed tube. These are also of a form suitable for carrying up the various parts of the girders while being drilled. When the girder has been built in this iron frame, all the holes are marked off to templet, and afterwards drilled by a radial, the centre of the column of which coincides with that of the girder.

Although some of the girders vary slightly in diameter at some parts, according to whether they are off or on joint covers, this is easily overcome by fixing temporary packing strips against the ring to suit the new dimension to which they are of necessity built. After drilling, the separate parts are all marked, removed, and now bolted together, awaiting erection on the mandrel.

The angle iron rings for these girders are curved on a large cast-iron segmental block. A pin is fixed into the centre of this segment, round which a large wrought-iron arm carrying a curving wheel is moved. This wheel is of a form suitable to bear against the outside or inside of the bar as may be required. When the angle is heated, one of the ends is grabbed behind the wheel to the segment, and the arm is now gradually moved round. The wheel bearing hard against the angle brings it close to the face of the segment, and it thus receives the proper curve. To assist the curving, a crow bar has to be used in front of the curving wheel to bend up the angle, while behind the wheel additional grabs keep it close to the segment. After the full bar has been curved in this manner, the grabs are removed and the wheel run backwards and forwards several times, and then it is ready to be removed, and with a little trimming up, fit to be used in any of the girders.

Having now considered the principal parts which compose the tubes, the building and drilling of these will naturally follow. This work is done out in the open, on what is called the drill roads. (See Fig. 4, Plate IV.) These are laid down to suit the drilling machines, and at such a distance and with such a length as to allow the bracing girders and connections thereto to be placed in position, as the work stands on the ground, prior to the final erection.

The roads are so arranged as to be all equally suitable of access for the steam travelling cranes used in carrying the material to position, and in building the tubes. This is accomplished by means of traversers, of which there are three, one in the centre and one at each end of the drill roads, those at the ends running on rails, at right angles, and close to the main roads, but fully twelve inches lower, while the centre one is run on cross rails, on the same level as the main roads. If it is necessary to change the position of a crane, it is run on to the traverser, and on it carried to the desired point, and there run off. In this way, the whole of the ground is commanded by the cranes. It has already been said that the material for the tubes is placed in position by means of these cranes, the work of building, as required in any of them, will now be described.

Fig. 2, Plate II., shows the style of building. First of all there is the mandrel M, 45 feet long by 5 feet diameter, raised on iron trestles T, to a height, at the centre, of 10 feet from the ground. This corresponds with the centre of the outer rings of the drilling machines.

The great length of mandrel required is to allow of its being carried up at the ends, when the H beams and plates are built in position. On this mandrel there are now secured, but in halves, temporary iron rings R, at the horizontal distance from each other of 8 feet. To these are fixed the radiating plates P, having holes punched in the outer end for bolting on the first part of the permanent work, viz., the inner angle A of the circular stiffening girders. The same bolts are also made to carry the web plates W, of those girders, on the outer edge of which is fixed the angle irons I, for making the final connection to the shell of the tube. The horizontal H beams H are now placed in position, being securely bolted through the inner angle of the circular girders. On these beams are now placed the shell or tube plates S, the ends forming butt joints, while longitudinally they lap one another, this taking place over the solid flange of the H beams. The end joint of the one plate breaks opposite the centre, or solid part of those on either side. The first plates to place in position are the inner, or those lying close against the flange of the beams, beginning generally at the bottom, and coming up on each side. Owing to the passing of the one plate beyond the other, one half of each remains free to put grabs and drawwashers on, without interfering with the placing of the outer ones in position. So soon as the outer ones have been put on, and fixed in a similar manner, there is passed round all a couple of angle iron rings, for binding and drawing them up to their proper position. The tightening up is done by means of iron wedges, between the plates and the rings. After the bottom plates have been fixed in position, the tube is borne up by wooden blocks, built between it, and the cradle underneath.

The true position of the tubes, both as regards horizontal distance apart and height, is found by means of a theodolite, placed at one end of the roads, on a fixed platform, in a position such that when it is in line with a stationary point at the other end it always fixes the centres 120 feet apart throughout, and horizontally in the same plane. If the centre of the mandrel is not in this line, then it is made so by being raised, lowered, or shifted sideways to suit. When the mandrel is right, the tube must of necessity be so also, seeing the centres coincide.

When the building of one ring of plates has been completed, the drilling machine is moved forward, the blocks in front being taken out of the way, and rebuilt behind, as it is travelled along. To enable the drilling to go on continuously, the building of the tube in front is being proceeded with, while the machine is still at work on the portion immediately behind. These tube drilling machines (of which there are four) are shown by Figs. 1, 2, and 3, Plate IV.). Each is self-contained, and on being run along the rails carries all with it. The principal parts are, the wrought-iron underframe or carriage A, on the one side of which is fixed the engine E and boiler B, and two large cast-iron rings C, firmly bolted to the main cross girders. These rings have an internal diameter of 13 feet, sufficient to enable them to pass freely round the tube, when the machine is being moved along. Five cast-iron slides D are fixed thereon, and held in position by means of small slipper blocks F, fitting into a recess, in each of the rings C. On each of the slides, are the two heads H H. Each head is provided with a single drill, and is capable of being rapidly run from one point of the slide to another by rack and pinion gearing. The slides are kept in position, and also turned round the rings C, in either direction, by means of two worms W, carried in brackets F, one gearing in each ring in the circular racks R. These racks being bolted to the rings, serve also as guides, for steadying the whole upper portion of the machine. All the drills point to the centre of the tube, and having, as shown, both a circular and longitudinal motion, can with ease be made to reach every hole in any part of the structure, some of which are through a depth of as much as four inches of solid metal.

It might be here mentioned, that some of the slides were specially designed to overcome the difficulty of drilling, say, a flat part in any of the tubes. The difficulty lies in the fact, that the drills on any of the fixed heads always point to the centre of the tube, whereas in the case just mentioned, the holes require to be drilled at right angles to the special or flat part. The mode adopted to overcome this was to make both ends of each slide circled, fitting them into separate heads, which in turn were bolted to the slipper blocks F, as in the others. On the head at one end is placed a worm, while on the same end of the slide there is keyed a wheel into which the worm is geared, by turning which the slide can be made to place and keep the drill pointing in any required direction.

The whole of the drills are fed into their work by an automatic arrangement, the motion being imparted to the longitudinal shaft L, by a band driven of the main driving pulley. On this shaft slides, and by it also are driven the worms W^1, necessary for turning the worm wheel I, which at will can be made to drive the hand wheel R, thereby feeding the drill into its work. At one end of each of the main slides is overhung the driving pulley P, the power being transmitted from the engine to the whole of these by means of a cotton rope, guided where necessary by supplementary pulleys. The slack is taken up by a shifting quadrant Q, moving about the engine shaft as a centre, assisted by auxiliary pulleys on the wrought-iron frame close by the engine.

When starting work on any tube, a drilling machine is moved forward to the point at which operations are to begin. Each of the five slides is now moved round the rings until the points of the drills face truly any series of holes in the longitudinal beams. The holes in this line, or series, are all drilled, two drills being at work on each line, then the slides are again placed so as to suit a new set, and so on until the whole of the tube commanded by the machine in its present position is finished. This is equal in length to 8 feet, and includes the full circumference of the tube. The number of holes in such is about 800, and the time required to drill all, when working continuously, is from 24 to 28 hours, varying thus much principally on account of the difference of thickness of the various parts of the tubes. The machine is in like manner made to drill the whole length of the tube.

At several of the ends of the first four of these tubes are presently being erected the skewbacks, each a complicated connection of five different tubes, including one end of these just described, and also several heavy bracing girders. Into this, however, it is not proposed to enter at this time. At present other tubes are being treated in a manner similar to that already described, which shows if anything is yet required, that special work can only be grappled with to advantage by the free use of special plant.

After the reading of the paper,

Mr F. W. DICK said he had listened with very great pleasure to the paper read. As a steel maker he might be expected to refer to the cracked plate which had been so pointedly brought to notice. and the failure of which some people might wish to class with the mysteries with which it was the fashion at one time to surround steel. He thought it was due to the Steel Company of Scotland with which he was connected, to say that the plate referred to was not manufactured by them. He had no doubt that the cracking was due to internal strains which might have been occasioned by the plate being put out to cool in the wet in rainy weather. He had noticed in the *Engineer* of this week a letter to the Editor, in which the writer affirms that at the Forth Bridge Works the steel was treated in an extremely barbarous manner--he said, indeed, that it was most cruel treatment; and also that if he had had the work to do, he would have left it to the steel makers. He (Mr

Dick) did not know that steel manufacturers could curve plates better than they were being done by Mr Arrol. He had no doubt that the writer of the letter had in his mind the rolling of the plate in long strips and to the proper curve, but he (Mr Dick) did not think that any steel maker would undertake such a job. The heating of the plates before bending was a beneficial measure. It prevented the danger of any failure of the plates from such causes as those to which the failure of this plate in question was due. It was really an annealing, and he thought the plates were treated in an admirable manner. The final setting of the plates after they had become cold was so slight that it would not set up much strain. A test piece from every one of these plates was brought to a cherry red-heat cooled in water, and then doubled up till the inner radius of the curve was 1½ times the thickness of the plate, so that the plate had to stand a bending test the same as was required by the Board of Trade or by Lloyd's Registry. He thought they were very much indebted to Mr Biggart for his paper.

The further discussion of the paper was adjourned till next meeting, when a vote of thanks was unanimously awarded the author for his paper, on the proposition of the PRESIDENT.

On Energy and Entropy and their Applications to the Theories of Air and Steam.

By Mr Henry Dyer, C.E., M.A.

Received and Read 25th November, 1884.

Introduction.

When I received an invitation from the Secretary to read a paper before this Institution, it occurred to me that I might give an account of some of the most recent applications of thermo-dynamics to problems connected with heat engines, and especially with the steam engine, as I found in looking over the Transactions that comparatively little had been done in that direction. It was, however, suggested to me that before entering into consideration of applications it might be well to give, especially for the benefit of the younger members, a resumé of some of the more important formulæ which occur in such investigations. In attempting this in the following paper I have avoided the detailed consideration of principles, and have simply shown how some of the expressions for air and steam may be obtained, with the object of affording a starting point from which investigations relating to their applications may be made.

It is a well known fact that in the earlier days of the steam engine the most important improvements made in it were always the consequence of the discovery of some physical law, or property of steam, and that Smeaton and Watt endeavoured to make their practice conform to what they knew of the principles involved, as nearly as possible as the circumstances under which they worked would permit. Notwithstanding the progress which has been made in recent years the same cannot be said of present mechanical engineering practice, for that is a long way behind the theoretical knowledge we possess, and the improvements which have been made seem to be due, not so much to keeping the principles of the action of steam clearly in view, as to the experience gained by a system of trial and error. This, of course, is to be accounted for partly from commercial considerations, but I am afraid it is chiefly due to the impression among engineers that the study of thermo-dynamics, however useful it may be as an intellectual exercise, and interesting as an accomplishment, is only of slight practical value. Hence we find that its laws have not hitherto guided practice to any great extent, but have rather been used to explain progress which had been accomplished after many experiments and trials. Such experiments and trials are of the utmost importance, and I have no wish to undervalue them in the smallest degree, but much time and money may be wasted if they are entered upon without a knowledge, at least, of the results of what has been done by others in the way of theoretical investigations, for science surely ought, if it be worth anything, to anticipate to a certain extent the lessons of experience.

On the Continent, in recent years, much more has been done than in Britain to advance the knowledge of the practical applications of thermo-dynamics to the theory of heat engines, and such works as those of Clausius, Zeuner, Hirn, Röntgen, Combes, Hallauer, Ledieu, and others are beginning to exercise a most important influence on the practice of mechanical engineering, an influence which will very soon reduce to zero the great advantage we have had in practical experience, and in cheap coal and iron, unless we are prepared to go in for the more thorough study of the application of principles. For many years Professor Rankine's treatise remained the only one in the English language in which a theory of the steam engine founded on thermo-dynamics was given, but it is now known that from 15 to 50 per cent. of the steam used in engines is not accounted for by his theory. D. K. Clark in England, and Isherwood in America, many years ago investigated the cause of this loss, but it is to Hirn that we are chiefly indebted for the advances which have been made on Rankine's work. I am afraid, however, that the results of his investigations are not well known to British engineers. Professor

Cotterill, in his work on the Steam Engine, has given some account of them, as well as of some other experiments and investigations bearing on the subject, and thus has been able to show the progress made in the theory of the steam engine since Rankine left it, and this is almost all that has been done in Britain. The investigations of Hirn, Cotterill, and others, do not in any way lessen the value of Rankine's achievements, which must for ever be memorable in the history of science, but rather by making them more complete, place them more strongly in relief.

Other fields of inquiry in connection with thermo-dynamics, in addition to what is included under the ordinary forms of heat engines, are gradually opening up. Machines worked with compressed air, and apparatus for cooling and freezing have made rapid progress in recent years, both in Britain and on the Continent, and these form almost the only exceptions to the statement that theory has not guided practice, for I think it will be admitted that without a knowledge of thermo-dynamics no such progress would have been possible.

Investigations relating to thermo-dynamics are peculiarly appropriate to an Institution which had Rankine for its first President, which has Sir William Thomson, Joule, Clausius, and Helmholtz as honorary members, and which has now Professor James Thomson for its President, and as these men placed the laws of thermodynamics upon a firm basis, it is surely not too much to hope that the engineers of the Clyde will strengthen their position by taking greater advantage of the assistance which a more thorough knowledge of these laws and their applications would afford them, and place before the Institution some of the results of their work.

SECTION 1.—GENERAL EQUATIONS.

The first law of thermo-dynamics, which expresses the fact that heat and mechanical energy are mutually convertible, is represented algebraically by the equation

$$H = JQ \qquad \text{(1)}$$

$$\text{or } Q = AH \qquad \text{(1a)}$$

where H represents the number of units of work required to produce Q units of heat. J is Joule's equivalent and A its reciprocal.

If a number of operations be carried on at the same time this equation may be written

$$H = J\Sigma Q \quad \dots\dots\dots\dots\dots\dots\dots (2)$$

or since H may be either positive or negative

$$H + J\Sigma Q = 0 \quad \dots\dots\dots\dots\dots\dots\dots (2a)$$

an equation which should be considered the *generalised expression of the First Law.*

If the element of the external work done by an expanding body be represented by $p\,dv$, where p is the external pressure, and dv the increase of volume, then from this law it follows that if dQ be the heat expended that

$$J\,dQ = dH = dU + pdv \quad \dots\dots\dots\dots\dots\dots\dots (3)$$

where dU is equal to the dynamical equivalent of the heat expended, minus the external work done. The quantity U is called by Sir William Thomson and Clausius the *energy* of the body, and is equal to what Zeuner and Cotterill call the *inner* or *internal work.*

Equation (3) is not generally integrable, but it may be made so by multiplying by a certain factor. It follows from the Second Law of Thermo dynamics that the reciprocal of the absolute temperature is such a factor, so that $\frac{J\,dQ}{\tau}$ or $\frac{dH}{\tau}$, a quantity which we will always designate by $d\phi$, is an exact differential. ϕ is what is called the *entropy* by Clausius, the *thermic weight* by Zeuner, and the *thermodynamic function* by Rankine. We will adopt the first of those names.

If we have a source of heat at the absolute temperature τ_1, and a refrigerator at the temperature τ_2, and if JQ_1 be the quantity of heat absorbed by any apparatus for converting heat into work, then the available energy, that is the energy which can be converted, is given by the expression

$$J\,(Q_1 - Q_2) = JQ_1\left(\frac{\tau_1 - \tau_2}{\tau_1}\right) \quad \dots\dots\dots\dots\dots\dots (4)$$

where Q_2 is the heat rejected by the apparatus. Equation (4) was first given in this *shape* by Sir William Thomson, but its principle was enunciated long before by Carnot, and hence is generally called *Carnot's Law of Efficiency,* and for all practical purposes it may be considered the *Second Law of Thermo-dynamics.* In this form it shows the relation between the First and the Second Laws, a relation which is easily understood when we consider the case of the steam engine. By the First Law the dynamical equivalent of a given quantity of heat Q is JQ, while the Second shows us that only a fraction of this can be converted into work, a fraction which depends for its value on the relative temperatures of the boiler and condenser, and as the range between these two temperatures is necessarily limited, we cannot utilise all the energy which, according to the First Law, is resident in the fuel. The idea which naturally presents itself to those who know the First Law and are ignorant of the Second is, that heat being a form of energy, in a perfect engine the whole of it might be converted into work, that is to say that the actual work as measured by a brake on the crank shaft should be equal to the dynamical equivalent of the heat expended in the furnace. Such a view the Second Law shows to be entirely false.

Equation (4) may be written in the form

$$\frac{Q_1}{\tau_1} - \frac{Q_2}{\tau_2} = 0 \quad \text{.......................} \quad (4a)$$

from which we infer that the *quantity of work which can be performed by a body is solely proportional to its absolute temperature.* Hirn remarks that this is the most striking, clear, and simple statement of the Second Law of Thermo-dynamics. Clausius and Sir William Thomson extended this expression to the form

$$\Sigma \frac{Q}{\tau} = 0 \quad \text{...........................} \quad (4b)$$

so that if any body undergoes a complete cycle of operations of a perfectly reversible kind, as for instance in Carnot's reversible engine, the algebraic sum of the quantities of heat it receives, divided respectively by their corresponding absolute temperatures, is equal to zero, or in other words the sum of the quotients is unaltered by the passage of the heat through the body.

If the temperature of the different parts of the working substance alter gradually during the process, then equation (4b) may be written

$$\int \frac{dQ}{\tau} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4c)$$

equation (4a) is simply the definite integral of this between the limits τ_1 and τ_2.

If the cycle be non-reversible this equation is no longer true, for evidently then the left hand member is a positive quantity, if we consider heat taken in as positive, and suppose the engine to be one in which work is produced from heat, so that we may write

$$\int \frac{dQ}{\tau} \geqq 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4d)$$

an equation which may be considered the *generalised expression of the Second Law of Thermo-dynamics.*

The preceding explanations show us that the consideration of the quantities named in the title of the paper really embraces the whole field of thermodynamics, so that all that can be given in a single paper is a mere outline. It will be convenient to give, in the first place, a short resumé of the usual analytical expressions for the two chief laws of thermo-dynamics, for details of which, however, special treatises or papers must be referred to, our chief object now being to show their applications to the properties of air and steam.

An equation of the form $f(p,v,\tau,) = 0$, where p is the pressure, v the specific volume, and τ the absolute temperature, may be called the *characteristic equation*, and any two of these quantities may be taken as independent variables in the equations we form for showing the effects produced by heat.

Consider first v and τ as variables and p constant, so that when τ becomes $\tau + d\tau$ v becomes $v + dv$, $d\tau$ and dv both being very small. Then the heat required to produce those changes will be

$$dQ = c_v\, d\tau + l_v\, dv \quad \ldots\ldots\ldots\ldots\ldots\ldots (5)$$

where c_v is the *apparent specific heat at constant volume*, and l_v is what is usually called the *latent heat of expansion.*

Taking p and τ as variables we have

$$dQ = c_p\, d\tau + l_p\, dp \ldots\ldots\ldots\ldots\ldots\ldots (5a)$$

where c_p is the *apparent specific heat at constant pressure*, and l_p is a thermal capacity without special name.

Lastly taking p and v as variables we have

$$dQ = h_v\, dp + h_p\, dv \dots\dots (5b)$$

where h_p and h_v are thermal capacities without special names.

The following relations exist between the quantities c_v, c_p, l_v, h_v, and h_p,

$$c_p - c_v = l_v \left(\frac{dv}{d\tau}\right) \dots\dots (6)$$

$$l_p = l_v \left(\frac{dv}{dp}\right) \dots\dots (6a)$$

$$c_p - c_v = - l_p \left(\frac{dp}{d\tau}\right) \dots\dots (6b)$$

$$h_v = c_v \left(\frac{d\tau}{dp}\right) \dots\dots (6c)$$

and

$$h_p = c_p \left(\frac{d\tau}{dv}\right) \dots\dots (6d)$$

By means of the last two expressions we may write equation (5b) in the form

$$dQ = c_v \left(\frac{d\tau}{dp}\right) dp + c_p \left(\frac{d\tau}{dv}\right) dv \dots\dots (5c)$$

The quantities c_p, c_v, &c., are evidently partial differential coefficients of U with respect to the different variables, and the following relations may be deduced.

For v and τ as variables,

$$\left(\frac{dl_v}{d\tau}\right) - \left(\frac{dc_v}{dv}\right) = A \left(\frac{dp}{d\tau}\right) \dots\dots (7)$$

For p and v as variables

$$\left(\frac{dh_p}{dp}\right) - \left(\frac{dh_v}{dv}\right) = A \dots\dots (7a)$$

or, since $h_p = c_p \left(\frac{d\tau}{dv}\right)$, and $h_v = c_v \left(\frac{d\tau}{dp}\right)$,

$$\frac{d}{dp}\left[c_p \left(\frac{d\tau}{dv}\right)\right] - \frac{d}{dv}\left[c_v \left(\frac{d\tau}{dp}\right)\right] = A \dots\dots (7a)$$

Lastly for p and τ as variables

$$\left(\frac{dc_p}{dp}\right) - \left(\frac{dl_p}{d\tau}\right) = A \left(\frac{dv}{d\tau}\right) \dots\dots (7b)$$

We have in equations (7), (7a), and (7b complete analytical expressions of the First Law of Thermo-dynamics with different independent variables.

It follows from the Second Law (as already remarked) that the expressions for the quantities of heat given in equations (5), (5a), and (5b) are made exact differentials by multiplying them by the reciprocal of the absolute temperature, and from this condition we obtain the following equations

$$\left(\frac{dl_v}{d\tau}\right) - \left(\frac{dc_v}{dv}\right) = \frac{l_v}{\tau} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$$

Comparing this with equation (7) we have

$$\frac{l_v}{\tau} = \mathrm{A}\left(\frac{dp}{d\tau}\right) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (8a)$$

or,

$$\tau\left(\frac{dp}{d\tau}\right) = \mathrm{J}l_v \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (8b)$$

$$\therefore \left(\frac{dc_v}{dv}\right) = \mathrm{A}\tau\left(\frac{d^2p}{d\tau^2}\right) \ldots\ldots\ldots\ldots\ldots\ldots\ldots (8c)$$

similarly we obtain

$$l_p = -\mathrm{A}\tau\left(\frac{dv}{d\tau}\right) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (8d)$$

$$\therefore \left(\frac{dc_p}{d\tau}\right) = -\mathrm{A}\tau\left(\frac{d^2v}{d\tau^2}\right) \ldots\ldots\ldots\ldots\ldots\ldots (8e)$$

and

$$\left(\frac{dh_p}{dp}\right) - \left(\frac{dh_v}{dv}\right) = \mathrm{A} = \frac{1}{\tau}\left[h_p\left(\frac{d\tau}{dp}\right) - h_v\left(\frac{d\tau}{dv}\right)\right] \ldots\ldots (8f)$$

Equations (8), (8a), &c., are complete analytical expressions of the Second Law with different independent variables.

Substituting in equation (5) the value of l_v given in equation (8a) we have

$$d\mathrm{Q} = c_v\, d\tau + \mathrm{A}\tau\left(\frac{dp}{d\tau}\right) dv \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (9)$$

or,

$$d\mathrm{H} = \mathrm{J}d\mathrm{Q} = \mathrm{J}c_v\, d\tau + \tau\left(\frac{dp}{d\tau}\right) dv \quad \ldots\ldots\ldots\ldots\ldots (9a)$$

and similarly from equations (5a) and (8d) we have

$$d\mathrm{H} = \mathrm{J}d\mathrm{Q} = \mathrm{J}c_p\, d\tau - \tau\left(\frac{dv}{d\tau}\right) dp \quad \ldots\ldots\ \ldots\ldots (9b)$$

By integrating equations (8c) and (8e) we obtain

$$\mathrm{J}c_v = k + \tau\int\left(\frac{d^2p}{d\tau^2}\right) dv \quad \ldots\ldots\ldots\ldots\ \ldots (10)$$

and

$$Jc_p = \mathfrak{k} - \tau \int \left(\frac{d^2 v}{d\tau^2}\right) dp \qquad \ldots\ldots\ldots\ldots\ldots(10a)$$

The quantities k and $\mathfrak{k}$ are called the *real dynamical specific heat* at constant volume and constant pressure respectively, and they are the dynamical equivalents of the amount of heat required to raise unit mass through one degree absolute temperature, and are constant and do not depend on the state of aggregation of the body whether solid, liquid, or gaseous. The *apparent specific heat*, on the other hand, includes in addition to this the heat required to overcome molecular resistances and external pressures.

From equation (6) we have seen that

$$c_p - c_v = l_v \left(\frac{dv}{d\tau}\right)$$

and from equation (8a)

$$l_v = A\tau \left(\frac{dp}{d\tau}\right)$$

so that

$$A\tau = (c_p - c_v) \left(\frac{d\tau}{dp}\right) \left(\frac{d\tau}{dv}\right) \qquad \ldots\ldots\ldots\ldots\ldots(10b)$$

and the pressure being constant, we have the equation,

$$dp = 0 = \left(\frac{dp}{d\tau}\right) d\tau + \left(\frac{dp}{dv}\right) dv$$

or,

$$\left(\frac{d\tau}{dv}\right) = -\frac{\left(\frac{dp}{dv}\right)}{\left(\frac{dp}{d\tau}\right)}$$

substituting in equation (10b) we have

$$c_p - c_v = -A\tau \frac{\left(\frac{dp}{d\tau}\right)^2}{\left(\frac{dp}{dv}\right)} \qquad \ldots\ldots\ldots\ldots\ldots(10c)$$

or,

$$c_p = c_v - A\tau \frac{\left(\frac{dp}{d\tau}\right)^2}{\left(\frac{dp}{dv}\right)} \qquad \ldots\ldots\ldots\ldots\ldots(10d)$$

and,

$$c_v = c_p + A\tau \frac{\left(\frac{dp}{d\tau}\right)^2}{\left(\frac{dp}{dv}\right)} \qquad \ldots\ldots\ldots\ldots\ldots(10e)$$

substituting in equation (9a) from equation (10) we have

$$dH = JdQ = \left[k + \tau\int\left(\frac{d^2p}{d\tau^2}\right) dv\right] d\tau + \tau\left(\frac{dp}{d\tau}\right) dv \ldots\ldots(11)$$

and in equation (9b) from equation (10a) we have

$$dH = JdQ = \left[k - \tau\int\left(\frac{d^2v}{d\tau^2}\right) dp\right] d\tau - \tau\left(\frac{dv}{d\tau}\right) dp \ \ldots(11a)$$

The first of these two expressions for the value of dH is the one which we will find most generally useful. Equating it with that given in equation (3) we have

$$dH = dU + p.dv = \left[k + \tau\int\left(\frac{d^2p}{d\tau^2}\right) dv\right] d\tau + \tau\left(\frac{dp}{d\tau}\right) dv \ldots(12)$$

or,
$$dU = kd\tau + \tau\int\left(\frac{d^2p}{d\tau^2}\right) dv.d\tau + \left[\tau\left(\frac{dp}{d\tau}\right) - p\right] dv \ \ldots(12a)$$

so that
$$U = \text{const.} + \int kd\tau + \int\left[\tau\left(\frac{dp}{d\tau}\right) - p\right] dv \ldots(12b)$$

as, however, in the applications of this equation we only require to know the variations of U, and not its absolute amount, we may neglect the constant and simply write

$$U = \int kd\tau + \int\left[\tau\left(\frac{dp}{d\tau}\right) - p\right] dv \ldots\ldots\ldots\ldots(12c)$$

an expression which may be considered the general equation for the energy of unit mass of a substance, v and τ being the variables.

From equation (11) we have

$$d\phi = \frac{dH}{\tau} = k\frac{d\tau}{\tau} + \int\left(\frac{d^2p}{d\tau^2}\right) dv.d\tau + \left(\frac{dp}{d\tau}\right) dv \ \ldots\ldots(13)$$

$$= k\frac{d\tau}{\tau} + dF \ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(13a)$$

or,
$$\tau.d\phi = dH = k\,d\tau + \tau.dF$$

so that
$$\phi = \text{const.} + \int k\frac{d\tau}{\tau} + \int\left(\frac{dp}{d\tau}\right) dv \ldots\ldots\ldots\ldots\ldots(13b)$$

for the same reason as in equation (12c) we may neglect the constant and write

$$\phi = \int k\frac{d\tau}{\tau} + \int\left(\frac{dp}{d\tau}\right) dv \ldots\ldots\ldots\ldots\ldots\ldots(13c)$$

$$= k\log_e\tau + \int\left(\frac{dp}{d\tau}\right) dv \ \ldots\ldots\ldots\ldots\ldots\ldots(13d)$$

$$= k\log_e\tau + F \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(13e)$$

an expression which may be considered the general equation for the entropy of unit mass of a substance, v and τ being the independent variables. Equation (13a) is sometimes called the General Equation of Thermo-dynamics, and from it we see that a given quantity of heat (in units of work) which a body receives, or gives out, during any infinitely small change of figure or dimensions, is expressed in every case by $\tau . d\phi$ where τ is the absolute temperature, and $d\phi$ is the infinitely small variation of the entropy. The function F is what Rankine called the *metamorphic function*, $\tau . dF$ being the quantity of heat transformed into mechanical work, whether external or internal, during an infinitely small change in the condition of the body, while the first part of the expression for the entropy, that is $k \log_e \tau$, we may consider the entropy of the sensible heat imported, and which causes change of temperature.

We will now proceed to apply these general equations to determine some of the properties of air and steam.

SECTION II.—ON AIR, CONSIDERED AS A PERFECT GAS.

For a perfect gas since

$$\left(\frac{d^2p}{d\tau^2}\right) = 0, \text{ and } \left(\frac{d^2v}{d\tau^2}\right) = 0$$

we see from equations (10) and (10a) that

$$Jc_v = K_v = k \qquad \text{(14)}$$

and

$$Jc_p = K_p = k \qquad \text{(14a)}$$

that is, the *apparent* is equal to the *real* specific heat; and since

$$\left(\frac{dv}{d\tau}\right) = \frac{R}{p}, \text{ and } \left(\frac{dp}{d\tau}\right) = \frac{R}{v} = \frac{p}{\tau}$$

from equation (10b) we have

$$c_p - c_v = AR \qquad \text{(14b)}$$

or,

$$K_p - K_v = R \qquad \text{(14c)}$$

and

$$K_p = K_v + \frac{p_0 v_0}{\tau_0} \qquad \text{(14d)}$$

the same results may of course be obtained by substituting

$$\left(\frac{dp}{d\tau}\right) = \frac{R}{v}, \text{ and } \left(\frac{dp}{dv}\right) = -\frac{R\tau}{v^2}$$

in equation (10c).

If, in equation (12c), we substitute

$$k = \mathrm{K}_v, \text{ and } \tau\left(\frac{dp}{d\tau}\right) = p$$

and integrate, we have for the difference of energy in the states 1 and 2

$$\mathrm{U}_2 - \mathrm{U}_1 = \mathrm{K}_v(\tau_2 - \tau_1) \dots\dots\dots\dots\dots\dots (15)$$

that is, it is a simple function of the temperature.

From equation (12) we have for the heat expended in raising the temperature of the gas from τ_1 to τ_2

$$\mathrm{H} = \mathrm{JQ} = \mathrm{J}c_v(\tau_2 - \tau_1) + \int_1^2 p.dv \dots\dots\dots\dots (16)$$

$$= \mathrm{K}_v(\tau_2 - \tau_1) + \int_1^2 p.dv \dots\dots\dots\dots (16a)$$

Taking the indefinite integral of the general equation for the entropy (equation 13b) between the limits 2 and 1 we have

$$\phi_2 - \phi_1 = k_v \log_e \frac{\tau_2}{\tau_1} + \int_1^2 \left(\frac{dp}{d\tau}\right) dv \dots\dots\dots\dots\dots (17)$$

and since in a perfect gas

$$\left(\frac{dp}{d\tau}\right) = \frac{\mathrm{R}}{v}, \text{ or } \tau\left(\frac{dp}{d\tau}\right) = \frac{\mathrm{R}\tau}{v} = p,$$

we have

$$\phi_2 - \phi_1 = \mathrm{K}_v \log_e \frac{\tau_2}{\tau_1} + \mathrm{R} \log_e \frac{v_2}{v_1} \dots\dots\dots\dots\dots (17a)$$

$$= \mathrm{K}_v\left[\log_e \frac{\tau_2}{\tau_1} + (\gamma - 1)\log_e \frac{v_2}{v_1}\right] \dots\dots\dots\dots\dots (17b)$$

Where

$$\gamma = \frac{\mathrm{K}_p}{\mathrm{K}_v}$$

and since

$$\frac{p_2 v_2}{p_1 v_1} = \frac{\tau_2}{\tau_1}$$

we have

$$\log \frac{p_2}{p_1} + \log_e \frac{v_2}{v_1} = \log_e \frac{\tau_2}{\tau_1}$$

or,

$$\log_e \frac{v_2}{v} = \log_e \frac{\tau_2}{\tau_1} - \log_e \frac{p_2}{p_1}$$

substituting this value of $\log_e \frac{v_2}{v_1}$ in equation (17a), we have

$$\phi_2 - \phi_1 = \mathrm{K}_p \log_e \frac{\tau_2}{\tau_1} - (\mathrm{K}_p - \mathrm{K}_v) \log_e \frac{p_2}{p} \dots\dots (17c)$$

$$= \mathrm{K}_p \left[\log_e \frac{\tau_2}{\tau_1} - \left(\frac{\gamma - 1}{\gamma}\right) \log_e \frac{p_2}{p_1} \right] \text{............(17d)}$$

similarly substituting the values of $\log_e \frac{\tau_2}{\tau_1}$ in the same equation, we have

$$\phi_2 - \phi_1 = \mathrm{K}_v \left[\gamma \log_e \frac{v_2}{v_1} + \log_e \frac{p_2}{p_1} \right] \text{............(17e)}$$

expressions which give us the difference of entropy for any two states of a gas.

To apply these to special cases, suppose first, that the pressure at 1 and 2 is the same, so that

$$\frac{v_2}{v_1} = \frac{\tau_2}{\tau_1}$$

and for

Pressure constant

$$\phi_2 - \phi_1 = \mathrm{K}_p \log_e \frac{\tau_2}{\tau_1} = \mathrm{K}_p \log_e \frac{v_2}{v_1} \text{............(18)}$$

when the

Volume is constant

$$\phi_2 - \phi_1 = \mathrm{K}_v \log_e \frac{\tau_2}{\tau_1} = \mathrm{K}_v \log_e \frac{p_2}{p_1} \text{............(19)}$$

when the

Temperature is constant

$$\phi_2 - \phi_1 = (\mathrm{K}_p - \mathrm{K}_v) \log_e \frac{v_2}{v_1} = \frac{p_1 v_1}{\tau_1} \log_e \left(\frac{v_2}{v_1}\right) \text{......(20)}$$

$$= (\mathrm{K}_p - \mathrm{K}_v) \log_e \frac{p_1}{p_2} = \frac{p_1 v_1}{\tau_1} \log_e \left(\frac{p_1}{p_2}\right) \text{............(20a)}$$

hence,

$$\mathrm{K}_p - \mathrm{K}_v = \frac{p_1 v_1}{\tau_1}$$

or,

$$\mathrm{K}_p = \mathrm{K}_v + \frac{p_1 v_1}{\tau_1}$$

as already obtained in equation (14d)

If we multiply equation (20) by τ we have for the external work done during *isothermal* change of condition

$$\tau\,(\phi_2 - \phi_1) = p_1 v_1 \log_e \left(\frac{v_2}{v_1}\right) = p_1 v_1 \log_e \left(\frac{p_1}{p_2}\right) \text{......(20b)}$$

from which we see that the amount of heat (in units of work) which is absorbed during expansion, or given out during compression, is equal to the difference of entropy into the absolute temperature.

In *adiabatic* expansion since no heat is given out or absorbed we have

$$\phi_2 = \phi_1$$

so that

$$\frac{\tau_1}{\tau_2} = \left(\frac{v_2}{v_1}\right)^{\gamma - 1} \text{ from eq. (17b)}\ldots\ldots\ldots(21)$$

and

$$\frac{v_2}{v_1} = \left(\frac{p_1}{p_2}\right)^{\frac{1}{\gamma}} \text{ from eq. (17e)}\ldots\ldots\ldots(21a)$$

hence

$$\frac{\tau_1}{\tau_2} = \left(\frac{v_2}{v_1}\right)^{\gamma - 1} = \left(\frac{p_1}{p_2}\right)^{\frac{\gamma - 1}{\gamma}} \ldots\ldots\ldots\ldots\ldots\ldots(21b)$$

and the equation of the adiabatic curve is $p_1v^{\gamma} = p_2v_2^{\gamma}$ = constant, and the external work done is

$$W = \frac{p_1v_1 - p_2v_2}{\gamma - 1}$$

substituting in terms of equations (21) and (21a) we have

$$W = \frac{p_1v_1}{\gamma - 1}\left[1 - \left(\frac{v_1}{v_2}\right)^{\gamma - 1}\right] = \frac{p_1v_1}{\gamma - 1}\left[1 - \left(\frac{p_2}{p_1}\right)^{\frac{\gamma - 1}{\gamma}}\right] = \frac{p_1v_1}{\gamma - 1}\left[1 - \frac{\tau_2}{\tau_1}\right].(22)$$

If we interchange the subscripts attached to the symbols in these formulæ they apply also to adiabatic compression of air.

Section III.—On Saturated Steam.

If we apply the general equation

$$dH = \tau.d\phi = \left[k + \tau\int\left(\frac{d^2p}{d\tau_2}\right) dv\right] d\tau + \tau\left(\frac{dp}{d\tau}\right) dv$$

to the case of a body changing its state at constant temperature and pressure we obtain an expression for the amount of heat which becomes *Latent*. Under these circumstances the first term on the right hand side becomes zero, and the factor $\tau\left(\frac{dp}{d\tau}\right)$ of the last term is constant since the pressure and temperature do not vary, so that the heat expended, or the *latent heat* is (in units of work),

$$\int dH = L = \tau\left(\frac{dp}{d\tau}\right)(v_1 - v_0)\ldots\ldots\ldots\ldots\ldots(23)$$

v_0 and v_1 being the volume of unit mass of the body in its first and second states respectively, τ the absolute temperature at which the change takes place and $\left(\frac{dp}{d\tau}\right)$ the reciprocal of the rate at which the temperature varies with the external pressure.

If the amount of heat expended be estimated at the instant when the whole of a solid is liquified, we obtain the value of the latent heat of fusion or liquifaction. Professor James Thomson has made some interesting investigations relating to change of molecular condition. We cannot enter into details of those at present, but may remark that one of his conclusions follows directly from the last equation, although he arrived at it by quite another process of reasoning. That equation may be written in the form

$$\left(\frac{d\tau}{dp}\right) = \frac{\tau\,(v_1 - v_0)}{\mathrm{L}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(23a)$$

and it is evident in such substances in which the volume of a given mass in solid state exceeds that in the liquid state, that is when v_0 is greater than v_1, as is the case for water and some other substances, then $\left(\frac{d\tau}{dp}\right)$ is negative, that is the temperature of fusion is lowered by increasing the pressure, a conclusion which was verified experimentally by Sir William Thomson.

If the amount of heat expended be estimated at the instant when the whole of a liquid has been evaporated, we obtain the latent heat of evaporation, that is the heat expended in causing a body to change from the liquid to the gaseous state, and it is equal to

$$\mathrm{L} = \mathrm{A}\tau\left(\frac{dp}{d\tau}\right)(v_1 - v_0) \text{ units of heat} \quad \ldots\ldots\ldots(23b)$$

where v_0 is the volume occupied by unit mass of the substance in the fluid state at the absolute temperature τ_1 and v_1 the volume of the same mass when in the state of dry saturated steam.

The value of $\left(\frac{dp}{d\tau}\right)$ that is the rate of increase of the pressure with the temperature may be obtained from tables giving the results of Regnault's experiments, or computed from empirical formulæ which have been formed to represent these. In practice, however, the value of the latent heat of evaporation is generally taken from tables or calculated from the empirical formula

$$\begin{aligned} L &= 1092 - 0{\cdot}7\,(T^\circ - \ \ 32^\circ) \\ &= \ \ 966 - 0{\cdot}7\,(T^\circ - 212^\circ) \qquad \text{(23c)} \end{aligned}$$

the temperature T being in degrees Fahrenheit. From this expression we see that L diminishes very nearly at the rate of seven-tenths of a unit of heat for each degree of rise of the boiling point.

The corresponding expression for the total heat of evaporation, or the total heat of steam as it is usually called, being the quantity of heat required to raise unit mass of water from the temperature of melting ice to a given temperature, and to evaporate it at that temperature, is

$$H = 1146 + {\cdot}305\,(T^\circ - 212^\circ) \qquad \text{(23d)}$$

a quantity which increases nearly at the rate of three-tenths of a unit of heat for each degree of elevation of the boiling point.

One of the earliest and most important applications of thermodynamics was the calculation by Rankine and Clausius of the volume, and hence the density of dry saturated steam, which can be obtained from equation (23b) since

$$v_1 = \frac{L}{A\tau\left(\dfrac{dp}{d\tau}\right)} + v_0 \qquad \text{(23e)}$$

v_1 being the volume of unit mass of the steam, and v_0 of the water, and the values of L, and $\left(\dfrac{dp}{d\tau}\right)$ being known that of v_1 can be computed.

The results calculated from this formula agree very closely with Fairbairn's experiments, from which he formed the empirical formula

$$v = {\cdot}41 + \frac{389}{p + {\cdot}35} \qquad \text{(23f)}$$

v being the specific volume in cubic feet, and p the pressure in pounds on the square inch. This formula is nearly exact for pressures up to 100 pounds per square inch; beyond that limit it gives too large a result.

For purposes of calculation connected with steam engines the empirical formula

$$pv^{\frac{17}{16}} = \text{const.}$$

proposed by Rankine, is sufficiently exact. For pressures in pounds on the square inch, and volumes in cubic feet the value of the constant is about 475.

The expression for the difference of entropy in any two states obtained from equation (13c) may be written in terms of the latent heat (units of work) thus

$$\phi_2 - \phi_1 = k \log_e \frac{\tau_2}{\tau_1} + \frac{L_2}{\tau_2} - \frac{L_1}{\tau_1} \ldots\ldots\ldots\ldots (24)$$

an equation of great importance, but of which we will delay the application till we derive it in a more general form in which it can be applied to steam in any degree of saturation, for it must be observed that we have hitherto only considered *dry saturated steam.*

In the case of *supersaturated steam*, since it is not all in the same physical condition, the characteristic equation breaks up into two; and we require an equation of the form

$$f\ (p.\tau) = 0$$

and another for the specific volume in terms of either p or τ, and the variable which expresses the proportion of pure steam, in unit mass of saturated steam. Let x denote this proportion, a quantity which becomes smaller the wetter the steam becomes. If v_1 be the volume in cubic feet of one pound of water, and v_2 the specific volume of dry steam, then since each pound of wet steam contains x pound of dry steam and $(1 - x)$ pound of water, the specific volume of wet steam must be

$$V = v_2 x + (1 - x)\, v_1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots (25)$$

for ordinary temperatures, and when the steam is not very wet we may make

$$V = v_2 x \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (25a)$$

an approximation which is often exact enough in calculations relating to steam engines.

The general equations which we have had for the effects of heat, although they hold for a liquid and its vapour, are not applicable for a mixture of the two substances such as wet steam, in which case we proceed as follows. If p be the common pressure of the liquid and the vapour, and L the latent heat (in heat units) of the latter, then to change unit mass of the mixture from the condition denoted by the variables x and τ, to that denoted by $x + dx$ and $\tau + d\tau$ the heat required to be imparted is

$$dQ = L.dx + c'' x.d\tau + c' (1 - x) \, d\tau \;\ldots\ldots\ldots (25b)$$

where c' is the specific heat of the liquid c'' that of the vapour. The specific heat used here is not that at constant volume, nor yet that at constant pressure, but is a compound quantity involving changes both of volume and pressure, and is the amount of heat required to raise the temperature of unit mass of either form of the substance one degree, under the condition that the two forms remain in equilibrium during the process. Thus c'' is that quantity of heat which saturated vapour requires to heat it through one degree, if the pressure is at the same time raised so that at the higher temperature the vapour remains in the same state of saturation, and in the case of vapour of water it is called the *specific heat of saturated steam.* The increase of pressure has very little influence on the specific heat c' of the liquid, since for such pressures as we consider the liquid is only slightly compressible.

From equation (25b) we have

$$d\phi = \frac{dH}{\tau} = J \left[\frac{L}{\tau} \, dx + \{c' + (c'' - c') \, x\} \, \frac{d\tau}{\tau} \right] \ldots\ldots (25c)$$

but as $d\phi = \frac{dH}{\tau}$ is an exact differential, we have

$$\frac{d}{d\tau} \left(\frac{L}{\tau} \right) = \frac{c'' - c'}{\tau} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (25d)$$

so that

$$c'' - c' = \frac{dL}{d\tau} - \frac{L}{\tau} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (26)$$

and for the *specific heat of saturated steam* we obtain the expression

$$c'' = c' + \frac{dL}{d\tau} - \frac{L}{\tau} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (26a)$$

which may be written in the form

$$c'' = \frac{dH}{d\tau} - \frac{L}{\tau} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (26b)$$

where H is the total heat of evaporation, and $\frac{dH}{d\tau}$ is the rate at which

H varies per degree of increase of temperature which in the case of dry steam we have seen is equal to ·305, so that

$$c'' = \cdot 305 - \frac{L}{\tau} \quad\text{.........................(26c)}$$

and as $\frac{L}{\tau}$ for all practical temperatures must always be greater than ·305, it follows that c'' must be negative. From this we infer that when dry saturated steam expands performing work, the temperature falls, and heat must be added to keep it in the dry saturated condition, and conversely if dry saturated steam be compressed, heat must be abstracted in order to keep it from becoming superheated. For wet steam the value of H varies according to the degree of wetness.

From equations (25c) and (25d) we have

$$d\phi = J\ d\left(\frac{Lx}{\tau}\right) + Jc'\frac{d\tau}{\tau} \quad\text{..................(27)}$$

integrating between the states 2 and 1 we have

$$\phi_2 - \phi_1 = \frac{L_2 x_2}{\tau_2} - \frac{L_1 x_1}{\tau_1} + J\int_{\tau_1}^{\tau_2} c'\frac{d\tau}{\tau} \quad\text{.........(27a)}$$

the latent heat being in units of work in equation (27a), which is a general expression for the entropy which must be imparted to any mixture of a substance and its vapour, to change it from the state $\tau_1\ x_1$ to the state $\tau_2\ x_2$.

In the case of a mixture of steam and water c' is sensibly equal to unity, so that we have

$$\phi_2 - \phi_1 = \frac{L_2 x_2}{\tau_2} - \frac{L_1 x_1}{\tau_1} + J \log\frac{\tau_2}{\tau_1} \quad\text{.........(27b)}$$

a result which might also have been easily obtained from the general equation for the entropy. If we suppose the steam to have been originally dry and to have remained dry, that is, that $x_2 = x_1 = 1$, then we have equation (24). From equation (27b) we can easily obtain the amount of heat required to raise the temperature of steam in any state of saturation by a given amount.

The applications of this formula are very numerous and important. We will only in the meantime consider one or two connected with the expansion of steam. As in this case the initial temperature is generally the highest, we may write

$$\phi_1 - \phi_2 = \frac{L_1 x_1}{\tau_1} - \frac{L_2 x_2}{\tau_2} + J \log_e \frac{\tau_1}{\tau_2} \ldots\ldots\ldots (27c)$$

If we suppose the steam to expand *isothermally*, so that $\tau_1 = \tau_2 = \tau$, and $L_1 = L_2 = L$, we have

$$\tau\,(\phi_1 - \phi_2) = L\,(x_1 - x_2) \ldots\ldots\ldots\ldots\ldots\ldots (28)$$

an expression which gives us the amount of heat which is required to be imparted to unit mass in order that the temperature may remain constant between the states 1 and 2.

It is instructive to compare equation (28) with the corresponding equation for air (20b), as we then see why the quantity of heat required for a given change of state differs so much in the two cases.

If the steam expand *adiabatically* $\phi_1 = \phi_2$, and we have

$$\frac{L_1 x_1}{\tau_1} = \frac{L_2 x_2}{\tau_2} - J \log_e \frac{\tau_1}{\tau_2}$$

or,

$$J \log \frac{\tau_1}{\tau_2} = \frac{L_2 x_2}{\tau_2} - \frac{L_1 x_1}{\tau_1} \ldots\ldots\ldots\ldots\ldots\ldots (29)$$

as the equation of the adiabatic curve, for a unit mixture of steam and water of which $\tau_1 x_1$ is one point.

The amount of water in the steam after expansion, when the temperature and pressure have fallen by a given amount, is

$$x_2 = \frac{\tau_2}{L_2} \left[\frac{L_1 x_1}{\tau_1} + J \log_e \frac{\tau_1}{\tau_2} \right] \ldots\ldots\ldots\ldots\ldots (29a)$$

we may generally write (see equation 25a)

$$V_1 = v_1 x_1, \text{ and } V_2 = v_2 x_2$$

so that

$$x_1 \frac{V_2}{V_1} = \frac{D_1}{D_2}\, x_2 = r x_2$$

where r is the ratio of expansion, D_1 the density of dry steam at the temperature τ_1, and D_2 the density at τ_2, therefore approximately.

$$r = \frac{\tau_2}{x_2 L_2} \, \frac{D_1}{D_2} \left[\frac{L_1 x_1}{\tau_1} + J \log \frac{\tau_1}{\tau_2} \right] \ldots\ldots\ldots (29b)$$

from which if we suppose $x_1 = x_2 = 1$, that is, that the steam is dry originally, and remains dry, we obtain the expressions given by Rankine (Prime Movers p. 384)

As, however, neither the one nor the other of these suppositions is usually true, the theory of the action of steam in the cylinders of engines, founded on them, leads to results which are far from correct.

The expansion curve for a mixture of steam and water may be expressed by an equation of the form pv^n = const., so that the external work

$$W = \frac{p_1 v_1}{n-1}\left[1 - \left(\frac{v_1}{v_2}\right)^{n-1}\right] \ldots\ldots\ldots\ldots(30)$$

Zeuner has shown that the value of n depends on the amount of water in the steam, so that if x represent the weight of water in 100 parts of steam at the beginning of the adiabatic expansion,

$$n = 1{\cdot}135 - {\cdot}001\,x.$$

For $x = 0$ $n = 1{\cdot}135$
$x = 10$ $n = 1{\cdot}125$
$x = 20$ $n = 1{\cdot}115$
$x = 30$ $n = 1{\cdot}105$

from which we see that the curve approaches an equilateral hyperbola as the quantity of water contained in the steam increases. Rankine gave $n = \frac{10}{9}$ for dry steam, but for that value the steam contains 24 per cent. of water. He obtained this value from a large number of actual indicator diagrams, but he overlooked the initial condensation of the steam caused by the action of the sides of the cylinder. Grashof in his discussion of steam engines takes $n = 1{\cdot}125$.

SECTION IV.—ON SUPERHEATED STEAM.

In considering the action of superheated steam, it is usual to assume that superheating has been carried to such an extent that it may be treated as perfectly gaseous, and therefore follows the same laws as those for perfect gases.

Thus Rankine gave the equation

$$pv = 42140\,\frac{\tau}{\tau_0} = 85{\cdot}44\,\tau \ldots\ldots\ldots\ldots\ldots\ldots(31)$$

where τ is the absolute temperature of the steam, τ_0 the absolute temperature of the water at the freezing point according to Fahrenheit's scale, p the pressure in pounds on the square foot, and v the volume in cubic feet.

If p be given in pounds on the square inch then equation (31) may be written

$$pv = \cdot 676\ \tau \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (31a)$$

Rankine further assumed that the *total heat of the steam*, or as it is usually called in the case of superheated steam, *total heat of gasefication*, is equal to the latent heat of evaporation at the freezing point, plus the heat required to raise the steam from that point to the temperature at which it exists calculated in the following manner.

Taking equation (11a), viz.:—

$$\tau d\phi = d\mathrm{H} = \mathrm{J}d\mathrm{Q} = \left[k - \tau \int\left(\frac{d^2v}{d\tau^2}\right) dp\right] d\tau - \tau \left(\frac{dv}{d\tau}\right) dp \quad (32)$$

being the general equation for the element of heat expended in terms of the temperature and pressure as independent variables, we have in the present case since the pressure is supposed to be constant and the steam to be in the perfectly gaseous state

$$\mathrm{H}_1 = \mathrm{K}_p (\tau_1 - \tau_0) \ldots\ldots\ldots\ldots\ldots\ldots (32a)$$

and when $\tau_1 = \tau_0$,

$$\mathrm{H}_0 = \mathrm{L}_0$$

where L_0 is the latent heat of evaporation in foot pounds, of one pound of the substance at the temperature τ_0, so that for the integral of equation (32) we may write

$$\begin{aligned} \mathrm{H} &= \mathrm{L}_0 + \mathrm{K}_p (\tau_1 - \tau_0) \\ &= \mathrm{L}_0 + 0{\cdot}48 \times 772\ (\mathrm{T}^\circ - 32^\circ) \\ &= 842872 + 371\ (\mathrm{T}^\circ - 32) \\ &= 659895 + 371\ \tau \text{ foot pounds } \ldots\ldots\ldots (32b) \end{aligned}$$

T being the temperature of the steam according to Fahrenheit scale, and τ the corresponding absolute temperature.

These formulæ possess the practical advantage of great simplicity, but it is evident from the assumptions which have been made that they can only be considered as first approximations to the exact formulæ for superheated steam, and as the steam approaches the saturation point, the deviation from exactness is so great that they are no longer applicable.

Different investigators have attempted to give expressions for steam, which will apply not only to the superheated but also to the saturated condition when the necessary substitutions are made, but owing to the want of experimental data none of these can be considered perfectly satisfactory. We will consider shortly those of Zeuner,* which give results agreeing very closely with experiment and are not inconvenient for purposes of practical calculation. By taking advantage of some of our previous results we can somewhat simplify Zeuner's methods.

The following fundamental equations which we have already obtained will serve as a basis for our investigation,

Eq. (7a) $$\mathrm{A} = \frac{d}{dp}\left[c_p\left(\frac{d\tau}{dv}\right)\right] - \frac{d}{dv}\left[c_v\left(\frac{d\tau}{dp}\right)\right] \quad\ldots\ldots\ldots\ldots\ldots \text{I.}$$

Eq. (10b) $$\mathrm{A}\tau = (c_p - c_v)\left(\frac{d\tau}{dv}\right)\left(\frac{d\tau}{dp}\right) \quad\ldots\ldots\ldots\ldots\ldots\ldots \text{II.}$$

Eq. (5c), Eq. (9), Eq. (9b)
$$\left.\begin{aligned} dQ &= c_v\left(\frac{d\tau}{dp}\right)dp + c_p\left(\frac{d\tau}{dv}\right)dv && \text{(a)}\\ dQ &= c_v\,d\tau + \mathrm{A}\tau\left(\frac{dp}{d\tau}\right)dv && \text{(b)}\\ dQ &= c_p\,d\tau - \mathrm{A}\tau\left(\frac{dv}{d\tau}\right)dp && \text{(c)}\end{aligned}\right\}\ldots\ldots \text{III.}$$

and the *condition equation*, that is the equation which gives the relation of p, v, and τ since the absolute temperature is a function of the pressure and volume is

$$d\tau = \left(\frac{d\tau}{dp}\right)dp + \left(\frac{d\tau}{dv}\right)dv \quad\ldots\ldots\ldots\ldots\ldots\ldots \text{IV.}$$

We will assume that superheated steam is in a condition intermediate between that of a perfect gas and saturated steam, and that dry saturated steam is thus its limiting condition, that the specific heat at constant pressure c_p is constant, and that the relation given in equation (17d), viz.:—

$$\phi_2 - \phi_1 = \mathrm{K}_p\left[\log_e\frac{\tau_2}{\tau_1} - \frac{\gamma-1}{\gamma}\log_e\frac{p_2}{p_1}\right]\ldots\ldots\ldots(33)$$

holds for superheated steam as well as for air, where γ has, however, a different value from what it has in the case of air. As there are probably slight variations in the quantities c_p and γ our resulting equations can only be considered approximate.

From these equations and assumptions the following differential equation of condition is derived,

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* Zeitschrift des Vereins deutscher Ingenieure, Bd. XI.

$$d\tau = \frac{A}{c_p\,(\gamma - 1)}\left[v\,dp + \gamma\,p\,dv\right] + \frac{\gamma - 1}{\gamma}\cdot\frac{\tau}{p}\cdot dp \quad \text{...(34)}$$

of which the integral is

$$pv = B\tau - Cp^{\frac{\gamma-1}{\gamma}} \quad \text{......................(34a)}$$

where the constant

$$B = \frac{c_p\,(\gamma - 1)}{A\,\gamma}$$

From Regnault's experiments we know that for superheated steam $c_p = 0{\cdot}4805$ and $\gamma = 1{\cdot}333$, so that A being $\frac{1}{424}$ we find B equal to 51 nearly. The value of C is found by assuming equation (34a) to hold in the limiting condition of the steam, that is when it is dry saturated, for which the values of the quantities v and τ are known for a given pressure, say of one atmosphere, and thus we obtain

$$pv = 51\,\tau - 192{\cdot}5\,p^{\frac{1}{4}} \quad \text{...................(34b)}$$

p being expressed in kilograms per square metre, v in cubic metres, and τ in absolute degrees Centigrade. This is Zeuner's formula.

If, in equation (34a), we make, on the right hand side of the equation, $p^{\frac{\gamma-1}{\gamma}} = v^{-n}$, we obtain

$$pv = B\tau - Cv^{-n} \quad \text{......................(34c)}$$

an expression of this form was given by Hirn* as representing the results of his experiments on superheated steam, but it can also be derived from theoretical considerations, as has been done in different ways by Hirn† himself, and by Schmidt,‡ the constants, however, having different values from those in Zeuner's expression. Equation (34c) may be derived from Zeuner's if we consider as constant the specific heat at constant volume, in place of the specific heat at constant pressure. These expressions may be regarded as *second approximations* to the law of superheated steam, of which an expression of the form $pv = R\tau$ is the first approximation.

Ritter§ has shown that the results of Hirn's experiments may be

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* Hirn—Mémoire sur la Thermo-dynamique, 1867.

† Hirn—Theorie Mecanique de la Chaleur, 1875. Tome second.

‡ Zeitschrift des Vereins deutscher Ingenieure, 1867.

§ Wiedemann's Annalen, 1878.

represented by an equation of the form

$$\tau = \frac{pv}{\text{B}} + \frac{\text{C}}{pv^{\frac{3}{2}}} \text{.........................(34d)}$$

where B = $\frac{1}{220}$ and C = 28, when the pressure p is given in atmospheres, the volume v in cubic metres, and the absolute temperature τ according to the Centigrade scale. This expression fulfils certain theoretical conditions of superheated steam more perfectly than the others, since the second term on the left hand side, which shows the variation from the first approximation, is a function both of p and v, while in that of Zeuner it is only a function of p, and in that of Hirn and Schmidt it is only a function of v. As, however, Zeuner's expression is more convenient for purposes of practical calculation than the others, and is exact enough for such purposes, we will in the meantime confine our attention to it.

If in Zeuner's expression p the pressure be given in atmospheres. v the specific volume in cubic metres, and τ in absolute degrees Cent.

$$pv = {\cdot}0049287\ \tau - {\cdot}187815\, p^{\frac{1}{4}} \text{..............(34e)}$$

The results calculated from this equation agree very closely with the results of Hirn's experiments on superheated steam, as is shown by the following short table :—

Pressure in Atmospheres.	Temp. Cent.	Specific volumes in cubic metres.	
		Hirn.	Equation (34c).
1	118·5	1·74	1·7417
1	141	1·85	1·8526
3	200	0 697	0·6947
4	165	0·4822	0·4733
4	200	0·522	0·5164
4	246	0·5752	0·5731
5	162·5	0·3758	0·3731
5	205	0·414	0·4150

If p be in *pounds on the square inch*, v in cubic feet, and τ in absolute degrees Fahr. then

$$pv = \cdot 645\,\tau - 23\cdot 8\,p^{\frac{1}{4}} \ldots\ldots\ldots\ldots\ldots\ldots (34f)$$

By comparing this with equation (31a) we see clearly the difference in the results of Rankine and Zeuner's suppositions. Multiplying by 144 we obtain the product pv in foot pounds.

If in equation (34f) we substitute for τ in terms of p from the results of Regnault's experiments and apply it to find the specific volume of dry saturated steam at constant pressure, we obtain results which agree with those got by the ordinary formulæ, as is shown in the following short table:—

Absolute Pressure in lbs. per square inch.	Specific volumes in cubic feet.	
	Ordinary formula	Equation (34f).
14·7	26·37	26·40
30	13·48	13·43
60	7·02	7·00
100	4·34	4·33
150	2·96	2·91
200	2·26	2·24

From the fundamental equations III. the following expressions may be deduced,

$$\left.\begin{aligned} JdQ &= \frac{1}{\gamma - 1}\left(v\,dp + \gamma\,p\,dv\right) && \text{(a)} \\ JdQ &= Jc_p\left(d\tau - \frac{\gamma - 1}{\gamma}\frac{\tau}{p}\,dp\right) && \text{(b)} \\ JdQ &= Jc_p\left(d\tau + (\gamma - 1)\,\frac{\tau}{v}\,dv\right) && \text{(c)} \end{aligned}\right\} \ldots (35)$$

These are the well-known equations of Clausius and Zeuner for perfect gases, but when applied to superheated steam the values of c_p, c_v, and γ are (as already remarked) different from those for such gases.

Taking the general equation

$$dQ = AdU + A\,p\,dv$$

and substituting from equation (35a) we have

$$AdU = \frac{A}{\gamma - 1}\left(v\,dp + \gamma\,p\,dv\right) - A\,p\,dv \ldots\ldots\ldots (36)$$

$$= \frac{A}{\gamma - 1}\, d\,(p\,v)$$

integrating this from a given initial condition we have

$$A\,(U - U_1) = \frac{A}{\gamma - 1}\,(pv - p_1 v_1) \text{............(36a)}$$

if we assume the initial condition to be that of water at 0°C. then $A\,(U - U_1)$ gives the increase of what is usually called the *steam heat* by Continental writers, and denoted by S, and we have

$$S = S_0 + \frac{A}{\gamma - 1}\, pv \text{....................(36b)}$$

S_0 being a constant to be determined by applying the equation to the case of dry saturated steam, and is found to be equal to 858. If we add to this the heat equivalent of the external work $A\,p\,v$ we have for the total heat of unit mass of the steam

$$\begin{aligned} H = \quad & S_0 + \frac{A\gamma}{\gamma - 1}\, pv \\ = \quad & S_0 + c_p\left(\tau - \frac{C}{B}\, p^{\frac{\gamma - 1}{\gamma}}\right) \text{.......................(36c)} \\ = \quad & 858 + {\cdot}4805\,\tau - 17{\cdot}7\, p^{\frac{1}{4}} \text{ units of heat......(36d)} \\ = {} & 662376 + 371\,\tau - 13664\, p^{\frac{1}{4}} \text{ units of work......(36e)} \end{aligned}$$

the temperature being absolute according to the Fahr. scale, and p the pressure being in pounds on the square inch. This expression shows that for such pressures and temperatures as are used at present, Rankine's formula does not differ much in its results from those given by Zeuner's. For higher pressures and temperatures, however, the latter is the more exact, and, moreover, it has the advantage of being applicable to temperatures down to that of the saturation point.

When equation (36c) is applied to dry saturated steam (substituting for τ in terms of p) it gives us the total heat, and the results thus obtained agree very closely with Regnault's experiments, as is shown in the following short table :—

Absolute Pressure in lbs. per square inch.	Total Heat.	
	Regnault's Experiments.	Equation (36d).
14·7	1146·60	1146·68
30	1158·28	1158·22
60	1171·17	1170·85
100	1181·86	1180·96
150	1191·20	1191·15
200	1198 34	1196·32

These results do not pretend to exactitude beyond the first decimal figure.

The general formulæ we have had may be applied to the different problems connected with the expansion and heating of superheated steam under different conditions. We have only space to notice one—that of *adiabatic expansion.* In this case we place $JdQ = 0$ in equation (35a), and then

$$v\,dp + \gamma p\,dv = 0$$

or,

$$\frac{dp}{p} + \gamma\frac{dv}{v} = 0$$

integrating between the limits $(p_1 v_1)$ and $(p_2 v_2)$ we have

$$\log_e \frac{p_1}{p_2} = \gamma \log_e \frac{v_2}{v_1}$$

or,

$$\frac{p_1}{p_2} = \left(\frac{v_2}{v_1}\right)^{\gamma} \quad\text{..........................(37)}$$

hence $p_1 v_1{}^{\gamma} = p_2 v_2{}^{\gamma} =$ &c., as in the case of air, only for superheated steam $\gamma = 1·333$.

If we compare this equation with the corresponding one for saturated steam, we see that in superheated steam the adiabatic curve approaches the axis of abscissæ somewhat more rapidly than that for saturated steam.

The external work done by unit mass of the steam is

$$W = \frac{p_1 v_1}{\gamma - 1}\left[1 - \left(\frac{v_1}{v_2}\right)^{\gamma-1}\right] \quad\text{..............(38)}$$

We have now considered as far as is possible, within the limits of a single paper, how the chief formulæ used in thermo-dynamical investigations are obtained. A systematic treatise would, however, be necessary to do the subject justice. Although some of the methods adopted have, from want of space, suffered from undue compression, I hope the results given will serve as a basis for the future discussion of the applications of thermo dynamics to problems connected with heat engines.

On the motion of the PRESIDENT, a vote of thanks was cordially awarded Mr Dyer for his paper.

On Mr Mansel's and the late Mr Froude's Methods of Analysing the Results of Progressive Speed Trials.

By Mr William Denny.

(SEE PLATES VI., VII., VIII., IX., X., XI., XII., XIII., XIV., XV., XVI., AND XVII.)

Received and Read 23rd December, 1884.

In the spring of the present year, Mr Robt. Mansel issued a pamphlet (see p. 102) in which he criticised with considerable severity some remarks I made in the discussion of a paper read by the late Mr William Froude before the Institution of Naval Architects, on the 7th of April, 1876. As Mr Mansel addressed this pamphlet to the President and Council of this Institution, it seems a right and proper thing that any reply should be made before the members, which will give as much publicity to the reply as Mr Mansel, by a wide circulation, gave to the pamphlet. On the 14th of May I addressed a letter to Mr Mansel, acknowledging receipt of his pamphlet, and on the 9th of the following month I wrote to him again, intimating the manner in which I proposed to deal with it. From the first of these two letters I make the following quotation, as it deals with a point in Mr Mansel's criticism against which I thought it was necessary to protest promptly. I said:—"Even, however, from the short perusal of your letter, I gather that you consider my remarks to have been made in some underhand way, and, as it were, behind your back. To this I feel compelled at once to demur. What I said was said publicly, and before a public Institution. Indeed, as the Institution of Naval Architects is exclusively devoted to Naval Architecture and Marine Engineering, and, I believe, the only Institution in this country exclusively devoted to these subjects, it never occurred to me that its proceedings would be unknown to a professional man of your high standing. Whatever points there may be for discussion between us, I hope you will understand that your failure to learn of my remarks made in 1876, is not a matter for which I am prepared to accept any blame." These sentences sum up my reply to this portion of Mr Mansel's criticism, and I shall add nothing further to them now.

Mr Mansel condenses the statements which I made in the discussion by saying, that they amount to:—"R. Mansel, taking advantage of a private communication to him of a discovery of Dr Froude, had devised another means of representing the same idea, and proffered it to Mr William Denny as his own discovery." I do not think any one who reads my remarks as they are quoted in Mr Mansel's Letter of Reclamation will take such a meaning out of them. What I said amounted to this—that, in a conversation with Mr Mansel, I had conveyed to him Mr Froude's idea of determining the initial friction by reducing the indicated horse-power curve to a curve of indicated thrust, and prolonging this curve to the vertical axis raised from the speed zero. In reply to Mr Froude, I said:—"I do not think Mr Mansel originally did more than proceed on the notion I had given him of what you had done, and I believe he forgot all about where the notion came from, which is common enough for all of us to do. I have, myself, occasionally borrowed from my friends, but when I have been reminded of it I have acknowledged it, although I could not say at the time where the idea came from." This is a very different statement from accusing Mr Mansel of taking advantage of a private communication. When any great subject is under discussion, and when ideas about it are prevalent, it is often very difficult to determine to whom the credit of their origination should be given, and it is often as difficult to determine whether they originated spontaneously in several minds at once, or were conveyed from one mind to another by suggestion. At the time I made the statements which Mr Mansel has quoted I was under the impression that the idea which I conveyed to him had lain in his mind, and was the seed from which germinated his method of dealing with the initial friction. I have had a most careful search made into every scrap of evidence which might indicate that Mr Mansel originated his method of dealing with the initial friction previous to my conversation with him in the autumn of 1875, but I cannot find any such indication. I have found one letter from Mr Mansel on the subject of the trials of the "Hawea" and "Taupo," which he addressed to me on the 17th of May, 1875, and in which he speaks of the initial friction, or rather the pressure necessary to work the engines unloaded, as being probably the cause of the differences between the trial results of these two steamers. Any one who will carefully read this letter—printed in the appendix—will observe that Mr Mansel did not show any way of determining the amount of the initial friction, but made two arbitrary corrections, and that at the two extremes of speed, upon the five pounds which he had allowed for it. He thus made the results of his analysis square with the trial results. If Mr Mansel can bring me any proof that he had worked out the idea expressed in his paper, read before this Institution in March, 1876, previous to the conversation which we had in the autumn of the year before, then I shall admit that I was mistaken in my impression about the matter. If the idea of defining the amount of initial friction from a line passing through force ordinates as distinguished from power ordinates, which underlies the continuation of the indicated thrust curve and the continuation of the straight line passing through the logarithms of the corresponding pressures in the cylinder, occurred to Mr Froude and Mr Mansel independently, then I shall have pleasure in acknowledging that I was mistaken. At the time of the discussion I was under the impression that Mr Mansel had taken the idea of working with piston pressures from my conversation with him. Wherever I spoke under this impression I admit myself in error. Mr Mansel had the idea of using piston pressures from the first, and he also knew that initial friction must be an element in the power developed. What Mr Froude showed me when I met him at the meeting of the British Association at Bristol in the autumn of 1875, was that by means of the indicated thrust curve it was possible to determine the *amount* of this initial friction. It was this idea which I discussed with Mr Mansel in the conversation to which he refers, and it was the possibility of so determining the initial friction from the indicated thrust curve which he rejected. On the occasion of our meeting, Mr Mansel denied the possibility of so obtaining the initial friction. His statement—"There is more than friction, I can prove it," I do not remember to have heard. I do not wish to be too severe in commenting upon any of the expressions used by Mr Mansel in his Letter of Reclamation, but there is one to which I must draw the attention of this Institution. On page 4 of his pamphlet, at the end of the first paragraph, Mr Mansel says, referring to myself—"I never, however, imagined his misunderstanding would have carried him so far as to necessitate a public proof of his having made himself ridiculous about a fallacy!" I do not think this fit language to use in controversy, and it is my purpose throughout this paper, and any discussion which may follow upon it, to use this sentence as an indication of what should be avoided and not as an example to be followed by myself, or, I hope, by others.

When Mr Mansel began to analyse the progressive trial results, which he found in my paper of March, 1875, he did so in ways which in many points differed from his later analysis. He began with the attempt to correct the Admiralty formula connecting area of midship section, indicated horse-power, and speed, by substituting a better measure of resistance for the midship area, and by differentiating in the power an assumed value for the amount of the initial friction. After the discussion closed he added a memorandum to it, splitting up the indicated horse-power into six different items. Three of these were factors. 1st—the factor involving initial friction, or the pressure necessary to work the engine unloaded; 2nd—the factor for slip; and 3rd—the factor for friction due to the working load. Dividing the gross indicated horse-power by these three factors, he obtained E_v, or the effective power. This he split into three terms—E_f, the power due to the skin friction of the wetted surface of the steamer; E_m, the power due to the movements communicated to the fluid in the vicinity of the steamer; and E_r, the power recovered from the wake by the propeller—a quantity which was deducted from the sum of E_f and E_m. Mr Mansel calculated the amount of each of these elements by allowing for the constant decrement of the pressure five pounds per square inch on the high-pressure cylinder, for the friction due to the load $\frac{1}{10}$th of the residual power, and for the slip a percentage of the residual power corresponding to the slip percentage. E_f he calculated by an approximate estimate of the wetted surface, and a formula of Beaufoy's; E_m he calculated by a formula founded on Poncelet's principle of investigation. E_r at first he allowed as equivalent to $\frac{1}{2}$ E_f, but this giving rise to some discrepancies in the results, he calculated—only by a more complicated formula—the value of E_r as well as the values of E_m and E_f. The result of these various calculations was the production of figures which very closely agreed with the experimental trial results of the "Goa" and "Africa." These are the methods with which Mr Mansel began in 1875. In 1876 he changed to a very different method, under the impression that he had discovered constant laws for the revolutions, pressures, and gross developments of power, true for each steamer, although differing between steamer and steamer in the values of certain quantities, which are constant for each steamer. He still mentions at the end of his paper his original analysis of power, but thereafter this portion of his investigation shrinks in amount and diminishes in value, absorbed by the theories of the straight lines. The leading elements of his 1876 paper are the revolutions and the piston pressures, and their combination in the gross power. On the underside of a speed axis he sets down as ordinates the logarithms of the speeds and so constructs a curve. Setting up from this curve at each of the speeds the logarithm of the corresponding revolutions he obtains a straight line. On the logarithms of the revolutions he sets the logarithms of the piston pressures at the various speeds, and he obtains another straight line inclined to the base line of revolutions from which it has been plotted. The pressure corresponding to the logarithm at the zero of these two lines he calls the constant decrement. For the purpose of this paper, as the points in discussion with reference to Mr Froude's work relate to pressures, it is unnecessary to take in the question of the revolutions, and the gross power, or even the residual pressures subsequently introduced. In bringing Mr Mansel's theory to the test of a larger experience than it has yet touched, I shall therefore start from the basis of proposition No. 1 in his paper read before this Institution in March, 1876. This proposition is as follows :—"Experimental law of the pressures.—If a steam vessel be tried at various speeds, and if along an axis, at points representing the speeds, parallel lines be laid off, upwards, representing the logarithmic values of the corresponding piston pressures, the ends of these lines will range in a straight line slightly inclined to the axis, and having its ordinate at the origin, equal to the logarithm of the statical friction of the machinery." In this proposition Mr Mansel calls the "constant decrement" the "statical friction of the machinery." I hope he will permit me to use the words of Mr Froude, and to call this quantity for convenience during the remainder of this paper "initial friction."

On page 5 of the Letter of Reclamation Mr Mansel says:—"I do not doubt that Mr Denny honestly believed that Dr Froude's method and mine were alike at bottom, and gave the same result, and in this belief he repelled any explanation." I am surprised at Mr Mansel making this statement, when what I really said as quoted by him on page 2 of his Letter of Reclamation was :—"You will see that a different method of analysis has provided us with, although not an exact confirmation, yet a very close confirmation of what Mr Froude has said." Any one who reads my remarks will see what I meant was this—that while the general idea in Mr Mansel's method of finding the initial friction corresponded with the general idea upon which Mr Froude had worked, the methods were different, and the results were also to some extent different. I have compared their results by the values found for them in a selected list of progressive measured mile trials which was compiled for the following purpose. My firm constructed about two years ago an experimental model tank very similar to that erected for the Admiralty by the late Mr Froude at Torquay. As soon as we got it into working order, and had organised its experimental staff, we determined to go back upon our data of progressive trials, and to analyse them by its means. For this purpose early in last year a list was selected of those which were most faultless in the matter of propeller immersion and weather. No trial was put upon this list in which the weather was not of such a nature as to permit reliable results to be obtained, or in which the propeller was not fully immersed. From this list I have further omitted, for the present purpose, all ships for which the number of double runs on the measured mile was less than four. So far as the accuracy of observation is concerned, all our trials are of equal value, as they are all conducted by a numerous and well-trained staff. It is fairer to test Mr Mansel's later formula by such a selected body of results, than by the total number of our progressive trials, in which there are many of doubtful or secondary value, owing to the nature of the weather or the immersion of the propeller. On Table I. in the appendix will be seen the extreme differences in the percentage ratios of the initial friction expressed in indicated thrust to the maximum indicated thrust exerted during the trial. By Mr Froude's method the highest ratio is 18·5 per cent., and by Mr Mansel's method the ratio for the same ship is 8·8 per cent., while the highest ratio is 10·6 per cent., the lowest ratio is by Mr Froude's method 2·8 per cent., and by Mr Mansel's 1·7 per cent. It will be observed that in constructing this table use is made of piston pressure as well as of indicated thrust. In the diagrams of curves indicated thrust alone is used. The 30 trials used in the table are arranged in three divisions. The first of these contains those vessels of which the lines of indicated thrust, set off in logarithms, come most nearly to Mr Mansel's straight lines. They correspond to the illustrations given in Fig. 4, Plate VII. The second division corresponds to the illustrations given in Fig. 5, Plate VII., and includes those steamers of which the lines of indicated thrust, set off logarithmically, diverge further from straight lines although still capable of being fairly continued to the speed zero so as to give some measure of the initial friction. The third division, corresponding to Fig. 6, Plate VII., contains those steamers of which the indicated thrust lines set off logarithmically are so irregular in curvature that they can not fairly be continued to the speed zero. Twelve steamers are included in each of the first and second divisions, and six in the third division. Although there is a very real similarity between the methods of Mr Mansel and Mr Froude there is a considerable difference in the reasons which each of these gentlemen has given for his method. From Mr Mansel's Letter of Reclamation it is evident he misunderstands Mr Froude's method and in so strange a manner as to leave the impression that he never took the trouble to make himself acquainted with Mr Froude's ideas upon the subject. Mr Froude started with the knowledge, which he had obtained from his experiments, that skin friction varied pretty nearly as the square of the speed, or more exactly as the power 1·87. He inferred that the lower portion of the indicated thrust curve might be safely continued according to this power of the speed, because in all steamers at the lower speeds the surface friction constitutes very nearly the entire resistance. As the curve rises above these low speeds the other elements of the resistance involved in the formation of waves and eddies begin to make themselves felt, and increase the resistance in a very much greater ratio than the square of the speed. In the letter which Mr Mansel received from Mr Froude, and which he so frequently quotes, this is made abundantly clear, as is shown, by the quotation on page 12 of the Letter of Reclamation, where the following remarks occur:—"The meaning of all this is that, as a matter of fact, the resistance of the 'Merkara' is practically as the square of the speed, up to quite 9 knots, above this speed, the resistance increases in a higher ratio, and then of course for higher speeds deviates from the parabola, which correctly expresses it so far." Mr Mansel must have had this sentence before his eyes in the composition of his Letter of Reclamation, and yet on page 7 he writes as if Mr Froude would use the square of the speed throughout the whole range of the speeds. This imputes to Mr Froude an idea which had no place in his mind. Mr Froude knew very well from his model experiments that resistance curves in no case followed the supposed law of the square of the speed, excepting in such portions of them as came within the dominating influence of skin friction. Besides, confirmatory proof, from progressive trials, of the fallacy of this square of the speed theory had been furnished by myself in the paper which I read at Bristol before the British Association in 1875. In this paper I showed that the corresponding theory of the power required to overcome a vessel's resistance varying as the cube of the speed was quite untenable. Having on page 7 of his Letter of Reclamation assumed for Mr Froude the notion of the resistance varying continuously as the square of the speed, it was very easy for Mr Mansel to bring this notion to a *reductio ad absurdum*, and to write the following sentence on page 8:—"But in this Dr Froude simply *begs the question* for he had no more right to assume this value than the 4·06 of the pair above, and the obviously absurd —2·09 of the upper pair." In the same vein Mr Mansel continues—"It is true, a limitation is laid down that 'the resistance is not to outrun the square of the speed,' which amounts to saying that the method is not applicable to a steam vessel at all; for I not only deny, but can offer most satisfactory proofs that in *no* instance does this assumed law, of resistance varying as the square of the speed, hold good." It is very easy to defeat an opponent, however great that opponent may be, if you are at liberty to make his opinions for him. Mr Froude based his continuation of the resistance curve upon his knowledge from a very large number of experiments, most accurately performed and observed, that at low speeds the resistance of any steamer was composed almost entirely of skin friction, the law of which he had also discovered by experiments to be a variation according to a power of the speed ranging from 1·83 to rather over 2. Mr Froude, in this same letter to Mr Mansel, gives a tabulated comparison with a group of pressures deduced from his own method and also from that of Mr Mansel. He carries this comparison the length of 10 knots, and I assume he carried the comparison to this point because it was the next whole figure above 9·2, one of the two lowest determined speeds. But he did it also for the purpose of showing that the error in the assumption of the square of the speed began to tell above 9 knots, as will be found in a quotation made from his letter on page 12, which is as follows:—"The curve calculated by the equation $P = a + b V^2$, cuts exactly the two points of pressure given by the 'Merkara' experiments, for the 6·2 knots and the 9·2 knots. This, of course, it was bound to do, because it was calculated from them; it, however, cuts below the point which belongs to the 11·09 speed, and still more below that which belongs to the 12·91 speed." It is quite evident from this that Mr Froude did not intend his comparison to be carried at the outside beyond 10 knots, nor do I believe he intended it even to be carried so far, excepting for the purpose of demonstrating the variation which began to take place between it and the curve deduced from the experiments. Yet, in the face of all this, Mr Mansel carries on the square of the speed up to nearly 13 knots, and in doing so furnishes himself with what he fancies is a crushing argument against the method of Mr Froude. On page 11 Mr Mansel writes as follows:—"It is obvious, from 13 knots to 3, according to Dr Froude's figures for this last speed, there is as perfect agreement as could be expected between the formula values and the experimental. Now, from its nature, this curve of mine must develope into a straight line when the logarithmic ordinates are set up to the speed abscissas, and upon no reasonable principle can it be contended that the law giving a straight line from 13 to 3 knots should not continue true for the remaining three knots; and thus by going to the origin we get the value of m belonging to the limits of experience. It is, however, a matter of certainty that experiments made between the 6·2 knot speed and zero, would have shown a change of value of m due to changed circumstances explained as the lower *conjugate* solution. The value of 10·04, however, being derived from the 9·2 and 6·2 knot speeds, is not in any way connected with the unknown region under the 6·2 speed, and consequently, is neither true for the experimental nor the unknown lower speeds with the steam vessel. Dr Froude's curve is hopelessly erroneous at the higher speeds, and is only true for the 9·2 and 6·2 speeds, because he compelled it to take the true values at these points." Of course, Mr Froude's curve is hopelessly erroneous for the higher speeds if it is made out in direct contradiction to Mr Froude's ideas, and the explanation given by him in his letter to Mr Mansel. Mr Mansel's assumption "that upon no reasonable principle can it be contended that the law giving a straight line from 13 to 3 knots should not continue true for the remaining three knots" is an assumption and nothing more, based upon his happening in the case of the "Merkara" to have hit upon a straight line. It must, also, be remarked that the experiments justifying Mr Mansel's straight line range only from 13 to 6 knots. They do not extend to three knots. Is it possible that Mr Mansel, after condemning Mr Froude's method, went to three knots on the ground of their agreement down to this point? Mr Mansel considers the lower end of the pressure or indicated thrust line between the lowest speed obtained on trial and the speed zero to be an unknown region, for which inferences can only be drawn from the portion of the pressure line above the lowest trial speed. But we are not in such complete ignorance of this portion of the pressure line or indicated thrust curve, since we know from Mr Froude's investigations that at these low speeds the resistance of the steamer is made up almost entirely of skin friction, and, further, that the power in which this resistance varies is pretty nearly the square of the speed. If it is true that, deducting the element of initial friction, the remaining elements in the gross power are practically proportional to the effective power, *i.e.*, the product of ship's resistance and speed divided by 33,000, then it may be held that the curve of pressure or indicated thrust, less the amount due to initial friction, will follow in its form the curve due to the actual resistance of the steamer. In this case, we can with Mr Froude complete the lower end of the resistance curve as a parabola. But if we do this we shall not obtain a straight line with Mr Mansel's system of logarithmic off-setting, but a line which will have in it a contrary flexure, and will finish against the vertical passing through the speed zero with a curve convex to the axis of speed. Mr Froude has shown this at length in the letter which he addressed to Mr Mansel. Mr Mansel can hardly object to the idea that the indicated thrust or pressure less that due to initial friction bears a fairly constant ratio to the resistance, because in his original analysis of power added to the discussion of my paper in 1875 he practically worked upon such a basis.

I must now show that Mr Mansel's straight lines are exceptions and not the rule, and that, judged by such a body of experiments as I shall lay before this Institution, they must be called haphazard coincidences. This proved, the whole fabric of his reasoning falls to pieces, being supported by nothing else than such coincidences. On the other hand, Mr Froude's method remains founded upon an idea deduced from many experiments, and is therefore of greater interest. I do not say of greater value, because I do not think it is possible either by Mr Mansel's or Mr Froude's method to arrive at a quantitative measure of the initial friction. The variation of the results as shown by both these methods is sufficient to shake confidence in them, and the late Mr Froude, who was eminently given to hold lightly to ideas which were not confirmed by manifold experiments, had doubts upon his method which do not seem to have occurred to Mr Mansel with reference to his method. These are illustrated in the following quotations from letters of Mr Froude addressed to me on 24th May and 4th July, 1876, in which he was writing about the trials of H.M.S. "Shah." In the former he says—"As to the 'Shah' we got what seemed *primâ facie* a very fair curve of power and pressures, but the particulars, when plotted, were, in the first place, somewhat inconsistent with each other at the low speeds, and, in the second place, gave at all events a measure of the constant friction which was scarcely credible from its smallness—one interpretation made it equivalent to only about $\frac{1}{40}$ of the maximum working pressure, the other about $\frac{1}{10}$. But at the higher speeds, interpreting the indicated thrusts by the ship's ascertained curve of resistance which our experiments here had supplied, it appeared that the working friction was excessively great, getting on for equivalent to, or even in excess of, the ship's true resistance at the respective high speeds." In the second letter he says—"The 'Shah's' constant friction does not *seem* to be as much as $\frac{1}{18}$ of the maximum load; but the lower speeds were not good. It seems hardly intelligible too—for one of the crank pins was continually heating."

If you will refer to the table giving the comparative initial friction by Mr Froude's and Mr Mansel's methods from the list of selected progressive trials already described you will observe that the number of trials included in that table is 30. The whole of the power and speed curves of these trials have been reduced to curves of logarithms of indicated thrust, and out of their number only 12 can be said to approximate to straight lines. In order to show you how leniently these cases have been judged, Fig. 4, Plate VII., shows the best and worst of the 12 curves with reference to straightness. These are the trials corresponding to the first division of the initial friction table in the Appendix. In this division the straightness ranges from that of the "Goa" to the curvature of the "Quetta's" line.

In Fig. 5, Plate VII., are given the indicated thrust lines corresponding to the second division of the initial friction table, lines which, although inferior in straightness to those in the first division, are such as to allow of their being, with some fairness, carried on to the speed zero for the purpose of defining the initial friction. In this division the "Wairarapa" (2nd trial) represents the nearest approach to straightness, and the "Booldana" the greatest departure from it. In Fig. 6, Plate VII., is given the indicated thrust line of the "Clyde," which is one of the worst of the third division of the table. The lines in this division are so irregular in their curvature as to render them useless for the purposes of determining the initial friction ordinate. Of all the steamers enumerated in the table, the "Merkara," "Goa," and one other only have logarithmic lines which can really be called straight. As a matter of curiosity, I have given also a diagram of the trials of four vessels out of several built by my firm, of which Mr Mansel has at various times published the logarithmic lines. These four vessels have been selected because the five indicated thrust lines produced from their trials have not less than four trial spots each. On this diagram the "Merkara" and "Goa," just mentioned, appear.

But it is not alone by data collected from the special records of my own firm that the exceptional nature of Mr Mansel's straight lines can be shown. If you will refer to Figs. 1, 2, and 3, Plate VI., you will find the results of the progressive trials of six Admiralty vessels plotted by Mr Mansel's method, some of the plottings having been previously published by Mr Mansel himself. Many of the lines are very far from straight, the best approximations being two lines for the "Iris," one for the "Carysfort," and the one for the "Shah." These trials have also been selected on the ground of their having not less than four trial spots and complete immersion of the propeller. Fig. 1 shows the lines deduced from the "Carysfort's" trials, Fig. 2 those of the "Prince Consort," the "Hecla," the "Heroine," and the "Shah," and Fig. 3 those of the "Iris."

Again it is not only by progressive speed trials, in which, so far as the indicated horse-power is concerned, many variables are involved, that Mr Mansel's method can be shown to be inherently wrong. We have, thanks to the genius of Mr Froude, accurate methods of model experiments by which to judge such propositions, and their application to a very much wider range of speeds than is possible on the measured mile in all but very exceptional types of steamers. It is not difficult to show that Mr Mansel's method of straight lines is only applicable when the pressure or indicated thrust curves have no contrary flexure, but all experiment curves deduced from models have contrary flexures, and in steamers such as the torpedo boats, where the speed trials are carried far enough, the contrary flexures become apparent even in the indicated horse-power curves. The late Mr Froude, in his most interesting and valuable paper upon the effects of the addition of middle body to models, first indicated the causes of these contrary flexures or humps and hollows in the curves.

These peculiarities, an example of which—taken from our own model results—is shown on Fig. 10, Plate X., exist in the resistance curves of all models, and the first hump, which is only slightly defined, begins at speeds far below those proportioned to the very high speeds of the torpedo-boats: for example in the "Merkara," the first hump which wehave been able to trace is at a speed for the ship below 13 knots. Faint indications of small humps existing even much below this rate of speed are sometimes traceable in resistance curves. As shown both by Mr Froude and his son, the positions of these humps and hollows on the resistance curves are determined by the varying positions of the two kinds and series of waves accompanying the model. But this is a subject far too large to be treated within the limits of this paper. Ample materials for its study and further development exist in their papers. They contain the results of their experiments and observations upon these interesting points. In connection with this subject of waves it may interest you to see a comparison we have been able to make between the wave profiles of two of our paddle steamers—the "Minerva" and "Lucinda"—the latter tried only last Saturday, as these wave profiles were observed upon the mile and as they were observed at the corresponding speed of the model in the experimental tank. The wave profiles are for two speeds in the one case and for one speed in the other, and their comparisons are shown in Fig. 11, Plate XI., and Fig. 12, Plate XII. These wave profiles were observed and plotted quite independently, and were traced to the same scale upon corresponding profiles of the steamer. When the tracings were laid upon each other the result was as shown in the diagram. It will be observed that the profiles do not correspond abaft the paddle wheels, but this want of correspondence is easily accounted for by the effect of the paddle race upon the water surface. The correspondence between the wave raised by the model in the tank and that raised by the full-sized steamer on the mile, which is the basis of Mr Froude's law of comparison, has a most useful bearing, in connection with our tank, upon the prediction of the speeds of fast paddle steamers. As the efficiency of the paddle wheel depends upon the proper immersion of its floats, it is evident, if the steamer is driven at a speed which causes a wave crest beneath the paddle wheel, there is a risk of the floats being over-immersed. On the other hand, if the speed of the steamer causes a wave trough beneath the paddle wheel, there is a risk of the floats being insufficiently immersed. A most interesting practical illustration of this last condition was given at the 1881 meeting of the Institution of Naval Architects by one of the members of this Institution, Mr James Hamilton, jun. In the discussion following Mr Hamilton's paper Mr R. E. Froude pointed out that the occurrence of a wave trough or wave crest beneath the paddle floats has a further importance from the fact that in the case of the crest coming beneath the wheel the floats will be working in a forward moving current, whereas in the case of a hollow they will be working in a sternward current. As the humps on the resistance curves, to which I referred above, occur at comparatively moderate speeds, the measured mile trials of ordinary steamers, if the spots were sufficiently numerous, would very probably show irregularities which are at present unapparent. In the only case known to me where very frequent spots have been obtained from the trial of a steamer such a hump and hollow are very distinctly shown. This trial was carried out on the "Spartan" by Mr Biles, naval architect at the Clydebank Shipyard. The method of trial and the apparatus employed on the occasion we owe to his invention and energy. At the time Mr Biles read his paper upon this new method, I gave it a hearty welcome, upon the ground that it promised to link up much more closely the results of tank work and actual trials. I refer to the "Spartan" as a proof of the necessity which exists for progressive trials involving more frequent spots than are now common. For the speed at which they occur, the hump and hollow in her curve appear more pronounced than one would expect.

In order to illustrate the effect of Mr Mansel's method upon model experiments, I have had the resistance curves of two models tried in our tank, which are in no way exceptional, plotted out in Fig. 7, Plate VII. The abscissæ represent speed, and the ordinates the logarithms of the resistances of ships similar to the models at the various speeds. It will be seen from this diagram that there is not even an approach to straightness in these lines.

With Mr Mansel's method of analysing the power developed in progressive trials by means of logarithmic straight lines, there are four quantities which require to be determined before speed prediction can be attempted.

1. The amount of the initial friction.
2. The zero ordinate of the revolution logarithmic line.
3. The angle of the pressure logarithmic line; and
4. The angle of the revolution logarithmic line.

Mr Mansel reduces these four items to two by working with the gross power line and its angle, but even for the use of this in prediction no guiding principles are laid down by him.

I conceive it quite possible that from a sufficient amount of data the first two quantities could be assumed for any given new vessel with such a margin of allowance as to make them safe, but I have never yet been able to see on what principle Mr Mansel could select the angles of the two straight lines or of the one straight line, nor does he show in any of his papers how he would propose to do this or what he would make the test of these angles. He gives in the discussion on his paper "On some points in the theory of thermodynamics" a rough general rule, but no guiding principles. In truth, one of the weaknesses of his method, apart altogether from its want of correspondence with large experience, is its comparative uselessness for purposes of prediction; I am not aware of any naval architect who has so used it. Is this failure in the power of prediction not in itself corroborative proof of the error of the whole formula?

Mr Mansel, in at least two of his writings subsequent to March 1876, found occasion to doubt the universality of the application of the laws of his straight lines. The most evident occasion for such doubts certainly exists in the torpedo boats. The humps and hollows apparent upon their power and speed curves are sufficient to show that they are not likely to be reducible to straight lines. On this account Mr Mansel, in the paper last referred to, found himself obliged to resort to a series of articulated straight lines instead of the single straight line which he had been previously using. To show the general nature of the indicated thrust curve of the torpedo boats, and of the logarithmic line deduced therefrom, I give one example in Fig. 9, Plate IX. This represents a boat built by Messrs Yarrow & Co., the trials of which were published in *Engineering* on the 17th October, 1879, and are, I believe, thoroughly trustworthy. Mr Yarrow informs me the screw was four inches out of water when the boat was at rest, but was completely immersed when running. But the case of the torpedo boats does not differ from that of any other ordinary steamer, except that they have an exceptionally high allowance of power, and can consequently be driven at speeds which are impossible for the ordinary types. We know from our model experiments that steamers now running at speeds not exceeding ordinary expectations would, if supplied with power proportionate to that in the torpedo boats, perform as great feats, and show the same humps and hollows in their speed curves. Mr Mansel, in his note on the trials of H.M.S. "Iris," published in *The Engineer*, 22nd March, 1878, also admits that a straight line does not adequately suit the case of the "Prince Consort," although that ship is not at all powered as the torpedo boats are powered. It is a very great pity that these side lights of doubt, which met him in the way of his investigations, did not lead him to see that instead of the character of universality his method had only the character of occasional fitness.

I notice that Mr Mansel speaks in many cases as if there were one speed suitable and proper for each steamer, and I am surprised to find him, in the present state of our knowledge, giving currency to such an idea. There is no *single* speed proper to any type of vessel, but if we are to judge from the resistance curves found by experiment from models there is a series of speeds indicated by the humps in these curves unfavourable, and another indicated by the hollows of these curves favourable, for easy propulsion. But these humps and hollows rather indicate groups of favourable and unfavourable speeds than determine individual speeds. The changes in the curves are not of the nature of cliffs, as one would expect from the old notion of special speeds, but gentle undulations from hollow meadows to rounded hills. In speaking of favourable and unfavourable I am only doing so in a popular sense, because these conditions may be traversed by other considerations which would lead one to choose, even after a careful investigation, a "hump" instead of a "hollow" speed for particular purposes. Each model has these humps and hollows arranged and formed differently, and of different amounts, according to the variation of the proportions of its dimensions and the variation of its fineness, and for each model, if perfect accuracy is desired in prediction at high rates of speed, such characteristics must be found by experiment. No doubt the notion of appropriate speeds peculiar to different types of steamers had some value in it as a stimulant to further inquiry, and we owe much to the late Mr John Scott Russell for its utility in this sense. But the notion has now ceased to be useful. Doubtless the fact that in some steamers tried progressively the power and speed curve tended to become vertical at the higher speeds somewhat revived this old notion. But experiments and better knowledge have since taught us not to trust to the finality hinted at by such speed curves, but to believe in further possibility. The addition of more power, changing the propeller if necessary, would, I am certain, in all such cases produce still higher speeds. In several of his papers Mr Mansel asserts that his logarithmic line of the pressures turns up when the steamer is overdriven, but this is an incomplete statement, owing to an incomplete appreciation of the nature of resistance curves. When a steamer is driven at exceptionally high rates of speed the logarithmic straight line will be found not merely to turn up but also to turn down from its angular direction, owing to the contrary flexure in the ordinary curve.

I think it well to say a few words upon the analysis of power with which Mr Mansel nine and a half years ago started his discussion of my progressive trial data. In this analysis he divided the gross indicated horse-power as has been already described in the earlier portion of this paper. I may remind you that he divided the effective horse-power, E_v, into:—E_f, the power consumed in overcoming the surface friction of the vessel's hull; E_m, the power consumed in movements communicated to the water in the vicinity of the vessel; and E_r, a credit quantity for the power recovered by the propeller from the wake. At the speeds with which he was dealing he asserted that E_m absorbed the larger portion of the effective horse-power. This is an erroneous statement, and had he carefully studied the late Mr Froude's experimental work in connection with the analysis of resistance he would have found the reverse to be the case, and that at the most of the speeds involved the skin friction formed the largest portion of the resistance, as can be well seen on Fig. 10, Plate X. But Mr Mansel was unable to pursue this analysis further, because he had not the command of the necessary apparatus for experiments to carry out the investigation.

In Mr Froude's analysis of the expenditure of the indicated horse-power made in his paper upon this subject read before the Institution of Naval Architects in 1876, he split up the indicated horse-power into two factors, the one being the ratio of the speed of the propeller to the speed of the ship, or what Mr Mansel calls the slip ratio, and the other an item to which Mr Froude gives the name Ship's Horse Power. This latter element he divides up into the following terms:—

1. The effective horse power corresponding to the resistance of the vessel if towed.
2. The power spent on augmentation of the resistance due to the action of the propeller, which causes a suction on the run of the vessel, and consequent decrease of pressure favourable to her propulsion.
3. The power spent on friction of the propeller.
4. The power spent on the initial friction of the machinery.
5. The power spent on the friction of the load; and
6. The power required for pump duty.

In the course of his analysis, which he did not lay down as absolute, he recommended the reduction of the power term to a force term by converting it into indicated thrust, indicated thrust being the indicated horse-power multiplied by 33,000, and divided by the speed of the propeller in feet per minute. By the use of a multiplier, constant for any given steamer, this indicated thrust can be immediately reduced to Mr Mansel's term of $P + rp$. It will be noticed by any one who reads Mr Froude's paper carefully that he gave very grave prominence to the effect of the propeller in augmenting the resistance of the vessel, but he did not, in the course of his paper, make any mention of the help afforded the propeller by the wake. In Mr Mansel's analysis of power it is curious that while he takes no notice of the augmentation of resistance caused by the propeller, which is a discovery of Mr Froude's—the result of experiment—he laid very considerable stress upon what he called the recovered power, *i.e.*, the power recovered by the propeller from the wake. I understand Mr Froude summed up all such effects in the item of slip, but he does not develop the matter, and in this connection I think it right to draw attention to the prominence given to it by Mr Mansel. The series of tank experiments in which the propeller truck was added to the model resistance truck commenced by the late Mr Froude, and continued by his son, have led to some very curious information being obtained on this point. I believe Mr R. E. Froude looks upon the augmentation of resistance, or thrust deduction as he prefers to call it, and the gain obtained for the propeller by working in the wake, as quantities which very nearly balance each other, but which may even leave a remainder in favour of the steamer. Both father and son have worked very hard and very steadily at this question. It is one which has not yet reached a complete solution, nor will it reach even an approximation to complete solution without a great deal more work being expended upon it. Without the help of model experiments it would be impossible to take any steps in it with certainty.

By means of these experiments, and by means of Mr Froude's law of comparison, it is now possible to predict the amount of the resistance of any given steamer at any speed with a very fair degree of accuracy, excepting in cases where the form is such as to produce considerable eddy resistance. Mr Froude summed up the resistance due to the motion of a vessel in the water as being composed of three elements:—Skin friction, the resistance due to the formation of waves, and the resistance due to the formation of eddies. In an ordinary well-formed steamer the resistance due to the formation of eddies is so small that it scarcely affects the total results. Assuming this to be the case, then, knowing the law of resistance due to the formation of waves as it connects models and full-sized steamers, and knowing also the law in accordance with which the surface friction varies with fair approximation, we are able to predict the resistance of a full sized vessel, and to state the amount of indicated horse-power which would be required to drive that vessel at a given speed if the whole of the power developed in the engines were applied to this purpose without any loss. I may remark that the variation of the element of skin friction is not constant with increase of length, but decreases, as measured per unit of surface, with increase of absolute length of the surface. Mr Mansel uses a formula of Beaufoy's in which no allowance is made for this peculiarity of variation; very probably it had not been observed in Beaufoy's time; but it is of very vital importance to these investigations, as without it the prediction of the resistance of the full-sized vessel from the model experiments would be more hazardous. But the subject of the analysis of indicated horse-power, of which the vessel's resistance forms such an important part, is too large for treatment in this paper. I have, therefore, relegated to the appendix a few further remarks upon it, together with comparative diagrams showing with as much clearness as possible not only the methods of Mr Mansel and the late Mr Froude, but also the present condition of that analysis. From what has gone before, it is evident that it is only by means of experiment we can arrive at any really valuable information regarding power analysis, and I think it is much to be regretted that Mr Mansel, with his great ability, was so easily tempted away from this really promising side of the subject. There is an immense deal yet to be done in it by patient and careful experimenters, and there is no reason why a man of Mr Mansel's ability should not have taken his share in such useful and pleasant work. Indeed, in the discussion upon my paper in April, 1875, he states on page 210, referring to the initial friction:—" The real value of this quantity in our ordinary direct-acting compound engines would be an important piece of information, which it is hoped some engineering member may see his way to experiment upon." In this sentence Mr Mansel points out the true method of investigation, and the only one which will be productive of valuable fruit. Something also may be done to help forward such investigations by more exhaustive methods of progressive trials, either in the way of obtaining many more spots of observation, or in the addition of Mr Biles' method to the ordinary measured mile trials. Further, if it is correct that the analysis of indicated horse-power is practically hopeless without some definite knowledge of the resistance of the vessel, it is apparent that without the help of an experimental tank real progress in the direction of effective analysis is impossible. The only substitute I know for tank experiments, but one useless for purposes of individual prediction, would be the towing of the full-sized steamers at the various speeds on the measured mile; but excepting in rare cases the expense of such a method of investigation would be enormous, and beyond the financial powers of any individuals or firms, however willing these individuals or firms might be to spend money upon such investigation. I am, therefore, convinced that experimental tanks will become common in the future. My own firm could very easily employ two tanks instead of one, and we are at the present moment by means of log propellers and improvements in the towing machinery, attempting to increase the experimental output by at least 50 per cent. I do not believe a public experimental tank has much chance of success, for over and above the elements of jealousy and distrust, which would be pretty sure to enter into its use, there is the difficulty that unless each individual can command not only the special item of information he requires, but practically the resultant of all the information obtained in the tank, the single item of information is of very little use to him. In this respect an experimental tank entirely differs from a chain-testing house, or such establishments for general testing purposes as are conducted by Mr Kirkaldy and Professor Kennedy.

In conclusion, I would urge upon Mr Mansel not further to press his method, because, if accepted by any large portion of the practical world (which I hope it will not be) it would certainly have three effects:—First, by the assumption that the pressure or resistance ordinates, set off logarithmically, will produce straight lines, to obscure the real need for many spots in progressive trials. What we want is more accurate and full progressive trials, and not less complete trials. Second, Mr Mansel's method, if accepted, would do harm, by setting up a quite incorrect standard of accuracy for the results of trials. I have noticed in several of his papers he has pointed to variations from his straight line as indicating inaccuracies in the trials. The probability is that the variations indicate important changes rather than inaccuracies. Third, Mr Mansel's method would do harm, as pointed out by Mr Froude in his very admirable letter which by Mr Mansel's courtesy I have had the opportunity of perusing, in the reduction of the scale of the ordinates produced by setting them off logarithmically tending to hide exceptions and small differences, instead of defining them. I do not think it possible to put this view of the subject better than it is put by the late Mr Froude, and I believe no greater advantage could result to the Institution from this discussion than that Mr Mansel should be induced to print this letter in full in connection with it. Mr R. E. Froude has drawn my attention to another objection to the use of Mr Mansel's logarithmic straight lines, and it is this—that, while a logarithmic notation is convenient for quantities which have to be analysed into factors, it is unsuitable for quantities which have to be analysed into terms. But the analysis of I.H.P. into its various elements corresponding to the elements of resistance and also to the elements of loss is more an analysis of terms than an analysis of factors. Is it not possible that this very peculiarity of logarithmic notation, pointed out by Mr R. E. Froude, may have led Mr Mansel to abandon his original and more excellent method of analysis, which was one of terms as well as of factors, for the more restricted and easier but less fruitful analysis of pressures and revolutions? I may here acknowledge my indebtedness to Mr R. E. Froude for his great kindness in many suggestions, and much valuable information afforded me as to the latest steps in the analysis of indicated horse-power.

If we look to the future it is evident there lie before us the possibilities of very extraordinary performances in speed. Both for purposes of war and for purposes of quick passenger traffic, speeds are now being required and thought of which a few years ago would have been deemed impossible and absurd. Besides, it is to be remarked that great hopes have been raised in the popular mind by the performances of the small sized torpedo boats, and it is not easy to make the ordinary public understand what a very wide interval divides the performances of the torpedo boats from even the fastest performances which have been lately attained by Atlantic steamers. Those who know the realities and the difficulties of the speed and power question are aware that in the torpedo boats there is a development of power sufficient to carry them in many cases into the region beyond the humps of their resistance curves. And they know that in no case has this region of the resistance curve ever been approached by any of the fast full-sized steamers. We are not likely soon to see torpedo boat performances on a large scale, but short of this there may soon be very wonderful speeds attained at sea, and far beyond those attained at the present moment. Looking to all these possibilities, it seems an absolute necessity that our ideas upon speed and power and the resistance question should be large and catholic, and not cramped by insufficient and empirical formulæ. There is an immense deal in these questions still awaiting solution, and I have not found it possible in this paper to do more than touch in a general way upon some of the most important points. This may stimulate the minds of those interested in the subject to further investigations and study on their own account. There is ample material for such students in the papers of the late Mr Froude and his son, and there is much hope for the future. But we must not underrate the difficulties of the subject, nor expect to get any rapid or haphazard solution of them. Like all solutions worth obtaining, they must be sought with great labour, great patience, multiplied experiments, and a readiness to doubt upon every point which experiment and practice do not fully confirm.

APPENDIX

COMPARATIVE TABULATED STATEMENTS OF ANALYSIS OF INDICATED HORSE-POWER.

In Tables Nos. II. and III. are given tabulated statements of power analysis; viz., that with which Mr Mansel started in 1875, and the late Mr Froude's as explained by him in 1876. Comparing these two analyses, it will be found that Mr Mansel has four main factors—the slip ratio, the factor involving constant decrement of pressure, the factor for friction due to the working load, and E_v. The last of these factors he divides into the three terms E_f, E_m, and E_r, there being in all six root elements in his analysis. In Mr Froude's analysis there are two main factors, one for slip and the other for S.H.P, or ship's horse-power as he calls it, which he again divides into six main terms:—The power spent on the net resistance of the ship, E.H.P.; the power spent on the augmentation of resistance; the power spent on the water friction of the screw; the power spent on the constant friction of the engines; the power spent on the working friction of the engines; and the power spent on air pump resistance. Further, the first term, E.H.P., is divided into speed and resistance; the latter being of three kinds—wave-making, eddy-making, and surface friction. There are thus in Mr Froude's analysis nine root elements as compared with the six in Mr Mansel's.

In Table No. IV. is given the present condition of the power analysis in so far as I have been able to gather it from the papers of Mr R. E. Froude, and from information which he has very kindly afforded me. It involves experiments in the tank with models of ship and screw propeller, taken both separately and in combination. The indicated horse-power is here split up into five main terms, which are:—The power spent on the constant friction of the engines and shafting; the power spent on the working friction of the engines; the power spent on pump duty; the power spent on the thrust block friction; and the D.H.P., or dynamometer horse-power, as found by such an instrument as the late Mr Froude's turbine dynamometer, less the thrust block friction. This last term is subdivided into three factors, the factor for hull efficiency, the factor for screw efficiency, and the effective horse-power. To make this power analysis clearer, this subdivision is preceded by a grouping of the factors under the heads of screw items and hull item. The factor for hull efficiency and the factor for screw efficiency are bracketed together under the head of screw items, and the E.H.P. is named—in opposition to them—the hull item. The meaning of this is that the E.H.P., or hull item, is found from experiments upon the model alone without propeller. On the other hand the screw items involve the action of the propeller; the factor for hull efficiency being obtained from experiments combining the propeller and the model, and the factor for screw efficiency from experiments with the propeller alone. Two sub factors of the factor for hull efficiency are shown, viz., the factor involving augmentation of resistance due to the action of the propeller, and the factor involving gain due to the wake. The former is obtained as follows: knowing the net resistance of the model at any given speed without the propeller, the *augmented* resistance is found for that speed with the propeller working behind, the slip being so regulated as to cause delivery of a thrust equal to that augmented resistance; the ratio of the augmented resistance to the net resistance of the model is the factor involving augmentation. The factor involving gain to the propeller due to the wake is the speed of the model divided by the same speed less speed of wake, the latter speed being found by the increased facility for obtaining thrust, which the wake gives to the screw when the model is in front. It will be seen that it is reasonable to call the combination of these two factors the factor for hull efficiency although the screw is involved in both of them, because in both the screw is considered with reference to its connection with the hull. The two sub factors of the factor for screw efficiency—viz., that involving true slip, and that involving water friction of screw are factors which are due entirely to the action of the propeller clear of the hull. They are not at present deduced separately, but are obtained conjointly from experiments with the propeller alone in the following way :—At the speed under consideration the propeller is driven with varying amounts of slip, and the driving force and thrust measured for each; the forces are brought into comparison by the principle of virtual velocities, each force being multiplied by the movement made in the same unit of time, and the ratio of the two—*i.e.*, of the driving force to the thrust—taken. This ratio, when the thrust is that which is required, is the factor sought; its value varies with the thrust obtained or with the slip which is required to give that thrust; when the thrust is very small the ratio is very large; as the thrust increases it falls to a minimum, and then again increases as the thrust continues to increase. The hull item or third factor composing the D.H.P. less power for thrust block friction, corresponds exactly to the E.H.P. of the late Mr Froude, and being reduced to a resistance term is split up as with him into surface friction, eddy-making, and wave making. The wave item of the resistance is further split up into two terms, one of the transverse, and the other of the diverging series; these two series being capable of sub-division into bow and stern groups; although this further possible analysis may be omitted at the present stage of the question.

In the last analysis the number of root elements will be seen to be 11. It is to be noted that all these elements have not as yet been either fully defined or quantified, nor is it by any means certain that we are at the end of the statement of the items of the analysis.

One lesson we should learn from these three tables is, that progress in power analysis means, in so far as our steps have yet led us, progress into unsuspected difficulty, and not advance into facile solutions. The work which lies before the experimenter and investigator in power analysis is not easy, and its importance and difficulty should not be underrated. Another lesson which we may learn from the study of these tabular statements of analysis is that the main element required is the resistance of the vessel. This is the kernel of the analysis in each case, and must be known before any progress can be made in it. Hence the absolute necessity of model experiments in any attempt to elucidate this difficult question. Round the element of the ship's resistance, or rather round that element transformed into effective horse-power, gather all the other elements in the expenditure of the power. To say this indeed is only to say that the power to be expended depends on the work to be performed; it agrees with the argument already set forth in the body of this paper, that the curve of power expenditure must, if carefully plotted from sufficiently exact observations, bear a close relationship to the curve of the vessel's effective horse-power.

It may be asked, if the analysis of power is so difficult, how can the experimental tank be made useful for the prediction of the speed and power of any new type of steamer? It is fortunate that we do not need to wait until we have a complete solution of the power analysis difficulty before being able to make such prediction, and as it may be of interest to this Institution to know the method pursued in such predictions by my own firm, I give the following explanation:—When the resistance of any model has been obtained, then by means of Mr Froude's law of comparison, and by means also of his method of proportioning the skin friction of the full-sized ship to the skin friction of the model, the resistance of the full-sized vessel in lbs. is approximately found. When this is known it is converted into effective horse-power by multiplying it by the speed of the vessel in feet per minute, and dividing by 33,000. To convert the effective horse power into indicated horse-power, it is necessary to use ratios which have been obtained from past measured mile trials and tank experiments. These ratios, in our experience, we have found to vary from 45 per cent. to 60 per cent. of the gross indicated horse-power. This may seem a large range of variation, but the extremities of it are accounted for by very exceptional cases, of which the causes are pretty well known. The real range of the ratios which we use for regular work is very moderate in extent and confirmed by a large amount of data. But here, too, there is an immense field for investigation which can only be explored by most careful experimental work directed to the examination of every possible cause of the loss of power that occurs.

LETTER FROM MR MANSEL TO MR W. DENNY.

Slip Dock, Kelvinhaugh,
Glasgow, 17th May, 1875.

DEAR SIR,

I am proud to write that I have got the most important and novel part of my formulas in a much better shape, and I am sure you will be pleased with the very close results to experiment and consequences indicated. I wrote my last letter to you hurriedly, and I know I made mistakes in the recovered power formula. I hope you will be able to follow out the calculations now enclosed, by the aid of the explanatory sheets; if not, I will very gladly go over them with you. I have condensed the calculation on one sheet, and briefly summarize results :—

"GOA."	No. I.	No. II.	No, III.	
E_m =	648	347	101	
E_f =	436	246	81	NOTE.
E_r =	—222	—155	—44	
Calculated =	862	438	138	Results closer than could well be expected.
Experimental E_v =	869	438	139	
Differences,	—7	0	—1	

"AFRICA."	No. I.	No. II.	No. III.	
E_m =	792	475	232	
E_f =	534	333	174	NOTE.
E_r =	—265	—259	—147	
Calculated =	1061	549	259	Also a very close result.
Experimental E_v =	1066	541	254	
	—5	+8	+5	

The "Africa" at first proved a regular *bête noir*, and bothered me sadly. Of course, like most philosophers, I often neglect facts lying under my nose, and go on a wild goose chase after explanation of difficulties, which after all are simply important elements seeking recognition in imperfect formulas. In the present case it led me to a better appreciation and more suitable form for the important elements, draft aft and angle of trim :—

"HAWEA."		No. I.	No. II.	No. III.	No. IV.	
E_m	=	406	251	144	37	NOTE.
F_f	=	273	176	106	32	Absurdly correct; don't blame me. I could not help it. It is just as the figures came out.
E_r	=	—6	—72	—64	--17	
Calculated	=	673	355	186	52	
Experimental E_v	=	673	353	186	52	
		0	+2	0	0	

"TAUPO."		No, I.	No. II.	No. III.	No. IV.
E_m	=	440	233	126	27
E_f	=	285	158	91	23
E_r	=	+142	0	0	+215
Calculated		867	391	217	265
Expt. E_v	=	814	390	216	68
		53	1	1	197

Here we have a most singular result. No recovered power in any case. No. I. shows instead a large extra expenditure, and in the same direction No. IV. shows a result which is simply monstrous! The source of these inebriate figures from our previously steady-going formula, at the two extreme experiments, is not difficult to fathom. We have imported into the formula a quantity E_v., which is the gross indicated power, with certain deductions for working engines and slip, which, whenever *under or over valued*, the excess being raised to the fifth power, is enormously magnified, and this forms an excellent means of arriving at the true value of friction and other losses involved in the difference between E and E_v. Thus, if in these two experiments we take instead of the calculated values

70·74 and 14·90 lbs., the effective pressures at 68·82 and 12·16 lbs, respectively—no really serious difference—our formulas would have yielded the following figures:—

		No. I.	No, II.	No. III.	No. IV
E_m	=	440	233	126	27
E_f	=	285	158	91	23
E_r	=	+67	0	0	5
Calculated		792	391	217	55
Expt. E_v		792	390	216	55

Now, here we have a pretty little problem: how does the pressure 5 lbs., which seems to suit working the engines in all former experiments and the two middle ones of "Taupo," require to be 6·92 lbs. at the high speed and 7·74 lbs. at the low speed? This requires some thinking over, but I daresay you will have quite enough in the accompanying papers[*] to occupy your attention for a few days. I assure you it has fully occupied my every spare moment for a few weeks.

I remain, yours very truly, in haste,

ROBT. MANSEL.

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[*] These accompanying papers contain the arithmetical work by which the figures in the letter were obtained.

TABLE NO. 1.

Comparison of Initial Friction as obtained by Mr Froude's Method with that obtained by Mr Mansel's Method.

		Name of Ship.		Lowest Trial Speed.	A. Maximum Indicated Thrust. Lbs.	B. Initial Friction in Lbs. of Indicated Thrust.		Percentage Ratio, B to A.		Pitch of Propeller.		Diam. of High Press're Cylinder in Inches.	Stroke.		Initial Friction in Lbs. pr. Sq In. of Area of High Pressure Cyl'der.		REMARKS.
						Froude.	Mansel.	Froude.	Mansel.	Ft.	In.		Ft.	In.	Froude.	Mansel.	
Division 1.	1	"Merkara,"		6·20	46210	6325	4886	13·6	10·6	22	0	47	4	0	10·0	7·7	
	2	"Goa,"		6·30	29510	2740	2900	9·3	9·8	18	6	36½	3	6	6·9	7·3	
	3	"Hauroto,"	1st Trial	6·13	36660	3900	2570	10·6	7·0	21	0	38	3	9	9·6	6·3	
	4	"	2nd Trial	5·66	34806	4400	2692	12·6	7·7	21	0	38	3	9	10·9	6·6	
	5	"Waihora,"		5·28	39451	4100	3090	10·4	7·8	18	6	38	3	9	8·9	6·7	
	6	"Manapouri," ...	1st Trial	5·05	41700	4600	3236	11·0	7·8	20	6	41	4	0	8·9	6·3	
	7	"Wairarapa," ...	1st Trial	5·99	43354	4850	2985	11·2	6·9	20	6	41	4	0	9·4	5·8	
	8	"Bancoora,"	1st Trial	6·29	48587	2350	2377	4·8	4·9	19	0	38	4	3	4·6	4·7	
	9	"	2nd Trial	5·20	48582	3820	3214	7·9	6·6	19	0	38	4	3	7·5	6·8	
	10	"Quetta,"		4·92	58363	5730	4169	9·8	7·1	23	0	50	4	6	7·5	5·4	
	11	"Antonio Lopez,"		7·77	72427	3000	3199	4·1	4·4	28	6	57	5	0	3·4	3·6	
	12	"Snark,"	•	5·08	3189	88	54	2·8	1·7	10	0	12½	1	3	2·9	1·8	
Division 2.	13	"Hawea,"	2nd Trial	5·31	22253	1932	1698	8·7	7·6	17	3	35	3	0	5·8	5·1	Although figures are given for Initial Friction, as determined by Mr Mansel's method, yet the want of straightness of the lines makes this determination very far from satisfactory.
	14	"Booldana,"		4·33	49110	5500	2512	11·2	5·1	19	0	38	4	3	10·8	4·9	
	16	"Sirdhana,"		4·62	40458	2310	2455	5·7	6·1	19	0	38	4	0	4·8	5·1	
	16	"Manapouri," ...	2nd Trial	5·09	48560	4800	3162	11·0	7·3	20	6	41	4	0	9·3	6·1	
	17	"Wairarapa," ...	2nd Trial	5·26	42173	3700	1996	8·8	4·7	20	6	41	4	0	7·2	3·9	
	18	"Pau-Tah,"		5·38	31873	2955	1950	9·3	6·1	14	9	37½	3	0	6·6	4·3	
	19	"Rotomahana,"...	1st Trial	6·84	53296	3450	2291	6·5	4·3	21	6	47	4	0	5·8	3·5	
	20	" ...	2nd Trial	6·03	51815	3450	2138	6·7	4·1	21	6	47	4	0	5·3	3·3	
	21	"Bhundara,".....		6·62	46275	1500	2399	3·2	5·2	19	0	38	4	3	3·0	4·7	
	22	"Pretoria,"		5·50	60830	5450	3236	9·0	5·3	23	4	50	4	6	7·2	4·3	
	23	"Ravenna,"		6·04	66738	6600	4037	9·9	6·1	26	6	54	5	0	7·6	4·7	
	24	"Omapere,"		3·83	14309	2650	1259	18·5	8·8	12	6	24	2	6	14·6	7·0	
Division 3.	25	"Shunlee,"		4·18	34915	3525		10·1		15	0	37½	3	0	8·0		Initial Friction cannot possibly be determined by Mr Mansel's method, owing to the want of straightness of the lines.
	26	"Chilka,"		4·88	31083	230		·7		18	6	34	3	6	6·7		
	27	"Fung Shun," ...		7·32	31062	2870		9·2		14	6	37½	3	0	6·3		
	28	"German,".........		5·10	59352	8260		13·9		23	6	50	4	6	11·0		
	29	"Clyde,"		6·42	87480	2400		2·7		28	6	58	5	3	2·5		
	30	"Venetia,".........		7·29	45690	250		·6		25	0	49	4	6	·37		

TABLE No. II.—Analysis of Indicated Horse Power.

Mr Mansel's Analysis, 1875:—

$$E\ (= \text{I.H.P.}) = \left\{\begin{array}{l}
\times\ \text{Factor for slip}\ \left(\frac{n \times H}{V}\right)\\
\times\ \text{A factor involving constant decrement of pressure}\quad \frac{P + rp}{P + rp - 5}\\
\times\ \text{A factor for friction due to working load}\quad \frac{100}{90}\\
\times\ E_v = \left\{\begin{array}{l}
+\ E_f\ \text{power spent on surface friction.}\\
+\ E_m\ \text{power spent on other movements communicated to the water in the vicinity}\\
\qquad\qquad\text{of the vessel.}\\
-\ E_r\ \text{power recovered from the working of the propeller in the wake.}
\end{array}\right.
\end{array}\right.$$

The first three factors above are the reciprocals of those used by Mr Mansel, the object of the table being to show how the I.H.P. is made up, while Mr Mansel's object was to deduce E_v from the I.H.P.

TABLE No. III.—Analysis of Indicated Horse Power.

The late Mr Froude's Analysis, 1876:—

$$\text{I.H.P.} = \left\{\begin{array}{l}
\times\text{Factor for slip.}\\
\times\text{S.H.P.} = \left\{\begin{array}{l}
+\text{E.H.P. power spent on nett resistance of ship} = \left\{\begin{array}{l}
\times\text{Resistance of ship} = \left\{\begin{array}{l}
+\text{Wave-making.}\\
+\text{Eddy-making.}\\
+\text{Surface friction.}
\end{array}\right.\\
\times\text{Speed.}
\end{array}\right.\\
+\text{power spent on augmentation of resistance.}\\
+\quad\text{,,}\quad\text{water friction of screw.}\\
+\quad\text{,,}\quad\text{constant friction of engines.}\\
+\quad\text{,,}\quad\text{working friction of engines.}\\
+\quad\text{,,}\quad\text{air pump resistance.}
\end{array}\right.
\end{array}\right.$$

TABLE No. IV.

Present condition of Analysis of Indicated Horse Power.

- I.H.P.
  - + Power spent on constant friction of engines and shafting.
  - + " working " "
  - + " pump duty.
  - + " thrust block friction.
  - + D.H.P. less power for thrust block friction.
    - Screw items
      - × Factor for hull efficiency *
        - × Factor involving augmentation of resistance due to action of screw.
        - × Factor involving gain due to wake.
      - × Factor for screw efficiency*
        - × Factor involving true slip.
        - × Factor involving water friction of screw.
    - Hull item
      - × E.H.P.
        - × Ship's nett resistance
          - + Surface friction.
          - + Eddies.
          - + Waves
            - Transverse series.
            - Diverging series.
        - × Speed.

---

* These factors are the reciprocals of the efficiencies proper. The object of the table being to show how the I.H.P. is made up, the factors necessarily become the inverse of what they would be, if the useful power were being deduced from the gross power.

After the reading of the paper,

Mr **Mansel** said he would not attempt, at that time, to answer Mr Denny's paper, the evening being too far spent. He would like, however, to notice one statement in the paper where Mr Denny says—"I think it well to say a few words upon the analysis of power with which Mr Mansel, nine and a half years ago, started his discussion of my progressive trial data." He thought Mr Denny was here assuming rather much credit to himself; since, long before the time referred to, he had worked at such matters. He held in his hand the original papers of calculations made in 1859, when the Royal Mail paddle steam vessel "Scotia" was being designed. The following table, exhibited to the meeting, included various calculated elements of the distribution of power in some large paddle vessels, as taken from those papers, and were such calculations as he had been in the habit of making for Messrs Napier & Sons, for nine years previously, say from 1850!

	H.M.S. 'Victoria and Albert.'	R.M.S. 'Persia' (Clyde).	R.M.S. 'Persia,' at sea light.	R.M.S. 'Persia,' at sea load.	R.M.S. 'Shannon' (Clyde).	R.M.S. 'Scotia.' Estimate before building
Length at mid depth,	300	351·5	do.	do.	329·0	362·5
Breadth, - -	40·25	44·75	do.	do.	43·75	47·0
Draft, - -	13·75	18·0	18·8	23·3	17·0	17·0
Mid area, - -	435	650	688	892	610	660
Displacement, -	2160	4000	4396	5844	3880	4420
Co-effict. of fineness,	·47	·53	...	...	·59	·57
Indicated Power,	2406	4250	3798	3340	2928	estimate 4500
Speed, - -	15·	15·4	14·58	11·29	13·9	15·5
Slip per cent., -	22·0	23·3	...	...	25·8	23·0
Calculated Distribution of Power.						
Displacing water,	902	1458	1311	789	1000	1500
Fluid friction, -	734	1200	1071	627	850	1300
Working engines,	360	620	565	487	430	670
Slip, - -	450	840	711	626	650	880
Excess for oblique action of float,	feathering	132	140	801	feathering	150
	2446	4250	3798	3340	2930	4500

As many of the members had not seen his Letter of Reclamation, he thought it ought to have been read before Mr Denny's paper to show what was the ground on which the latter proceeded. Mr Mansel proceeded to read some quotations from his Letter, and eventually it was agreed, on the suggestion of Mr Denny, to print in the Transactions, both the Letter of Reclamation and Mr Froude's letter to Mr Mansel, which are appended.

LETTER OF RECLAMATION.

TO THE PRESIDENT AND COUNCIL OF THE
INSTITUTION OF ENGINEERS AND SHIPBUILDERS IN SCOTLAND.

GENTLEMEN—

I have suffered a wrong which I cannot submit to; and, on full consideration, have come to the conclusion that you are the appropriate body to which my representation of the matter ought to be addressed.

About a fortnight ago, by mere chance, I noticed, at page 178 of "Transactions of the Institution of Naval Architects, Vol. XVII." on the 7th April, 1876, when speaking in the discussion of a paper "On the Ratio of Indicated to Effective Horse Power," &c., by the late Dr William Froude, Mr William Denny considered himself justified in making the following statements:—

"I must really enter upon one point which I feel a little unpleasant to myself. I have the pleasure of the friendship of Mr Froude, and of another very able man, Mr Mansel, who is known to many of our scientific shipbuilders here. When Mr Froude discovered by his analysis the way in which it was possible to measure the initial friction, I remember having a conversation with my friend Mr Mansel, and I put it to him that Mr Froude had thoroughly explained the discrepancies of the 'Hawea' and 'Taupo' trials by reducing them to indicated thrust. My friend Mr Mansel at the time did not seem to think that this had been attained. Shortly afterwards however he came to me, and I believe he had forgotten what I had said to him, but he had come upon the same idea, and he has worked out this idea in a very interesting way. You will perhaps excuse my making use of the black board to show you this, as it is a point which I am very anxious this Institution should clearly understand. Mr Froude has, and has alone, the priority for the discovery that the amount of initial friction could be found out from progressive trials, and it is perhaps the most interesting discovery which has been made with regard to speeds for a very long time. You will see that a different method of analysis has provided us with, although not an exact confirmation, yet a very close confirmation of what Mr Froude has said. Mr Mansel's method of analysing was this. You will suppose that these are two scaled arms at right angles of a diagram, similar in all respects to that of the 'Pachumba.' Upon the horizontal arm is set off in ordinary arithmetical notation the speeds 1, 2, 3, 4, 5 knots, or whatever they may be. Upon the vertical arm, not the indicated horse-power, but the mean piston pressures equivalent to the indicated thrust (the idea of which, as I tell you, Mr Mansel had unconsciously borrowed I believe from my suggestions about Mr Froude) are set off not in arithmetical but logarithmic notation. In fact, the principle of setting off this arm is the principle of Gunter's scale carried out in only one scale. Now if any gentleman would set off on the speed ordinates the amount of mean cylinder pressure or indicated thrusts according to the logarithmic scale already described, as Mr Froude has done in arithmetical notation, a very curious thing happens [*illustrating on the black board*]. Strangely enough, Mr Froude's curve becomes a straight line. With regard to what Mr Mansel did, and I have the deepest respect for Mr Mansel, you must acknowledge his great ability in seeing that it was possible by this means to show what Mr Froude has also shown. By simply producing the straight line, and measuring its zero ordinate, he shows the amount of the initial friction of the engines.

"Mr FROUDE: May I ask does that proceed on the assumption as to the law that governs the resistance? It must involve some such fundamental rule.

"Mr DENNY: I do not think Mr Mansel originally did more than proceed on the notion I had given him of what you had done, and I believe he forgot all about where the notion came from, which is common enough for all of us to do. I have myself occasionally borrowed from my friends; but, when I have been reminded of it, I have acknowledged it, although I could not say at the time where the idea came from.

"Mr FROUDE: I fully agree with that; but I merely wanted to understand the principle on which that line comes out a straight line.

"Mr DENNY: I do not know the principle, but in every case this line has come out a straight line, with one exception. Allow me to say this, because it will be confirmatory of something Mr Froude will have to tell you in a second paper. In some of the ships—I think notably the 'Merkara'—where the speed was for us high, 13 knots, this last curve did not come out straight but turned up here; showing you, at that point, something—which you will see afterwards, and which Mr Froude will explain to you fully—had happened. What I may call an augmented increase of resistance had taken place there, which of course must have been due to the form of the ship. I know I am taking up a great deal of time; but you will acknowledge that, as to one part of this, it has been a duty forced upon me, and not a part of my own inclination."

Which, concisely stated, amounts to: R. Mansel, taking advantage of a private communication to him of a discovery of Dr Froude, had devised another means of representing the same idea, and proffered it to Mr William Denny as his own discovery.

It is further an implied claim for Dr Froude of a principle embodied in the paper, "Propositions on the Motion of Steam Vessels," which, on the 21st of the preceding month, I had read before your Institution, for which paper the Marine Engineering Medal of that year was afterwards awarded me.

For eight years, during two of which I filled the honourable office of President of your Institution, a charge of dishonourable conduct has been recorded in the Transactions of a kindred institution, of which I was not then a member, and no opportunity of offering an explanation was given me when it was placed there!

Notification of the existence of this charge was never made known to me, or it should immediately have been replied to, and shown to be absurd. During the summer of 1876, I *did* hear that Mr Denny had said something in this strain, "in London." I did not inquire where, and even had I known, I should have concluded that a sense of fair dealing, directed to a comparison of Dr Froude's paper with mine, would have ensured the alteration or suppression of the objectionable statements I now find in the foregoing report of the discussion.

Mr Denny had been doing a great work for naval science; he had known me but a few months, and I was not so weak as to seek to take notice of any impulsive statements he might have made. I trusted he would come to see that they were unfounded, and regret that he had been so hasty. I never, however, imagined his misunderstanding would have carried him so far as to necessitate a public proof of his having made himself ridiculous about a fallacy! At all events, this was my idea.

Mr Denny's mistake was the supposition that I agreed with Dr Froude; who, had he been in life, was the person to whom I should have appealed in this matter. It is, however, fortunate that in a letter to me, dated 23rd September, 1876, Dr Froude has left his own views on record; and this letter may be briefly referred to as illustrative of the serious difference between us, leading directly to the most conflicting views on far more important subjects than the one imagined by Mr Denny. I might have been all wrong in differing from Dr Froude, and yet have been perfectly honest: I had my own opinions, and knew both how to state and defend them.

My recollection of this regretful affair is quite clear. In the summer of 1875, Mr Denny having sent me trial data of two sister ships, my simple and direct mode of investigation, then unpublished, showed the friction in one to be abnormally great, and I wrote suggesting inquiry into the engineering data; also, personally meeting Mr Denny's partner, Mr Walter Brock, in Helensburgh, put pointed inquiries as to whether there was not a possibility of some error with the indicator springs. Mr Brock described the care taken in testing these matters, and said there was nothing in his department to explain the discrepancy. Late in the autumn, after Mr Denny had been meeting Dr Froude in England, happening to be in Dumbarton, I had a hurried interview with Mr Denny in a lane, when about to start for the train. Mr Denny informed me Dr Froude had explained the whole matter to him—it was simply friction; by drawing a curve and its tangent, and continuing the curve to the axis, he got the friction. I at once knew what Dr

Froude had done, and challenged the accuracy of the value thus obtained. Mr Denny was indignant that I should question Dr Froude's method; high words passed between us (a party in hearing jocularly shouted out, "I say, boys, don't fight"), and I left him with the significant statement, *There is more than friction; I can prove it.* Mr Denny cannot have forgotten this, for he taxed me with it on our next meeting; no doubt expecting that I would resile from, what he considered, an absurd position. I object to the sentence, "Mr Mansel, at the time, did not seem to think that this (initial friction) had been attained." Then, and after, attainment of the object by Dr Froude's method was denied!

I do not doubt that Mr Denny honestly believed that Dr Froude's method and mine were alike at bottom, and gave the same result, and in this belief he repelled any explanation; but I must openly protest against Mr Denny's mistaken notions being recorded as the measure of my knowledge, and the biassed judge of my honour. In the letter referred to, written by Dr Froude some months after, and with the advantages of study of my papers, and even a short personal interview, at page 19* we find, "I will continue to work out in detail the differences which arise out of the two modes of treating the question, as they issue in reference to the case of the 'Merkara,' which is one we have both investigated;" and at the foot of page 21,† states the results, "7·39 is the pressure due (by your method) to the constant friction, whereas by mine it is 10·04."

Dr Froude also shows that his curve of pressures cannot develop into a straight line, as was the case with my curve, and therefore argues I must be wrong.

Mr Denny writes, "In every case the curve develops into a straight line," noticing an exception, which his data does not justify, so that, on his own showing, Mr Denny credited Dr Froude with a method which Dr Froude distinctly repudiates.

The matter under consideration was interesting in itself, and led to many very important issues. I shall endeavour to give a brief discussion of the chief features. Mr Denny's paper on the "Difficulties of Speed Calculation" was discussed, at the Institution of

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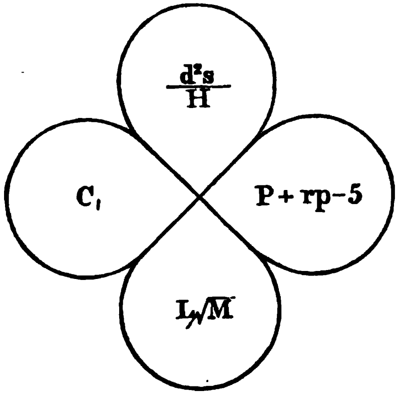
* See page 126.

See page 127.

Engineers and Shipbuilders in Scotland, in April, 1875. The old "mid area" formula was taken by me as a basis, and, by some obvious modifications, I showed that it might be reduced to the formula in the looped figure.

$$\text{I.} \qquad \frac{MV^3}{E} = C.$$

II. $V^3 =$



a formula which, in the case of progressive speed trials on the same ship, the quantities in the upper and lower loops being then constant, simplifies, as is easily seen, into,

$$\text{III.} \qquad V^3 = C\,(P + rp - 5).$$

(This was figured on the same diagram as II., but was not published in the discussion) So far, *three* mechanical principles are involved.

First, Mechanical effects are only properly judged when referred to the powers producing them. (Smeaton's principle.)

Second, Like kinds of mechanical effects are in constant ratio to the respective powers producing them. (Theory of the Admiralty co-efficients.)

Third, When power is developed, and producing mechanical effect, the manifested pressure has a constant decrement which is independent of the velocity with which the effect is produced. (This decrement is known as Morin's constant.)

Let us now refer to the data of the "Merkara," as furnished by Mr Denny.

S.S. "Merkara."

Speed, V.	Co-efficient, $\frac{MV^3}{E}$	Pressure, (P + rp).
12·91	579·4	73·30
11·09	585·2	53·55
9·20	551·0	38·12
6·20	417·5	22·79

On substituting these data in III., replacing the assumed special value 5 of Morin's constant by the general value m, we obviously get four equations,

$$\text{IV.}\quad \begin{aligned} 12{\cdot}91^2 &= C\,(73{\cdot}30 - m).\\ 11{\cdot}09^2 &= C\,(53{\cdot}55 - m).\\ 9{\cdot}20^2 &= C\,(38{\cdot}12 - m).\\ 6{\cdot}20^2 &= C\,(22{\cdot}79 - m).\end{aligned}$$

Now, if the investigation was free from error, by the third principle we should have the same value of m in all. To try this, we have to get rid of the common factor C, which is easiest done by dividing the members of the first equation by those of the second, those of the second equation by those of the third, &c.; and by transposition we thus get:

First and second,

$$m = 53{\cdot}55 - \frac{73{\cdot}30 - 53{\cdot}55}{\left(\frac{12{\cdot}91}{11{\cdot}09}\right)^2 - 1} = -2{\cdot}09.$$

Second and third,

$$m = 38{\cdot}12 - \frac{53{\cdot}55 - 38{\cdot}12}{\left(\frac{11{\cdot}09}{9{\cdot}20}\right)^2 - 1} = 4{\cdot}06.$$

Third and fourth,

$$m = 22{\cdot}79 - \frac{38{\cdot}12 - 22{\cdot}79}{\left(\frac{9{\cdot}20}{6{\cdot}20}\right)^2 - 1} = 10{\cdot}04.$$

Dr Froude's method of finding this quantity (the initial friction, as he supposed) was *not to take* his published method of the indicated thrust curve, and the geometrical construction at the origin, as given in his paper of April, 1876, about which Mr Denny and I differed. Dr Froude wrote (page 9), "The algebraical mode is more definite, and may be relied on as accurate, if we have reason to believe that the resistance has not sensibly outrun the square of the speed." In effect, Dr Froude simply takes the two last of the foregoing four equations, and works out the value 10·04, which he assumes to be the true value of the "initial friction."

But in this Dr Froude simply *begs the question*, for he had no more right to assume this value, than the 4·06 of the pair above, and the obviously absurd —2·09 of the upper pair. It is true, a limitation is laid down that "the resistance is not to outrun the square of the speed," which amounts to saying that the method is not applicable to a steam vessel at all; for I not only deny, but can offer most satisfactory proofs that in *no* instance does this assumed law, of resistance varying as the square of the speed, hold good! This is a matter of paramount importance, and can best be approached by consideration of the relation between the *second principle* and the quantity $\frac{MV^3}{E}$, when derived from the progressive speed trials on a steam ship. The numerator represents the mechanical effect, the denominator the power doing it, and by the mechanical principle these should have a constant ratio. The fact is uniformly impressed upon us, that it is not so, and the reason is, that the numerator involves the V^2 hypothesis, assumed as a law, and the mechanical principle is quite obscured until the V^2 is removed, and something much nearer the truth is put into its place.

Take, for example, the "Merkara's" co-efficients, and multiply each of them by the factor $\frac{\text{Log}^{-1} \cdot 0735 \text{ V}}{V^2}$, which is the process I have indicated, the V^2 denominator removing the V^2 influence from the co-efficient. The calculation is simple, and as follows :—

Speeds, - -	12·91	11·09	9·20	6·20
Co-efficients, -	579·4	585·2	551·0	417·5
Log co-efficients,	2·7630	2 7673	2·7412	2·6207
Add value ·0735 V,	·9489	·8152	·6762	·4557
Subtract Log V^2,	2·2218	2·0898	1·9276	1·5848
Logs, -	1·4901	1·4927	1·4898	1·4916
Constant Ratio,	30·92	31·10	30·89	31·02

This vindicates the second principle, and indicates the V^2 hypothesis, throughout the range of experiment in the "Merkara," to be an utter fallacy: for these new ratios are as nearly constant as the nature of the problem could lead us to expect. That this is not an exceptional case, is best shown by giving the results of three more vessels, in which the fallacy of the V^2 hypothesis is even more clearly shown.

H.M.S. "Shah,"			H.M.S. "Iris,"			"Charles Quint,"		
Factor, $\frac{\text{Log}^{-1} \cdot 0792\,V}{V^2}$.			Factor, $\frac{\text{Log}^{-1} \cdot 0750\,V}{V^2}$.			Factor, $\frac{\text{Log}^{-1} \cdot 0842\,V}{V^2}$.		
Speed.	Co-effts.	Ratio.	Speed.	Co-effts.	Ratio.	Speed.	Co-effts.	Ratio.
16·45	587·3	43·58	18·59	594·9	42·70	15·11	702·8	57·21
12·13	702·4	43·60	15·75	690·5	42·39	13·42	770·4	57·33
8·01	656·3	44·10	12·48	770 0	42·72	9·82	823·4	57·30
5·32	466·9	43·53	8·32	676·7	41·12			

In every case, multiplying the co-efficients by a factor of this same form, gives a ratio, constant in the same vessel, so long as the conditions other than power and speed remain the same. The inference from all this is irresistible, there is no proof whatever that V^2 is the law of the resistance in a steam vessel. Every formula in which this assumption is involved thereby becomes contradictory, both to facts and mechanical principles, and worse than of no value: it becomes misleading!

The correction of equations IV., so as to remove the original sin due to the V^2 hypothesis in the Admiralty co-efficient, from which it was derived, and thus vindicate the third mechanical principle, is not difficult. We have simply to replace the first member V^2 by the quantity, $(1 + aV)\ b\ \text{Log}^{-1}\ aV$. Only, it may be noticed, the formula is very sensitive, and some collateral influences have to be allowed for to remove resulting variations in m. (See equation (d), sequel.) The contrast of Dr Froude's method and mine, however, is more readily seen, as Dr Froude has shown, by taking his formula for the curve of pressures, which he writes, $P = a + bV^2$ and from the data of the 9·2 and 6·2 knot trials calculates, $a = 10{\cdot}04$ and $b = {\cdot}3318$, so that,

$$(P + rp), \text{ or, as he writes, } P = 10{\cdot}04 + {\cdot}3318\ V^2.$$

At page 26, the calculated values by this formula are given up to 10 knots, and in another column, the values derived by Dr Froude from my method, as follows;—

Ordinates of Curve of Pressures.

Speeds.	Dr Froude's Curve.	Logarithmic Mode.
0	10·04	7·39
1	10·37	8·86
2	11·37	10·63
3	13·03	12·75
4	15·35	15·29
5	18·33	18·33
6	21·98	21·99
8	31·27	31·63
10	43·22	45·55

But my reading of the "Merkara" data is, in the general formula, $P + rp = f\ \text{Log}^{-1}\ a\ V$. We have $f = 7{\cdot}745$, and $a = {\cdot}0756$. The formula for the "Merkara," is,

$$(P + rp) = 7{\cdot}745\ \text{Log}^{-1}\ {\cdot}0756\ V.$$

As an example of the extreme simplicity and accuracy of this formula, the full calculation of the values for the trial, and a few assumed speeds, is as follows:—

Ordinates of Pressure Curve of "Merkara."

Speeds, -	12·91	11·09	9·20	6·20	3·00	1·00	·00
Value, ·0756 V,	·9760	·8384	·6955	·4687	·2268	·0756	·0000
Log 7·745, -	·8890	·8890	·8890	·8890	·8890	·8890	·8890
Sum, or Logs (P + rp),	1·8650	1 7274	1·5845	1·3577	1·1158	·9646	·8890
By formula, -	73·30	53·39	[*]38·42	22·79	13·06	9·22	7·745
„ trial, -	73·30	53·55	38·12	22·79	?	?	?
„ Dr F.'s curve,	65·34	50·84	38·12	22·79	13·03	10·37	10·04

Here, the formula values are contrasted with the experimental and Dr Froude's values in the succeeding lines. It is obvious, from 13 knots to 3, according to Dr Froude's figures for this last speed, there is as perfect agreement as could be expected between the formula values and the experimental. Now, from its nature, this curve of mine must develop into a straight line when the logarithmic ordinates are set up to the speed abscissas, and upon no reasonable principle can it be contended that the law giving a straight line from 13 to 3 knots should not continue true for the remaining 3 knots: and thus by going to the origin we get the value of m belonging to the limits of experience. It is, however, a matter of certainty that experiments made between the 6·2 knot speed and zero, would have shown a change of value of m, due to changed circumstances explained as the lower *conjugate* solution. The value 10·04, however, being derived from the 9·2 and 6·2 knot speeds, is not in any way connected with the unknown region under the 6·2 speed, and consequently is neither true for the experimental nor the unknown lower speeds with the steam vessel. Dr Froude's curve is hopelessly erroneous at the higher speeds, and is only true for the 9·2 and 6·2 speeds, because he compelled it to take the true values at these points Let us now

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[*] Had this speed been given 9·16 instead of 9·20, the value would have been 38·14, but the whole data indicates this speed to be 9·10 only, giving (P + rp) = 37·76, which is increased to 38·12, by the simultaneous reduction of the revolutions from 45·07 to 44·75, the observed value; the pressures varying inversely as the revolutions.

take formula III., and substituting for 5 the general value m, and for (P + rp), the equivalent general value f Log^{-1} aV, we have,

$$V^2 = C\,(P + rp - 5).$$

$$V^2 = C\,(f\,\text{Log}^{-1}\,aV - m),$$

which, at the limit when V = 0, gives,

$$0 = C\,(f\,\text{Log}^{-1}\,0 - m),$$

and consequently f = m, since $\text{Log}^{-1}\,0 = 1$.

Now, in the four equations of IV., whenever V has any definite value, the mechanical principle is violated; since, as shown, we have a variety of values of m, with nothing but empirical guessing to determine which is the right one. The reason is, the form V^2 of the first member is a fallacy, and only when the V has no value, vanishing with the first member, does the second member indicate its involved truth that f = m, showing the origin ordinate of the curve to be the value of Morin's constant; which, in virtue of the third principle, continues uniform through all the successive values of V.

Dr Froude's explanations, given at page 27 of his letter,* are as follows: "The curve calculated by the equation $P = a + bV^2$, cuts exactly the two points of pressure given by the 'Merkara' experiments, for the 6·2 knots and the 9·2 knots. This, of course, it was bound to do, because it was calculated from them; it, however, cuts below the point which belongs to the 11·09 speed, and still more below that which belongs to the 12·91 speed. . . The meaning of all this is that, as a matter of fact, the resistance of the 'Merkara' is practically as the square of the speed, up to quite 9 knots, above this speed, the resistance increases in a higher ratio, and then of course for higher speeds deviates from the parabola, which correctly expresses it so far; on the other hand, the line which is defined by the logarithms of the pressures is not absolutely straight, though it is nearly so, and when an absolutely straight line is substituted for it, and the pressures are calculated backwards from the ordinates of the straight line, the resulting pressures are far more different from the true pressures than the logarithmic line is from the

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* See page 130.

assumed straight line, and definite tangible error is thus introduced into the data. Nevertheless, it happens that the series of pressures which are thus obtained from the straight line, indicates a resistance which grows more rapidly than as the square, and which thus has a rough sort of agreement with the real growth; only the agreement is arbitrary and haphazard, and has no real relation with the special law of pressures which each form of ship requires (a law which is materially different for different forms), and will certainly contradict the law for almost any possible form of ship as the speed increases."

There is nothing in this to bridge the interval between Dr Froude and me, or invalidate the strong reasons upon which I have challenged the very basis of Dr Froude's speculations. I will in conclusion quote from my letter in reply, to show the attitude taken by me at that time to be the same as now. The origin of this correspondence may be explained. I had one short and only interview with Dr Froude, at the Glasgow meeting of the British Association, in September, 1876. He had a printed copy of the first part of a paper I read two days after, and began to discuss some points which he thought erroneous. Shortly after I had a letter in which, after regretting that a sudden call to the south had prevented him from being present at the reading and discussion of my paper, resuming the discussion, he at much length proceeded to strengthen his position, and show forth the weakness of mine. My answer shows that, in my opinion, he had effected neither; and, omitting some trivial matters, began as follows :—

Letter to Dr Froude.

"9th October, 1876.

"William Froude, Esq., LL.D., F.R.S.

"Dear Sir,—Your valued communication came to hand in due course, and I am much obliged and gratified by the great amount of trouble you have taken in order to explain your views on certain points of the difficult and important problem which, I believe, for a long course of years, has been a study for both you and me. Doubtless there are several points on which, like sensible men, we may *agree to differ*, without, in the least, impairing the good understanding and mutual respect which ought to characterise the discussion of any subject, and in an especial degree the discussion of a purely scientific one. . . As to the transactions, Section G, B. A.: you lost very little by being called away. My paper was set down last for Monday, and a Council meeting coming on at three o'clock, I think about twenty minutes was the time in which it was to be read and discussed. When about one third through I had to stop, . . . discussion there was none. Mr Denny described your mode of continuing the pressure curve to the origin ordinate, but, I thought, in a way that would lead a stranger to suppose we had arrived at precisely the same result, and had time permitted I would have noticed the points of difference.

"My paper opens with a declaration of its object—'the application of certain propositions to the experimental trials of steam vessels,' . . . 'the propositions are the statement of these experimental laws in this way exhibited and tested within the limits of experience.' I might simply challenge the production of actual trials of steam vessels inconsistent with these propositions, and might also assert 'known dynamical data of the case' to lie between the least and greatest experimental speeds of these steam vessels, declining to be judged by data drawn from *unknown seas* at and about the origin. . . . However, I gladly waive all this, and beg to offer the following reply to your objections.

"A steam vessel is a machine in which power is developed to do work, and is subject to the same laws as other machines. Now, a machine in motion developing power has a constant deduction from the acting pressure which is independent of the velocity of its motion. I have denoted this defect of pressure by the symbol f, and, at page 4, you will notice I affirm that this quantity is not the measure of the moving friction alone, but this friction, in most cases, very much modified by external circumstances, of which, except by the resultant effect, we possess no definite measure.

"To determine this resultant effect of the whole. I affirm two experiments at moderate speeds are alone necessary, and that the straight line drawn through the logarithms of the gross pressures, set up as ordinates to the trial speeds as abscissæ, cuts off from the origin ordinate a line representing in like manner the logarithm of the quantity f. Pardon me for observing that in challenging this you labour under the disadvantage of not knowing the real ground of this assertion, which would have been more obvious if you had my paper in full before you. I send you the rough proof slips of the part immediately following. . . .

"Allow me to indicate my propositions in a concise and symmetrical form.

> "First, The *logarithms of the gross pressures* increase, from the value Log f at the origin, by the product of the speed and a constant factor α; or,
>
> $$\mathrm{Log}\,(P + rp) = \alpha V + \mathrm{Log}\, f.$$
>
> "Second, The *logarithms of the number of revolutions per mile of the propeller*, increase, from the value Log n_0 at the origin, by the product of the speed and a constant factor β; or,
>
> $$\mathrm{Log}\,\frac{N}{V} = \beta V + \mathrm{Log}\, n_0.$$
>
> "Fourth, The *logarithms of the ratio of effective pressure to the speed* increase, from the value $\mathrm{Log}\,\frac{n_0}{l_0}$ at the origin, by the product of the speed and a constant factor $(\beta + \gamma)$: or,
>
> $$\mathrm{Log}\,\frac{(P + rp - f)}{V} = (\beta + \gamma)\, V + \mathrm{Log}\,\frac{n_0}{l_0}$$

"The third proposition being a mere corollary of the two first, is left out of consideration, and my argument is: these propositions are not independent, but are conjointly satisfied by conditions which exist in a steam vessel, and express the mechanical principles involved in its direct motion. We may take one of these equations and put in any arbitrary values we please, but what sort of a steam vessel would answer to these suppositions? Would it be 'such as are designed and constructed by competent parties?' I submit that arguments founded on abstract and partial considerations of numbers have no force whatever, and, supposing we cannot agree upon principles between the origin and lower limits of observation, does it much matter? The question: do the propositions fairly interpret the phenomena between the limits of observation? is surely of vastly greater importance.

"In my remarks I have stated two speeds only as being requisite; this involves their being fairly accurate, which many are not; hence the virtue of Mr Denny's more numerous observations, enabling us to get a fairer average of the two for calculation purposes."

Finally: in justification of my views on the whole matter, and in illustration of their perfect agreement with the facts of experience, I add the results of their application to this special vessel the "Merkara," in respect of the Power, Revolutions, Piston Pressures, and Residual Pressures. The values of these elements being calculated by the following formulas, are contrasted with the values given or involved in Mr Denny's data for this vessel.

"Merkara"

Power, $E = 16{\cdot}90 \ V \operatorname{Log}^{-1} {\cdot}0735 \ V.$ (a)
Revolutions, $N = 5{\cdot}176 \ V \operatorname{Log}^{-1} - {\cdot}0021 \ V.$ (b)
Piston Pressures, $P + rp = 7{\cdot}745 \ \operatorname{Log}^{-1} {\cdot}0756 \ V.$ (c)
Residual Pressures, $P + rp - 7{\cdot}745 = 1{\cdot}226 \ V \operatorname{Log}^{-1} {\cdot}0478 \ V.$ (d)

Calculated Results.

Trial Speeds.	Power.		Revolutions.		Piston Pressures.		Residual Pres'res.	
	Trial.	Form.(a)	Trial.	Form.(b)	Trial.	Form.(c)	Trial.	Form (d)
12·91	1948	1940	63·23	62·78	73·30	73·30	65·555	65·555
11·09	1225	1225	54·35	54·40	53·55	53·39	45·805	46·09
*9·10	718	718	44·75	45·07	38·12	37·76	30·375	30·38
6·20	299	299	31 15	31·15	22·79	22·79	15·045	15·045

ROBERT MANSEL.

WHITEINCH, GLASGOW, 21st April, 1884.

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* The speed given by Mr Denny for this experiment is 9·20 knots, but the results indicate 9·10 as the more probable value. This trial also illustrates the interchange of revolutions and pressure, so that with the power following the normal law, the revolutions and pressures, viewed separately, seem to depart from their normal law.

Letter from Dr Froude to Mr Robert Mansel.

Chelston Cross, Torquay, 23rd September, 1876.

DEAR SIR,—I was much obliged for the corrected copies of your paper of "Propositions on the Direct Motion of Steam Vessels" which you were so good as to send me at Sir W. Thomson's, and I regretted greatly that I was cut off from the opportunity of hearing the paper read and joining in the discussion, by being summoned (prematurely as it proved) to superintend some experiments at Portsmouth for the Admiralty. The experiments will not, in fact, take place till next week.

I have not heard whether your paper was, in fact, read and discussed. Had I been able to join in the discussion, I should have had to express my regret, that while in many respects I went along with you, that is, that my independent investigations had brought my ideas into pretty nearly the same course as yours, there were some very definite and not unimportant differences between us both in the mode of treating the data before us and in the conclusions arrived at.

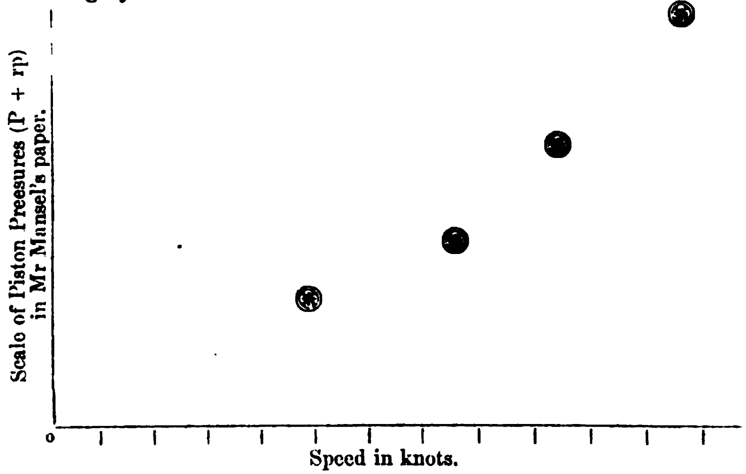
I have no wish to run into a controversial discussion, but as you have given me an opportunity of understanding your views better than I have done before, I should like in return to explain to you at least the most fundamental of the grounds of difference between us.

Perhaps the first remark I shall have to make will seem to imply that I forget the saying which, I think, has a real force when applied to modes of scientific investigation, as to the circumstance to which it primarily belongs—" Each glazier can best use his own diamond." (By the way the improved method of mounting diamonds has greatly invalidated the original force of the saying.) No doubt each mind has its own grooves of thought in which it works best, and if it did not seem to me that your mode of handling Mr Denny's data had led you into an erroneous conclusion, I should have felt you were but working in your own groove, and I should not have felt entitled to say that by laying down—not the pressures themselves, but the logarithms on the speed abscissæ—the real significance of the record was obscured, for if the step had not led you into error I should have been bound to believe that your own method fitted your own groove of thought best.

To me certainly the "logarithm of a pressure" conveys a less instructive meaning than the "pressure" itself, and though no doubt on finding that the series of logarithmic ordinates resulted in nearly a straight line, there must have been a fascination and a temptation to regard the line as probably straight up to the origin, and to assume this as a fact, I should myself have felt it dangerous to make the assumption, unless it in all respects rationally fitted the known underlying dynamical data of the case. Now these are certainly more readily associated with the "pressures" themselves than with their "logarithms," and to turn the former into the latter, seems to me only to obscure what is naturally clear.

When I received Mr Denny's data from him, more than a year ago, the first thing I did was to use the pressures as ordinates to speed abscissæ, and on doing so the result obviously at once, not only indicated the existence of the "constant" element of the engine friction, but suggested what is, to my mind at least, the true mode of determining its actual value, in virtue of the natural and obvious dynamic conditions on which the total pressures must depend.

Before putting the results into actual figures I will explain my meaning by sketches.



Laying off the piston pressures simply and nakedly, the four spots arranged themselves about as sketched. The arrangement was such as to make it quite clear they belonged to a curve which would not come down to the zero of pressure at the zero of speed. And the question which at once presented itself was, How would the curve have been continued if the ship could have been run at slower and slower speeds down to zero ? and again, What is the meaning of the curve not running down to zero ?

The answer to the latter branch of the question supplied the answer to the first branch also.

Obviously the meaning of this feature in the curve was, that it indicated that element of engine friction which may be called constant, being due to the dead weight of the working parts, and to the tightness of the piston packings, &c., elements of friction which may be regarded as practically constant; while, of course, there must also be another element of friction, due to the stresses which constitute the working load, being the fruit of the ship's resistance, which grows greater as the load increases.

We (you and I) are agreed here in principle, but as will be seen our modes of determining the constant friction lead to results differing in amount, and I am not without hope that I shall convince you that my method is the correct one.

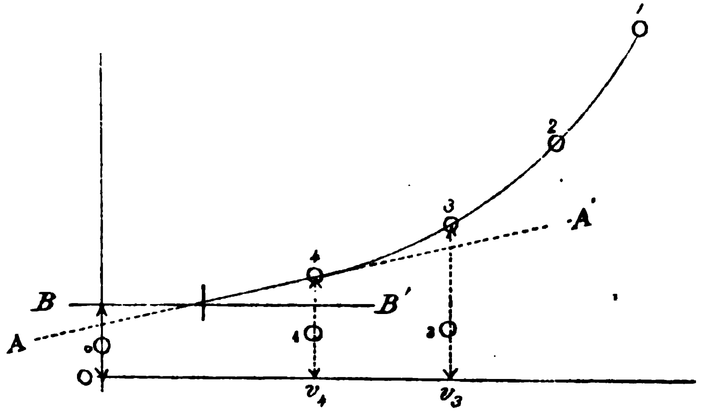
Bearing in mind the nature of the two causes of or demands for piston pressure—(1) the constant friction; (2) the load due to the ship's resistance and its derivatives—it must be approximately true, especially at low speeds, that the total residue of piston pressure, after deducting the constant friction, will be proportionate to the ship's resistance at each speed.

Moreover, we know quite well, and I believe you will agree with me here, that, at low speeds at all events, the ship's resistance is almost exactly as the square of her speed.

If this be so, it follows that, at all moderate speeds, the piston pressure may be expressed by an equation of the form,

$$\text{P (or P + rp as you write it)} = a + bv^2,$$

and the completion of the curve to the speed zero is accomplished by the easy process of constructing the parabola which this equation represents. This may be done in two ways, one geometrical, the other algebraical.



(1) Geometrically. Draw a fair curve through the four spots, and draw its tangent, A A', through point 4. Then draw B B' (parallel to the speed-base) so as to cut A A' halfway between point 4 and the speed zero. Then the line B B' will be the tangent to the parabola at the vertex and B0 is (to scale) the pressure which corresponds with the constant friction.

(2) The algebraic mode is more definite, and may be relied on as accurate if we have reason to believe that the resistance has not sensibly outrun the square of the speed at the speed belonging to point 3. If we are satisfied of this, the completion of the parabola is at once obtained by solving the following quadratic,

if ${}_0 0$, ${}_4 0$, and ${}_3 0$ be the parabolic ordinates, at the origin, at point 4 and point 3 respectively,

$$ {}_4 0 = {}_0 0 + b_4 v^2 \qquad {}_3 0 = {}_0 0 + b_3 v^2 $$

by which equations ${}_0 0$ and b are readily determined; and observe ${}_0 0$ is the same as (a) in the equation to the parabola which I gave just now.

I proceed to explain why I think that your method of treating the ordinates—namely, laying off their logarithms as the basis of the determination—is in several ways misleading; and how, by adopting it, you have erroneously estimated the constant friction.

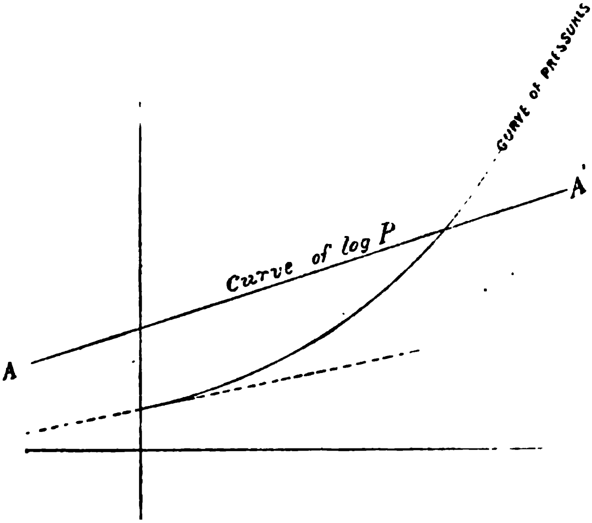
It is misleading because, as v increases, and the pressure ordinates increase to comparatively large figures, a given alteration of pressure ordinate produces a less and less alteration in the corresponding logarithmic ordinate, as indeed is obvious when one recollects the well-known differential expression,

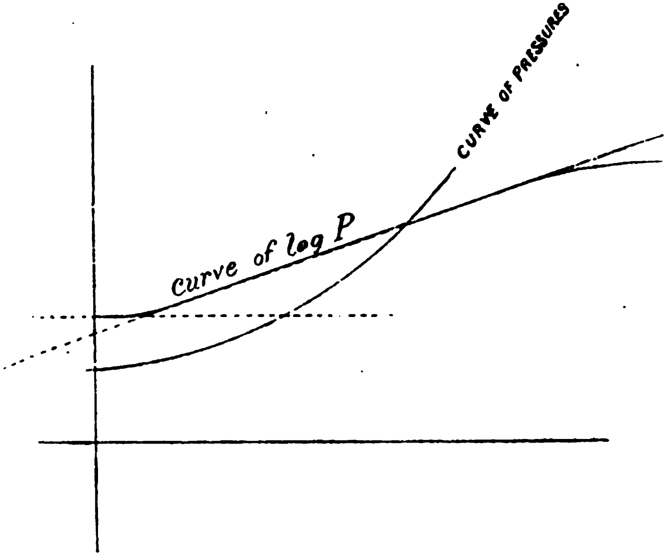
$$d \log y = \frac{dy}{y}$$

The greater y is, the less is $d \log y$ for a given value of dy, and hence large and instructive features of the curvatures in the pressure curve vanish in the curve given by the logarithms of the pressure, and the resulting curve becomes more and more temptingly like a straight line.

This tempting resemblance to a straight line has led you—*I* would say has *mis*led you—into supposing that the same degree of straightness would be continued to the origin. The supposition is entirely gratuitous. The line could only continue straight in virtue of the existence of a very peculiar law of pressures near the origin—a law of pressures which we have not the slightest reason to suppose can exist. And on the other hand, if we suppose the law of the pressures to be such as we know it must be with a very close approximation; that is to say, if we assume the pressure, in the region of moderate speeds, to be defined by the equation $P = a + bv^2$, it is easy to prove that the line given by the logarithmic ordinates is not straight down to the origin, but curved so as to cut the vertical axis at right angles.

If, for instance, we assume that A A′, or the line of logarithmic ordinates, continues to run straight right down to the origin, then (it is easily proved) the curve of pressures will cut the vertical axis obliquely as sketched, which we know cannot be true; or if the curve of pressures be described by an equation of the form $P=a+bv^2$, then the tangent to the logarithmic curve will, at the origin, be horizontal, as sketched. (See next page.)





The reason why the curve of the logarithms of the pressures *looks* so straight, is that it contains a very elongated region of contrary flexure, and at a contrary flexure the curvature is always zero; and it will be seen that whereas if the ship's resistance, and the steam pressure due to it, remained proportional to the (speed^2) up to the highest speeds, the curve of logarithms of pressures would turn downwards, as sketched; yet inasmuch as the resistance soon begins to increase faster than (speed^2), this circumstance provides a growth of pressure ordinates at the higher speeds, such that, in the curve given by the logarithms of the pressures, the downward tendency of the curve is more or less obliterated, or (according to circumstances) even reversed.

It is interesting to trace the history of this curve in its mathematical details, and this may be simply done, if we assume, as I feel sure we ought to assume, for the lower speeds at least, that

$$\text{P (or P + rp by your notation)} = a + bv^2.$$

This being so, your curve of pressure-logarithms is defined by the equation,

$$y = \log (a + bv^2);$$

and observe, for convenience of writing, I shall adhere to the use of hyperbolic logarithms in the discussion. This being so,

$$\frac{dy}{dv} = \frac{2\,bv}{a + bv^2} \qquad (1)$$

and

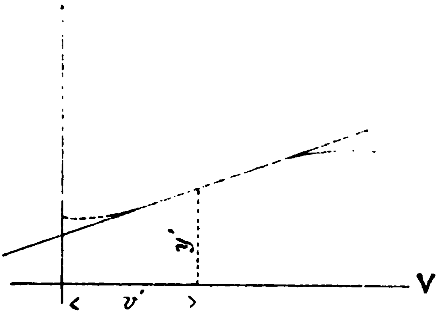
$$\frac{d^2y}{dv^2} = 2\,b\,\frac{a - bv^2}{(a + bv^2)^2} \qquad (2)$$

by (2) we see that the curve has a contrary flexure at the point where $(a - bv^2) = 0$, or v, say v', $= \sqrt{\frac{a}{b}}$, and where the corresponding value of y, say y', $= \log 2\,a$; also, $\frac{dy'}{dv'} = \frac{b\sqrt{\frac{b}{a}}}{a} = \sqrt{\frac{b}{a}}$.

If we take the equation of the tangent to the curve at the point where this contrary flexure exists—that is, the point where the co-ordinates are y' and v'—the equation is,

$$y - h = v\frac{dy'}{dv'} = v\sqrt{\frac{b}{a}},$$

where h is the height at which the tangent cuts the axis of Y.



In this equation we can determine h by inverting the conditions which belong to the point y', v', and where, since it is also a point in the curve of pressure-logarithms at the point of contact,

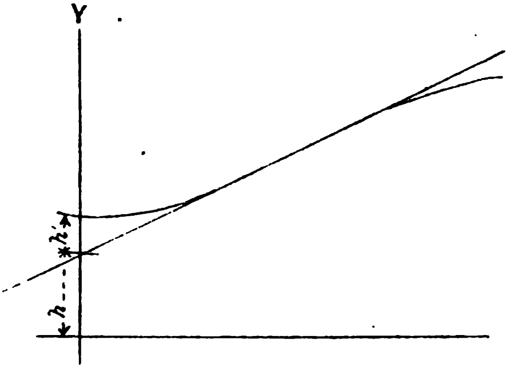
$$y' = \log(a + bv'^2) = \log 2\,a.$$

Substituting this value in the equation of the tangent,

$$\log(2\,a) - h = \sqrt{\frac{a}{b}} \times \sqrt{\frac{b}{a}} = 1;$$

$$\text{or, } h = \log(2\,a) - 1.$$

Now, in your way of estimating the constant friction (which in your notation is f), you make $h = \log f$, but in reality $f = a$ in the equation of the original parabolic curve, and $h + h' = \log f$ (see sketch).



You will perhaps say that you have as good a right to your method as I have to mine; and the only reply I have is that the assumption you make—namely, that the line of pressure logarithms is a straight line, and continues straight till it cuts the axis of Y at the height h—is a gratuitous assumption, founded on no mechanical or dynamical principle; but that my conclusion rests on the justifiable and indeed plainly correct assumption that (at all events for low speeds) $P = a + bv^2$. If this assumption be a true one, my conclusion is plainly true also; and it is only because I think you will, on reflection, probably agree with me as to the constitution of P, that I have a right to intrude this long discussion on you.

Without further apology, therefore, I will continue to work out in detail the differences which arise out of the two modes of treating the question, as they issue in reference to the case of the "Merkara," which is one we have both investigated; and in treating it I will take the steam pressures in the terms in which you have expressed them, as "unit piston pressures, P + rp," but for simplicity I symbolise the figure as P.

According to my way of treating them, then, it is only necessary to see what are the elements of the parabola which is expressed by the equation $P = a + bv^2$; that is to say, to determine (a) the pressure due to constant friction, and (b) the factor which expresses the relation of the residue of the pressure to the square of the speed.

As in point of fact the resistance of the "Merkara" does not, up to 9 knots, sensibly deviate from the quadratic law (as I know by experiments with her model), these values may be as exactly determined by what I called the algebraical method (see page 57), as by the geometrical, and I adopt the former.

The two experiments, at 6·2 knots and at 9·2 knots, give data which when substituted in the equation $P = a + bv^2$, result in the two equations following.

The data are,

(1)	Speed, 6·2,	Pressure, 22·79,
(2)	Speed, 9·2,	Pressure, 38·12,

and the equations are,

$$(1)\quad 22{\cdot}79 = a + b \times (6{\cdot}2)^2.$$

$$(2)\quad 38{\cdot}12 = a + b \times (9{\cdot}2)^2.$$

The solution of the equations gives

$$a = 10{\cdot}04 \qquad b = \frac{1}{3{\cdot}0136} = 0{\cdot}3318,$$

and the equation of the parabola is

$$y = 10{\cdot}04 + 0{\cdot}3318\,v^2.$$

Here, 10·04 is the constant friction pressure.

If I frame your curve of the logs of the pressures, these values of a and b supply the data.

By the equation in p. 61,

$$h = \log 2\,a + 1.$$

Reminding you again that I am using hyperbolic logarithms,

$$h = 1{\cdot}9997,$$

and since $h = \log {}_0P$, ${}_0P = 7{\cdot}39$; and 7·39 is the pressure due (by your method) to the constant friction, whereas by mine it is 10·04.

The equation of the straight line which is tangent at the contrary flexure, and which cuts the axis Y at the height h—the straight line, in fact, which fits the logarithms of the pressures of the "Merkara" trials—is

$$y - 1{\cdot}9997 = v\sqrt{\frac{b}{a}} = \frac{0{\cdot}3318}{10{\cdot}04} = {\cdot}1818\,v;$$

and if the ordinates of this line be calculated out for each of the values of v, 0, 1, 2, &c., and if again we set side by side with these values the tabulated values of log. P, calculated for the same values of v, by the equation $P = a + bv^2$ we get the following columns, and you will see how closely the two run together for all the lower part of the curve for which the "Merkara" trials gave data, and how resolutely they differ for all the still lower values of v, namely values below 6·2 knots.

v.	Ordinates of tangent on Mr Mansel's straight line, which approximately fits the logs of "Merkara" Pressure.	Ordinates of curve formed by log P when $P = a + bv^2$.
0	2·0000	2·3065
1	2·1818	2·3389
2	2·3636	2·4309
3	2·5454	2·5672
4	2·7272	2·7311
5	2·9090	2·9085
6	3·0908	3·0901
8	3·4544	3·4426
10	3·8180	3·7663

It will be seen that the figures agree very closely for a long range, and if the second column were shorn of the figures for $v = 0, 1, 2$, at the beginning of the scale, and $= 10$ at the end, the intermediate figures when laid off as ordinates at their appropriate speeds would be suggestive of an absolutely straight line; and yet they would lead into error any one who adopted and relied on the suggestion.

There are two other columns of tabulated figures which I will tabulate accordingly.

(1) The series of pressures calculated by the equation $P = a + bv^2$ the other, the *numbers*, of which the ordinates of the straight line are the logarithms, and which are implicitly assumed to have been the pressures, if you assume the line given by the logarithms to have been straight throughout.

You will observe that the figures in the two columns agree very closely at the medium speeds, that at the zero end of the speed scale there is a difference of about 1 in 8·3, and at the 10 knot speed there is a difference of about 1 in 20. And it is deserving of notice that if we take the differences between the corresponding pairs of figures in the table in p. 23, while the difference at the zero end of the scale is there only about 1 in 7, the difference at the 10 knot speed is only 1 in 75, so that by converting the pressures into their logarithms an operation has been performed which masks or reduces in visible magnitude the differences of the pressures, especially of the larger pressures, and makes it easy to overlook, especially in a graphic exposition of results, linear or geometrical quantities which represent quantities which are really large but which are made small by their logarithmic dress. (I have already referred to this circumstance in pages 11 and 12.) The table is as follows:—

v.	Pressures inferred from the Ordinates of the straight line, regarded as the logs of the Pressure.	Pressures calculated by the equation $P=a+bv^2$.
0	7·39	10·04
1	8·86	10·37
2	10·63	11·37
3	12·75	13·03
4	15·29	15·35
5	18·33	18·33
6	21·99	21·98
8	31·63	31·27
10	45·50	43·22

Observe, this illustration of the consequence of converting pressures into their logarithms, which I just now pointed out, is a real one, even if it were shown that my equation $P = a + bv^2$, which I used in calculating the tables is erroneous—for the point of the illustration is that it shows how a given difference in the large pressures is reduced in visible magnitude by that method of representing the pressures.

To complete the comparison, I have pretty carefully "plotted" the two pairs of "tables" on the accompanying sheet of ruled paper—which, however, as I have not made a duplicate, I will ask you to return to me when you have sufficiently examined it.

I see there is one point in reference to the curve of pressures which I have omitted to notice.

You will see that the curve calculated by the equation $P = a + bv^2$ cuts exactly the two points of pressure given by the "Merkara" experiments, for the 6·2 knots and the 9·2 knots; this of course it was bound to do, because it was calculated from them; it, however, cuts below the point which belongs to the 11·09 speed, and still more below that which belongs to the 12·91 speed. On the other hand, the curve built on the pressures calculated back from the ordinates of the straight line, regarded as pressure logarithms, cuts distinctly above the experimental pressure for the 9·2 speed, and if continued would cut a little above that for the 11·09 speed, but would cut a little below that for the 12·91 speed.

The meaning of all this is that as a matter of fact, the resistance of the "Merkara" is practically as the square of the speed, up to just 9 knots; above this speed, the resistance increases in a higher ratio, and thus of course for higher speeds deviates from the parabola which correctly expresses it so far. On the other hand the line which is defined by the logarithms of the pressures is not absolutely straight though it is nearly so, and when an absolutely straight line is substituted for it, and the pressures are calculated backwards from the ordinates of the straight line, the resulting pressures are far more different from the true pressures than the logarithmic line is from the assumed straight line, and the definite tangible error is thus introduced into the data.

Nevertheless it happens that the series of pressures which are thus obtained from the straight line indicate a resistance which grows more rapidly than as the square, and which thus has a rough sort of agreement with the real growth; only the agreement is arbitrary and haphazard, and has no real relation with the special law of pressure which each form of ship requires (a law which is materially different for different forms), and will certainly contradict the law for almost any possible form of ship as the speed increases.

To give greater clearness to the view I am endeavouring to express, I send with this a couple of lithograph sheets (extracted from a report of mine to the Admiralty, sent in a year ago after I had made experiments with a model of the "Merkara"), one of which gives the lines of the "Merkara," and of three other types of form which had been included in our general course of experiments here—the other gives the resistances of ships of all four types, the "Merkara," and the other three, all reduced to ships of the same displacement, namely 3,980 tons.

In that report I went fully into the question of "constant friction," treating it on the principles which I have endeavoured to explain in this letter, so I need not send you the report, since the greater part of it has been already said here, and this in conjunction with the discussion of new matter which has grown out of your different treatment of the same data.

I have, however, added to the resistance diagram four lines based on the logarithms of the four resistance curves according to your method.

These lines are all so nearly straight in appearance for the greater part of their length as to offer great temptation to treat them as straight lines in the manner which your paper describes, and yet it is plain that in doing so the exactness and the significance of the original curves of resistance would be lost and falsified.

In calling attention to this diagram, I ought perhaps to refer to a feature in the "Merkara's" curve of resistance which will, perhaps, at first sight appear to you unnatural, and may lead you to mistrust the representation. I mean the peculiar "hump" in the curve, between the speeds of 14½ and 16½ knots.

Let me say first that our experiments are made with models of large size—that of the "Merkara" was about 14 feet long—and with the most exact automatic apparatus for recording speed and resistance, and our work already comprises the complete investigation of the resistance properties of several hundred varieties of form. And I may say it is rather the exception than the rule to find a *perfectly* "FAIR" curve of resistance. These humps occur with more or less pronouncedness in the case of all forms which have the sides straight for any length. If you scrutinise curve D closely you will see a trace of such a hump in the part of the curve between 11 and 13 knots.

And we have been able to identify the circumstance which seems the proximate cause of the hump feature, which is as follows:—

When the speed is such as to produce waves of sensible magnitude, that wave which is created by the bow reappears as a secondary wave with its crest against the ship's side—the position of the secondary crest being determined by the ship's speed, which requires that the wave which keeps pace with her shall have a definite length from crest to crest.

When the secondary crest abuts against the parallel part of the ship's side, (if she has parallel sides, like the "Merkara's,") the pressure due to the wave height has no local effect on the ship's resistance—it neither pushes her backward nor helps her forward—but when her speed is so increased that the wave length is so far increased that the wave crest infringes on or abuts against the converging lines of the run, then the extra head given by the wave crest becomes an auxiliary propulsive force, and makes a sensible diminution in the resistance.

Now I have said all I have to say in reference to the question of the ship's natural resistance and to the mode of treating the steam pressures supplied by experiment.

There remains a point of great importance, of which you make no mention in your paper, and to which perhaps your attention has never been directed.

I refer to the augmentation of resistance which results from the action of a screw or other stern propeller, from the circumstance that the sternward pressure which it throws on the water on which it operates, necessarily lessens the natural pressure of the contiguous water against the ship's stern post, and against the nearer parts of her run.

It has so long seemed to me inevitable that the loss of pressure must be thus produced, and it has been so long known to me that it does in fact exist to a most formidable extent, and yet I have found it so difficult to get shipbuilders to attach to the circumstance the importance it deserves, that I am almost weary of arguing about it.

And now, as this letter has already run to a portentous length, I will, instead of arguing about it, merely give you the categorical and quantitative proof of its existence which our experiments have supplied.

To make this clear, I must roughly describe our dynamometric arrangements.

Our "sea" is a *covered* tank or canal, nearly 300 feet long;—the working length is about *a mile on the scale of the model's dimensions compared with the biggest ships;*—it is 36 feet wide and 10 feet deep —*as deep in proportion to the model as the British channel to the biggest ships.*

A railway, suspended from the stiff roof, traverses the length of the tank 15 inches above the water.

A truck runs on the railway, driven by a double cylinder engine by a wire rope on a drum, which by help of a delicate governor and change wheels, administers any required steady speed to the truck.

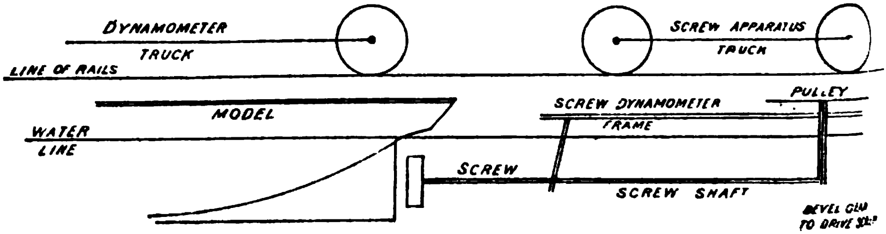
The model is carried forward by and under the truck, being guided or kept from sheering by a light counterbalanced frictionless knee joint motion at each end (which allows perfectly free motion in the vertical plane, or fore and aft, but is inexorable against sheering), and being pulled forward by a dynamometer spring, the extension of which by the model's resistance is automatically recorded. This spring is virtually the *tow rope*, and indicates the towing strain simply.

Let us now suppose that we have thoroughly determined the natural resistance of say the "Merkara" model at all speeds.

We now bring forward and "couple on" a second truck, under which there is bracketed down into the water a horizontal shaft, which I will call a screw shaft, which protrudes forward ahead of the truck, and is at the level which would be that of the screw shaft of the model if she had one. A screw, suitable to the model, is fitted on the end of the shaft, and the screw truck is coupled up astern of the dynamometric truck with the model under it, at such a regulated distance that the screw occupies exactly the position behind the model which it would occupy if the model were regularly fitted with a screw. The screw, however, does not touch the model.

As the two trucks run on together, the screw is made to revolve by independent power, at any required speed, being driven by gearing connected with the truck wheels, and it may be kept going at the speed proper for propulsion, or at any greater or less speed.

The frame which carries the screw shaft is delicately suspended, so that it possesses a power of moving horizontally under its truck, in a fore and aft direction, and the position of the frame is defined by a dynamometric apparatus which automatically records the drag or thrust of the screw. This rough sketch may help you to follow the description.



This apparatus, in the first place, brings out in the clearest way the augmentation of the model's resistance to which I have referred. When the screw is so speeded as to produce a drag equal to the model's total resistance; that is to say, to the natural resistance + the augmentation, it proves that from 40 to 50 per cent. has been added to the resistance. The augment is, in fact, 40 or 50 per cent. of the natural resistance at each speed.

In the next place the apparatus brings out in the clearest way the value of the wave or current which follows the ship, on which I am glad to see you lay stress.

I frequently show the following illustration which simultaneously exhibits both the augmentation of resistance, and the increase of screw drag which results from the following current at given speed of screw.

We first run the screw truck with its screw, but without the model—and we speed the screw to its true pitch—so that it neither drags at its frame nor pushes it back, but simply *cuts the water.*

We then harness on the model, and repeat the run.

The model's *natural* resistance at the speed (indeed at all speeds) is already known. Now that it has the screw at work behind it, the mutual reaction of the model and the screw becomes apparent. The screw, instead of merely cutting the water, is exerting a strong drag, probably more than half of what is required to overcome the model's natural resistance, the whole of this drag being due to the screw's encounter with the current which the model has created, for if the current were not there the screw would have no drag. The resistance of the model has received an augment of some 20 per cent., though the conditions of resistance are unchanged except by the drag which the screw exerts on the water behind it.

We shall shortly pursue this question with more completeness and exactness than we have already attained. There have been some difficulties in ascertaining the exact drag of the screw, because we have to clear it of the resistance of the thin knife-like brackets which carry it, and of the spindle and bevel gear which drive it, and to obtain this clearance with exactness, that is to say, to obtain its *exact* measure, has involved some difficulties which we have only recently seen how to master; but the broad features of the experiment are glaringly conclusive, and the residuary errors or uncertainties are relatively inconsiderable.

I may add that if the screw is removed sternwards to a space of about one-fourth of the model's beam, the augmentation of resistance is *greatly* diminished, and yet the benefit realised from the following current is scarcely if at all impaired.

When I began this letter I had not fully calculated the length to which it would run, and perhaps if I had done so I should have hardly felt entitled to seek to impose on you the burden of reading it. But it seems to me of such high importance that the subject of ship propulsion and ship resistance should be treated in a really scientific manner, and that therefore those who are endeavouring to arrive at and disseminate the true principles of the treatment should exchange ideas on the subject and come as far as possible to an agreement respecting it, that one should not be nervously hesitant about making such communications. Only, I must add, I hope you will not feel obliged to write at as great length in reply, for probably that would be too heavy a tax on your time, already perhaps too fully occupied by the demands of business.

I am yours truly,

W. FROUDE.

The discussion on this paper was resumed on 27th January, 1885.

Mr MANSEL said—The printed documents of last month's proceedings of the Institution furnished a full basis and introduction to the subject of discussion for to night, and showed quite clearly that while there existed a subject of scientific interest in dispute between the late Dr Froude and myself, Mr William Denny had enlarged it by a second question, of little general interest, which was personal between Mr Denny and myself. This latter most people, I think, would allow had better have been avoided as much as possible; and, better still, ought never to have been raised, for, I submit, any one who reads Dr Froude's letter will see, that while asserting the independence and superiority of his own interpretation, Dr Froude advances nothing which can be construed into a support of Mr William Denny's assertion, that my investigations on the same subject were founded upon notions of Dr Froude's method, which Mr William Denny had communicated to me in the autumn of 1875. When Mr Denny made this charge of plagiarism against me, his attempted explanation on my methods at once led Dr Froude's truer instinct in such matters to the real point at issue between himself and me, and he put the question as to the law of the resistance which I had assumed, stating quite truly "it must involve some such rule." Mr Denny admitted "he did not know the principle," also, "in every case, with one exception," the data had satisfied a remarkable condition, which he had stated to be a consequence of my principle. If so, Mr Denny ought to have been more guarded in his statements, and sought further information before, in my absence, making me the subject of an infamous charge, the existence of which, in a definite shape, only came to my knowledge about nine years afterwards! Mr Denny has printed a letter of mine, dated 17th May, 1875, in which I gave him an extended application of the formulas I advanced in the discussion of his paper on the "Difficulties of Speed Calculation," in this institution in April, 1875. Mr Denny's answer to that letter was accompanied by the trial data of two Chinese vessels, the "Pau Tah" and "Fung Shun," the latter of which was tried on 25th September, 1875. I replied shortly after, but having only a pencil draft of my letter, cannot specify the exact date.* Certainly, however, it was before my interview with him on his return from England, when he spoke of Dr Froude's graphical solution of my published equation, which, by Mr Denny's own admission, I at once challenged as unsuited for its object. To have written this letter after this meeting would, on my part, have exhibited a height of folly or depth of humility which I shall never try to attain! Mr Denny's answer to my letter of 17th May, 1875, was somewhat critical thereon, for which he made apology, and hence the tone of my reply; which, omitting some irrelevant matter at the commencement, read thus:—

"Your data of the Chinese vessels and last note and enclosures came to hand in due course, and I will forward you something on same shortly. Meanwhile I send you a few notes, which will probably be of interest. Don't imagine that I am in the least offended by any criticism that can be offered, so long as it is honest and discriminate! I have found so few in our profession who will give themselves the trouble to think at all, on scientific matters, either correctly or otherwise, that I have real pleasure in their discussion,

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* Mr Wm. Denny having, since the meeting, forwarded the letter referre to, it was sent to Mr Mansel for his inspection. The date is Dec. 4, 1875.—Ed.

and I am only sorry that we are so much out of one another's way; it is such a bore writing long explanations.

" Most people when they get a dose of Mansel's "peculiar" either turn on me with the sniff ineffable, *cui bono*, which is irritating, or speedily exhibit symptoms of intense stupefaction, *quite bovine*, which is pitiable; but blessed are they who expect little. There is a system of curve analysis, which is very applicable to the cases we are discussing, namely, taking the abscissas in natural numbers, and measuring the ordinates by a logarithmic scale: the curves are much more easily drawn, and their relations exhibited. I send you speed curves of the 'Goa,' 'Hawea,' 'Merkara,' and 'Mecca,' drawn in this way; these are denoted by the blue curves, actual observation spots being noted by the small circles. The red curves next to these are the calculated values of E_v· or power expended on the vessel. The black curves are what I hold to be the value of the work done on fluid resistances, distinguished in my notes as E_f and E_m, but W_f and W_m, *i.e.*, work on friction, plus work on movement, is a more correct notation, as power should be equated to work. Between the black curve and the red we have the values of the quantity E_m, which I call the recovered power. You notice correctly that I have made an alteration on its value. When I first examined your 'Goa' I thought proper to give the after term the value

$$\left\{ \alpha V + \frac{49 - \alpha}{82 \cdot 5} \Delta^{\frac{2}{3}} \right\} \left(\cdot 2059 \frac{E_v}{E_r} \right)^5 .$$

This gives very close results with some of your vessels, but it does not answer over a great range of vessels so well as the simpler formula :—

$$\left\{ 200\, \alpha^{\frac{1}{2}}\, V + (100 - \alpha)\, \Delta^{2} \right\} \left(\cdot 2059 \frac{E_v}{E_r} \right)^5 .$$

In later calculations, I find it simpler to take out the last constant; and put it under the form,

$$\left\{ \frac{\alpha^{\frac{1}{2}} V}{700} + \frac{(100 - \alpha)}{140{,}000} \Delta^{2} \right\} \left(\frac{E_v}{E_r} \right)^5 .$$

I enclose you the 'Mecca's' calculation as an example.

" Your remarks as to application of the expression of the term

E_v are acute and quite proper, from your point of view. It is involved and indefinite, but I have a notion of a flank movement which will make these difficulties untenable! I will close for the present by marking on the several vessels the straight line which gives the relation between V, N, E, and its equation on the severa vessels. You cannot fail to observe the beautiful certainty with which the spots or experimental values of logs $\frac{E}{N}$ fall in the straight lines. On the 'Hawea,' up to about $10\frac{1}{3}$ knots, we have the same; after this, notice the divergence of the ends by E from its fair line, and the corresponding rise of the red spot above its line; also, the rapid diminution of the recovered power line about this spot. This is the mode by which I would exhibit the effect when a vessel is driven beyond a speed corresponding to her dimensions and fineness. Your 'Chinee' gives another excellent illustration, of which I cannot give you the result now. The correlation of a lot of facts will be necessary to further remarks."

As will be seen, this letter contains an outline of the mode of investigation fully followed out in my paper in spring of 1876; and, in so far, is an answer to Mr Denny's inquiry for information as to my working out of this matter, prior to the interview when we differed over Dr Froude's method. I shall now follow with a short essay on an extensive subject:—

NEW DEPARTURES IN NAVAL SCIENCE.

I prelude this subject with a little myth, and its moral. On hearing the Templar's latest intelligence from seat of war in the East —truce for fifty years concluded with the Paynim—" Jester Wamba" naively remarked: Inasmuch as in his lifetime he remembered *three* such treaties, he must needs be getting a very old man! And so it is with workers in the obscure fields of naval science: they become cognisant of so many vaunted new departures, if only a small portion of those represented wise directed labour over some manifest mystery, Jacob like wrestled with and not let go till the dawn came with a vouchsafed blessing: the world had certainly been older, and mankind probably wiser. But, in recent times, new departures do not seem to mean much: fresh minds recognising old facts or fancies, thinking much of them and more of some real or supposed novel point of view in which they are managed to be presented; and, in the end, apparent progress turns out to have been mere marking time or scientific goose step, not by any means steps in advance; and, in cases, doubtful whether they have not been retrograde rather.

On the first applications of steam to ship propulsion, the successive tentative experiments with paddle vessels of increased dimensions, power and speed, were correlated and compared by the reasonable hypothesis that, for different vessels at different speeds, the power would be found to vary in the ratio of the products of the square of a lineal dimension by the cube of the speed: and, in the case of similar shaped vessels proportionately immersed, and having engines and propellers of like efficiency, the co-efficients of such formalas, (usually known as the Admiralty formulas) ought to be constant. Also, since the square of a lineal dimension was involved in the immerged mid-ship area, the immerged surface of hull, or the two-third power of the displacement, it was a matter of indifference which of these elements, or even a combination of them, was employed, as the hull factor in this comparison. Experience, however, soon exhibited many marked deviations from this assumed law, which could not reasonably be accounted for by slight structural variations which might exist, and it came to be well understood that the fundamental hypothesis, in some way, was faulty, and that great care was necessary in order to draw correct conclusions from its indications.

In a "First Report of a Committee of the British Association," published in 1869, "separately for the use of the Institute of Naval Architects," an extract from a pamphlet by M. Dupuy de Lôme is given, stating "It is near the truth to say, that, for similar forms, the resistance per square mètre of midship section, at the same speed, decreases as the vessel increases, in the ratio of the square roots of the radii of curvature of its lines, these radii being themselves proportional to the lineal demensions of the ships; it is therefore wrong to compare the resistance of different ships by many of experiments made on models to reduced scales," and the same author refers to long anterior lectures, in which this subject was discussed by M. Rèech. In this report at page 28, it is also stated, "and some time later (than 1827-28) M. Rèech pointed out that models of different sizes intended for comparison should be made to move at velocities varying as the square root of their lineal dimension: in this case the actual resistances would vary as the cube of the lineal dimension. This would follow from the theory of the resistance of submerged bodies on the supposition that the resistance varies as the square of the speed."

Let us suppose two similar vessels of different sizes, the power, lineal dimension, and speed of the larger and of the smaller being denoted by the letters E, L, V, and e, l, v, respectively.

Stated as analogies, the original hypothesis being :—

$$(1) \quad E : e :: L^2V^3 : l^2v^3.$$

By Rèech's hypothesis, concurrently, we ought to have

$$(2) \quad V : v :: \sqrt{L} : \sqrt{l}.$$

Which, being fulfilled, (1) then changes into

$$(3) \quad E : e :: L^{\frac{7}{2}} : l^{\frac{7}{2}}.$$

Since, by (2), $V^3 : v^3 :: L^{\frac{3}{2}} : l^{\frac{3}{2}}$,

consequently, *the speeds of similar vessels being in the ratio of the square root of a lineal dimension, the powers for those speeds will be in the ratio of the seventh power of their square roots.*

For example—Suppose three similar paddle vessels, 200, 250, and 300 feet long, respectively; also, that the one 250 feet long is driven 10 knots, when the engine developed power at the rate of 1000 indicated horses.

Distinguish the elements of the different vessels by the suffixes 1, 2, and 3, then the speeds being in the ratios defined by (2),

$$V_1 : V_2 : V_3 :: \sqrt{200} : \sqrt{250} : \sqrt{300},$$

which the factor $\frac{10}{\sqrt{250}}$ changes into

$$: : 10\sqrt{\frac{200}{250}} : 10 : 10\sqrt{\frac{300}{250}}$$

$$: : 8{\cdot}945 \quad : 10 : 10{\cdot}955 \text{ knots.}$$

The respective comparative speed for the three vessels. By (3) the corresponding powers for these speeds,

$$E_1 : E_2 : E_3 : : (200)^{\frac{7}{2}} : (250)^{\frac{7}{2}} : (300)^{\frac{7}{2}},$$

which the factor $\frac{1000}{(250)^{\frac{7}{2}}}$ changes into

$$: : 1000\left(\frac{200}{250}\right)^{\frac{7}{2}} : 1000 \ : \left(\frac{300}{250}\right)^{\frac{7}{2}}.$$

$$: : 458{\cdot}3 \quad : 1000 \ : 1893 \text{ indictd. horses.}$$

But the simplest applications of these principles to practice is enunciated thus: *Similar vessels driven at speeds proportional to the square root of a lineal dimension, will have their Admiralty co efficients all of the same value.*

This is easily seen to be the case: suppose the 250 feet vessel had an immerged mid area of 450 square feet, the midship area formulas,

$$\text{co-efficient} = \frac{\text{mid area} \times \text{speed}^3}{\text{power}} \text{ obviously are:}$$

$$\text{co-effict.} = \frac{450\left(\frac{200}{250}\right)^2 \times 8{\cdot}945}{458{\cdot}3} = \frac{450 \times 10^3}{1000} = \frac{450\left(\frac{300}{250}\right)^2 \times 10{\cdot}955^3}{1893} = 450$$

Now, instead of being driven at the respective speeds found above, more power is developed in the 200 feet vessel, and less in the 300 feet one, so that the speeds of all three are 10 knots: then, the one is overdriven and the other underdriven by about one knot from the speed which would justify the one value of co-efficient; and, if from the experiment on the 250 feet vessel, we had set out to calculate, by formula, the power for the other vessels; according to the experience of naval architects a less co-efficient would be adopted for the less vessel and a larger co-efficient for the greater: tantamount to saying; in the same vessel, *the Admiralty co-efficients diminish with the increased speed.* Or, we may write this, co-efficient $= \frac{\text{constant}}{f(v)}$ where $f(v)$ denotes some unknown function of the speed, and the formula would stand thus:

$$\text{power} = \frac{\text{mid area} \times \text{speed}^3}{\text{co-efficient}} = \frac{\text{mid area}}{\text{constant}} \times \text{speed}^3 \times f(v).$$

Now $f(v)$ increases with the speed; and, hence, *the power increases in a faster ratio than the cube of the speed.*

This is the necessary deduction from Rèech's hypothesis; but, to a limited extent, agrees with the facts of experience, and the special explanations generally advanced, are admitted failures. The report already referred to, commences by stating "resistance may be treated in two ways . . . either in gross, as regarding the power required to drive a vessel of certain force and dimensions at a specified rate; or, in detail, as regarding the exact way in which the vessel and propeller act and react upon the water which they disturb . . . the former of these is not understood with any reasonable degree of certainty, and the latter also being far from being settled with precision." Rèech's hypothesis is strictly the one which under the name of "Froude's Law" has been much lauded in recent years. For example, in the "Watts Anniversary Lecture" at Greenock, by Mr Wm. Denny, it is stated: "model experiments had been made before Mr Froude took this matter of resistance of vessels in hand, but it was not till his time that we were enabled to relate accurately the resistance of a model to that of a large size, or to that of a full-sized vessel. Partly by speculation and partly by experiment Mr Froude discovered the law of this relationship. What Mr Froude discovered amounts to this, that for vessels of the same proportional dimensions, or, as we say of the same lines, there are speeds appropriate to these vessels which vary as the square roots of the ratio of their dimensions; and at these appropriate speeds the resistances will vary as the cubes of their dimensions." This is exactly what Rèech deduced some 50 years before. It seems to me, those statements are not fair towards the memory of M. Rèech and very injudicious towards that of Dr Froude. In all this inquiry the primary object is to enable us to foretell the power required to propel a given vessel at required speeds, and to compare the efficiency of different vessels. Dr Froude's method of solving the first of these objects by means of model experiments, only yields approximations by the use of empirical assumptions, which are questionable both in point of novelty and accuracy. As shown, put in the most direct shape, the principles amount to these: when the speeds of similar steam vessels are in the ratio of the square root of a lineal dimension, the power for these speeds, are as the seventh power of this square root; and, the "Admiralty co-efficients," such as, co-efficient $= \frac{\text{mid area speed}^3}{\text{power}}$ have the same value.* Now, employed between vessels of not greatly varied dimensions and forms when subject to like actions and reactions and losses in machinery and propeller; there may be a reasonable expectation of agreement between deductions obtained by these principles and experience, which can not be expected from their partial application to a small scale model, in which the absence of the propeller entirely alters the character of the phenomena, and leaves the determination of the power for the corresponding vessel much a matter of guess work. Dr Froude's signature is attached to this report in which M. Rèech is credited with the discovery and publication of this so called "law," and though Dr Froude dissented from the conclusion of other members of committee; that for certain data full-sized ship experiments were desirable, and in a supplementary report upheld the value of model experiments, which he declared "when rationally dealt with, by no means deserve the

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* To illustrate this subject let us refer to a recent illustration from a paper by one of Messrs Denny's employees. A 12-feet model is drawn through the water at three knots with an observed strain of four lbs., hence $4 \times 3 \times 101{\cdot}3$ foot pounds per minute; that is to say, an indicated power of ·03683 indicated horses, and since $\sqrt{12} : \sqrt{300} :: 3 : 15$, a 300 feet similar hull, would be drawn through the water at 15 knots, by $\cdot03683\left(\frac{300}{12}\right)^{\frac{7}{2}}$ = horses = $\cdot03683(5)^7$ = 2879 horses. Also, if a be the immerged mid area of the model,

$$\text{co-efficient} = \frac{\text{mid area speed}^3}{\text{power}} = \frac{a\ 3^3}{\cdot03683} = \frac{a\left(\frac{300}{12}\right)^2 15^3}{2879} = 733\ a \text{ for both.}$$

To 2879 horses as above, is to be added slip losses, action of propeller and working engines increasing the gross, so as to compare with experience, by some very doubtful assumptions; amounting on the gross to something very like an old rule "*guessing the half and doubling it.*"

mistrust with which they are usually regarded" so far as the model is implicated in his explanations, the dealing is simply the direct application of Rèech's hypothesis. Beyond this, however, there are other deductions, which, whatever merit they possess, may with more propriety be ascribed to Dr Froude. I would here remark; beyond giving a good summary of the views of some eminent men who have interested themselves in naval science, the labours of this committee do not seem to have gone far to advance that science. Indeed, properly examined, we have statements contradictory to a degree; and, viewed as a whole, singularly adapted to unsettle in men's minds, any small degree of certainty which might there exist on any particular branch of the subject considered! Take, for example, the very innocent looking deduction "vessels of a certain form" (corresponding to the minimum of resistance) have a "fair entrance and run and an absolute length of not less than the length of the trochoidal wave moving with the same velocity." On employing one of these torpedo boats, which are uncommonly efficient with one-third of the length of the trochoidal wave moving with its velocity, to blow the superimposed length condition into nebulosity, we are then confronted with Dr Froude's conclusions "an abnormal form (suggested simply by the appearance of water birds when swimming) if moving with a high, but not excessive velocity experienced considerably less resistance than the wave line form, the accredited representation of the form of least resistance particularly at high speeds." Also: "for symmetrically shaped bodies of 'fair' lines, not excluding by that description certain very blunt ended ovals, when wholly submerged, the entire resistance depends on the conditions of imperfect fluidity, of which surface friction is the only one so considerable that we need take account of it, if we deal with bodies of rational dimensions." Looking at the form figured by Dr Froude would suggest to some minds exceedingly strong doubts as to the value of model experiments; and the statements quoted are samples of a soil suited to the development of "new departures" which anon shall blossom and fructify into "Popoffkas," length and breadth synonymous; and war ships "short and handy" which a

Reed, not shaken by, but controlling the winds, shall *extol* magniloquently; and Old Neptune, most misanthropical of "pike keepers," by his unpublished table of rates, shall *toll* most exorbitantly. Nay, worse than that, cap his misdeeds by Lady "Livadia," peerless amongst the tribe, being treated with frightful contumely.

We hear of the glorious uncertainty of the law, but whenever want of certainty is at a premium, or requires to be illustrated, the laws affecting the elements of steamship propulsion can be confidently referred to as having attained a stage which, contrasted with glorious, may well be termed seraphic. This does not apply to deeper considerations involved in the question, but to simple data lying on the threshold of the subject, which ought to be matters of honest, careful observation. For example, what is the amount and law of the resistance due to the friction of a square foot of a clean painted iron surface, at different velocities?

Eighty-seven years ago the extensive and costly experiments associated with the name of Beaufoy were instituted, and included an answer to the kindred question pertaining to timber vessels, which, if accurate, ought to furnish a close approximation to our question. We may first consider the values deduced from his own experiments by Dr Froude some years ago. In the Transactions of the Institute of Naval Architects for 1874; there is a carefully drawn curve of Dr Froude's deduced values of the fluid friction upon immersed surface of H.M.S. "Greyhound." On measuring the ordinates at various speeds up to 12·64 knots, as carefully as the smallness of the scale permitted, the coppered surface being taken at 7260 square feet as a divisor, yields the following tabular values as the friction on one square foot at the respective speeds noted, as follows:—

Table I.

Speed	400	640	880	1120	1280	feet per minute
or	3·94	6·32	8·69	11·05	12·64	nautical miles
Friction	·130	·300	·502	·763	·951	lbs.
Values by formula $f = \cdot 0127\ V^{1\cdot 7}$.						
Friction	·131	·293	·503	·757	·950	lbs.

From this we see that the above simple formula, in which the speed v is taken in nautical miles, reproduces with great accuracy the curve values; and for 10 knots the calculated value is $f = \cdot 638$ lb.

Taking the Beaufoy experiments in 1798 upon a painted 50 feet plank, at 4 and 8 knots, extended by induction to 10 and 16 knots, the values seem to be :—

Table II.

Speeds,	4 knots.	8 knots.	10 knots.	16 knots.
(1798) Experiments,	·144	·432	·605	?
Formula $f = \cdot 0127\ v^{1\cdot 7}$	·135	·437	·638	1·415
Formula $f = \frac{V(V+3\cdot 5)}{221}$	·136	·416	·611	1·411

In this Table II. the values of f by the 1798 experiments are contrasted with the values derived from the formula $f = \cdot 0127\ v^{1\cdot 7}$, involving a fractional power of the speed; and, again, with the values derived from the formula $f = \frac{V(V+3\cdot5)}{221}$, which is Prony's form of the frictional equation applied to surfaces, with constants derived by me from the Beaufoy experiments, slightly modified by other data, and is noteworthy as the first formula deduced from the consideration of the loss of head due to the internal surface friction on long lines of pipes employed for water distribution. The general form was published by Citizen Prony, in the year I. or II., not the Christian dispensation, however, but its antithesis—the *liberte, egalité, fraternité* affair.

In the Greenland Dock Experiments referred to, when this question was experimented upon, a smooth painted plank, having about 50 sq. ft. of surface, was employed. In 1796, when used thoroughly water-soaked, gave values somewhat greater than in 1798, when the plank was not roughened by long immersion. The highest speeds for which experimental values were obtained was about 8 knots; but extending the curve to 10 knots we have the following table:—

Speeds, . .	4 knots.	8 knots.	10 knots.
1796, . . .	·155	·501	·688
1798, . . .	·144	·432	·605
Foregoing formula values,	·135	·437	·638

These figures justify the deduction as to the apparent law of Dr Froude's curve, and show its fair agreement with these old experiments.

It is somewhat surprising, therefore, on looking into the "Transactions of the Institution of Naval Architects," to find in Dr Froude's paper "On the Elementary Relation between Pitch Slip and Propulsive Effect," at page 47, vol. xix., 1878. The assumption that this quality, for a screw blade, "even when its surface is quite smooth, is as much as 1¼lbs per foot, at 10 knots, and is nearly as the square of the speed, and as each square foot of blade involves two square feet of skin, the resistance of each is over 60lbs." Obviously, in the proposed case of the blade travelling at 50 knots, on both sides, $2\,f = 2 \times 1\frac{1}{4} \times \left(\frac{50}{10}\right)^2 = 62\cdot 5$ lbs., and in another place by an assumed co-efficient ·004, this is reduced to 57·1lbs. Now it seems to me, if the "Beaufoy" and "Greyhound" deductions are of any value, the true figure is very much less, viz.:—

$$2\,f = 2 \times \cdot 01\dot{2}7 \left(\frac{50}{10}\right)^{1\cdot 7} = 19\cdot 7 \text{ lbs. ?}$$

and we may ask upon what grounds the friction is doubled to begin, and then nigh trebled by neglecting the known and obvious fact, that so far as experiment goes, frictional resistance *does not increase* so fast as the square of the speed! I keep in view the fact, in 1858, Dr Rankine published as a deduction from Weisbach's pipe formula, that for a painted plate, at 10 knots speed, the resistance might be taken at 1lb. per square foot, and then assumed to vary as the square of the velocity. I have to submit, however, that the complete and exhaustive experiments on the movement of water in straight pipes, published about one year earlier by Mons. D'Arcy, go to show that the majority of the experiments upon which Wisbeach and other hydraulicians had based their formulas, were of such a nature that they must, necessarily, have given values much in excess of the truth.

To illustrate this matter from another point of view. Recurring to Dr Froude's figure for the "Greyhound," and measuring with as much accuracy as the small scale will permit, it appears that Dr Froude deduced that the frictional resistance upon 7260 square feet at the respective velocities, in feet per minute, was as stated in the following table; whence, taking the product of the resistance and this speed, on dividing by 33,000 we have the power expended in overcoming this resistance, in terms of the Watt conventional horsepower.

Table of "Greyhound" Friction (Dr Froude).

Speed in feet per minute.	Speed in nautical miles.	Resistance in lbs.	Power in Indicated Horses.
1200	11·85	6200	225·5
1040	10·27	4850	152·8
880	8·70	3600	96·0
720	7·11	2550	55·6

Now by this Prony form of the frictional equation, employed in the Clyde since 1850, and published in the Transactions of this Institution (March, 1875), the indicated horse-power required to overcome the frictional resistance on 7260 square feet at the respective speeds noted above is given by the equation:

$$\text{indicated power} = \text{surface}\ \frac{V^2\,(V+3{\cdot}5)}{68{,}000},$$

which, for the given data, employing logarithms for the calculation, yields the values—

Speed in knots,	11·85	10·27	8·70	7·11
Log surface, =	3·8609	3·8609	3·8609	3·8609
Log V^2, =	2·1472	2·0228	1·8790	1·7034
Log $V+3{\cdot}5$, =	1·1861	1·1359	1·0864	1·0257
Subtract—				
Log 68,000, =	4·8388	4·8388	4·8344	4·8388
Log Power, =	2·3504	2·1808	1·9875	1·7512

∴ Power =	224·1	151·6	97·2	56·4	I.H
By Dr Froude =	225·5	152·8	96·0	55·6	,,
Differences,	— 1·4	— 1·2	+ 1·2	+ 0·8	,

These differences are unimportant, and obviously less than the possible and probable range of errors of observation.

I must here remark upon the extraordinary superficial views which Mr Denny has been pleased to express upon my methods of investigation, as if they merely depended upon an after deduction, which, though surprisingly simple and valuable, it is manifestly absurd to suppose it to have no deeper meaning than a blundering and indefinite approximation to a straight line. Let me explain. It is an obvious fact, or at least can be seen with a slight exercise of the reasoning powers, if a vessel is being propelled by steam, the indicator cards of her compound engine being combined, will give the gross indicated power being developed, in one term:

$$E = \frac{d^2 s}{21{,}010} N (P + rp) \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

in which, E is this gross power, N the revolutions of the engine, P and p the mean diagram pressures upon a unit of the high and low pressure pistons respectively; d and s the diameter and stroke of the high-pressure piston, and r the ratios of areas of the two pistons. Now, in the second member of this equation I have shown that in terms of V, the speed of the vessel and constant co-efficients, we have,

$$(2) \qquad N = mV \operatorname{Log}^{-1} nV;$$

also,

$$(3) \qquad P + rp = f \operatorname{Log}^{-1} (a - n) V;$$

whence, it follows, by substituting these in (1), we have,

$$(4) \qquad E = bV \operatorname{Log}^{-1} aV;$$

and also deduce the explicit value of f, which mechanicians distinguish as Morin's constant, as,

$$(5) \qquad f = \frac{21{,}010}{d^2 s} \frac{E}{N} \frac{1}{\operatorname{Log}^{-1} (a - n) V}.$$

The perfect agreement of these formulæ with the facts of experience has been illustrated so often that it is almost a waste of time to adduce further examples. I will note, for a few vesssels, the values of a and b which enter equation (4), which is the approximate true form of the "Admiralty" equation, the ordinary well known forms being founded on a false law of the resistance, are entirely misleading and erroneous.

Name of Vessel.	Value a.	Value b.
H.M.S. "Shah," .	·0792	22·63
H.M.S. "Iris," .	·0750	16·30
"Merkara," .	·0735	16·90
"Charles V." .	·0842	7·30
"Dunrobin Castle,"	·103	7·94
"Warsaw," .	·111	2·97
H.M.S. "Heroine,"	·085	8·57
H.M.S. "Firebrand,"	·138	1·78

According to this, the relation between the power and speed, say, for example, in the small vessel "Warsaw," is,

$$E = 2\ 97\ V \operatorname{Log}^{-1} \cdot 111\ V;$$

hence, for the following speeds,

Trial Speeds	= 4·73	7·54	9·53	10·90 knots.
Product by ·111	= ·5250	·8369	1·0578	1·2099
Log V	= ·5449	·8774	·9791	1·0374
Log 2·97	= ·4728	·4720	·4728	·4728
Log E	=1·6727	2·1871	2·5097	2·7201
∴ E	= 47·1	154·0	323·4	525
By Trial,	47	153	324	525

The agreement of these figures is closer than we have any right to expect, considering the many sources of uncertainty which affect the data by which the relation of power and speed is determined in progressive speed trials. I shall now illustrate the calculation of the value of "Morin's" constant by formula (5), taking, for example, H.M.S. "Heroine," which has been referred to by Mr Denny, I presume, as an example of the absurdity of my method of treating the question. The data for this is given in the last issue, "Results of trials made in Her Majesty's screw ships and vessels," as follows:—

"Heroine," 7th September, 1882.

Trial Speeds, V.	Revolutions, N.	Power, E.
(1) 13·12	113·2	1466
(2) 12·43	108·1	1243
(3) 11·47	97·1	922
(4) 9·16	76·2	471

The remaining necessary data are, d = 36; s = 2·5; whence, $\frac{d^2s}{21{,}010} = -1{\cdot}1881$; and I calculate the quantity a — m = ·081.

Hence,

		(1)	(2)	(3)	(4)	True Speed for (2) ?
Trial Speeds	=	13·12	12·43	11·47	9·16	12·51
Multiplied by ·081	=	1·0627	1·0068	·9220	·7420	1·0133
Add, Log N	=	2·0538	2·0338	1·9870	1·8820	2·0338
Add, Log $\frac{d^2s}{21{,}010}$	=	– 1·1881	– 1·1881	– 1·1881	– 1·1881	– 1·1881
Sum	=	2·3046	2·2287	2·1044	1·8121	2·2352
Subtract from Log E,	=	3·1661	3·0945	2·9647	2·6730	3·0945
Leaves value Log f	=	·8615	·8658	·8603	·8609	·8593
i.e., Morin's constant f	=	7·27	7·34	7·25	7·26	7·27

In the foregoing each trial gives its own value and testimony to the truth of the mechanical principles involved in the equation, and the very exception shown by the slight excess of value in (2) is only an indication that the reported speed 12·43 for that trial is slightly erroneous, and that the data belong to the speed, 12·51! Thus, in the last column, taking this as the speed, we get f=7·27; same as

(1), while for (3) and (4) it comes out very slightly less, which is quite in accordance with Morin's deduction regarding this quantity, and borne out in the actual trials of the machinery of the "Warrior" and "Black Prince." In confirmation of the speed of (2) being slightly underestimated, let us take the power and revolution formulas,

$$E = 8{\cdot}56\, V \operatorname{Log}^{-1} {\cdot}085\, V;$$
$$N = 7{\cdot}65\, V \operatorname{Log}^{-1} {\cdot}004\, V.$$

taking the speed 12·51, the calculation is as follows:—

H.M.S. "Heroine."—Power Calculation.

	(1)	(2)	(3)	(4)
Trial Speeds =	13·12	12·51	11·47	9·16
V × ·085 =	1·1152	1·0634	·9750	·7786
Log V =	1·1179	1·0973	1·0596	·9619
Log 8·56 =	·9328	·9328	·9328	9328
Sum Log E =	3·1659	3·0935	2·9674	2·6733
∴ E =	1465	1241	927·7	471
By trial =	1466	1243	922	471

Revolution Calculation.

	(1)	(2)	(3)	(4)
Trial Speeds =	13·12	12·51	11·47	9·16
V × ·004 =	·0525	·0500	·0459	·0366
Log V =	1·1179	1·0973	1·0596	·9619
Log 7·65 =	·8835	·8835	·8835	·8835
Sum Log N =	2·0539	2·0308	1·9890	1·8820
∴ N =	113·2	107·4	97·5	76·2
By Trial =	113·2	108·1	97·1	76·2

Let me further illustrate the application of this formula (5) to the calculation of Morin's constant for the "Merkara" and H.M.S. "Shah," these being amongst the first vessels to which my methods were fully applied, and the first, the vessel where Dr Froude's method and mine came into collision. For the "Merkara" the trial data furnished by Mr Denny is published at the end of my "Letter of Reclamation," where it will be seen,

$$a - n = \cdot 0756; \text{ also, Log } \frac{d^2 s}{21{,}010} = -1\cdot 6238.$$

"Merkara."

	(1)	(2)	(3)	(4)	Suppose (3)
Trial speeds =	12·91	11·09	9·20	6·20	9·16
V × ·0756 =	·9760	·8384	·6955	·4687	·6925
Log N =	1·8007	1·7356	1·6572	1·4942	1·6512
Log $\frac{d^2 s}{21{,}010}$ =	− 1·6238	− 1·6238	− 1·6238	− 1·6238	− 1·6238
Sum =	2·4005	2·1978	1·9705	1·5867	1·9675
Subtract from Log E =	3·2896	3 6881	2·8561	2·4757	2·8561
Difference, Log f=	·8891	·8903	·8856	·8890	·8886
∴ Morin's const. f=	7·745	7·77	7·685	7·74	7·74

Here, again, the slight variation of value in (3) may be seen to be due to the speed of (3) being slightly overstated: instead of 9·20 knots it ought to be 9·16, and then all give practically the same value.

Dr Froude, making use of the same equation I published in the spring of 1875, from (3) and (4) deduced the value f=10·04, which is simply the result of an assumed false law of the resistance, masking the mechanical principle involved in Morin's constant.

Again, for H.M. "Shah" we have,

$$a - n = \cdot 0763; \text{ also, } \operatorname{Log} \frac{d^2 s}{21{,}010} = 1 \cdot 4161.$$

H.M.S. "Shah."

	(1)	(2)	(3)	(4)
Trial speeds V =	16·45	12·13	8·01	5·32
$V \times \cdot 0763$, =	1·2551	·9255	·6112	·4058
Log N =	1·8152	1·6573	1·4729	1·3034
$\operatorname{Log} \frac{d^2 s}{21{,}010}$ =	·4161	·4161	·4161	·4161
Sum =	3·4864	2·9989	2·5002	2·1253
Log E =	3·8737	3·3930	2·8876	2·5024
Difference, Log f =	3873	·3941	·3874	·3771
∴ Morin's constant f =	2·440	2·478	2·440	2·383

These results are in fair agreement with my formulas, and it is thus shown how it is possible to calculate this quantity f by a definite mathematical process; but however carefully gone about, the observation of the quantities entering these equations, by their very nature involve causes of variation. For example, tidal and wind drift, residual errors of which are not properly eliminated by the usual methods of observation, and hence it is not at all surprising to find, that instead of ranging in straight lines, as when treated by the proper methods they ought to do, we find aberrations, such as those which seem to have perplexed Mr Denny; and in reference to the cases which Mr Denny refers in illustration, it can only be said that they display an evident tendency to rectitude, *so far as the infirmi-*

ties of human nature permit them. However, I am perfectly aware of, and have published explanations on some curious phenomena connected with discontinuity and changes of angularity in the lines in which the observation values range, which await fuller investigation. Probably the most complete series of observations, upon the same vessel, ever published, were those conducted upon H.M.S. "Iris." The following table contains my deductions of the various coefficients which enter my formulas, and the contrasted, calculated, and observation values of the power, revolution, and piston pressures for the different trials :—

Power.

$$E_{\text{I.}} = 19{\cdot}30\ V \log^{-1} {\cdot}0830\ V$$
$$E_{\text{II.}} = 21{\cdot}00\ V \log^{-1} {\cdot}0713\ V$$
$$E_{\text{III.}} = 19{\cdot}40\ V \log^{-1} {\cdot}0725\ V$$
$$E_{\text{IV}} = 17{\cdot}60\ V \log^{-1} {\cdot}0733\ V$$

Revolutions.

$$N_{\text{I.}} = 5{\cdot}08\ V \log^{-1} {\cdot}0020\ V$$
$$N_{\text{II.}} = 5{\cdot}72\ V \log^{-1} - {\cdot}0006\ V$$
$$N_{\text{III.}} = 4{\cdot}545\ V \log^{-1} {\cdot}0033\ V$$
$$N_{\text{IV.}} = 4{\cdot}510\ V \log^{-1} {\cdot}0022\ V$$

Piston Pressures.

$$(P + rp)_{\text{I.}} = 3{\cdot}957 \log^{-1} {\cdot}081\ V$$
$$(P + rp)_{\text{II.}} = 3{\cdot}827 \log^{-1} {\cdot}0719\ V$$
$$(P + rp)_{\text{III.}} = 4{\cdot}445 \log^{-1} {\cdot}0692\ V$$
$$(P + rp)_{\text{IV.}} = 4{\cdot}055 \log^{-1} {\cdot}0711\ V$$

Trial Speeds.	Powers.		Revolutions.		Pressures.	
V	Formula.	Trial.	Formula	Trial.	Formula.	Trial.
16·58	7608	7503	90·93	91·04	87·18	85·86
15·12	5251	5251	82·37	82·15	66·40	66·57
12·06	2334	2560 ?	64·77	65·11 ?	37·52	40·94 ?
8·19	756	755	43·21	43·36	18·23	18·14
15·73	4368	4368	87·96	88·89	51·75	51·18
14·52	3306	3306	81·34	81·18	42·35	42·41
11·58	1628	1637	65·12	65·07	26·03	26·21
7·95	616	571 ?	44·94	45·05	14·27	13·19 ?
18·57	7996	7714	97·20	97·19	85·68	82·68
16·56	5100	5108	85·39	85·39	62·22	62.31
12·28	1850	1833	61·28	61·34	31·46	31·13
7·98	587	606 ?	38·54	40·96 ?	15·86	15·41
18·59	7543	7556	92·13	93·25	85·27	84·40
15·75	3957	3958	76·95	76·93	53·56	53·59
12·48	1805	1765 ?	59·95	59·39 ?	31·36	30·95 ?
8·32	596	596	39·15	39·15	15·87	15·86

Mr JAMES HAMILTON, jun., said he would confine the few remarks he was about to make to subjects of general interest, as distinguished from the personal element in the matter before them; for there were many important points connected with this paper which to him were of very much more interest than the conversations which took place in 1876, and which they had heard so much about. He thought it was a very good thing that Dr Froude's letter had been published in the Transactions. That letter would be the best defence that could be made for Dr Froude. As regards Mr Mansel's plotting down of curves, he had also plotted down a good many at different times, and he had found a good many straight and a good many crooked. He held in his hand a few, and it would be seen that one or two of them were very straight; and there was one of them to which Mr Mansel had referred—that of the "Livadia," which took jumps. It required two parallel lines to represent it.

Mr MANSEL said he had brought that before the Institution, and showed it some time ago.

Mr HAMILTON replied that it might not be amiss to draw attention to it again. He thought that this showed that if the lines were all as correct as that they might be better to call them curves. With reference to the initial friction, when those parallel lines were continued to the origin, they found that about one-third of the power was absorbed by skin friction. There was another ship also, the "Mendoza," that gave perfectly straight lines through all the five different points. The initial friction came to nearly one-third of the total indicated horse-power developed on trial, while on another occasion it came to about two-thirds of the indicated horse-power, leaving only one-third of the maximum power got out of the engines available for the propulsion of the vessel; so that he thought there must be some better way of accounting for the loss.

Mr W. DENNY asked whether it was force or power ordinates he had made use of?

Mr HAMILTON replied that he ought to have said that in the cases of the "Livadia" and the "Mendoza" that it was two-thirds and one-third respectively of the length of the ordinate representing the gross indicated horse-power. Mr Mansel had referred to the "Iris" trials, and had mentioned that very capital experiments were made with the propellers. Since these experiments were made the "Phæton" had been tried, with the result of attaining much greater speed with the same horse-power, so that doubtless it was all owing to the difference of propellers. Now he ventured to think that if such differences in propellers occurred those differences would not be uniform throughout the curve, and that the propeller would be more efficient at one part of the curve than the other. He had not plotted down the "Phæton's" results, but all the information he had led him to believe that the results of the trials of the "Iris" and the "Phæton" would show straight lines. Then it was known that the position of the wave produced by the vessel made a vast difference in the resistance of the ship, and it was scarcely reasonable to think that those lines would be straight lines. As far as he could see they were straight lines when the vessels were of good form, and when requiring the ordinary indicated horse-power to propel them; but when the ship was hard-pressed a much greater amount of power was absorbed outside of the useful effect produced with the consequences, that instead of straight lines they were crooked. He would like to refer to the diagram of the "Kangra." Mr Denny, in seeking to refute Mr Mansel's arguments, seemed to say—or at least he gave to him the impression—that the resistance was greatly made up of skin friction, as a general rule. He did not say what the speed was, and he would like to ask Mr Denny what the dimensions of the ship were, and the maximum power and speed got out of her, for at 14 knots, as given on the curve of the diagram, with the force there shown, the resistance appeared to be just about a half of what he was led to expect it to be, so that he must conclude that skin friction had very little to do with it.

Mr Denny replied that the "Kangra" was a vessel of 285 feet in length, and by no means a sharp ship. On trial she attained a speed of not more than 12 knots, so that 14 knots were far beyond her capability of being driven with the power on board.

Mr Hamilton said that this information demolished his argument altogether. He should like to refer to the question of wave formation. To his mind it gave an indirect but a very complete confirmation of Dr Froude's law of the corresponding speed. Mr Mansel had said that the idea belonged to Mr Rèech; but be that as it may, the confirmation of the law was very satisfactory and complete. A study of the diagrams before referred to would show that at different speeds there were different curves of wave formation, but it must be remembered that the one was from the model and the other from the actual trial of the vessel; and although they differed slightly, the correspondence was remarkably close. Now was that correspondence not a confirmation of Dr Froude's law?

On the motion of Mr Henry Dyer, the further discussion of this paper was adjourned to next General Meeting of the Institution.

The discussion of this paper was resumed on 24th February, 1885.

Mr Henry Dyer said—

In resuming the discussion on Mr Denny's paper, it may be useful to the members if I explain in as brief a manner as possible, the basis of Mr Mansel's formulæ, so that they may see how much they depend on general principles and how much on hypothesis.

In treatises on engineering, there are generally three classes of formulæ to be found. In the first class, an attempt is made to take into account all or nearly all the conditions of the problem, and thus expressions are obtained which are very complicated, and the application of which is very tedious; in the second class, certain assumptions are made in order that we may simplify the resulting expressions, which are, however, exact enough for all practical purposes, within a considerable range of conditions, while, in the third class, by confining the applications to a limited range of conditions, we obtain the so-called empirical formulæ derived from experience. These three classes of formulæ are well illustrated in the expressions for the efficiency of the steam in engines. If we attempt to take into account all the circumstances which affect that efficiency, we obtain expressions which fill several lines in an ordinary text book;

if we make certain assumptions which experience proves lead to results which are not very far from exact we get expressions of manageable length; and lastly, if we assume data derived from ordinary practice, simple expressions are obtained which are approximately correct within the limits of that practice.

The question for us to consider is to which of those classes do Mr Mansel's formulæ belong. When Mr Mansel first published his expression connecting the Indicated Power and the speed of a vessel, he obtained it by correcting the Admiralty formula, and in his paper* on "Propositions on the Motion of Steam Vessels," read before this Institution he says, "it is not founded on any theory, but merely expresses the result of experiment on well designed vessels according to the practice of our best constructors," although in a later paper he shows that by making certain assumptions it may be derived from the principles of dynamics; and he claims for it a much wider range of application than some of his critics are disposed to admit. His method of deducing the formulæ from theoretical considerations is somewhat abstract and difficult to follow by those who have not had a special training, so that I am afraid few have taken the trouble to find out the basis on which his expressions rest. For a long time Mr Mansel has been kind enough to send me his papers, and some years ago I wrote a note to him from Japan suggesting how his formulæ might be obtained in what I considered a comparatively simple manner, and which showed distinctly the assumptions he made, and how they affected the general application of his expressions. What I propose to say to-night is in great part a resumé of what I then wrote.

For convenience of comparison, I will use Mr Mansel's notation throughout.

A steam vessel is a machine in which the energy exerted by the steam is spent in overcoming resistances, that is to say, in doing work, and it is subject to the same laws as other machines. Now.

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* Transactions of the Institution of Engineers and Shipbuilders in Scotland, Vol. XIX.

we know that the *rate of doing work*, or the quantity of work performed in a given interval of time by a machine is

$$E_1 = RV, \tag{1}$$

in which R is the resistance and V the velocity with which it is overcome. When E is expressed in the special unit of power usually adopted in engines, viz., 33,000 foot pounds per minute, then it is what is called the Horse Power of the engine.

When we apply the expression to a steamship, and measure the resistance by the pull of a towing rope, when the ship is drawn along by an external force which does not interfere with the free flow of water past her hull, and if we denote by V the velocity corresponding to that resistance, then E gives what is usually called the Effective Horse Power of the engine.

On the other hand, the Indicated Horse Power is

$$E_2 = PV, \tag{2}$$

where P is the mean total effective pressure on the pistons, and V the mean piston speed.

In the ordinary calculations relating to the propulsion of steamships, we want an expression which connects the Indicated Horse Power, which can be calculated from Indicator Diagrams, and the speed of the ship which can be ascertained from observations, so that if we multiply equation (1) by a factor involving the efficiencies of the mechanism of the engines, and of the propeller, we may then use it for expressing the Indicated Horse Power. This fact should be carefully noted so that we may see what is included in the constants of Mr Mansel's formulæ.

Differentiating equation (1) we have

$$\begin{aligned} \frac{dE}{dV} &= R + V\frac{dR}{dV} \\ &= \frac{E}{V} + V\frac{dR}{dV} \\ &= \frac{E}{V}\left(1 + \frac{1}{R}\frac{dR}{dV}V\right) \end{aligned}$$

or if we write

$$\alpha = \frac{\frac{dR}{dV}}{R}$$

that is,

$$\frac{dR}{R} = \alpha\, dV$$

we have

$$\frac{dE}{E} = \frac{dV}{V} + \alpha\, dV.$$

Integrating on the supposition that α is constant, we have

$$\mathrm{Log}_e\, E = \mathrm{Log}_e\, V + \alpha V + \mathrm{const.};$$

or, putting the constant equal to Log b, and using common logarithms,

$$\mathrm{Log}\, E = \mathrm{Log}\, V + \alpha V + \mathrm{Log}\, b \qquad (3)$$

or

$$\mathrm{Log}\, \frac{E}{V} = \alpha V + \mathrm{Log}\, b, \qquad (3a)$$

which is evidently the equation of a straight line

Equation (3a) may be written in the form

$$\frac{E}{V} = b\, \mathrm{Log}^{-1}\, \alpha V, \qquad (3b)$$

or

$$E = bV\, \mathrm{Log}^{-1}\, \alpha V, \qquad (3c)$$

the factor $\mathrm{Log}^{-1}\, \alpha V$ denoting the number whose common logarithm is the product of α into the speed V, so that (3c) is the equation of a curve.

Assuming

$$b = \frac{(L\sqrt{M})^{\frac{7}{8}}}{C}$$

where

L = length of the ship,
M = the immersed midship section,
C = a constant,

and substituting in equation (3), Mr Mansel obtains

$$\mathrm{Log}\, E = \tfrac{7}{8}\, \mathrm{Log}\, (L\sqrt{M}) + \mathrm{Log}\, V + \alpha V - \mathrm{Log}\, C, \qquad (3d)$$

or

$$\frac{E}{V} = \frac{(L\sqrt{M})^{\frac{7}{8}}}{C}\, \mathrm{Log}^{-1}\, \alpha dV. \qquad (3c)$$

Let us now consider the assumptions which have been made. In the first place it has been assumed that

$$\frac{dR}{R} = \alpha V,$$

that is to say, in a vessel receiving a small increase of speed, the ratio of the increase of the resistance thus produced to the resistance, is equal to a small fraction of the increase of speed α being found from experience to be a small quantity varying in different vessels from $\frac{1}{16}$ to $\frac{1}{6}$, but constant so long as the conditions are the same.

The second assumption is the expression for the value b, which is simply what Mr Mansel considers a corrected expression for the factor $\frac{M}{C}$ in the Admiralty formulæ.

How far these two assumptions are allowable must be determined from experience. Mr Mansel has shown by numerous examples that they hold within considerable limits. To use his own words on this subject, "the statement that the law of the power is expressed by equation (3d) must be understood in the sense that it expresses a hypothesis which is found to agree with the facts of experience when another implied condition is allowed for, namely, the special values of the quantities α and b which enter that equation are not the same for all speeds, they may remain constant through a great range of speeds, and then suddenly and simultaneously take other constant values through a wider range so that we may have two or three sets of values for the same vessel." Mr Mansel gives this as the result of his experience, and I must say I am not prepared in the meantime to explain why these sudden changes should take place. I would ask you to observe, however, that as E in Mr Mansel's formula denotes the Indicated Horse Power that the constants are affected by the efficiencies both of the mechanism of the engines and of the propeller, so that it is possible to imagine two similar ships with different kinds of engines and propellers, having considerably different constants, as a variation in any one of those elements will produce a corresponding variation in the constants, and on the other hand, we might have two dissimilar ships with nearly the same constants, if the efficiencies of the engines and propellers were different.

I need scarcely explain that the word *constant* is here used not as signifying a quantity which is absolutely constant, but simply one which is constant under given conditions.

Mr Denny, on the other hand, asserts that Mr Mansel's straight lines are the exceptions and not the rule, and are merely haphazard coincidences. The discussion of how far Mr Mansel's formulæ give results, agreeing with experience, I should like to leave to those members who are actually engaged in shipbuilding.

I think it is probable that the majority will agree with the opinion expressed by Mr Hamilton at the last meeting when he said that "as far as he could see the lines were straight when the vessels were of good form, and when requiring the ordinary indicated horse power to propel them; but when the ship was hard-pressed a much greater amount of power was absorbed outside of the useful effect produced, with the consequences that instead of straight lines they were crooked," and I do not think, at least, so far as I understand him, that Mr Mansel claims more for his formulæ.

Mr Hamilton further remarked, "that it was known that the position of the wave produced by the vessel made a vast difference in the resistance of the ship, and it was scarcely reasonable to think that those lines would be straight lines." That may be admitted, but still, as I have already pointed out, this increase of resistance *might* so affect the efficiencies of the engines and of the propellers as to correct in great measure the deviation from straightness of the line of indicated horse power. However, this is a point which can only be settled by further experiment.

With regard to the *shape* of the formulæ, I would remark that the use of logarithms leads to the easy construction of a straight line, but as Mr Froude has noticed in his letter to Mr Mansel, a given alteration of the pressure ordinates, produces a less and less alteration in the corresponding logarithmic ordinates, so that although the logarithmic form may be more convenient for purposes of practical calculation, an expression not involving the use of logarithms might be preferable for purposes of investigation.

The only other point in the discussion, between Mr Mansel and Mr Denny, which I will touch, is the method of obtaining the initial friction of the engines, but which I prefer to call the pressure necessary to work the engines unloaded.

The Indicated Horse Power of an engine is expressed in terms of the mean effective pressure of the steam, the length of the stroke, the diameter of the piston, and the number of revolutions per minute by the formula.

$$\text{I.H.P.} = \frac{\text{P } 2n \frac{\pi}{4} d^2 s}{33{,}000} = \frac{\text{P } n d^2 s}{21{,}010} \qquad (4)$$

for a compound engine with two cylinders, Mr Mansel writes this in the form

$$\text{I.H.P.} = \frac{n\, d^2 s}{21{,}010} (\text{P} + rp) \qquad (4\text{a})$$

where P and p are the respective mean pressures on the small and large pistons, and r the ratio of the greater to that of the smaller piston. As the quantities in the factor $\frac{n\, d^2 s}{21{,}010}$ remain constant for a given speed we may write

$$\text{I.H.P.} = \text{C } (\text{P} + rp),$$

if we denote by m the pressure necessary to overcome the friction of the engines, we have the power delivered to the crank shaft

$$= \text{C } (\text{P} + rp - m),$$

and this is equal to the effective horse power, multiplied by a factor of the form $\frac{n\text{ H}}{\text{V}}$ where H is the pitch of the screw in feet, and n the number of revolutions per minute, from which we deduce Mr Mansel's expression,

$$\text{V}^2 = \text{C } (\text{P} + rp - m),$$

which, however, he derives in a similar manner to the equation to the power, that is by correcting the Admiralty formula. He calculates the numerical values m from two experiments at moderate speeds, by eliminating C between the two values of the equation thus obtained. In doing this he makes two assumptions; in the first place, that C is absolutely constant, and also that m remains the same not only for the two trial velocities, but also down to zero velocity, two assumptions which may give approximations to the truth, but which cannot be regarded as exact.

On the other hand, I agree with Mr Denny when he says he does not think it possible to arrive at an exact quantitative measure of the initial friction by means of Mr Froude's method, as I am of opinion that the only data of any value of this subject are those obtained from actual experiments with steam engines.

It will be observed that Mr Mansel's expressions belong to the second class of formulæ which I mentioned at the beginning of my remarks, as they do not take into account all the varying conditions of the problem, but are simplified by certain hypotheses which Mr Mansel considers justified by experience. To-night, I have confined myself to showing the basis on which they rest, and the assumptions made with a view to simplifying them, so that these latter may be discussed by members who have had opportunities of applying the formulæ in practice, and I have not entered into the general question of steamship propulsion, which would require a special paper for itself.

For the further successful prosecution of the various subjects, raised by Messrs Mansel and Denny, we want many more experiments, not only with models, but also with actual ships and engines, and I trust that those members of the Institution who have opportunities of making them will do so, and publish some of the results in the Transactions, in order that those of a more mathematical turn of mind may find out either how Mr Mansel's formulæ may be improved, or better ones put in their place.

Mr George Thomson said—

The remarks made by Mr Dyer are possibly pretty scientific, but, Mr Mansel in opening the discussion of Mr Denny's paper has, I think, brought into play his well-known abilities. We must all admire a gentleman like Mr Mansel, who has devoted so much of his time, so assiduously, during the last 35 years, to the study of steamship propulsion.

The figured data he has put before us, explanatory of his investigations, is of great practical value, and convey more to my mind than the zig-zag lines Mr Denny has shown up to us, as the exponents of the principles Mr Mansel advances.

Model experiments may be very good, only, we are distinctly told that these model experiments are supplemented by the results of trial data from known ships. Theoretically then, Mr Denny makes up a ship's horse power, plus some intangible quantities, but like every practical and sensible shipbuilder, retreats to practical deductions from known facts, and draws his inference therefrom.

Mr Mansel's object has been to simplify, as far as I can see, our mode of procedure, by bringing all the results to one constant, instead of having so many conflicting co-efficients, and there is no disputing that this must be of great advantage.

In the last issue we have the results of the trial of H.M.S. "Iris," and applying Mr Mansel's formula in the 4th series of trials we have 7543 I.H.P., for a speed of 18·59 knots; supposing we wanted that speed with the 1st set of propellers, we would have to expend 12,525 I.H.P. or 66 per cent. more power for the same speed; this fact agrees very closely with results I plotted on the same vessel some months ago, and my conclusion is, that we have a prodigious hump to get over here, in which the bow spray and minikin waves sink into insignificance in Mr Denny's illustrations.

Hollow wave formation in the vicinity of paddle wheels and the waste of power caused from that source is a tradition of long standing; it was pointed out to me by draughtsmen who have studied marine painting many years ago. It will be my endeavour to get the results of resistance reduced to straight lines or fair curves, rather than pursue a policy of humps and hollows. We have got torpedo boat humps in the craft referred to in Mr Denny's paper, at

19 knots	365 I.H.P.
19·5 „	402 „
20·0 „	425 „

We get here from 19 to 19·5 knots at the expense of 37 H.P., and the next half knot so cheap as 23 H.P. Now, I believe, the observations for this craft were taken as near as was possible, but practical people will put the 37 horse power to the opposite side, and we are not yet so credulous as to accept without a doubt, that a boat could spin for us exactly 17, 17·5, 19, 19·5, and 20 knots, as the data shows. Those who have studied the diagrams in connection with this boat must admit not only the difficulties of taking correct cards at so high velocities of the piston, but also the true mean pressure from them. Mr Denny's progressive methods of trial have thrown a flood of light on erroneous theories, but in actual practice I think it best for every man to use his own ways, although if time allowed to try the modes of others for comparison; these must, however, be like Mr Mansel's descriptive ship, "short and handy." I have often looked at the S.S. "Merkara's" power and speed curve compared with a simple curve of logarithms, but it is the only ship's curve I have seen follow this law so closely, and have merely brought this matter up to make people cautious in accepting formulæ unless thoroughly investigated, this, of course, just being a partial coincidence. Making the base of the logarithm 100 = 0 speed we have

I.H.P.
399 = 6 knots.
818 = 9·12 „
1325 = 11·22 „
2048 = 13·11 „

then deducting 100, which I made the base, from these results, we have the horse powers of the "Merkara" with a wonderfully good approximation to her trial speeds, and it is clearly seen that if I put off abcissæ representing these speeds in logarithms and this horse power plus 100, I have a straight line, in fact for a logarithm pure and simple, the angle of 45 degrees.

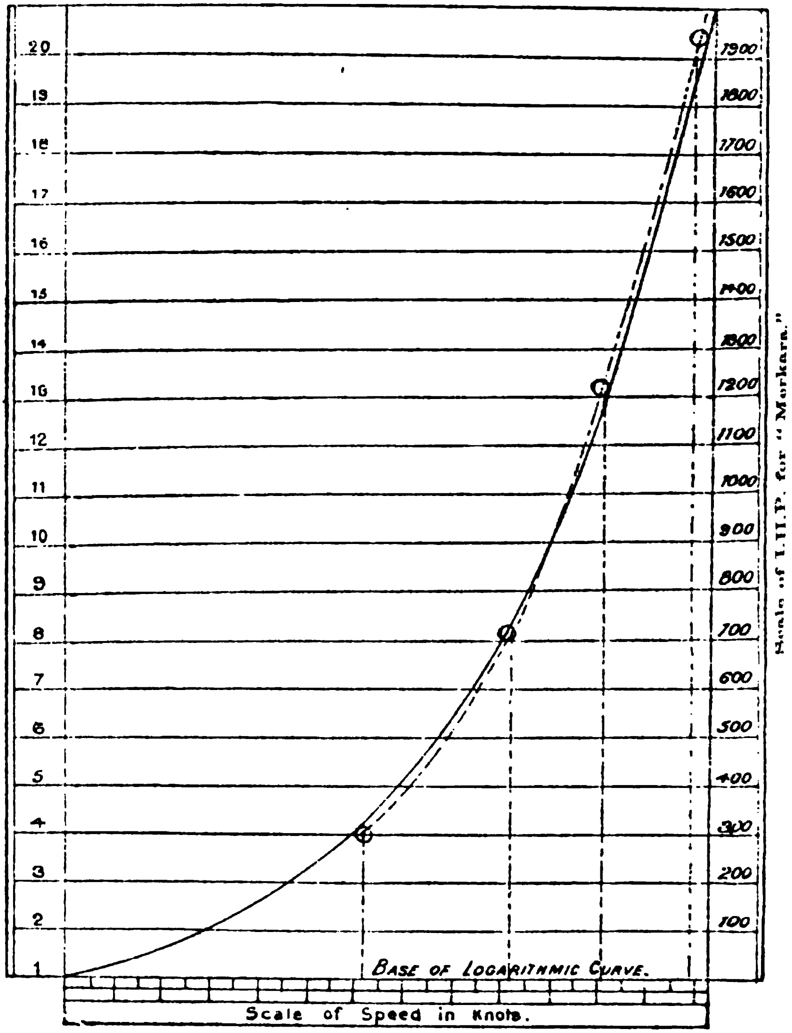
I quite agree with Mr Denny that there is no defined limit to the speed of any vessel if force enough is put into her to gain it, but the speed wanted may be got at great cost. An engine and propeller have, however, as is too well-known, a limit in their capabilities, above the maximum capability, no more speed is got. There is also another point to keep in view, and that is the minimum ability of the engine to keep up constant momentum of the propeller without making intervals. This will give a speed below which the ship cannot be driven with the engine in steady motion, I would call this initial speed, and these first impulses have a great deal to do with after effect.

Until a thorough analysis is made on screw propellers, and their actual values as propellers elucidated, it appears futile to deal with anything but real data deduced from actual trial with known qualities of the ship and propeller results.

Supposing I ask a scientific gentleman what horse power it will take to drive a vessel 16 knots an hour, giving him particulars of displacement, &c., but at the same time I bolt on a couple of stout flat plates for blades on the boss for a propeller; getting round on trial trip as if disappointed with the result of speed, I ask my friend what he was thinking about in leading me to understand the speed would be no less with so much horse power.

As soon as Mr Mansel and Mr Denny are prepared to solve the propeller difficulty, it will be time enough for us to adopt their ways and means of saying a certain horse power will make a ship go so many knots per hour.

Curve of Common Logarithms showing Close Approximation of "Merkara's" Speed and Power. (Referred to by Mr G. Thomson.)



Professor **Elgar** said that he did not mean to refer to the personal questions which were involved in the subject of the paper that had been read, except to say that personal questions were perhaps not always without advantage to the interests of a scientific institution. They were sometimes the means of attracting attention towards points which might otherwise be regarded with comparative indifference; and they sometimes gave to technical discussions an interest and thoroughness which they would not otherwise possess. In this case the personal discussion, which had been raised by Mr Mansel, had had the effect of eliciting from Mr Denny some extremely interesting information, relating to model experiments and progressive speed trials. A large quantity of useful data had thus been furnished, which would make the volume of Transactions of the Institution for the present year exceedingly valuable to naval architects. They had also obtained, from Mr Mansel, a great many instructive examples of the application of his method of estimating the speed of ships. Mr Dyer had that evening given to the meeting a clear and concise explanation of the principles upon which Mr Mansel's methods of calculation are based. He had not been quite clear, before, whether Mr Mansel put his system forward as an empirical mode of describing facts and predicting results; or whether he claimed for it mathematical accuracy. He gathered, from what Mr Dyer had just said, that what is claimed for Mr Mansel's method is, that it is mathematically exact, provided certain conditions are fulfilled; but that those conditions are themselves subject to variation according to circumstances. The precise circumstances which cause such variation, and the possible limits of variation, can only be decided by experimental trials. That being so, some of the irregularities in the curves produced by Mr Denny to show that Mr Mansel's method does not always give straight lines, may very well be accounted for. At the same time he would not be surprised if, as Mr Mansel pointed out in the discussion, some of the departures from straightness arose from errors of observation in recording the results of the speed trials, and from the conditions not being absolutely perfec One of the most astonishing results of the discussion upon Mr Denny's paper, to him, is that Mr Mansel should have contested, in the terms he did, the propriety or fairness of calling the law of comparison, which is so intimately associated with Mr Froude's name, Froude's law. Mr Mansel spoke of M. Rèech's labours in the same direction, and of the British Association Report which referred to them; and he asserted that M. Rèech first discovered and established the law of comparison. He (Professor Elgar) was aware that M. Rèech many years ago deduced this law by strict mathematical reasoning from a theorem of Newton's. Rèech's investigation was published in one of his scientific works, but no one appears to have considered it of any practical importance, or of more than abstract interest. No use was made of it, even in France, so far as he was aware, and it was apparently never submitted to experiment. Certainly, no one in this country knew anything of, or believed in the existence of, such a general law. Professor Macquorn Rankine—who knew as much of the science of the subject, and of what had been proved with reference to the laws of resistance, as any one—said at the Institution of Naval Architects in 1864—"As for mis-shapen and ill-proportioned vessels, there does not exist any theory capable of giving their resistance by previous computation." Professor Rankine could not have said this had the law of comparison been completely established and proved—as it was later on by Mr Froude—to be applicable to the purpose. That was in 1864, and the report of the British Association Committee in 1869, upon which were Professor Rankine, Mr C. W. Merrifield, and Mr Froude, states—"There exists no generally recognised theory or rule for calculating the resistance of a ship." Where was the law of comparison which we are now told had long before been clearly demonstrated and made available by Rèech? Rèech may have described it as a theoretical deduction from a dynamical theorem, and may have expressed it in that form; but the law was not proved, nor supposed to furnish a practical means by which the resistance of ships might be determined, from the observed resistance of models. It certainly was believed in by no one. In the year 1869, Mr Froude first publicly propounded the general law of comparison, which bears his name, in its broad and general form, in the Appendix to the British Association Report. He (Professor Elgar) can well remember Mr Merrifield reading a paper before the Institution of Naval Architects in 1870, in which it was intimated that Mr Froude was about to experiment on models, for the information of the Admiralty, in order to ascertain what results might be obtained by applying the law of comparison. Few thought that this would do more than give a little innocent amusement to those who were interested in such matters. Mr Merrifield said—"The present theory is that the velocities should vary as the square roots of the lineal dimensions when the resistance of vessels of different sizes is to be compared; there is reason to believe that the comparison under these conditions very fairly represents the facts, but we are at present very far from knowing how much this law approaches to the truth, or what are its limitations." Now, these remarks fairly represent the state of knowledge in this country upon the subject, when Mr Froude was yet in the early stages of his model experiments. Mr Scott Russell, who had probably done more than any one else in that direction, remarked that "he must prepare the meeting not to expect reliable results from experiments with small models. He had once made a series of experiments with 120 small models, extending in one instance from 24 inches to 12 feet in length, and in another from 30 feet to 60 feet in length, and the most interesting fact he ascertained was that the results upon a large scale were precisely contrary to the results on a small scale." How can it now be said with justice that Mr Froude's work had been done years before by Rèech, and that the truth of the law of comparison had been established. Now, he did not think it right, nor fair to the memory of Mr Froude, considering that the state of knowledge of the subject, when he took up this difficult and complicated problem, was as thus described, to say that he borrowed a law already proved by Rèech, and that "Rèech's hypothesis is strictly the one which under the name of Froude's law has been much lauded in recent years." Mr Mansel makes a charge either of plagiarism or of unconscious imitation against Mr Froude? He did not suppose for a moment that Mr Mansel really believed that Mr Froude had been guilty of plagiarism. A great many of those present had had the honour of knowing Mr Froude, and they were perfectly satisfied that he would be one of the last men in the world to deck himself in borrowed plumes. The idea is absurd in connection with such a man as the late Mr Froude. No one was ever more modest than Mr Froude in valuing, or claiming credit for, his work; or was more generous in according merit to others, or in acknowledging any assistance he might derive from the labours of others. If such a charge were made no one who knew Mr Froude would consider that a serious reply to it was necessary. But he also considered they might confidently say that Mr Froude did not even unconsciously imitate Rèech. The work of the two men was entirely different, and although, up to a certain point and in one important respect, the results arrived at were similar, it must be remembered that these results were obtained by totally different modes of reasoning and research—Rèech, by pure mathematical deduction from an abstract dynamical theorem; and Mr Froude, by means of the modern stream-line theory, derived the same general law. It is quite certain that Mr Froude had no knowledge of what Rèech had long before published. But the theoretical proof of the law was but a step towards a practical comparison of the speeds of models and of ships. The law does not hold good for the gross amount of resistance, as Rèech appears to have assumed it would practically do. It applies to certain elements of resistance, but not to others. The law was not applicable to any useful practical purpose until the gross resistance had been resolved into its principal component elements: and until those elements had been eliminated and separately investigated which do not conform to the law. The reason why Mr Froude's name is better known in connection with the law or scale of comparison than M. Rèech's, is because of the way in which—after first of all deriving it from the the stream-line theory—he fitted it for, and brought it into, successful practical use; and on account of the ingenious and exhaustive manner in which he analysed the different elements of resistance, separating those to which the law rigidly applies from those to which it does not. Mr Froude proved the truth of the law of comparison, and the conditions under which it holds good; and he discovered and defined its limitations by means of a wonderful combination of theory and experiment. I sincerely trust that the effect of this discussion will not be to leave an impression upon the mind of any one, that Mr Froude's memory does not deserve all the respect and honour that an Institution of Shipbuilders and Engineers such as this can possibly pay to it.

Mr **Biles** said that Mr Denny had kindly referred to some work in which he had engaged in regard to the speed of ships. He would like to endorse what Professor Elgar had said with regard to Froude's law of comparison. He believed those who had studied Froude's writings must know quite well that he would not have attempted to detract in the slightest degree from the merits of any one's discovery. He thought that this law of comparison, valuable as it was, was a very simple deduction from a well-known theorem, so that there was very little merit in its discovery, but much in its application to the question of the resistance of ships. It might, however, be said that until Froude applied this law, the question of the speed of ships was not a law at all; and it was only because of the careful experiments he carried out that they had been able to make the slightest step in advance in that matter. With regard to the question of speed trials, as he understood the method that Mr Mansel claimed—that the usual friction produced the straight lines. Now, he had had some experience in steamer trials, and he must say he had had great difficulty in getting true results at low speeds. Would Mr Mansel inform them what kind of springs he had used in his indicators? Then there were other causes of error which might come into the question of speed trials, especially at the measured mile on the Clyde, at Skelmorlie. Two runs on the mile distance there was considered enough to determine the true speed of a ship. That might be correct in high speeds, although there were several things might come in to disturb them—such as the state of the tide, the time of the moon, and also variations of distance from the shore where the vessel was running—all matters difficult to eliminate from the facts of the case in two runs on a mile. But when the trials were at low speed, with a 5000-ton ship, and a force of 200 or 300 indicated horse-power propelling her at four or five knots, the forces that might come in to cause variations were considerably greater than the force that was necessary to propel her; and therefore it was most difficult to ascertain the power that was required to drive a ship at low speeds. In order to get clear of these difficulties, he suggested that in trials of a ship's speed they ought to take a few speeds at the mile distance, and then they could run straight ahead down the Frith and take a close series of revolutions and indicator cards. Or they might use the method which had been employed on the Clyde before now—the Dutchman's log. If that were done for a few ships, the difficulty of getting the true power at low speeds would become apparent, and it would be a good step towards finding a method for obtaining the true power of ships at low speed. He therefore recommended the running of a vessel a greater number of times at low speed in order to throw some light upon this question and upon the accuracy of Mr Mansel's law. Froude's reply to Mr Mansel had—as far as he could understand it, and as far as he understood Mr Mansel's paper—raised difficulties which it was impossible to forget. That question of the mathematical analysis of the curve at the lowest point seemed to him to be fatal to Mr Mansel's law.

Mr Wm. Denny, in reply, said he thought Mr Dyer had put into a very neat form the variation of thrust with variation of speed necessary for the production of a straight line. Beyond this he did not pretend to state *a priori* the grounds of the straight line theory. In any case he thought Mr Dyer had given good advice to this Institution, in asking the members of it to do all in their power to produce a large amount of accurate experimental data bearing on the subject. As he had very correctly remarked, such data formed the foundation on which alone theory could be properly raised. Mr Dyer had made reference to a remark of Mr Hamilton's—that he had found the lines to be straight in vessels of good form, when requiring the ordinary indicated horse-power to propel them; but to become crooked when the ship was hard-pressed. He (Mr Denny) was not cognisant of the data, or the extent of the data, from which Mr Hamilton had deduced this idea; but it would require a very considerable amount of data to prove his statement. Mr George Thomson—whose voice he was glad to hear that night as an old pupil of his own—had stated his preference for straight lines, and he thought that in that preference everyone present would concur. Had Nature favoured them with straight lines they would all have been happier, and the problem would have been easier to solve; but instead of facility Nature had given them such difficulty as would draw out what was best in them. Professor Elgar, in some of his remarks, had anticipated some things which he intended to say in reference to Mr Mansel's argument; but he would not hesitate to repeat them, because he had taken some trouble to set them forth with perhaps more completeness than Professor Elgar had been able to do, from a short study of the subject, and he was quite sure they would forgive him for reiteration in this matter. Mr Biles' remarks about the difficulties of obtaining accurate results in speed trials at low speeds were very important. He could only blame him for his modesty in not taking the credit to himself for inventing the method of trial which he had suggested to them. That method was gaining ground among experimenting firms on the Clyde. It was by no means a difficult method; and it had the advantage of being applicable without serious inconvenience in cases where it was not possible to carry out fully measured mile trials with a steamer. Mr Denny then went on to say—In Mr Mansel's reply, I observe, he repeats his assertion that I have charged him with plagiarism. I therefore repeat, that I have not charged him with plagiarism. My position regarding his relationship to Mr Froude is clearly stated at the foot of the second page of the paper I lately read to you. There I said that "At the time I made the statements which Mr. Mansel has quoted, I was under the impression that the idea which I conveyed to him had lain in his mind, and was the seed from which germinated his method of dealing with the initial friction. I distinctly pointed out in the remarks quoted by Mr Mansel that I believed this was an unconscious stimulation of his mind. I am afraid Mr Mansel is so sensitive about such questions that he is apt to fall into similar misconceptions, as, for instance, in the few remarks he made at the end of my paper. He there says: "He would like, however, to notice one statement in the paper, where Mr Denny says—'I think it well to say a few words upon the analysis of power with which Mr Mansel, nine and a-half years ago, started his discussion of my progressive trial data.' He thought Mr Denny was here assuming rather much credit to himself, since long before the time referred to he had worked at such matters." This remark of Mr Mansel's could only have been justified by my having said that he started this analysis of power at the discussion of my progressive trial data, instead of, as I did say, that he started the discussion with this analysis of power. Such misunderstandings offer both opportunity and temptation for the employment of banter rather than reasoning in replying to them. It is not, however, my purpose to employ any such methods in these remarks, but rather to do all in my power to set before you as fairly and impartially as I can an answer to Mr Mansel's reply; and I am all the more anxious to do so, as I have resolved that this shall be my final answer to Mr Mansel on this subject. I have taken much care to study the points raised by him in his Letter of Reclamation, and consider that he now receives from me all the satisfaction it is within my power to give, or that he has any right to claim. On the question of priority I have nothing further to add to what I have already said, as neither in my own researches, nor in the reply of Mr Mansel, has anything come to light calling for further remark. The letter quoted by him has no bearing on the question, as it is of date 4th December, 1875; while the conversation between us described by him occurred in the autumn of the same year. Mr Millar, it will be observed, has drawn attention to the date of this letter in a foot-note, and on the 9th inst., previous to the final correction of his proofs, I wrote Mr Mansel, drawing his attention to the date.

Turning from the personal question to the scientific aspect of this discussion, I regret Mr Mansel has been unable to appreciate the facts which I have brought forward regarding his logarithmic lines. In order to help him in this, I now show the logarithmic lines of the 30 trials, furnished by my firm, extended upon three supplementary diagrams (see Figs. 13, 14, 15, Plates XIV., XV., XVI.), with a straight line under each. In addition, all the four spot trials of the Admiralty vessels, previously referred to, have been grouped in another sheet, (Fig. 13, Plate XIII.), and treated in the same way. Regarding my firm's trials, please note that owing to some mistake three of them do not fulfil completely the conditions laid down, viz.: that they should all be trials having four spots, and with the propeller completely immersed. These three errors are noted on the diagrams, but they do not affect the general question. You have only to look at the four diagrams to see that straight lines are exceptions and not the rule. In my paper, I said that Mr Mansel's method, instead of universality, had only the character of occasional fitness. Even this occasional fitness can be explained in a very simple way. I show a diagram, (Fig. 17, Plate XVII.), giving the resistance curve deduced from model experiments of the "Manora' set off logarithmically the same as already exhibited on Fig. 7, Plate VIII. You will observe this is a very wavy curve, and yet that it is possible to draw through it a straight line cutting it in six different points. Supposing that on the trial of this steamer you had happened to hit upon these six different points you would have felt you had very fair justification in saying that the "Manora's" resistance curve set off logarithmically formed a straight line. But it is not very likely you would hit upon these six spots, the probability being that your measured mile trial had only supplied you with four spots. With four spots, especially if you allow yourself the privilege of rejecting one of the four as incorrect, you have a certain reasonable possibility of getting a straight line. If the irregular spot occurred on the top of one of the humps, then this rejected spot instead of being erroneous would be really the only one giving you a hint as to the true nature of the curve with which you were dealing. With three measured mile spots the possibility of getting a straight line is still further increased, and if two spots only are obtained, which Mr Mansel at one time considered quite sufficient, the possibility becomes a certainty. I believe, even among the straight lines obtained, the foregoing explanation holds good with regard to very many of them. Measured mile progressive trials at the best are, in the number of spots, very imperfect and quite inadequate to form the foundation for such a broad theory as Mr Mansel has embodied in his three laws of the revolutions, the pressures, and the gross power. In a mass of voluminous figures, Mr Mansel shows some cases of agreement between his formula and the results of experience occurring among the large number of cases he has analysed. Even, however, in these selected cases exceptions occur which have to be summarily dismissed as due to errors in observation or other causes not likely to conflict with the theory.

At our last meeting, Mr Mansel again pressed upon you the value of his method as affording a standard by which the accuracy of all measured mile trials could be judged. I do not think you will endorse such a recommendation, or be prepared to admit that scientific laws are dependent upon exceptions instead of upon generalised facts to which there are either no exceptions, or to which the exceptions are completely and satisfactorily explained. But Mr Mansel asks you to do more than this, and to make a theory the test of practice when that theory is still upon its trial. He asks you to stretch or shorten every ordinate which does not agree with his formula, and the same reasoning will oblige you to go even further and to reject every trial which produces curved or irregular logarithmic lines, and to condemn all model experiments because they show clearly that the resistance does not vary as it ought to do if a straight line is to be produced. This is not science but dogmatism —a dogma being an opinion which demands the unreasoning rejection of every opinion and fact which may prove contrary to it.

Mr Mansel objects to my having, in my Watt lecture of January, 1882, upon the speed and carrying of screw steamers, given Mr Froude the honour of the law of comparison in having called it by his name. He says: "This is exactly what Rèech deduced some fifty years before." And he further says: "It seems to me those statements are not fair towards the memory of M. Rèech, and very injudicious towards that of Dr. Froude." I wish to do justice to the memory of Rèech, for I very greatly esteem the services which that distinguished Frenchman has rendered, not only to naval architecture, but to science in general. As to my statement having been injudicious to the memory of Mr Froude, I think I shall be able to show that while Rèech, at an earlier period, stated the theory of comparison, he did so only as a theory. He did not pretend to establish the law by experiment, in which case he would have found that the skin friction did not follow the law, but required a separate experimental and analytic investigation. Rèech died last year, were he still alive I should have taken the opportunity of consulting him personally upon the matter; but having the pleasure of knowing M. Jules Chaudoye, a distinguished member of the constructive staff of the French Admiralty, and a late pupil and friend of M. Rèech, I took the opportunity, when last week in Paris, to ask him if he knew in what light Rèech regarded the work of Mr Froude. M. Chaudoye told me that Rèech, while not personally knowing Mr Froude, had a very great admiration for his work, and that he (M. Chaudoye) had had never heard him speak of Mr Froude in other than terms of appreciation. Such are not the feelings of an injured man towards the person who has done him injury. There is no doubt Rèech, in his book entitled "Cours de Mécanique," published in 1852, enunciated in a broad theoretical way the law of comparison, and I think this Institution owes Mr Mansel its thanks for having drawn attention to this matter. Before he mentioned it I was ignorant of what Rèech had written upon this point. Since our last meeting I have examined Rèech's investigation, and I am glad to have done so because, in the British Association Report, the reference to Rèech seems to be based on a misunderstanding of his work. In that report it is stated that Rèech's conclusion "would follow from the theory of the resistance of submerged bodies, on the supposition that the resistance varies as the square of the speed." But the case of a submerged body is not a good basis for the law of comparison, which is strictly appropriate only to wave-making resistance. In a submerged body this element of resistance is wanting.

Even in Mr White's "Manual of Naval Architecture," published in 1882, as great a misconception exists regarding Rèech's reasoning. On this account I am adding, in the appendix, a translation of the chapter in the "Cours de Mécanique," which deals with the law of comparison. In printing this translation, I am compelled to notice that in some points Rèech had not arrived at correct ideas, and that some of his statements must be admitted to be erroneous. These points do not, however, impair the validity of his fundamental reasoning and conclusion. A careful study of Rèech's work, and a comparison with the late Mr Froude's published and unpublished papers, show that these two men arrived at their conclusions by quite different methods. It is evident that Rèech demonstrated his theory on abstract dynamical principles, basing it on the theory of similarity, as laid down by Newton. He acknowledges his great indebtedness to Newton, and as I had the pleasure of seeing in M. Jules Chaudoye's written notes of his lectures, he also acknowledges his indebtedness to Joseph Bertrand, the eminent French mathematician, who he declares first brought this theory of Newton into prominence. It is exceedingly interesting to note how this great thinker delights in acknowledging his indebtedness to the suggestions of other minds. We find the same spirit equally developed and evident in the late Mr Froude. Even from the rider to the British Association Report, this feature in Mr Froude's character is very apparent, as he mentions several times his indebtedness to Rankine.

I may point out that this British Association Report was prepared by the late Mr C. W. Merrifield. It was submitted as a draft to Mr Froude, and signed by him "subject to explanations." It is not, therefore, to be assumed that Mr Froude studied the original writings of all the foreign authors named in it. Indeed, it is evident from the rider that he trusted to Mr Merrifield for the correctness of these references. In this rider Mr Froude very clearly points out that he had based those researches which led him up to the law of comparison upon Rankine's stream line theory, and acknowledges that theory as the source from which he drew the law. From the mistaken way in which Rèech's theory is put forward in the British Association Report, it is very probable that Mr Froude passed it over as based upon an erroneous assumption, which, in his ignorance of Rèech's actual work, he would be entitled to do. It must be clear to you that these two distinguished men arrived at the idea of the law of comparison by different roads. In so far they were dissimilar, but in the generosity with which they acknowledged their indebtedness to their great teachers they were alike. But, admitting the principle of comparison to have been enunciated as a theory, and granting to Rèech the priority of such an enunciation, just as Mr Froude would have done, had he now been among us, we must admit that much labour would have to be put forth in experiment and analysis before such a theory could be raised to the security of a law. Any physical law which is to be counted of established and permanent value must not merely originate in theory, but be verified and confirmed by experiments. It is the doing of this work in addition to the independent origination of the theory which entitles Mr Froude to have his name permanently attached to this law. It is not irrelevant to remind you that by Newton's own acknowledgment the three laws which bear his name were understood before he formulated them and built them into the magnificent edifice of the Principia.

To establish the law of comparison, the total resistance has to be analysed into its separate elements, which in ship-shape forms consist of wave making and skin friction. It is evident from the remarks made by Mr Mansel that he totally misunderstands the work done by Mr Froude in dealing with the skin friction portion of the resistance. Mr Mansel takes much trouble to show that there were experimenters before Mr Froude, who had arrived at the same figures for the skin resistance of full-sized vessels. I welcome any corroboration of Mr Froude's experiments, whether ancient or modern. But Mr Mansel goes entirely beyond such a consideration when he assumes that Mr Froude's rates of surface friction are unreliable, because the rate per square foot of surface for a propeller blade was more than double of that for the "Greyhound." What Mr Froude did discover about the surface friction and what alone enabled him correctly to eliminate this element from the comparison of ship and model, was that the rate per unit of surface increased at the surface decreased in absolute length. Mr Froude further very largely defined the amount of such increase of friction with the diminution of length. The apparent discrepancy, therefore, between the surface friction of the "Greyhound" and the surface friction of the propeller, instead of being a proof of Mr Froude's being in error, was an indication of his discovery. But I am afraid Mr Mansel was too eager to find Mr Froude in the wrong to accept this hint, and so fell into error and mistook a truth for a blunder.

Mr Mansel objects to model trials on the ground that, "the absence of the propeller entirely alters the character of the phenomena." Would it not have been just to Mr Froude's memory to point out that he, first of all model experimenters, introduced the propeller as a means of analysis into such experiments? By doing this, he determined the existence of the augmentation of resistance, and the relationship between that element and the power recovered by the propeller from the wake.

In my paper I pointed out that in his Letter of Reclamation, Mr Mansel had (possibly unintentionally) very much misrepresented Mr Froude in his use of the square of the speed. After hearing my paper I thought he would have had the generosity, in his reply, to admit his error, and to do justice to Mr Froude. But instead of this he says in his reply, "Dr Froude making use of the same equation I published in the spring of 1875, from (3) and (4), deduced the value $f - 10{\cdot}04$, which is simply the result of an assumed false law of resistance masking the mechanical principle involved in Morin's constant." From this quotation it would appear that Mr Froude in determining the initial friction of the "Merkara" had used Mr Mansel's formula $V^2 = C\,(P' + rp - 5)$. But if we refer to Mr Froude's paper in the spring of 1876, and deduce the formula which underlies his graphic determination of the initial friction, we shall get the expression $V^{1\cdot 87} = c\,(T - f)$. Here T is equal to the indicated thrust, and f to the amount of initial friction expressed in indicated thrust, the power 1·87 expressing the rate—determined by Mr Froude's experiments—of variation of skin friction with varying speed. These formulæ on the very face of them are not the same, the most marked difference being certainly that between the figure 5 and the term f. In Mr Mansel's formula the initial friction is assumed known, and is put down in figures. In Mr Froude's formula the initial friction is an unknown quantity which has to be evaluated by the formula. Herein lies the whole gist of the matter. Mr Froude saw that the initial friction could be discovered by reducing the power ordinates to force ordinates—hence his formula. Mr Mansel assumed that he knew the initial friction, and therefore did not attempt to use his formula for the same purpose. But even ignoring this there is the further difference that while Mr Froude knew clearly the limits of application of his formula, Mr Mansel has as clearly shown in his Letter of Reclamation that he had no conception of these limits.

There is a sentence quoted by Mr Mansel from Mr Froude's rider to the British Association Report, which calls for some remark. As quoted by Mr Mansel, it has been mutilated by the omission of the first thirteen words—that is, of all the words down to "conclusion that." I give the sentence in full:—"Now, Professor Rankine's admirable stream line investigations have definitely established the conclusion that for symmetrically shaped bodies of 'fair' lines, not excluding by that description certain very blunt ended ovals when wholly submerged, the entire resistance depends on the conditions of imperfect fluidity, of which surface friction is the only one so considerable that we need take account of, if we deal with bodies of rational dimensions." Mr Mansel goes on to say, that "The statements quoted are samples of a soil suited to the development of 'new departures,' which anon shall blossom and fructify into 'Popoffkas,' length and breadth synonymous, and war ships 'short and handy,' which a Reed not shaken by but controlling the winds shall *extol* magniloquently and old Neptune, most misanthropical of 'pike-keepers,' by his unpublished table of rates, shall *toll* most exorbitantly." Mr Mansel in saying "statements" refers not only to the submerged body described in the previous quotation, but to the strange results given by the swan-breasted model upon which Mr Froude experimented before the formation of his tank. It is unfortunate for Mr Mansel that at this present moment facts seem to be against him and in favour of Rankine and Froude. I understand that Mr Whitehead, in his newest type of torpedoes, is adopting forward ends, not of the sharp form which these totally submerged bodies originally had, but like those described in the quotation made by Mr Mansel—viz., very blunt ended ovals. It is very curious also to observe, as I had the pleasure of doing lately in Mr Yarrow's yard at Poplar, that the fastest torpedo boats are being built with very much blunter forward ends than was the practice some few years ago. In cases of this kind, we must depend more upon practice and experiment than on what mere abstract theory would lead us to expect. It will therefore, perhaps, be well with regard to the swan-breasted model results, not to be positive that they too may not some day have practical confirmation.

In conclusion, I would express my regret that the course of this discussion has, on Mr Mansel's part, turned from a defence of his own work to a depreciation of Mr Froude's. To you, as to myself, it must have been painful to listen to the depreciation of a man whose generosity led him so amply to admit his indebtedness to your great townsman, Rankine. It was a poor return, before this Institution, of which Rankine was so great an ornament, to have heard Froude's achievements minimised, and the fruits of his genius and labour treated as of little value. He was almost the first among the scientific men who helped naval architecture, to think it a task not unworthy of his powers to make himself understood by practical men. There was no contempt in him for his fellow-men less skilful in mathematical processes than himself, and he never undervalued others because they were only plainly trained. He possessed in a remarkable degree and combination the gifts of a great thinker, a great experimenter, amd an admirable teacher

Even yet we only know a portion of his work, for there lie buried in the Admiralty archives many of his most valuable reports, and the results of very many of his experiments. I trust the day is not far distant when, instead of depreciation, his memory will receive the deserved honour of the publication of his completed works. This would be the best monument to a memory cherished by every one who had the honour and the happiness of his friendship. Without the morbid jealousy and vanity which often lead a man to crave for priority, and to despise his less accomplished fellow-men, Mr Froude, by his simple, constant, and unbiassed love of truth, passed a life in which the endowments of his heart and spirit were as apparent as the power of his intellect. Indeed, to have known him was to have had the opportunity of becoming a better man.

On the motion of the PRESIDENT, a vote of thanks was passed to Mr Denny for his paper.

[APPENDIX.]

Translation from the
"Cours de Mécanique," par F. Rèech,
Directeur de l'Ecole d'Application du Génie Maritime.
Section V.

On the Theorem of Newton on Similarity of Motions, considered as a General Principle for all Questions in Applied Mechanics.

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1. In geometry two figures are similar when one can be deduced from the other by means of a changeless ratio l between the homologous sides without change of the angles.

2. In the statics of rigid bodies, if any system of forces P, P′, P″, acting on any figure, be in equilibrium, then the same forces multiplied by any number f, will be in equilibrium on the same figure, or on any similar figure.

3. The general theorem of similarity in statics is thus very simple and very broad, because we can arbitrarily fix the two co-efficients or ratios l, f.

However, in terrestrial applications, we are obliged to take into account the weights of the bodies among the forces P, P′, P″ .... of a system, and thus there are two cases to be distinguished—the one ideal, the other real.

4. The ideal case is such as would be obtained by imagining bodies of finite volumes at great distances apart, their centres of gravity being united by rigid straight lines; the co-efficient l would then only have reference to the figure of the rigid lines, and the co-efficient f only to the weights Π, Π′, Π″......... of the different bodies, so that there would always be a complete independence between the co-efficients l, f, so long as the volumes of the different bodies did not exceed the limits beyond which these volumes would penetrate one another.

5. The real case is that in which there is only one ratio of

similarity l in all parts of the system; denoting then by d the ratio of the densities or the specific gravities we necessarily have

$$f = dl^3;$$

because the weights of similar bodies follow exactly this law, and all the other forces P, P′, P″, must in consequence do the same.

6. In the statics of flexible bodies the equations of equilibrium are

$$0 = X + \Sigma R \frac{dr}{dx}$$

$$0 = Y + \Sigma R \frac{dr}{dy}$$

$$0 = Z + \Sigma R \frac{dr}{dz}$$

for all points x, y, z, of a system, and the functions

$$R = \phi(r) = \frac{df(r)}{dr}$$

can certainly be such that, in two exactly similar positions, before and after the deformation of the system, the forces X, Y, Z, which produce this deformation, will not vary in a single and identical ratio f.

Thence, inversely, when the forces X, Y, Z vary in one same ratio f, the subsequent figure of the system can be not similar to the initial figure. In fact, if we imagine a prismatic bar laid horizontally on two supports, under a vertical load in the middle, we may see that the deflection of this bar increases directly as the load, without increase of the chord of the arc, which will evidently hinder the new figure of equilibrium from being similar to the previous one.

Likewise, a prismatic bar drawn out in the direction of its length only, would be lengthened without becoming thicker, &c., &c.

Thus we cannot pretend to extend the theory of similarity in statics to cover small changes of figure which a body under the influence of different forces will undergo.

We can, indeed, propose to establish a comparison between two bodies of large and small dimensions, supposed similar in their initial forms when there are no forces X, Y, Z at play, and similar still in their subsequent forms when the one is acted upon by forces

X, Y, Z, and the other by corresponding forces X′, Y′, Z′; because, applying the ordinary formulæ in the theory of the resistance of materials, we learn that in this case, for the same kind of material and with the same quality of elasticity, there exists for the two bodies only the single ratio

$$l^2$$

between the absolute forces which must be applied on the one body and on the other to homologous surfaces.

It follows that the theory of similarity in statics can be extended thus when we make

$$f = l^2, \text{ or } f = 1,$$

according as we denote by the letter f the ratio of the resultant forces on two homologous and similar surfaces, or else the ratio of the resultant forces per unit of surface at two homologous points. but we cannot submit to the same law the forces of gravity, the ratio of which, from the one body to the other, and for the same density, is

$$l^3.$$

Moreover, the forms of two bodies similar and similarly bent, of large and small dimensions, are not equally resisting forms, and for these reasons we can only extend the theory of similarity to the statics of flexible bodies by neglecting such small changes in the figure as the bodies undergo under the action of the force that is applied, which evidently amounts to considering only entirely determined and previously known figures as in the statics of rigid bodies, in which case the theory in question is reduced to what has already been stated.

7. To extend the same theory to dynamics, it is sufficient to note that the total exterior forces acting on a moving body can be represented by the quantities

$$X_1 = X - m\frac{d^2x}{dt^2}$$

$$Y_1 = Y - m\frac{d^2y}{dt^2}$$

$$Z_1 = Z - m\frac{d^2z}{dt^2}$$

and that, consequently, the complex quantities X, Y, Z ought to increase all in a single and identical ratio f, when we pass from any system to another exactly similar.

8. But in considering the co-ordinates x, y, z of a material point of mass m as functions of the arc s of the described curve, and the arc s as a function of the time, we have

$$\frac{dx}{dt}=\frac{dx}{ds}\,\frac{ds}{dt}=\frac{dx}{ds}\,v;$$

further, by considering also the velocity v as a function of the arc s, we find

$$\frac{d^2x}{dt^2}=\frac{dx}{ds}\,\frac{dv}{ds}\,\frac{ds}{dt}+\frac{d^2x}{ds^2}\,\frac{ds}{dt}\,v=\frac{vdv}{ds}\,\frac{dx}{ds}+v^2\,\frac{d^2x}{ds^2};$$

so that, denoting by

α, β, γ the angles that the velocity v makes with the rectangular axes of x, y, z,

r the radius of curvature of the path considered as essentially positive,

a, b, c the angles that the radius r drawn from the circumference towards the centre, makes with the axes of x, y, z,

we have

$$\frac{d^2x}{dt^2}=\frac{vdv}{ds}\cos\alpha+\frac{v^2}{r}\cos a$$

$$\frac{d^2y}{dt^2}=\frac{vdv}{ds}\cos\beta+\frac{v^2}{r}\cos b$$

$$\frac{d^2z}{dt^2}=\frac{vdv}{ds}\cos\gamma+\frac{v^2}{r}\cos c$$

9. It follows from this that from one system to another, the quantities

$$\frac{d^2x}{dt^2},\quad\frac{d^2y}{dt^2},\quad\frac{d^2z}{dt^2}$$

can vary in many different ways; but when we wish only to make comparison between the homologous points of two similar systems, and when we wish at the same time to have similar paths, in order that the similarity of the two systems may hold indefinitely, then the angles α, β, γ and a, b, c must be exactly the same two for homologous points, and the dynamic forces in question must vary partly as the term

$$\frac{v^2}{r},$$

and partly as the term

$$\frac{vdv}{ds}.$$

10. Thus, denoting by u the ratio of the velocities of two homologous points, and keeping the letter l to denote the ratio of the linear dimensions, we shall have, in the other system, the velocity

$$v' = uv,$$

and the corresponding forces,

$$\frac{v'^2}{r'} = \frac{u^2v^2}{lr} = \frac{u^2}{l} \times \frac{v^2}{r}$$

$$\frac{v'dv'}{ds'} = \frac{uv\,(udv + vdu)}{lds} = \left(\frac{u^2}{l} + \frac{vudu}{ldv}\right) \times \frac{vdv}{ds}.$$

by which we see that from the first system to the second the ratio of the centripetal forces will be

$$\frac{u^2}{l}$$

and the ratio of the tangential forces

$$\frac{u^2}{l} + \frac{vudu}{ldv}.$$

Then the ratios of these two kinds of forces cannot be the same unless we have

$$\frac{v'}{v} = u = \text{Const.} \qquad \text{.........................(1)}$$

and when this condition is accomplished, the common ratio of the quantities

$$\frac{d^2x}{dt^2}, \quad \frac{d^2y}{dt^2}, \quad \frac{d^2z}{dt^2},$$

between two homologous points of two exactly similar systems, becomes the perfectly determined number

$$\frac{u^2}{l},$$

that is, the quotient of the square of the ratio of the velocities divided by the ratio of the linear dimensions.

11. As the ratio of the homologous masses in two similar systems is

$$\frac{m'}{m} = dl^3,$$

where the letter *d* denotes the ratio of the densities, we see that the ratio of the forces of inertia

$$-m\frac{d^2x}{dt^2},\ -m\frac{d^2y}{dt^2},\ -m\frac{d^2z}{dt^2}$$

will be

$$f = dl^3 \times \frac{u^2}{l} = dl^2u^2 \dots\dots\dots\dots\dots\dots\dots\dots(2)$$

and consequently all other forces, the components of which have been denoted by X, Y, Z, will need to vary in the above ratio: that is to say, as the density, as the square of the linear dimensions, and as the square of the velocities.

12. The ratio of the durations t, t' of two homologous movements will be

$$\theta = \frac{t'}{t} = \frac{l}{u},$$

and this would be the whole theorem of similarity in dynamics, if we could neglect the forces of gravity.

13. It follows from this that in the absence of the forces of gravity, the resistances of floating bodies of similar forms and of absolutely smooth or polished sides would vary exactly as the densities of the liquids, as the homologous surfaces, and as the square of the speeds, provided that the atmospheric pressure per unit of area at the free surface of these liquids were to vary also according to the same law, and would consequently be proportional to the density as well as to the square of the velocity.

14. But in terrestrial applications we cannot generally neglect the forces of gravity, and then all the other forces must vary as these, that is to say, to the general relation

$$f = dl^2u^2,$$

we must link the condition

$$u^2 = l, \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots(3)$$

which involves that

$$f = dl^3$$

or the other forces as well as for the forces of gravity.

15. The condition

$$u^2 = l$$

is more necessary when the forces of gravity predominate over the others in a system: that is to say, when the motion is relatively slow; it is less necessary, on the contrary, when the forces of gravity mg are small in comparison to the forces of inertia,

$$-m\frac{d^2x}{dt^2}, \; -m\frac{d^2y}{dt^2}, \; -m\frac{d\,z}{dt^2}$$

or when the motion is relatively rapid.

16. It follows from this that the resistance of a floating body at different speeds, and with a suitable pressure at the free surface of the liquid, must approach indefinitely towards proportionality to the square of the speed, as the motion grows more rapid, and must depart, on the contrary, from such proportionality as the motion grows slower.

17. The exact proportionality to the surfaces and to the square of the speeds cannot hold good, in a word, unless we operate on similar forms, and with the condition,

$$u^2 = l.$$

Thus, in the case where we have determined experimentally, by means of certain known forces, the resistance of a ship's model, or rather the complete behaviour of a model of a paddle or screw steamer, we have only to make another model with linear dimensions l times larger, and to multiply all the observed speeds by the ratio

$$u = \sqrt{l},$$

that the new system may behave similarly to the previous one, requiring forces, the statical intensities of which are all increased in the proportion of the cube of the ratio of the linear dimensions, which increases the quantity of work of each of these forces as the product

$$dl^3u = dl^2u^3.$$

18. Or conversely, in the case where the dimensions and speed of a ship or steamer are known, and we propose to experiment upon a

small model so as to find all the relations of the forces and the speeds of the system, we have to use the same formula,

$$u = \sqrt{l},$$

in order to find the precise ratio of the speeds which we must establish between two homologous points of the two systems, so that the ratio

$$f = l^3$$

can serve for passing exactly from each of the forces of the large system to the corresponding force of the small model, or *vice versa.*

19. However, for perfect similarity, the atmospheric pressure at the free surface of the liquid, as well as the frictional forces or the forces of adhesion of the liquid particles against the sides of the system should follow the same general law as the other forces: that is to say, should be proportional to the cube of the ratio of the linear dimensions on two homologous surfaces, and consequently proportional to the ratio of the linear dimensions, or to the ratio of the square of the speeds, per unit of surface.

20. But it is very probable that for nearly incompressible liquids such a restrictive condition for the atmospheric pressure is not necessary unless the extreme rapidity of the motion of a body wholly immersed produces at the after end of the body a wake completely empty, or at least full of vapour, such as exists, perhaps, at the after end of a cannon ball, which moves at a speed of 400 to 500 metres per second in a sufficiently dense medium.

21. As to the adhesion or the friction of a liquid against a continuous surface, the very little we know up to the present day seems to indicate that the forces of this kind vary, in fact, about as the square of the speed.

22. What we have now said shows sufficiently that in mechanics the theorem of Newton on similarity is always the best, and often the only principle on which we can base numerous practical deductions.

23. This single theorem includes about all that men have managed to establish in hydrodynamics up to the present day by more or

less empirical or defective means, and when we add to it the theory of the *vis viva* by M. Coriolis, as well as our theorem on the forces of reaction, we shall possess a science more simple and much more powerful than any we could derive, up to the present time, from the special formulæ in hydrodynamics, the utility of which seems to be absolutely *nil* in a course of terrestrial mechanics.

Note on Tests of Turbines, by Professor R. H. THURSTON

Received 10th November, 1884

I note a few points which were brought up, in the course of debate upon the note sent to the Secretary, by me, last spring, which demand reply.

Mr Turnbull suggests that the tests referred to by me were made some months after those reported by him, and that there may have been some improvement in the interval. I would say that the builders of the wheel described are constantly experimenting with their turbines, testing them at the flume, and making alterations and improvements suggested by the results of such tests, and are thus constantly improving it. It is their avowed intention to thus improve all of their wheels until every pattern put on the market shall have an efficiency, under, say, 20 feet head, of above 80 per cent.

Mr Murray expresses doubts in regard to the figures reported by me, and asserts that I have given nothing they could well found upon, to improve their knowledge in this matter. He goes on to point out an apparent discrepancy between the data given, as evidence that I have been careless in reporting, or that the tests are not reliable. The apparent discrepancy is accounted for in the simplest way imaginable: the heads under which the wheel worked at full gate, on the two dates mentioned, were slightly different, in consequence of the variation of flow in the Connecticut River, and of rate of demand for water on the part of the adjacent mills. I did not consider it necessary to encumber the Transactions with extended tables of details, and gave simply results. If desired, I will gladly give the full logs of those tests, and of just as many more as may be needed to show that my statements may be absolutely relied upon. The probable error of the trials at the flume of the Holyoke Water Power Company is, I think, not more than one per cent. Any other apparent error that any critic may discover in the data furnished will be found, I doubt not, to have equally simple explanation. The fact is that these turbines did do exactly what has been claimed for them. I have superintended their tests, examined and measured up the flume, gone over the system of working up results, and may claim full credence for every assertion which I made in my original contribution. We are not unaccustomed, in the United States, to seeing 80 per cent. reached. We have many wheels that have given that figure, under favourable conditions; and the wheel in question is always expected to exceed that figure after the makers have worked their patterns into such a shape as satisfies them. But we are not alone in this matter. The turbines of M. Vallet, the distinguished French hydraulic engineer, have repeatedly given efficiencies exceeding 80 per cent., and I have no doubt that other good designers of turbines reach that figure. We do not consider it by any means wonderful, even for turbines with cast iron buckets. I have records of a number of wheels tested, under such conditions and by such skilled hands as to make it certain to my mind that the figures are correct, which go above 80 per cent.

I agree fully with Professor Thomson in his impressions in regard to the probable success of the untaught mechanic attempting to solve the problem here considered, and the fact of success in this case was to me, as to him, a very interesting and an almost incredible one. Nevertheless it remains a fact—a fact of which I am personally thoroughly certain. The man has produced turbines having efficiencies reaching up to 85 per cent. He cannot do this at every attempt, and, as might be expected, cannot with certainty repeat a success. Working with an educated and skilful engineer, I have no doubt that his successes would be more frequent and his work more uniform in quality.

HOBOKEN, N.J., U.S.A., Oct., 1884.

On Electrical Navigation.

By Mr ALLAN CLARK.

(SEE PLATE XVII^A.)

Received 16th December, 1884, and held as read 24th February, 1885.

IN treating this subject, we shall confine it to a short description of what has already been accomplished, and which may be found interesting, as showing the improvements which have been made up to date. Firstly, in regard to the batteries that furnish the current of electricity, and secondly, in regard to the motors which turn that current into mechanical work.

We shall, for the sake of comparison, given details of the spaces occupied, and the weights of the various batteries and motors, together with the horse-power furnished by the batteries, and the percentage of efficiency of the motors.

The batteries are either primary or secondary. The primary battery may be regarded as a kind of furnace where the fuel is zinc, the current of electricity being derived from the potential energy of zinc in the process of its dissolution and combination with oxygen to form oxide of zinc. Considered thus, the voltaic furnace can be shown to be a much more perfect and economical arrangement than the steam furnace, in which all heat of a low grade is wasted or lost. In the electric generator all potentiality is utilized. The secondary battery or accumulator may be regarded as a cistern for storing the current of electricity derived from a dynamo or primary battery. This current once stored, may be drawn off slowly or quickly as desired.

The motor that converts the current of electricity into mechanical power is an arrangement of soft iron in two parts, one fixed and the other movable, usually in the form of a drum revolving inside a frame. The current passes through an insulated copper wire wound round these—in the fixed frame in one direction only; but in the movable part the current is reversed at certain points whereby a continuous magnetic attraction and repulsion is kept up between them causing the drum to revolve.

The first experimenter in electrical navigation was Professor Jacobi, a Russian, who in 1838 succeeded in propelling a boat 27 feet in length, on the river Neva, at the rate of one and a quarter knots per hour. The battery used consisted of three hundred and twenty Daniell cells, occupying a space of sixty cubic feet, and the motor a space of ten cubic feet. The battery furnished a current of one-horse power, and the motor had an efficiency of ten per cent. The boat was wrought through the medium of paddle wheels. A sketch of this motor may be found interesting, especially as it was one of the first known to history.

An experiment similar to that of Jacobi was exhibited by Mr Llewelyn to the members of the British Association at Swansea in the year 1848. The motor used on this occasion was a great improvement on those previously invented, but the battery was too wasteful of zinc for practical purposes.

In 1866 Count de Molin succeeded in constructing a motor, that drove a small boat in the Bois de Boulogne. His motor developed one-seventh of a horse-power at the cost per hour of thirty-eight pounds of zinc per horse-power.

In 1881 Mr Gustave Trouvé, of Paris, constructed a twenty-foot boat that was worked in the little lake at the Exhibition. For this he made use of small double motors of the simple Siemen's armature kind, fixed on the rudder head, and connected to the propeller by means of an endless chain. The battery measured four cubic feet, developed two-thirds of a horse-power, and the motor an efficiency of nearly twenty per cent. A speed of between two and three miles per hour was obtained.

Late in 1882 the Electrical Power Storage Company, of London.

produced the launch "Electricity," a boat twenty-five feet long, and which differed from any of the preceding in having accumulators instead of primary batteries to furnish the driving power. Forty-five accumulators were used, each weighing half-a-hundredweight, or twenty-two and a-half hundredweights together, and the space occupied was fifteen cubic feet. The motors used were two Siemen's dynamos, weighing six hundredweights, and occupying a space of seven and a-half cubic feet. These were connected by belting to an overhead shaft, which in turn was connected to the propeller shaft. For reversing, this belting was shunted to a loose pulley, and a crossed belt connected up—an arrangement primitive and bulky. The total weight of accumulators, motors, gearing, and hull, was upwards of two tons. The accumulators furnished four-horse power, and as the motors had an efficiency of about seventy per cent., nearly three horse-power was obtained, and a speed of between five and six miles per hour.

Early in 1883 the author, who had been experimenting for some time, produced his first full-sized launch, which was twenty-one feet long. The batteries occupied a space of three cubic feet, and the motor a space of about half-a-cubic foot, and weighed together two and three-quarter hundredweights. The total weight of this launch was four and a-half hundredweights. The battery gave off one and three-quarters horse-power, and as the motor developed fully one and a-quarter horse-power, its efficiency was about seventy-five per cent. The propeller was a two-bladed one, twelve inches diameter and thirteen inches pitch, giving four hundred and fourteen revolutions, and a speed of four and a-half miles per hour. The reversing and stopping gear used was a simple cut off and current reversing bobbin, which weighed a few ounces only. This was the first electrically-driven boat that had the propeller shaft coupled directly to the motor, and marked a very important advance.

In obtaining this result several difficulties were overcome. The style of machinery that had given satisfactory results in the model was found to be useless owing to the lower rate of revolutions required. Electrically driven motors work best when allowed to revolve fifteen hundred to two thousand revolutions per minute, with a light load; but when loaded to revolve only four hundred per minute, the copper wire gets heated and the insulation destroyed, rendering the motor useless till re-wound with fresh wire. By increasing the size of the motor it was found that with the same current the large motor gave better results than the smaller one, and the wires did not heat. The bilge water was found also to damage the insulation of the copper wire, allowing the current to pass without going its round through the wires. By water-proofing the motor all over, this difficulty was got rid of; also several minor ones.

For comparison with the accumulator launch "Electricity" details of the author's launch "Electric," also 25 feet long, are now given. This boat was officially tried in May last. The battery occupied a space of six cubic feet, and the motor fully a cubic foot, weighing respectively three and one and a-half hundredweights; the total weight of hull complete with machinery was seven hundredweights. The battery gave off three and a-half horse-power, and the motor nearly three horse-power. The propeller was two-bladed, fifteen inches diameter, and eighteen inches pitch, giving three hundred and ninety-six revolutions per minute, and a speed of five and seven-eighth miles per hour.

In one of the same size now finishing the battery is four horse-power, and with a more efficient motor it is expected a speed of seven miles per hour will be got, which is about the maximum that can be got from a boat this size whether the power be electricity or steam.

It will thus be seen that the batteries have been improved from sixty cubic feet per horse-power to under two cubic feet per horse-power, and the motors from 10 cubic feet for one-tenth horse-power to about one cubic foot for three horse-power or about three thousand per cent. on each. The consumption of zinc has also decreased from thirty-eight pounds in 1866 to one and one-third pounds per horse power in 1884.

As to the future of these vessels, it is not expected by the most sanguine that they will ever supersede steam even on a small scale, but they will certainly obtain a footing for pleasure purposes where the utter absence of noise, smell, and soot is an advantage that users are willing to pay for, even were the cost much more than it is now. Among the many advantages these boats driven by primary batteries exhibit over steamers may be mentioned. They can be charged in one-third the time it takes to get up steam. When charged they can be used at once or weeks after without further trouble. After being used, they can be left without attendance, and used again when required. They do not weigh more than one-third the weight of steam launches the same size, can be easier hung on davits, are cleaner and noiseless, and do not require skilled attendance.

The accumulator launches require motive power to drive a dynamo to charge the accumulators, so that it is not likely these boats will come into use except perhaps for ferry or coast traffic, where the charging plant could be kept at the terminus quay and applied as required.

Regarding cost of driving, if steam be taken to represent 1, then accumulators may be taken at 2, and primary batteries at 10; but as improvements in primary batteries are being made continually, it is probable this figure will be much reduced ere long.

On the 24th March, 1885, the PRESIDENT, in proposing a vote of thanks to Mr Clark for his paper, said he had no doubt that the Institution were gratified by having the information contained in the paper put on record. Electrical propulsion was a very interesting subject, and while he did not think it would to any great extent supplant the use of steam in navigation, yet it might have its own uses; and it was highly desirable for them always to be on the outlook to find what were the capabilities and uses of electricity in various directions.

On a Continuous Regenerative Gas Kiln for Burning Fire-bricks, Pottery, &c.

By Mr JOHN MAYER, F.C.S.

(SEE PLATE XVIII.)

Received and Read 24th March, 1885.

FOR fully twenty years the subject of firing by using fuel in the gaseous form, and on the principle of heat-regeneration, has had a most intense attraction for me, partly on account of the scientific interest and beauty inherent in it, partly owing to its great importance in the industrial arts, and in no mean degree in consequence of its being made the theme of the last public discourse delivered in the Royal Institution, London, by the late Professor Faraday, which was the only occasion on which I had the great pleasure of listening to that distinguished chemist and physicist, and of seeing him perform many beautiful experiments with matchless skill and success. That was in the year 1862, while the Great International Exhibition of that year was being held in London; and on that occasion the great experimental philosopher was surrounded by many of the most notable scientific men of this kingdom, of the Continent of Europe, and of the United States of America. The late Sir Wm. Siemens, who was then rapidly making his great reputation in physical and mechanical science, had quite recently got his regenerative system of gas-firing into successful operation at the famous glass works of Messrs Chance Brothers, at Birmingham; and so greatly charmed was Faraday with the beauty of Siemens' valuable invention that he made a special journey to the capital of the Midlands in order that he might look into the glass-melting furnace with his own eyes, and thereby be enabled to appreciate the merits of the invention in their true and full significance. It was upon that subject that "the old man eloquent" delivered his most memorable discourse, and during all the many years that have since elapsed the Siemens system of gas-firing and heat-regeneration has never in the least degree diminished in its scientific beauty and value, while as regards its importance in the industrial arts it has gone on from year to year attaining for itself a stronger and stronger position; indeed, so very marked is that the case that the system in question may be spoken of as one of the greatest industrial inventions of the present generation.

Within those years there have been many and varied applications of gaseous firing and heat-regeneration, according both to the Siemens patents and to the patents of other inventors. It is the aim of this paper to describe another successful application of those two principles, almost at our own doors, as it were, and in a branch of manufacturing industry in which there was great room for economising fuel and preventing the fouling of the atmosphere by the discharge of immense volumes of dense black smoke. The industry to which I refer is more especially the burning of fire-bricks, though it may also be said to include all kind of goods that are made of clay; and the inventor of the system of kiln-burning about to be described is Mr James Dunnachie, who has been intimately identified with the manufacture of that most refractory and heat-resisting, and now and most familiar article—a "Glenboig fire brick"—for the last quarter of a century. Various persons, including Dr Siemens, had made attempts to produce a kiln for firing bricks on the heat-regenerative principle, but in no case were any efforts in that direction attended with practical and commercially-successful results until the matter was taken in hand by the proprietor of the Glenboig Star Fire-clay Works, some four or five years since.

One important factor for making kiln-firing by regeneration a success was a means of providing a continuous supply of gaseous fuel, in a cheap and easy manner, and at that time such an appliance was ready to hand in the gas-producer which was brought under the notice of the members of this Institution some time ago by Mr F. J. Rowan. Without in any way detailing its construction or mode of action, I may pass on and simply state that such a gas generator, similarly to that of Siemens, produces gaseous fuel whose combustible constituents usually form well-nigh 40 per cent. of the whole, the non-combustible diluent being chiefly atmospheric nitrogen; and it may be well here to state that the chief combustible and calorific ingredient of producer gas is carbonic oxide. As regards the kind of fuel to be employed in the proposed new mode of burning fire-bricks, Mr Dunnachie had long had his mind made up, his desire on this point being to follow in the footsteps of Siemens; and as to the mode of practically using the gaseous fuel to the greatest advantage he also had his mind made up. Of course, it involved the adoption of the principle of heat-regeneration, and in a way not only modified to suit the special circumstances of the case, but so radically differing from the Siemens system of regeneration that the device adopted practically amounted to a new and important invention.

Two producers were forthwith ordered, and in due course erected on a suitable spot within the works; and at the same time, Mr Dunnachie proceeded to erect a kiln embodying all the newest notions that seemed to accord with the most efficient method of developing the calorific powers contained in the gaseous fuel.

Fully three years ago the first continuous regenerative gas kiln, as it was evolved from the brain of the inventor, was brought into full work, and it at once established itelf as a very marked practical success. Since that time the system has been extended at the "Star" Works, and it has also been brought into use at the original Glenboig Works, and at the Cumbernauld Fire-clay Works—all the three establishments just named being now the property of one concern, the Glenboig Union Fire-clay Company (Limited). Of its applications elsewhere and of its prospective adoption in other directions something may be said further on.

As illustrated by the diagrams exhibited on the walls, and by the very excellent and instructive model placed on the table (the latter having been put at my service for this evening, prior to being sent to the International Exhibition of Inventions about to be held in London), it will be seen that the continuous regenerative gas-kiln under consideration is really a series—or, better still, two series—of separate kilns or firing-chambers which are well seen in the ground plan (Fig. 1, Plate XVIII.) That plan, taken along with Figs. 2, 3, 4 (Plate XVIII.), shows that there are two parallel masses of brick-work about 24 feet apart, each of which contains five separate firing-chambers, which are all connected with each other in a series, by means of flues situated underneath the floors and in the walls of the individual chambers. These flues are for conveying the gaseous fuel from the gas-producer and the air which is to be used in its combustion wherever it is required. Situated opposite the middle of the 24-feet open space, and at a short distance outside, there are seen represented the two gas-producers with their overhead effluent tubes, and the latter are seen to terminate in a series of underground flues, which again terminate in the individual firing-chambers. There are likewise shown on Fig. 1, Plate XVIII., a number of other flues which terminate in two common underground passages by means of which the waste gases, after having done their regenerating work in the way of yielding up their surplus heat to the incoming air, pass into the chimney stack. It may here be mentioned that the stalk at the "Star" Works, and which is shown in the model, and its position indicated in the ground plan, is about 120 feet in height, which (in addition to doing other work) is quite sufficient to produce a good draught. If a blower be used for the air, the chimney may be dispensed with, or one not so high may be employed. Still referring to Fig. 1, Plate XVIII., it may be observed that right over each individual gas flue leading to its respective firing-chamber, there is placed a valve for controlling and regulating the amount of gas passing into any chamber. Then, again, there are provided dampers for keeping the currents of entering air and effluent waste gas under the most perfect control. To an observer who sees this kiln in operation for the first time and can appreciate its merits, it would almost seem as if the ultimate effect of all the nicely harmonised arrangements were even more beautiful and scientifically perfect than the original conception of the inventor could have been.

It is perhaps scarcely necessary to give a detailed series of dimensions bearing upon the construction of one of these regenerative gas-kilns, but a few such data may be mentioned. The extreme length of each mass of brickwork containing five firing-chambers is 69 feet; the length, height, and width of the chambers internally are, respectively, 17 feet, 11½ feet, and 10½ feet; and the internal capacity of each chamber is equal to about 13,000 or 14,000 bricks—the number varying according to their size and shape. Such experience as has now been gained at Glenboig shows that it is possible by means of a set of ten chambers arranged according to the plans in the diagrams to fire 300,000 bricks per month. By reference to Fig. 2, Plate XVIII., which shows the end elevation of one complete kiln, or set of ten firing-chambers, it will be seen that the open 24-feet space, is covered in by means of a light iron roof, so that it is possible to carry on all the operations of charging and drawing, "steaming" and heating-up, regenerating, firing, and cooling-down, &c., in any kind of weather. It may also be noticed that over the space just referred to and over the two series of firing-chambers there is a floor, formed partly of wood and partly of iron, which is used as a drying stove, and on which the moulders pursue their business of brick-making so long as there is any room for doing so. This floor is admirably suited for drying purposes, as most of the waste heat that escapes from the kilns by radiation into the air is here utilised in this way.

A brief account of the way in which these firing chambers are employed in continuous series may now be given. Let us assume that two of the chambers have been burned off, say, Nos. 1 & 2 on the ground plan. The current of gas from the gas-producers, at a temperature of from 600 degrees to 800 degrees Fahrenheit, is turned on to No. 3 chamber, which, up to the present, may be regarded as being a "green kiln," one in which no distinct combustion of gas has yet taken place. The stream of air necessary for the burning of the current of gas, now directed into No. 3 chamber, is made to pass through the mass of finished brick in what we may call the burned-off kilns. Such kilns are the very best regenerators that it is possible to conceive of, one of extraordinary efficiency, and a "green kiln," properly so called, is the most natural recipient and store-room of what would, under other circumstances, be waste heat. But the term "waste heat" in connection with the Glenboig regenerative gas kiln is almost, if not quite, a misnomer, as there is practically no heat allowed to escape into the atmosphere without doing its allotted work, such as regenerating and "steaming" within the kilns, drying green bricks above them, or producing an ascensional current in the chimney stack. It may be taken as a sort of fixed rule in the working of these kilns that the working chamber—that is to say, the one on "full fire"—always has one or two, and sometimes even as many as three, burned off chambers in its rear in the series and a "green" chamber on the other side. In its passage through the regenerator the stream of air is soon raised to a brilliant steel-melting heat, and that is by-and-by imparted to the mass of bricks in the chamber which is now passing through the stage of "full firing"—an operation that is accomplished in from 24 to 36 hours. But while the last-named operation is in progress the next chamber in the series, No. 4, is in its turn made the recipient of the heat which is carried over by the effluent gases from the chambers where the producer gas is actually undergoing combustion; and in this way its contained bricks may become not only dried to perfection, but even heated up to redness, which is more or less bright on the side next to No. 3 chamber, though of a dull red on the opposite side. When bricks are stacked in any of these firing chambers, even though apparently dry, they always contain a certain amount of moisture which has to be driven off in the stage called "steaming" prior to that of full firing. In the ordinary course of things, the next chamber in the series, No. 5, is at this time the "steaming" chamber, and that operation may be effected by passing hot air into it from, say, No. 4 chamber, or it may be done by means of a jet of gas direct from the producer, so as thereby not to interfere with the kilns or chambers that are on full fire. The vapour as it is dispelled from the green bricks makes its escape by means of a number of openings in the roof, one of which is indicated at E in Fig. 3. These openings are only used when a chamber is undergoing "steaming" or being cooled down for drawing; when full firing or regeneration is in progress they are made as close as possible.

Hitherto I have omitted to state how the gas and air find admission into and egress from the individual firing chambers, ten in all, and in two series of five each. The gas valves indicated on the ground plan, and in vertical section in Fig. 2, can be used at pleasure to admit gas to any chamber by means of the underground flues, shown by dotted lines in Figs. 1 and 2, and in section at A in Fig. 3. By means of the same valves the admission of gas may be entirely cut off, or the amount of the current may be adjusted with the greatest nicety to meet the circumstances of the case. The gas passes from the flue into the burner, marked B in Fig. 4, and it ascends into the chamber by a series of openings immediately in front of the dividing wall of brickwork. What has just been called the "burner" is really a space of about 18 inches that is left between the partition wall and the mass of bricks to be fired when the latter are being charged. It extends all the way from side to side of the firing chamber. The air required for the combustion of the gas, and which is brought in a highly heated condition from the regenerator, passes through the floor of the kiln immediately on the other side of the partition wall, by means of a series of slits in the brickwork, into another flue, shown at F in the same Fig. Along this part of the dividing wall there are numerous small apertures for the exit of the air from the flue into the firing chambers. The hot air and gas meet in numerous streams at or near the floor level of the chamber, the resultant effect being most thorough combustion, followed by intense heat which is eventually raised to that required in steel-melting. Great sheets of flame pass upwards through the space above the so called burner, and which space may fittingly be termed a heat-radiating chamber or space—much of the value of the

Glenboig kiln being doubtless due to the great amount of radiation which proceeds from the wall forming the permanent portion of the burning chamber. Then, again, the arched crown of the kiln also forms a most valuable heat-radiating surface. And here it may be well to mention that the space between the arched roof of the kiln and the mass of bricks being fired goes on increasing up to the stage of full firing, owing to the shrinkage or contraction in the bricks to the extent of about one-twelfth of their bulk. With the formation of such a large space above the mass of bricks, the radiant heat has an opportunity of exerting its full measure of effect. The flames and highly-heated gases in their upward passage swirl over the top and through amongst the bricks, the effluent or so-called waste gases eventually finding their way to the floor of the chamber on the opposite or exit side, where there are numerous slits through which the gases pass down into the flue marked C in Fig. 4, and from which they may proceed either into the next chamber of the series or direct to the chimney, as may be desired. By means of the gas valves already spoken of, and movable dampers working in the flues marked F, the gas and air to be admitted into any chamber are under the most perfect control, as they may be decreased or diminished in quantity at will, and may be so proportioned as to give any quality of flame required. At D, in Fig. 4, there is shown another flue about half-way up in the dividing wall. This may be used to draw air from one chamber to another at a higher level, thus effectually exhausting the heat of the burned-off kiln and, at the same time, shifting the intensity of the heat nearer to the back part of the burning kiln or chamber. When it is required, the same flue (D) may also be used to admit cold air (by a simple arrangement of dampers) in sufficient quantity to mellow or tone down the intense heat of the front, and permit of the back part of the kiln being hard burned without injuring the front bricks.

Up to the present, the description of the mode of working the Glenboig gas kiln has scarcely dealt with more than one series of chambers, forming half of the complete kiln. But those of the other series may be regarded as having been in the various stages of cooling-down, drawing, re-filling, &c. As will be seen by again referring to Fig. 1, Plate XVIII., there are underground passages or flues which give communication between the respective end chambers —No. 5 with No. 6, and No. 10 with No. 1. In this way the two sets of chambers are made quite continuous, so that practically there is a circle in which neither terminal nor commencing chamber occurs.

In devising and working out his gas kiln to be a practical success, Mr Dunnachie freely admits that he has followed the lines of Siemens and Hoffman, neither of whom was successful in producing a kiln that should be fired with gas, worked on the heat-regenerative principle, and be continuous in its action. The former tried his hand most anxiously in the direction indicated, but he failed in his efforts, and departed from the idea, under the belief that it could not be realised. His kiln had not the means of keeping up the heat of the regenerator chamber to the high point necessary for completing the full-firing operation. A nice white heat is needed to finish off the burning, thereby requiring a high heat in the regenerator, which is amply provided by Mr Dunnachie's method. One of the chief causes of failure on the part of every person who attempted to burn bricks by the use of gas before the Glenboig kiln was brought to a practical success, was a want of the proper distribution of gas and air throughout the burning chamber, as also a proper admixture of the gas and hot air at every point. At one place the bricks would be roasted, while at another they would be under-burned; in the Glenboig kiln, on the other hand, there is no burning in streaks of hard and soft bricks, as there is an even distribution of heat throughout the whole mass of the bricks under fire. The Hoffman kiln is continuous in its action, and is worked on the principle of heat-regeneration, but the fuel used in it is small coal, which requires to be fed in from the top. No doubt, the kiln in question is very serviceable where the bricks can be burned by employing a moderate heat; but it would not serve for burning refractory fire-bricks, which require a high melting heat. One disadvantage attending the use of the Hoffman kiln is the tendency which certain earthy constituents of the coal have to form fusible silicates with the clay, many of the bricks being wasted in the burning operation by being fluxed. That difficulty never arises where gaseous fuel is used. Owing to the fact that the Glenboig kiln is under such perfect control, it may be advantageously employed in burning the most refractory fire-bricks, even ganister bricks, and down to common red bricks. While speaking of the fluxing of bricks by the use of solid fuel in ordinary kilns, it may be stated that the walls themselves also suffer seriously from the same cause, whereas the kiln under consideration seems scarcely to suffer at all from tear and wear.

In addition to the extensive adoption of this new kiln at the Glenboig Company's own works, progress has been, or is being, made in the way of adopting it elsewhere, the interest excited in regard to it being very great, as is evidenced by the fact that almost all the leading fire-brick manufacturers of the kingdom have either visited the works themselves or have sent responsible representatives to inspect the new kiln in operation. It is in use at Garnkirk burning fire-clay goods. At Tamworth, it is being used for the well-known Staffordshire blue bricks, and in this case there are some facts of very special interest, as they show how the kiln can be adapted to new circumstances. The raw coal is distilled or carbonised in ordinary gas retorts, and the by-products are collected and subsequently treated separately, while the gas which is obtained is used in firing the brick-kiln. It is got without the use of a separate producer, and as it contains no air it has probably five times the calorific or fuel efficiency of ordinary producer gas. The residual coke that is drawn from the retorts is a good marketable commodity. As evidence of the confidence which the proprietor of the Tamworth Coke Works has in the efficiency and economy of the Glenboig gas kiln, it may be stated that he has recently completed negotiations for the erection of another complete kiln of ten chambers. In another kiln of the same sort erected at Sheffield, Messrs Lowood & Co. are successfully firing their famous ganister bricks. Messrs Henry Sharp, Jones, & Co., of Poole, in Dorsetshire, are now erecting a set of ten chambers for firing sewerage pipes. Not only is the kiln in this instance identical with that first erected at the "Star" Fire-brick Works, but it has, in addition, an arrangement for the economical introduction of the common salt required for glazing the pipes. A kiln such as we are speaking of was erected sometime ago at the Rutherglen Pottery, the proprietor of which declined to accede to the suggestion of the patentee to construct a muffle within each chamber so that the ware in course of being fired might be completely protected from the direct action of the flame and heated gases passing through the chamber. As might have been expected, the ware, coated with its delicate glazes, did not "stand fire" under such conditions, and the use of the kiln for firing pottery was suspended; but as Mr Dunnachie is confident of the ultimate success of the kiln for firing either earthenware or porcelain, when his valuable suggestion as to the adoption of a muffle is acceded to, he does not regard the Rutherglen example of his invention as having been "put on the shelf" in perpetuity, but simply as being in abeyance in the meantime. The peculiar adaptability of the Glenboig kiln is abundantly shown by the great range of firing temperatures that may be got in it, extending from that which suffices for the Tamworth blue bricks, which is far below that required for burning fire-bricks, up to that which is needed for Sheffield ganister bricks—being almost as great an extreme in the other direction.

As an invention in connection with sanitary improvement in industrial districts, the Glenboig gas kiln ought to take a very high place; indeed, with smoke-prevention advocates, it has already gained such a position, from the fact that its general adoption in brick-making and pottery districts would reduce to a minimum smoke nuisance in many places that have acquired an unenviable notoriety for polluting the atmosphere. There is no breach of confidence in saying that the Duke of Sutherland, Lord Wharncliffe, Sir Thomas Brassey, M.P., and many other persons of greater or less eminence, and who have large industrial interests at stake, are now giving attention to the Glenboig gas kiln, on account, in some measure, of its intimate connection with the prevention of smoke.

There are many directions in which the use of this kiln is attended with economical results. Some of these have already been incidentally mentioned or alluded to, yet still one more may be adduced; it is in the kind of coal from which the required gaseous fuel may be obtained, for even the commonest or least valuable slack or dross amply suffices as the source of the gas. Furthermore, if we take weight for weight, a very much less quantity of it is needed when compared with what is necessary to do the same amount of work in an ordinary coal-fired kiln, in which, by the way, good round coal at a high price has often to be used.

It will serve a good purpose if I now lay before the members of the Institution one or two most reliable facts bearing on the relative fuel-economy of this kiln. At the request of the Directors of the Glenboig Union Fire-Clay Company, the managers of the several works recently made careful observations, and without any collusion with each other, in regard to the consumption of coal for firing purposes, over a period of some six or seven weeks, with the different kinds of kilns in use. The data obtained from the separate reports of the managers show that the average cost of fuel used by the Newcastle kiln was 8s 2d per 1000 bricks burned; that the cost of fuel used in the hopper kiln, invented by Mr Dunnachie about twenty years ago, was an average of 6s 6½d per 1000 bricks; and that the average cost of the fuel used in the form of gas, in what has already been called the Glenboig kiln, did not exceed 2s 9½d per 1000 bricks burned.

Mr Frederick Siemens, who was long and intimately associated with his distinguished brother, the late Sir William Siemens, in connection with heat-regenerative furnaces, &c., has become so profoundly impressed with the merits of this gas kiln that negotiations are in progress between his firm and the patentee, with the view of the former undertaking its introduction into various industrial districts at home and abroad, in conjunction with the newest form of Siemens gas-producer, which is doubtless well-known to many of the members.

Referring with a little more detail to this producer, it may be said that a jet of steam is introduced into the generator chamber in order to give the gas a slight "push," though it is stated that experience gained in some recent cases, shows that such a device is not actually necessary. In it "clinkering" is reduced to a minimum, if it is not practically altogether *nil;* and as no stoppage is required for cleaning operations, one generator will suffice for a kiln of ten chambers, whereas, up to the present, two producers have always been considered necessary.

In conclusion, I may be permitted to say a few words in regard to the relationship which this gas kiln bears to the Thomas-Gilchrist or basic process of making steel, a process which will doubtless soon bulk largely in this part of the kingdom. After having undertaken the sole manufacture in Scotland of the basic bricks for lining the steel converters, Mr Dunnachie found that an enormous expense would attend the firing of such bricks if raw coal had to be used as the fuel, as they would need quite a steel-melting heat; and it may almost be said that the necessities of the case led to the construction of the continuous regenerative gas kiln treated of in this paper, which is almost the first formal communication that I have had the honour and pleasure of making to the Institution.

In inviting discussion on the paper,

The PRESIDENT said he was sure all present had listened with pleasure to the interesting and instructive communication.

Mr E. KEMP had really great pleasure in listening to the paper on what appeared to be a really efficient invention. At the beginning of the paper it was stated that the kiln practically utilised all the heat that was in the gas, but that there was still sufficient escaping to give draught enough to pull the waste gas up the chimney. Perhaps Mr Mayer could tell them what temperature there was in the chimney to cause that draught, as it must be a percentage of the total heat.

Mr **Mayer** replied, that the chimney at Glenboig was used for other kilns besides the regenerative kiln. Not only did it do the work of that kiln, but also of some of the other kilns in the works, so that the heat in it was not due to the former entirely. He was not aware whether the temperature in the chimney had been tested, but as Mr Dunnachie's son had had three years' training in a laboratory in the City, and was devoting some attention to the production of a really serviceable pyrometer, something would doubtless be done in that direction by-and-by One main point, he repeated, was that practically there was no waste heat.

Mr **Kemp** said from the explanation given it was apparent that the draught in the chimney was kept up by other fires, and not by the escaping gases from the regenerative kiln.

Mr **Mayer** rejoined that the chimney had certainly to do other work.

The **President** recollected well the Hoffmann kiln for burning bricks, which had been erected at Belfast a good many years ago, and which he had frequent opportunities of visiting. The economy of heat in that kiln was remarkable, so much so, in fact, that it became necessary rather to throw away a little more of the heat purposely because the chimney was too cold almost, and the moisture evaporating out of the warmer damp bricks condensed upon the cooler ones, and so the economy was carried a little too far. Probably Mr Mayer had something to tell them on that subject in regard to this new kiln.

Mr **Mayer** said that the arrangements for getting rid of the escaping vapour in the steaming process were very perfect. The chamber in which the steaming operation was carried on was actually cut off from all the others. The vapour escaped through the crown of the chamber by some fifteen openings, and special means were taken to increase the activity of the current from the chamber during the steaming stage.

Mr S. G. G. **Copestake** noticed that in the model shown the crown was semi-circular and in the drawings on the wall somewhat flatter

He wished to know whether or not the shape had anything to do with the effectiveness of the kiln.

Mr MAYER said Mr David Johnston, who was to some extent responsible for the diagrams, might be able to answer that question.

Mr JOHNSTON explained that Fig. 1 was merely a ground plan, but that there was a longitudinal section in Fig. 4, Plate XVIII.

Mr COPESTAKE was anxious to find out if there was anythin special in the shape of the arch.

Mr MAYER did not think there was any essential element in the shape of the arch.

Mr KEMP thought the half-circle was better suited than the other form for the purpose in view.

Mr JOHNSTON wished it to be understood that in the diagrams he had merely followed lithographed drawings, given in the patentee's "blue book."

Mr KEMP was of opinion that the model showed the ordinary arch.

Mr MAYER believed that the model might show a more recent method than the diagrams, and that the semi-circular arch might give better results than the flatter one.

Mr JAMES M. GALE observed that there was less lateral thrust in the semi-circular than in the other form of arch.

Mr GEO. RUSSELL asked if men worked in the drying stove at a place indicated by him? It appeared to be a hot place, especially in summer, with furnaces below, and its roof of iron.

Mr MAYER replied in the affirmative, explaining that he had referred to it as the moulding floor.

The discussion of this paper was resumed on 28th April, 1885.

On the call of the Chairman,

Mr F. W. DICK said he had not been able to be present at the last meeting to hear the paper read, nor had he been able to find time to peruse it since; but he knew something of Mr Dunnachie's plans. Of course anything which utilised heat, instead of allowing it to radiate into space, must be good. On the whole he thought the plan an excellent one, but he would prefer to say nothing more at present, as he was ignorant of the contents of the paper.

Mr HENRY DYER simply wished to remark that as Mr Mayer's paper was descriptive of an invention, which seemed to be correct in principle and successful in practice, there was not much to be said about it in the way of discussion. The chief duty incumbent upon them on that occasion was to thank Mr Mayer for his excellent paper, and to congratulate Mr Dunnachie on his marked success. Perhaps he might be allowed to suggest that a few simple experiments should be made with the oven to ascertain if it was working under the conditions of maximum efficiency. These were—first, that there should be little or no external radiation; second, that the combustible gases should be consumed; and third, that the temperature of the chimney should be just sufficiently high to carry off the waste gases. In the oven described he thought that the chimney served for other purposes, so that it might be somewhat difficult to carry out some of the experiments, but he had no doubt that Mr Dunnachie would be able to ascertain the actual efficiency of his invention. In concluding, Mr Mayer had remarked that this was the first paper he had read before the Institution; but for his own part he hoped, now that the author had made a start, he would give other papers of the same class, especially as the Transactions of the Institution in the past had been somewhat deficient in papers relating to applied chemistry and metallurgy. Of course they knew that Mr Mayer had paid special attention to these subjects, and he would confer a great boon upon the members by supplying them with particulars of recent advances in these departments, especially in the industries connected with Glasgow and neighbourhood.

The CHAIRMAN (Mr C. C. Lindsay, Vice-President) said he had not the pleasure of hearing Mr Mayer read his paper, neither had he read it in the Transactions, but it was a class of paper—bearing upon the economical use of fuel—which he would like to see more of. He was not acquainted with the burning of fire-bricks and pottery as described, but he had some experience of steel making with gaseous fuel, and it seemed to him that Mr Dunnachie had been very successful so far in his work. He hoped Mr Mayer would, as Mr Dyer had suggested, give more papers of a similar class in the future.

Mr MAYER said with regard to the remarks made in the discussion, it was Mr Kemp, he thought, who referred to the chimney at the Glenboig Works doing more than the work of the simple kiln or set of ten chambers to which it was specially attached. In regard to that, Mr Dunnachie had informed him that some of the kilns which had been erected in England were doing work only into one chimney stalk, and that each chimney stalk was doing the work of only one set of chambers, so that doubtless some specific details might be got in the course of the next few months as to the results in one or more of those instances. He had no doubt Mr Dunnachie would be quite willing to receive a Committee of experts from the Institution, and make preparations for them carrying out certain observations, if that should, in the opinion of the Council, be deemed desirable. As Mr Dyer seemed to be much interested in the subject dealt with in the paper, he (Mr Mayer) suggested that that gentleman might act on that Committee. The question had been raised by Mr George Russell about the rather hot quarters that the brick-moulders and their attendants would have, working in the drying stove, shown in Fig. 2, cross section. But it must be remembered, however, that the drying stove was some 60 or 70 feet long, and extended not only over the open space between the two rows of firing chambers, but over those two rows of firing chambers themselves, so that while a drying stove, it could also be fitly employed as a moulding room, for the ten chambers were not all equally hot. The brick-moulder could, therefore, moving his moulding table about at pleasure, always have a cool place to work in, even in the height of summer. Then, lastly, with reference to the point raised by Mr Copestake about the arches of the kiln being flat rather than semi-circular. It would be remembered that the arches were flat in one of the diagrams shown at last meeting and semi-circular in the model, which was now in the Inventions Exhibition in London. In the plans as originally drawn out the arches were semi-circular; but the diagram was copied from the patent specification drawings where they were given flat by mistake by the patent agent without the knowledge of the patentee or inventor. The arches were, therefore, semi-circular from the first, thus securing what Mr Gale desiderated—the utmost strength possible. Personally he could assure them that some of the chambers he had seen lately had borne the test of age and experience without giving way in the slightest particular. In his opinion they had great durability and power of resisting wear and tear. The suggestion Mr Dyer had thrown out would, he hoped, be acted upon, so that the Committee might report to next Session of the Institution what had been done.

Mr Dyer thought Mr Mayer had mistaken his meaning. He did not propose that the Institution should appoint a Committee, because if they once commenced that they would have plenty of work to do. It was Mr Dunnachie's business, he thought, to make a few simple experiments, and give the results to the Institution. Of course, there was nothing wrong in such a proposal, but there were other important matters that equally required investigation at their hands, and if the Council took up such things they would have more than enough to do.

The Chairman asked Mr Mayer if he had anything further to say in view of Mr Dyer's remarks.

Mr Mayer replied that he had nothing to say beyond this, that he was quite willing to bring up a report in the shape of results, tested as far as he was able to test them, and that he would do his best to collect information on the subject in question from those persons whom he had referred to in the paper as using the regenerative gas kiln.

The CHAIRMAN said he had much pleasure in proposing a hearty vote of thanks to Mr Mayer for his paper.

On the Butt Fastenings of Iron Vessels.

By Mr Staveley Taylor.

SEE PLATE XIX.

Received 23rd February; Read 24th March, 1885.

My desire in this paper is to draw attention briefly to the question of butt fastenings as at present ordinarily applied in the structure of iron vessels.

In introducing this subject, I am sensible that it is not altogether new. As, however, there have not recently been many papers before this Institution dealing directly with the practical questions of ship construction embraced by this subject, I trust the matter may still prove of some interest. For myself, I believe it to be *the vital point* as regards procuring a perfect structure, and I hope therefore it will be thought worthy of further consideration.

The main object, from a commercial point of view, in the design of any iron structure, ought to be to get a maximum strength on a minimum of material. In a merchant ship, where weight of hull bears such a direct relation to the freight-earning capabilities of the vessel, the item of weight is a primary factor to be considered.

My contention is, that if we are to get the full value of the weight of material employed in building a ship, some reformation is wanted in the custom of securing butts from that at present observed in shipbuilding practice, and this to the effect of getting *increased fastening.*

I do not think it is an exaggeration to say that, after a little service, a considerable proportion of vessels, particularly those of large dimensions, "show the butts" of their shell plating amidships.

Observation at the graving docks and slipways of our various ports makes this only too obvious. If these ships could be built of the same scantling, or perhaps a considerable percentage less, and made a whole jointless mass, their efficiency and immunity from fault of this nature would be unquestionable; but as the weakest part is the index of the whole strength, the butt gives way—it may be merely to the extent of being "paint cracked"—but, whatever the extent, it is undoubtedly due to a weakness, local or otherwise, which, showing first at the butt, proves it to be the most vulnerable part.

I do not assert that this "showing" of butts is a really dangerous defect. Many vessels whose butts show perhaps badly after their first voyage, if carefully treated never subsequently appear to get any worse, their structural strength, as a whole, being ample to resist further aggressions and to meet all work to be done. There are, however, many cases where this showing of butts is first observed, in which the vessel is either doubled or strengthened and loaded with additional weight, much to the commercial disadvantage of the ship.

It is manifestly evident that to load a ship with great weight of scantling with the view of gaining increased strength is in principle wrong, unless the fastenings are correspondingly increased in proportion to the added section. A ship may be, and many are, built with excessive and extra weight of material and insufficient *proportionate* butt fastenings. Such vessels may do their work well, but this can only be due to the fact that the strength at the weakest point is sufficient to do the work required, and all weight above this requisite working strength is carried uselessly and to the exclusion of freight-earning cargo.

It has been frequently demonstrated that chain riveting is the most efficient method of butt connection. Assumed, then, that a butt is built in this manner, with the most approved proportions of rivet area to plate section, and together with good workmanship made as efficient as the means will admit. Such butt if subjected to a steady tensile strain would probably bear without fault a much greater strain than we can imagine it would ever have to meet in its place as part of a ship's structure; but the fact remains that ships are built with such well-arranged butts, yet still give trouble in the manner indicated. The cause I assume to be—where it cannot palpably be traced to inherent structural weakness—a local panting, a continual tremor or vibration, due to the sudden varying strains a ship is subject to in a seaway; to the working of heavy machinery, or to successive blows from waves and heavy seas. This tremor, reverberating on each plate as an individual and separate mass in itself, must be exhausted at the extremities. So the movement thus created—even in the closest fitting butt—has a tendency to start the caulking and cause the defect complained of to be observed; leakage and consequent corrosion unavoidably follows. In badly fitting butts, though they may only happen to be a small degree open when originally riveted, this action must operate more quickly, and the fault consequently becomes aggravated. Perhaps, on the whole, steamers are more readily affected in both these respects than sailing vessels, their dimensional proportions and motions at sea being more severe—the vibration from their machinery in itself, in some cases, being no doubt sufficient to cause the defect without cooperation from other causes.

Upon the assumption of butts cracking from this latter cause, it may be argued that the same defect should also show at the landings, and that no plate butt, however well proportioned, will ever and at all times be safe from the defect. No doubt both forces more frequently, and in a greater or less degree, are acting simultaneously; but it will be obvious that the strains affecting the plating will generally be much more severe upon the ends than upon the edges of the plates, and that any vibration will also be more acute at the termination of the greatest dimension or length of the plate rather than at the smaller dimension of breadth. The disposition of the caulking, being upon the edge of the plate, in the case of the landings, may probably have a better effect to resist movement from vibration than face caulking has.

I have, therefore, to urge the fitting of double or outside and inside butt straps as a remedy for the evil, and as a simple means of securing increased strength and efficiency without serious increase of weight. The proposal can, of course, have no claim for originality. Indeed it is mostly upon old ships that we find double straps fitted, they having either been put on subsequent to the vessel's completion to cover defective butts arising from the causes named, or in some isolated instances, perhaps, fitted originally with a view of securing greater efficiency. Lapped joints are also in a few cases to be met with, in fact the practice is at the present time still occasionally adopted by some East coast builders. This is not, perhaps, a system that it would be advisable to adopt throughout in a ship's structure; it might, however, be used for some parts of the plating and some inside work, but it can in no case be considered so effective as double or even single straps, the strength of the butt under conditions both of tension and compression being dependent alone upon the shearing resistance of the rivets. This may, however, be somewhat equalised by increasing the rivet area.

The main objection to both practices is, I suppose, "unsightliness." There will also be the objection of increased friction or resistance due to the projecting strap edges, and the liability of the straps—particularly those of the outside strakes—to be chafed against dock walls, &c. I do not, however, think any of these worthy of great weight when the advantages gained are considered. If by any means butts of shell plating can be made never to require attention after a ship is once built, and when the saving of trouble and expense this would be each time a vessel is docked is realised, the objection of unsightliness will, I think, soon be banished, and I take this to be the most general objection. To what actual extent frictional resistance would be increased by outside straps is a point upon which any information would be valuable.

There can be no doubt that the simple transfer of the caulking from the face of the butt to the edge of the butt strap, which outside straps would involve, would do much to dispose of the evil of "paint cracked" butts; and thus, apart from strength, outside instead of inside straps would be a direct advantage. This advantage would also follow the adoption of over-lapped butts.

The outside straps need not of necessity be fitted fore-and-aft over the whole plating; in most cases, probably a limited distance amidships would be quite sufficient. Nor need the outside straps be made thicker than from one-half to two-thirds the thickness of the plating itself, with the edges of the strap chamfered down to a bare caulking edge. In treble-riveted straps, as the pitch of rivets in the back row is too wide to ensure a firm caulking edge for the outside strap, the cover strap need only extend to the second row of rivets on each side of the butt.

The systems of double straps or over-lapped butts would apply to the ordinary in-and-out and clinker method of plating mostly in vogue; but the system of flush edge-and-edge plating, with outside cover seam strips, which is now becoming so general, might be arranged in a different manner. This method of plating cannot be too strongly recognised as the best that has yet been devised to equalise the strains both on plates and rivets, though it unfortunately has a disadvantage in these days of competitive economy of being more expensive than the common practice. I would suggest for this system a slight departure from the usual custom of butt fastenings, and it is merely to leave the butts of the outside seam strips about three-quarters of an inch wide, or sufficiently far apart to admit of the plate ends being caulked against the edge-and-edge plating, the usual straps of course being fitted inside. The butts of the flush edge-and-edge inside plating to have double straps. With this arrangement no butts ought ever to give trouble, provided the whole structural strength is sufficient.

I do not suppose I shall get many to agree in advocating a reduction of scantling from that at present in use, and in this matter I refer solely to the requirements of the various classification Registries. It is not a question to be only casually treated, and I do not propose in this paper to trespass very far. The two subjects are no doubt very closely allied, but I will now only venture an opinion, that with more attention to the means of fastening the many parts of a ship's structure together, and a more careful distribution of material, some reductions might be considered. The tendency, however, lately has been all in the way of increase and greater weight. It is not asserted that while these increases in scantling have been made that butt connection has been entirely ignored. Moderate increases have been enforced in this respect also, but had these increases in riveting requirements been made earlier, the necessity for additional weight would probably never have arisen. I may here mention that Mr Henry H. West, chief surveyor to the Liverpool Underwriters' Registry, has called special attention to this matter in a paper read by him before the Institute of Naval Architects last year.

In the consideration of reduced scantling the element of "stiffness" has to be taken into account, and it is said that this has been dangerously encroached upon in some steel vessels built with the reduced scantling sanctioned for that material. This is an important point and one to be duly guarded against, but I look almost entirely to better butt fastenings as the most immediate means of balancing any apparent difficulties in the way of reducing weight of scantling. What the particular observed results of this flexibility have been I have not been able to learn; any details would be of interest.

It is at all events an anomalism that rules should be framed to specify certain sizes of material for a certain ship and certain butt riveting, say double for this required thickness, but in the case of another ship, whose scantling is by rule the same, additional riveting—treble or quadruple—is required on account of some difference in the ratio of her proportions of length, breadth, and depth. The section of each plate used is the same in each case; why its full strength—represented in the latter case, we must presume, by the treble or quadruple riveting—should not be maintained in the first case is a palpable inconsistency. If it is not required in the one, then obviously the scantling may be reduced more proportionate to the strength of the butt, and retained, if it is needed, in the other. To some extent, no doubt, this will be found in the application of the rules to adjust itself, but there are many possible and actual instances where such adjustment does not occur. The inconsistency in these cases can only be condemned. A similar discrepancy also occurs in using the same size of rivet for varying thicknesses of plate; in this case the rivet area remains constant while the plate section increases with each thickness. The question of varying the size of rivets involves practical difficulties which it is perhaps better, if possible, to try and avoid.

In order to show the relative proportions which rivet area bears to plate section in butts, double, treble, and quadruple riveted, of plates varying in thickness and width, tables giving these results are annexed hereto. See Tables I., II. and III., Cols. 11 and 12. Also in Cols. 18 and 19 are given the percentages of strength of plate over rivets, or rivets over plate, as the case may be, calculated upon the basis of 20 tons as the breaking strain per square inch of the plate, and 18 tons as the shearing strain of the rivet. In these calculated results the full effective value of the rivet hammered—that is, $\frac{1}{16}$th of an inch larger than the cold rivet—has been taken and 17 per cent., or $\frac{1}{6}$th has been assumed as a fair average to be added to the section punched out of plate at row of rivet holes for a good countersink. A countersink with a face diameter of one and a-half times the diameter of the rivet will be equal to 25 per cent. more than a parallel hole, and 17 per cent. may therefore be considered too low an allowance. This factor was determined upon after measuring the countersinks adopted by a number of builders, so that it may be considered very closely to represent the general practice. As, however, in these calculated results the per centage has been admitted over the whole row of rivet holes, assuming them all to be countersunk, which will not actually be the case as regards the landing holes for an inside strake, the proportion will be somewhat raised.

The arrangement of butt rivets, upon which the tables are based, is such as would be adopted on the inside shell plate of a vessel's hull (see Figs. 1, 2 and 3, Plate XIX.), and the widths of plates comprise those most commonly in use. The exact rule spacing of four diameters cannot, of course, be correctly applied for constant widths of plate and varying diameters of rivet. In Tables I., II., and III. the widths are made to suit the exact four diameter spacing. Where this cannot absolutely be applied the nearest practicable division has been made; but it will readily be seen that any increase in the width of plate, however slight, will affect the rivet area adversely to the comparison of plate section, without materially affecting the rivet spacing.

In Table IV. a constant width of plate of 48 inches has been taken, and results given for the nearest division of rivet spacing, both above and below the four diameter spacing, the results show differences varying from 6 to 8 per cent. in the value of the rivet areas.

For $\frac{9}{16}''$ and $\frac{12}{16}''$ plates the Underwriters' Registry require $\frac{13}{16}''$ and 1" rivets respectively, against $\frac{3}{4}''$ and $\frac{7}{8}''$ required by Lloyds'. These distinctions are given in the tables, and the result appears all in favour of the larger rivets. With these exceptions, and within the compass of the tables, the requirements of both societies are the same.

The inside plate having a completely strapped butt, has been chosen for comparison in preference to the outside plate, whose butt strap—extending only from edge to edge of the adjoining inside strakes—is incomplete, the rivet strength of the butt, consequently, is less than that of the inside strake.

The rivet area, therefore, in this comparison, appears more favourably than would be the case at any other butt strap in the ship.

In these considerations, assuming the strap for treble riveting to be $\frac{1}{16}$ of an inch thicker than the plate connected, and for quadruple riveting to be $\frac{2}{16}$ thicker, the butt strap may be ignored as the conditions are either equal or favourable to the strap.

The values arrived at in the tables, beyond and including col. 13, are not advanced as final; they can only be considered as purely theoretical, and determine merely the apparent results for the values of material used in the ratio upon which they are based.

The true shearing stress of the rivets and the tearing strain of the plate can perhaps never be accurately ascertained for such butts as we are here dealing with, on account of the impossibility of distributing the load equally over the whole area of material under stress. The friction of surfaces, as affecting the result, is also an element upon which very little is known.

The tables require little further explanation. From the results it may be deduced generally that the smaller thicknesses of plate, say up to $\frac{8}{16}$, may be safely fastened at their butts with double riveted straps; that plates from $\frac{9}{16}$ to $\frac{11}{16}$, except for excessive widths, ought to be treble riveted; and that plates of $\frac{12}{16}$ and above, and of excessive width above $\frac{10}{16}$, ought to be quadruple riveted. By fitting double straps these limits might be considerably increased.

On comparing col. 16 of Tables I. and II., it will be seen that the relative result of butt strength for double and treble riveting is actually the same, because in the treble butt the strength of the plate at AB is increased by the shearing resistance of the back row of rivets, CD (Fig. 2), and the total shearing strength of the rivets is only increased to a like extent over the double riveting. The percentage of strength is, of course, in favour of the treble riveting, as the value respectively of the plate and rivets is increased, though the nett result appears the same.

If the full complement of rivets be inserted in the back row, the rivet value becomes increased to the extent of one-half more than the double riveting, while the plate strength remains constant. But by ordinary treble riveting the strength of the plate at AB, plus the back row of rivets, approximates very closely to the strength of the plate through the line of frame holes YZ, which is the normal strength of plate (see cols. 14 and 21, in Table II.): therefore, by increasing the back row of rivets to a complete row, we at the same time reduce the plate strength and gain no advantage. Consequently, we must look to increasing the rivet value without interfering with the plate strength, and this can only be done, either by adding an entire extra row of rivets, as in quadruple riveting, or by putting the rivets in double shear by using double butt straps. A difficulty in the way of the general adoption of quadruple riveted butts arises from the fact that the increased width of straps necessary will entail a somewhat wider spacing of frames, particularly for the larger sizes of rivets.

It may be urged that double straps mean additional expense, but any small apparent outlay in this respect will be more than met by the saving in weight which must follow any system which establishes a normal strength on a uniform basis throughout the whole structure. The efficiency thus secured will in itself be synonymous with economy.

With attention to this matter iron may still perhaps be considered to compete more favourably with steel; for in steel riveting where the shearing resistance of rivet steel falls so much below the tensile strength of steel plates, the plate limits for riveting, as given above, would require to be reduced proportionately, unless the rivet area be increased in a corresponding ratio. This discrepancy in the shearing resistance of iron and steel rivets, compared with the tensile strength of their respective plates, when taken full advantage of, puts the two materials, considered as constructive competitors, on a slightly more uniform footing.

Table V. records the results of some shearing tests on rivets in single and double shear, comprising lap joints, butt joints with single cover straps, and also with double cover straps. The mean of the single shear tests is 17·56 tons per square inch of rivet area for hand riveting, and of the double shear, taking the single area of rivet only, 31·6 tons, which gives a value in shearing resistance to the rivet in double shear, rivet for rivet, of 80 per cent. more than the rivet in single shear. The corresponding figures for hydraulic riveting are 19·13 tons, 33·5 tons, and 75 per cent. respectively.

Endeavour was made with some of the samples to observe the exact portion of the load at which slipping commenced, and this was found to vary for hand riveting from about 10½ to 33⅓ per cent. of the total stress, and for hydraulic riveting from about 27 per cent. to 40 per cent.—a very wide divergence indeed.

Professor Kennedy, in his investigations into the strength of riveted joints of steel plates and steel rivets, states that by the aid of a magnifying glass, slip was observed to commence at about one-tenth of the breaking load, and that it was visible at about one-fourth of the load.

There is an average difference between the shearing results of the rivets riveted by hydraulic power and those riveted by hand, favourable to the former to the extent of from one to two tons. This, is no doubt due to increased friction, and to the holes being better filled by the greater pressure brought to bear upon the rivet, which tends to prove the great superiority of power riveting over hand work. It must also have been due to some varying in the effectiveness of the riveted work which caused the proportion of load at which slip occurred to be so irregular.

One point is brought out prominently in considering the small percentage of the ultimate shearing stress at which slip commences; and it is, that at comparatively low strains it is a reasonable inference that slip will occur to an extent sufficient to start the slight face caulking which an ordinary shell butt receives, and so permit leakage and corrosion to begin. Also, that this will occur without materially affecting the rivets under strain; for, after slip was observed in the test samples to have begun, the weight was removed and the rivets found to be perfectly sound—the elasticity in the material and joint having recovered itself on the strain being relieved. This was especially noticeable in the pieces riveted by hydraulic power, for in some instances as much as 70 per cent. to 80 per cent. of the total load was applied—the slip measuring about one-eighth of an inch—and the rivets on being tested after the weight was removed were found sound. In the hand-riveting the rivets were generally slack after about 50 per cent. to 60 per cent. of the load was applied.

Under ordinary circumstances, it is a remote supposition that the rivets in a butt of shell plating would shear or the plate fracture, except at the sheer strake where the top edge of the plate is unsupported. In the body of a ship each plate is so supported and assisted by the adjoining plates and overlaps of the landings, that any failure must be general. Indeed, from actual fact, it must be admitted that we more frequently find in cases of absolute failure that the plate has torn while the rivets remain intact; and if this is accepted as final proof that the present practice is sufficient, the object of this paper is useless. But I submit there is the danger of the rivets in a weak butt working adrift or admitting of slip in the butt, sufficient to entirely destroy the real value of the riveted work, as constituting a solid joint, and so to throw an undue and intensified strain on the plates immediately adjoining. With strains ever varying in point and direction, such as we have to contend with in designing the structure of a ship, I think that it is desirable we should endeavour to increase the margin of safety as much as is consistent, and to make the structure as complete as possible by treating each plate and point of connection upon its own individual merits.

I would suggest, before closing this subject, that the riveting in the fore-and-aft plate landings in many cases is out of proportion to the riveting requirements for the butts, and might with prudence be somewhat reduced.

Fig. 4 (see Plate XIX.) is a scale which shows at a glance the rivet area in square inches, corresponding to any plate section up to 60 square inches, in the proportion of 20 tons, as the tensile strength per square inch of the plate and 18 tons as the shearing strength of the rivet.

To sum up the whole, my conclusions are that butt riveting should be wholly dependant upon the thickness or sectional area of the plates connected, and not upon any dimensions or proportions of the vessel; that butt riveting in relation to the thickness or section of plate ought to be considerably increased from present practice; and that the most efficient method of securing strength of joint is by using double butt straps.

Width in inches × Thickness „ of Plate.	Diameter of Rivet cold.	Diam. of Rivet hammered.	Spacing of Rivets, centre to centre.	Rule spacing of Rivets, centre to centre.	Greatest number of Rivets in one row.	Area punched out of plate at one row of rivet holes in butt, 17 % added for countersinks.	Effective sectional area of Plate.	Total number of Rivets in Butt.	Total effective Rivet area in Butt.	Percentage of excess Plate section.	Percentage of excess Rivet area.	Strength of plate at row of Rivet holes, at 20 tons per square inch.	Shearing strength of total Rivets at 18 tons per sq. inch.	Excess of Plate strength in tons.	Excess of Rivet strength in tons.	Percentage of excess Plate strength.	Percentage of excess Rivet strength.	
	inches	inches	inches	inches		sq. in.	sq. in.		sq. in.	%	%	tons	tons	tons		%	%	
36½ × 8/16	¾	·81	3·	3·	13	6·13	12·12	26	13·26		9·4	242	239	3		1·2		
36 × 10/16	⅞	·94	3·53	3·5	11	7·53	14·97	22	15·18		1·4	299	273	26		9·5		
36 × 12/16	⅞	·94	3·5	3·5	11	9·04	17·96	22	15·18	18·2		359	273	86		31·5		Lloyd's
36 × 12/16	1	1·06	3·9	4·	10	9·27	17·73	20	17·6	·7		355	317	38		12·		Liverpool
36½ × 14/16	1	1·06	4·	4·	10	10·81	21·12	20	17·6	20·0		422	317	105		31·1		
37 × 1	1⅛	1·19	4·5	4·5	9	12·49	24·51	18	19·8	23·8		490	356	134		37·6		
42½ × 8/16	¾	·81	3·	3·	15	7·08	14·17	30	15·3		7·9	283	275	8		2·9		
43 × 10/16	⅞	·94	3·5	3·5	13	8·9	17·97	26	17·94			359	323	36		11·1		
42 × 12/16	⅞	·94	3·42	3·5	13	10·68	20·82	26	17·94	16·1		416	323	93		28·8		Lloyd's
42 × 12/16	1	1·06	4·15	4·	11	10·19	21·31	22	19·36	10·		426	348	78		22·4		Liverpool
41 × 14/16	1	1·06	4·02	4·	11	11·9	23·97	22	19·36	23·8		479	348	131		37·6		
41½ × 1	1⅛	1·19	4·5	4·5	10	13·88	27·62	20	22·	25·5		552	396	156		39·4		
48½ × 8/16	¾	·81	3·	3·	17	8·02	16·23	34	17·34		6·8	325	312	13		4·1		
46½ × 10/16	⅞	·94	3·5	3·5	14	9·57	19·49	28	19·32	·9		389	348	41		11·7		
48 × 12/16	⅞	·94	3·65	3·5	14	11·51	24·49	28	19·32	26·7		490	348	142		40·8		Lloyd's
48 × 12/16	1	1·06	3·92	4·	13	12·05	23·95	26	22·88	4·6		479	412	67		16·2		Liverpool
45 × 14/16	1	1·06	4·02	4·	12	12·97	26·4	24	21·12	25·		528	380	148		39·		
46 × 1	1⅛	1·19	4·5	4·5	11	15·27	30·73	22	24·2	27·		615	436	179		41·		
54½ × 8/16	¾	·81	3·	3·	19	8·97	18·28	38	19·38		6·	365	349	16		4·6		
53½ × 10/16	⅞	·94	3·5	3·5	16	10·95	22·48	32	22·08	1·81		449	397	52		13·1		
54 × 12/16	⅞	·94	3·55	3·5	16	13·16	27·84	32	22·08	28·8		547	397	150		37·7		Lloyd's
54 × 12/16	1	1·06	4·11	4·	14	12·98	27·52	28	24·64	11·7		550	444	106		23·8		Liverpool
54 × 14/16	1	1·06	4·11	4·	14	15·18	32·07	28	24·64	30·1		641	444	197		44·3		
Col. 1	2	3	4	5	6	7	8	9	10	11	12	13	15	16	17	18	19	

TABLE II.—TREBLE RIVETING.

Width in inches of Plate.	Thickness ” of Plate.	Diameter of Rivet cold.	Diam. of Rivet hammered.	Spacing of Rivets, centre to centre.	Rule spacing of Rivets, centre to centre.	Greatest number of Rivets in one row.	Area punched out of plate at one row of rivet holes in butt, 17 % added for countersinks.	Effective sectional area of Plate.	Total number of Rivets in Butt.	Total effective Rivet area in Butt.	Percentage of excess Plate section.	Percentage of excess Rivet area.	Strength of plate at row of Rivet holes, at 20 tons per square inch.	Ditto, with shearing strength of back row of Rivets at 18 tons per square inch added.	Shearing strength of total Rivets at 18 tons per sq. inch.	Excess of Plate strength in tons.	Excess of Rivet strength in tons.	Percentage of excess Plate strength.	Percentage of excess Rivet strength.		No. of hole at frame.	Strength of Plate at frame.
		inches	inches	inches	inches		sq. in.	sq. in.		sq. in.	%	%	tons	tons	tons	tons	tons	%	%			tons
36½	× $\frac{9}{16}$	$\frac{3}{4}$	·81	3·	3·	13	6·13	12·12	34	17·34		43·	242	315	312	3		·96			6	308
36	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·53	3·5	11	7·53	14·97	29	20·01		33·6	299	386	360	26		7·2			5	380
36	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·5	3·5	11	9·04	17·96	29	20·01		11·4	359	446	360	86		23·9		Lloyd's	5	458
36	× $\frac{12}{16}$	1	1·06	3·9	4·	10	9·27	17·73	27	23·76		34·	355	466	428	38		8·8		Liverpool	5	447
36½	× $\frac{14}{16}$	1	1·06	4·	4·	10	10·81	21·12	27	23·76		12·5	422	533	428	105		24·5			5	580
37	× 1	1⅛	1·19	4·5	4·5	9	12·49	24·51	24	26·4		7·6	490	609	475	134		28·2			4	628
42½	× $\frac{9}{16}$	$\frac{3}{4}$	·81	3·	3·	15	7·08	14·17	39	19·9		40·5	283	366	358	8		2·2				
43	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	13	8·9	17·97	34	23·46		30·	359	458	422	36		8·5				
42	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·42	3·5	13	10·68	20·82	34	23·46		12·6	416	515	422	93		22·		Lloyd's		
42	× $\frac{12}{16}$	1	1·06	4·15	4·	11	10·19	21·31	29	25·52		19·7	426	537	459	78		17·		Liverpool		
41	× $\frac{14}{16}$	1	1·06	4·02	4·	11	11·9	23·97	29	25·52		6·4	479	590	459	131		28·5				
41½	× 1	1⅛	1·19	4·5	4·5	10	13·88	27·62	27	29·7		7·5	552	690	535	155		29·				
48½	× $\frac{9}{16}$	$\frac{3}{4}$	·81	3·	3·	17	8·02	16·23	44	22·44		38·	325	417	404	13		3·2			8	410
46½	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	14	9·57	19·49	37	25·53		30·9	390	502	460	42		9·1			7	485
48	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·65	3·5	14	11·51	24·49	37	25·53		4·2	490	602	460	142		30·8		Lloyd's	7	605
48	× $\frac{12}{16}$	1	1·06	3·92	4·	13	12·05	23·95	34	29·92		24·9	479	606	539	67		12·4		Liverpool	6	609
45	× $\frac{14}{16}$	1	1·06	4·02	4·	12	13·02	26·85	32	28·16		6·6	528	655	507	148		29·1			6	659
[illegible]	× 1	1⅛	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	..	3·8	615	754	574	180	..	31·3	...		5	781

TABLE II.—TREBLE RIVETING—continued.

Width in inches of Plate	Thickness „	Diameter of Rivet cold.	Diam. of Rivet hammered.	Spacing of Rivets, centre to centre.	Rule spacing of Rivets, centre to centre.	Greatest number of Rivets in one row.	Area punched out of plate at one row of rivet holes in butt, 17 % added for countersinks.	Effective sectional area of Plate.	Total number of Rivets in Butt.	Total effective Rivet area in Butt.	Percentage of excess Plate section.	Percentage of excess Rivet area.	Strength of plate at row of Rivet holes, at 20 tons per square inch.	Ditto, with shearing strength of back row of Rivets at 18 tons per square inch added.	Shearing strength of total Rivets at 18 tons per sq. inch.	Excess of Plate strength in tons.	Excess of Rivet strength in tons.	Percentage of excess Plate strength.	Percentage of excess Rivet strength.		No. of hole at frame.	Strength of Plate at frame
		inches	inches	inches	inches		sq. in.	sq. in.		sq. in.	%	%	tons	tons	tons	tons	tons	%	%			tons
$54\frac{1}{2}$	× $\frac{8}{16}$	$\frac{3}{4}$	·81	3·	3·	19	8·97	18·28	49	25·		36·7	365	466	450	16		3·5				
$53\frac{1}{2}$	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	16	10·95	22·48	42	28·98		28·8	449	573	522	51		9·7				
54	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·55	3·5	16	13·16	27·34	42	28·98		6·	547	671	522	149		28·5		Lloyd's		
54	× $\frac{12}{16}$	1	1·06	4·11	4·	14	12·98	27·52	37	32·56		18·3	550	693	586	107		18·2		Liverpool		
54	× $\frac{14}{16}$	1	1·06	4·11	4·	14	15·18	32·07	37	32·56		1·5	641	784	586	198		33·8				
$60\frac{1}{2}$	× $\frac{8}{16}$	$\frac{3}{4}$	·81	3·	3·	21	9·91	20·34	54	27·54		35·	407	517	496	21		4·2			10	511
$60\frac{1}{2}$	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	18	12·18	25·63	47	32·43		26·5	513	650	584	66		11·3			9	636
60	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·48	3·5	18	14·8	30·2	47	32·43		7·3	604	741	584	157		26·9		Lloyd's	9	752
60	× $\frac{12}{16}$	1	1·06	3·94	4·	16	14·84	30·16	42	36·96		22·5	603	761	663	98		14·7		Liverpool	8	750
60	× $\frac{14}{16}$	1	1·06	3·94	4·	16	17·36	35·14	42	36·96		5·4	703	861	663	198		29·8			8	877
Col. 1		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19		20	21

TABLE III.—QUADRUPLE RIVETING.

Width in inches of Plate.	Thickness „ of Plate.	Diameter of Rivet cold.	Diam. of Rivet hammered.	Spacing of Rivets, centre to centre.	Rule spacing of Rivets, centre to centre.	Greatest number of Rivets in one row.	Area punched out of plate at one row of rivet holes in butt, 17 % added for countersinks.	Effective sectional area of Plate.	Total number of Rivets in Butt.	Total effective Rivet area in Butt.	Percentage of excess Plate section.	Percentage of excess Rivet area.	Strength of plate at row of Rivet holes, at 20 tons per square inch.	Ditto, with shearing strength of back row of Rivets at 18 tons added.	Shearing strength of total Rivets at 18 tons per sq. inch.	Excess of Plate strength in tons.	Excess of Rivet strength in tons.	Percentage of excess Plate strength.	Percentage of excess Rivet strength.	
		inches	inches	inches	inches		sq. in.	sq. in.		sq. in.	%	%	tons	tons	tons	tons	tons	%	%	
36	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·53	3·5	11	7·53	14·97	40	27·6		84·3	299	386	497		111		28·8	
36	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·5	3·5	11	9·04	17·96	40	27·6		53·6	359	446	497		51		11·4	Lloyd's
36	× $\frac{12}{16}$	1	1·06	3·9	4·	10	9·27	17·73	37	32·56		83·6	355	460	586		120		25·7	Liverpool
36½	× $\frac{14}{16}$	1	1·06	4·	4·	10	10·81	21·12	37	32·56		54·1	422	533	586		53		9·9	
37	× 1	$1\frac{1}{8}$	1·19	4·5	4·5	9	12·49	24·51	33	36·3		48·1	490	609	653		44		7·2	
43	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	13	8·9	17·97	47	32·43		80·4	359	458	584		126		27·5	
42	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·42	3·5	13	10·68	20·82	47	32·43		55·7	416	515	584		69		18·4	Lloyd's
42	× $\frac{12}{16}$	1	1·06	4·15	4·	11	10·19	21·31	40	35·2		65·1	426	537	634		97		18·	Liverpool
41	× $\frac{14}{16}$	1	1·06	4·	4·	11	11·9	23·97	40	35·2		46·8	479	590	634		44		7·4	
41½	× 1	$1\frac{1}{8}$	1·19	4·5	4·5	10	13·88	27·62	37	40·7		47·3	552	690	733		43		6·2	
46½	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	14	9·57	19·49	51	35·19		80·5	390	502	633		131		26·	
48	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·65	3·5	14	11·51	24·49	51	35·19		43·6	490	602	633		81		5·1	Lloyd's
48	× $\frac{12}{16}$	1	1·06	3·92	4·	13	12·05	23·95	47	41·36		72·6	479	606	744		138		22·7	Liverpool
45	× $\frac{14}{16}$	1	1·06	4·	4·	12	13·02	26·35	44	38·72		46·9	528	655	697		42		6·4	
46	× 1	$1\frac{1}{8}$	1·19	4·5	4·5	11	15·27	30·73	40	44·0		43·1	615	754	792		38		5·1	
53½	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	16	10·95	22·48	58	40·02		78·	449	573	720		147		25·6	
54	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·55	3·5	16	13·16	27·34	58	40·02		46·3	547	671	720		49		7·3	Lloyd's
54	× $\frac{12}{16}$	1	1·06	4·11	4·	14	12·98	27·52	51	44·88		63·	550	693	808		115		16·6	Liverpool
54	× $\frac{14}{16}$	1	1·06	4·11	4·	14	15·18	32·07	51	44·88		39·9	641	784	808		24		3·0	
55	× 1	$1\frac{1}{8}$	1·19	4·5	4·5	13	18·09	36·91	47	51·7		40·	738	896	981		35		8·9	
60½	× $\frac{10}{16}$	$\frac{7}{8}$	·94	3·5	3·5	18	12·18	25·68	65	44·85		74·4	513	650	807		157		24·1	
60	× $\frac{12}{16}$	$\frac{7}{8}$	·94	3·48	3·5	18	14·8	30·2	65	44·85		48·5	604	741	807		66		8·9	Lloyd's
60	× $\frac{12}{16}$	1	1·06	3·94	4·	16	14·81	30·19	58	51·04		69·	603	761	919		158		20·7	Liverpool
60	× $\frac{14}{16}$	1	1·06	3·94	4·	16	17·36	35·14	58	51·04		45·2	702	861	919		58		6·7	
59½	× 1	$1\frac{1}{8}$	1·19	4·5	4·5	14	19·49	40·01	51	56·1		40·2	800	978	1010		32		3·2	

TABLE IV.—DOUBLE RIVETING.

Width in inches of Plate.	Thickness " of Plate.	Diameter of Rivet cold.	Diam. of Rivet hammered.	Spacing of Rivets, centre to centre.	Rule spacing of Rivets, centre to centre.	Greatest number of Rivets in one row.	Area punched out of plate at one row of rivet holes in butt, 17 % added for countersinks.	Effective sectional area of Plate.	Total number of Rivets in Butt.	Total effective Rivet area in Butt.	Percentage of excess Plate section.	Percentage of excess Rivet area.	Strength of plate at row of Rivet holes, at 20 tons per square inch.	Shearing strength of total Rivets at 18 tons per sq. inch.	Excess of Plate strength in tons.	Excess of Rivet strength in tons.	Percentage of excess Plate strength.	Percentage of excess Rivet strength.	
		inches	inches	inches	inches	inches	sq. in.	sq. in.		sq. in.	%	%	tons	tons	tons	tons	%	%	
48	× 7/16	3/4	·81	3·19	3·	16	6·53	14·49	32	16·32		12·6	290	294		4		1.4	
48	× 7/16	3/4	·81	2·96	3·	17	6·94	14·08	34	17·34		23·1	282	312		30		10·6	
48	× 8/16	3/4	·81	3·19	3·	16	7·56	16·44	32	16·32	·73		329	294	35		11·9		
48	× 8/16	3/4	·81	2·96	3·	17	8·02	15·98	34	17·34		8·5	320	312	8		2·5		
48	× 9/16	3/4	·81	3·19	3·	16	8·4	18·48	32	16·32	13·2		370	294	76		26·		Lloyd's
48	× 9/16	3/4	·81	2·96	3·	17	8·92	17·96	34	17·34	3·5		359	312	47		15·		”
48	× 9/16	13/16	·875	3·38	3·25	15	8·57	18·31	30	18·	1·7		366	324	42		13·		Liverpool
48	× 9/16	13/16	·875	3·12	3·25	16	9·14	17·74	32	19·2		8·2	355	346	9		2·6		”
48	× 10/16	7/8	·94	3·65	3·5	14	9·57	20·43	28	19·32	5·7		409	348	61		17·5		
48	× 10/16	7/8	·94	3·35	3·5	15	10·24	19·76	30	20·7		4·7	395	373	22		5·6		
48	× 11/16	7/8	·94	3·65	3·5	14	10·45	22·57	28	19·32	16·8		451	348	103		29·6		
48	× 11/16	7/8	·94	3·35	3·5	15	11·2	21·82	30	20·7	5·4		436	373	63		16·9		
48	× 12/16	7/8	·94	3·65	3·5	14	11·51	24·49	28	19·32	26·7		490	348	142		40·8		Lloyd's
48	× 12/16	7/8	·94	3·35	3·5	15	12·33	23·67	30	20·7	14·3		473	373	100		26·8		”
48	× 12/16	1	1·06	4·36	4·	12	11·13	24·87	24	21·12	17·7		497	380	117		30·5		Liverpool
48	× 12/16	1	1·06	3·92	4·	13	12·05	23·95	26	22·88	4.6		479	412	67		16·2		”
48	× 14/16	1	1·06	4·36	4·	12	13·02	28·98	24	21·12	37·2		580	380	200		52·6		
48	× 14/16	1	1·06	3·92	4·	13	14·1	27·9	26	22·88	21·9		558	412	146		35·4		
Col. 1		2	3	4	5	6	7	8	9	10	11	12	13	15	16	17	18	19	

TABLE V.

Description of Sample.	Thickness of Plates.	Diameter of Rivets.	Diameter of Holes.	Area of Holes.	Shearing Stress at Moment of Fracture. Total.	Shearing Stress at Moment of Fracture. Per sq. in. Rivet Area.	Method of Riveting.	Percentage of Total Shearing Stress at which Slip was observed to begin.	Remarks.
	inches	inches	inches	sq. in.	tons	tons			
Lap joint, one rivet,	$\frac{6}{16}$	$\frac{5}{8}$	·68	·36	6·5	18·	Hand		Rivet sheared
” ”	$\frac{7}{16}$	”	·71	·39	7·05	18·1	”		”
” ”	”	”	”	”	7·3	18·7	”		”
” ”	”	”	”	”	8·1	20·7	”		”
” ”	”	”	”	”	7·3	18·7	Hydraulic		”
” ”	$\frac{8}{16}$	$\frac{3}{4}$	·81	·51	9·	17·6	Hand		”
” ”	”	”	”	”	9·45	18·5	”		”
” ”	$\frac{5}{8}$	”	”	”	9·15	17·9	”		”
” ”	$\frac{1}{2}$	$\frac{13}{16}$	·85	·56	10·45	18·6	”	28·7	”
” ”	”	”	·84	·55	10·75	19·3	Hydraulic		”
” ”	”	”	”	”	11·25	20·4	”		”
” ”	$\frac{5}{8}$	”	”	”	10·5	19·	”	21·5 mean	”
” ”	”	”	”	”	9·55	17·3	Hand		”
” ”	”	$\frac{7}{8}$	·94	·69	10·875	15·7	”		”
” ”	$\frac{11}{16}$	”	”	·69	12·5	18·1	”		”
” ”	$\frac{5}{8}$	”	·96	·72	12·2	16·9	”	14·3	”
” ”	”	”	”	·72	15·65	21·7	Hydraulic		”
” ”	”	”	”	”	14·15	19·6	”		”
Lap joint, two rivets (chain)	$\frac{6}{16}$	$\frac{5}{8}$	·69	·74	12·75	17·2	Hand		”
” ”	$\frac{7}{16}$	”	”	”	13·	17·5	”		”
” ”	$\frac{5}{8}$	$\frac{13}{16}$	·84	1·10	18·2	16·6	”		”

Butt joint, single strap, one rivet	5/8	13/16	·85	·55	10·8	19·6	Hydraulic	37·0		Rivet sheared,	one end
" " "	11/16	7/8	·96	·72	13·05	18·2	"	32·5	33·7 mean	"	"
" " two rivets	1/2	11/16	·71	·78	13·75	17·6	"	34·5		"	"
" " "	"	"	"	"	13·125	16·8	Hand	13·3		"	"
" " "	5/8	13/16	·85	1·10	18·1	18·3	"	16·6	13·4 mean	"	"
" " one rivet	11/16	7/8	·96	·72	12·0	16·6	"	10·4		"	"
Butt joint, double strap, one rivet	1/2	11/16	·73	·41	13·65	33·3	Hydraulic			"	"
" "	5/8	13/16	·85	·56	19·6	35·0	"			"	"
" "	1/2	11/16	·72	·41	13·75	33·5	"	40·0	33·4 mean	"	"
" "	5/8	13/16	·84	·55	17·7	32·2	"	26·8		"	"
" "	1/2	11/16	·73	·41	14·35	35·	Hand			"	both ends
" "	"	"	·73	"	11·85	28·9	"			"	one end
" "	"	"	"	"	11·9	29·	"	33·6		"	"
" "	"	"	"	"	11·2	27·3	"	20·0		"	both ends
" "	5/8	13/16	·85	·56	17·95	32·	"	23·2	24·9 mean	"	one end
" "	"	"	"	"	17·95	32·	"	33·4		"	"
" "	"	"	"	"	21·	37·5	"	14·3		"	"

SYNOPSIS OF RESULTS.

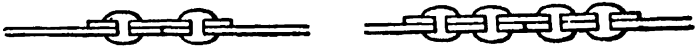
Mean shearing stress per square inch, rivet area of lap joints, hand riveted, = 17·8 tons.
" " " " hydraulic " = 19·8 "

" " " butt joints with single straps, hand riveted, = 17·33 tons.
" " " " " hydraulic " = 18·47 "

" " " " double straps, hand riveted, = 31·6 tons.
" " " " " hydraulic " = 33·5 "

Lap Joints.

Butt Joints, Single Straps.



Butt Joint, Double Strap

In the after discussion,

Mr **Robert Mansel** said he entirely agreed with nearly every deduction arrived at in this paper, and he had on his memory an instance of a case which went to show that it would be of great importance to improve butt fastening by introducing outside as well as inside straps. In 1858, the once well-known steam vessel "Persia," after some heavy transatlantic passages, gave decided indication of being deficient in longitudinal strength, and it became, necessary to arrive at some definite conclusions as to the amount of additional strengthening required. This vessel was heavily plated on the bottom and sides up to the main sheer, but the 'tween deck plating and upper deck stringers were lighter than would now be considered admissible. On examination, a plate at the gunwale, in the paddle-box space, was found to have been torn through the solid not at rivet holes, and it was judged that the extreme strain to have done this, very approximately, must have been about 20 tons per square inch. This definite fact, coupled with the reasonable and very approximate assumptions of the neutral axis being at the height of the centre of gravity of the strained metal of the section; that the extension, at any point of the section, would be proportional to the distance from the neutral axis; and, lastly, by Hooke's law, "*ut tensio sic vis*" (as the extension so the force), we were furnished with the principles necessary to calculate the amount of strength of the section (mathematically strictly proportional to its *moment of inertia)*, and we were then able to compare it with the breaking moment, taken as some fraction of the product of the length and displacement of the vessel; and, also, see the definite effect due to any other distribution or modification of metal in the section. On the direction of the late Mr James R. Napier, he (Mr Mansel) investigated these matters very fully, and the inaugural paper of the Association of Shipbuilders in Scotland, October, 1860, contained the results of the application of these principles to a number of cases. Mr Napier was a gentleman who took the greatest interest in scientific questions, and followed them up with unwearied industry; and, since it appeared that the plating showed started butts, where from the distance from the neutral axis the strains must have been very much less than at the gunwale, Mr Napier had full-sized models of double riveted butts prepared and tested; when, to their surprise, it turned out, with plates one inch in thickness or slightly over, these butts were started and practically ruined with a strain of about 8 tons per square inch of section; and, consequently, compared with the thinner topside plating, these thick plates did not give nearly the efficiency due to their metal, owing to want of strength in the butt fastenings. He thought this showed the necessity of double strapping, which had been experimentally proved to give a very great increase to the tensile strength of the butt. He might have other remarks to make upon the paper after he had read it carefully.

Mr HENRY DYER thought there was no question that as a mere matter of strength outside butt straps would be effective, but he would like to know whether they would not affect injuriously the speed of the vessel? Would the vessel's resistance in passing through the water not be greatly increased, not only by the butt straps but also by its bottom being more liable to become foul on account of the difficulty of cleaning at the ends of the straps? He was willing to admit the superiority of the plan suggested in the paper, so far as strength was concerned, but whether the objections to its adoption in practice would not be insuperable, was another question.

Mr MANSEL said no doubt Mr Dyer had put his finger on the objection to butt straps on the outside of vessels. No doubt the edges of these plates would very considerably increase the resistance of the ship in the water; but he thought that something might be done in the direction indicated in the paper.

Mr GEORGE RUSSELL referred to the small percentage of breaking strain at which slip commenced mentioned in the paper, and asked if there was authority for it?

Mr TAYLOR said that Professor Kennedy, at a meeting of the Institution of Mechanical Engineers, giving the result of some experiments he had made, stated that by the aid of a magnifying glass the slip could be seen to begin at about ten per cent. of the breaking strength of the joint, while, however, it was not visible to the naked eye till it reached about 25 per cent., or a quarter of the load, for the shearing of ordinary rivets. With regard to what Mr Dyer had said as to the resistance of outside straps, he might say that there was nothing known as to what that might be, with a vessel clean; but if they considered one with a good crop of barnacles on, it could not much matter whether the straps were fitted outside or not. Whether it would be worth while to sacrifice the speed that would be lost by outside straps was a moot point.

Mr RUSSELL asked if Mr Taylor had given any attention to the form of butt straps and arrangement of rivets to obtain the maximum tensional strength? Such a mode he had advocated 23 years ago. It is illustrated in Plate X. of Volume IX. of the Transactions of this Institution.

Mr TAYLOR did not think that that would be practicable in shipbuilding where they were confined by the spacing of the frames for the width of the butt straps.

The PRESIDENT suggested the desirability of making an experiment by adding butt straps to a vessel already built, to find what effect they would have on its speed. He proposed a vote of thanks to Mr Taylor for his very interesting and practical communication.

In the discussion of this paper, on the 28th April, 1885,

Mr W. T. C-DUTTON congratulated Mr Taylor, as a former colleague of his own on the staff of the Liverpool Registry, on the valuable paper he had brought before the Institution. The subject was one of the greatest importance, and would always awaken the interest of shipbuilders. There were one or two points that he would like specially to draw attention to. First with reference to the shearing strength of the rivets, it was well to notice the difference which Mr Taylor showed by his experiments between rivets closed by hydraulic pressure and those put in by hand. Mr Taylor in accounting for this difference, which appeared to be about nine or ten per cent. in favour of the former, said (see p. 237), "This is no doubt due to increased friction and to the holes being better filled by the greater pressure brought to bear upon the rivet, which tends to prove the great superiority of power riveting over hand work. It must also have been due to some varying in the effectiveness of the riveted work which caused the proportion of load at which slip occurred to be so irregular." There was no doubt much truth in this, but it appeared to him (Mr Dutton) that another cause might be looked for, which was that while in hydraulic riveting, the rivets, properly heated, were closed at once and done with; in hand work they were knocked down through all the stages from hot to cold, and the material would suffer to some extent in consequence. Their chief surveyor, Mr West, had given considerable attention to the question of riveting, and had read an interesting paper on the subject at last year's meeting of the Institution of Naval Architects, advocating increased fastening and the use of double straps, and if Mr Taylor would refer to the existing Rules of the Registry, he would find that both quadruple riveting, and double-strapping were provided for. As an instance of the former he might mention the case of the "Manora," a steel steamer of about 5,000 tons, built two years ago by Messrs Wm. Denny & Bros., in which all the alternate strakes of plating were quadruple riveted, and this vessel continued to do her work very satisfactorily. He did not find so much difficulty in fitting these wide straps as Mr Taylor anticipated. They could be checked out or thinned in way of the plating flange of the frame, so as to be fitted over it if required, or under it to take the place of the liner, and by that means full advantage of the 24-inch space could be taken. The fitting of outside butt straps was objected to on the score of appearance, and of the resistance offered to speed, but he considered both of these objections rather sentimental. When they found that the large steamer "America," built by Messrs Jas. and Geo. Thomson, and in the front rank of first-class passenger traffic, had these straps fitted on her sheer strake—not light straps such as Mr Taylor refers to, but of a very substantial character, and that another steel vessel the "Manaos," built by the same firm, and also engaged in passenger traffic, was similarly fitted he could not think that any importance could attach to the question of appearance. Then as regarded the resistance to speed, there was not much more in that, for although there was very little data to go upon, he could say that in cases where these straps of seven and eight-sixteenths in thickness had been fitted, there was actually no appreciable difference in the speed, and he looked forward to the time when double straps would be generally adopted.

Mr HENRY H. WEST, through the Secretary, said—I have read Mr Taylor's paper with very great interest, and I congratulate the Institution on having upon its records so thoughtful and practical a paper on such an important subject. I very cordially agree with Mr Taylor in his conclusion, that butt riveting in relation to the sectional area of the plates connected, ought to be considerably increased from present practice; but this conclusion immediately raises the question, "How much ought it to be increased beyond present practice?" The answer to this question, in the present state of our knowledge, is not easily given. In most engineering structures it is customary to apportion the riveting in a joint, so that the ultimate shearing resistance of the rivets, shall be approximately equal to the ultimate tearing resistance of the plate, at the line of probable fracture. This arrangement, I gather, Mr Taylor would adopt for ship work, and I freely admit that it would be a very great improvement upon present practice; but I venture to think that if we are to gain the full advantage of all the material we put into our large steamers, there are parts where even this allowance of rivet power must be exceeded. It is manifest that in such a structure as a ship, any slipping in a riveted joint throws an undue load on the adjoining material; and I think anyone who gives consideration to the reciprocating nature of the strains to which a large steamer is subjected when labouring in a sea way, the complexity of the twisting and racking strains she undergoes, and the violence of the blows she receives from the sea, simultaneously with the constant vibration of powerful machinery, must admit that these are conditions which will be favourable to the development of slip in even the best riveted joints. There is direct evidence that this frequently occurs. In a paper I read on this subject last year at the Institution of Naval Architects, and to which Mr Taylor has done me the honour to refer, I quoted a number of such instances, and the list could easily be extended. For my own part, I hold that the true measure of the strength of a riveted joint in the most severely strained parts of a ship is the resistance of that joint to slipping. Not simply its resistance to slipping under a continuously and steadily applied strain, but under strains analogous to those to which I have referred. Until we know approximately what that resistance is, it is impossible to say, with any approach to accuracy, how much our butt riveting ought to be increased. For a number of years the conviction has been growing upon me that our ship riveting arrangements are defective, and whatever may be the proportions which may ultimately be found to be the right and best proportions, we shall in the meantime be making a step in the right direction if we very materially increase the rivet power in the joints of the most severely strained parts of our large steamers. I hope that now Mr Taylor is engaged in practical ship construction, he will have the courage of his opinions, and carry out in an actual case the conclusions at which he has arrived.

On American Railway Freight Cars.

By Mr ALEXANDER FINDLAY.

(SEE PLATES XX. AND XXI.)

Received and held as read 28th April, 1885.

IN reviewing the history of the past half-century it doubtless will be admitted that nowhere in the world has there been such an enormous stride in social and material progress and prosperity as in the United States of America; and one of the prime factors—if not the most important—has been the development of railways.

Fifty years ago there was only a few miles—less than one hundred—running from Boston to Worcester in Massachusetts. Now there is over one hundred thousand miles running into every State, and opening up and peopling stretches of country which before were little else than a wilderness, and running through vast regions, from the 3000 miles stretches across the Continent to the overhead line round New York, all telling a tale of enterprise and progress.

Our purpose to-night, however, is not to enter into a history of railroad enterprise in the States, for who does not know about the great land grants given to railway contractors, or the colossal fortunes that have been made or lost in constructing, working, or speculating in those roads, of which the Vanderbilts, Fisks, Goulds, and others, were and are the leading lights among the "bulls" and "bears" of Wall Street?

Nor is our purpose to consider the construction, and many gauges of their roads, through flat prairies or over the Rocky Mountains, with their flat-bottomed rails spiked direct to the sleepers, nor their light trestle bridges, and viaducts, which will yet be a source of ever-increasing trouble to their railway engineers.

Nor is our purpose to deal with their locomotives, which show some good and some bad points, compared with our own. Nor their Pullman, Palace, Dining, Sleeping, or Ordinary Passenger Cars, a number of the former of which having been introduced here, have added much to the comfort of a long journey, when the purse admits of such a luxury; nor need their excellent ticket or care of luggage system detain us, fraught as these items are with so much comfort to the traveller compared with our own systems. All the foregoing might doubtless prove of general interest, but not quite suitable for this meeting, and with the short time at our disposal, we must content ourselves with considering freight cars and their construction.

Under freight cars, however, there are innumerable types and modifications for various requirements, such as grain cars, coal cars, ore cars, oil cars, timber cars, cattle cars (some of which are facetiously styled "palace cars"), refrigerator cars, dump cars, and many others, but as a general type we shall consider the grain and coal car commonly in use, though each railway may have some modification or variation.

I have chosen this subject, having had several years practical experience of this class of car-building with the Missouri Car and Foundry Company of St. Louis; and may say that I have had my own knowledge checked up to date, through the kindness of the president and superintendent of that company.

In the general construction of the class of car under consideration there has been little alteration for the past fifteen years, except the length increased from 28 feet to 33 feet, and the scantlings somewhat heavier, and we shall now proceed to dissect one of those standard cars for grain and general merchandise. About five feet from each end of the car floor framework there is a cross transom, at centre of which an 1¾ inch diameter pin passes down, and forms the pivot for four-wheeled trucks, which, with their short-wheel base, enable those long cars to pass easily round very sharp curves, the wheel base seldom exceeding five feet. There are many forms and modifications of those trucks, and the patents for wheels, axleboxes, drawbars, brake-gear, &c., are legion, but we shall take what seems to be the most general type in use, and consider, first—

Wheels.—With us, as you know, the wheels are usually 36 inches diameter, with cast-iron centres or hubs, wrought-iron arms and ring, and steel tyre, the hub being bored out, and key-seated for keying on to axle.

The almost universal wheel for freight-cars in the States is of cast-iron, and chilled on tread and flange. The most common form, and that which seems to give best results, is a double web wheel, as shown. Those wheels are cast from a special mixture of metal and whilst hot are lifted out of the chills or ring in which they have been cast, and placed in air-tight pits for two or three days to anneal and toughen.

The wheels are then bored out on a specially-constructed boring mill, the wheel being placed on a table which revolves, whilst a vertical boring bar, with roughing tool and finishing tool closely following, works quickly down, so that one man can do over seventy wheels in 9½ hours. The wheels are not keyed on axles, but simply pressed on to the gauge by hydraulic pressure of about fifty tons.

Those wheels weigh from 500 lbs. to 600 lbs. each, give good results, and have frequently been in continuous service for ten and even fifteen years. Now that cast-steel is making progress, one might think some such wheel could be introduced here with advantage, if cast-iron is looked upon with suspicion.

Axles.—The axles are usually forged from the best scrap, and weigh about 375lbs., and are subjected to severe tests before leaving the forge to make sure that all is sound. The standard size for 4 feet 8½ inch gauge road, as adopted by the Master Car Builders' Association, is 6 feet 11¼ inches long over all, 4⅜ inches diameter at wheel-seat, and 4 inches diameter at centre of length; and with journals 7 inches long by 3¾ inches diameter. Those axles are turned on special double-end lathe, which enables one man to do 20 axles in 9½ hours. The wheel-seat only gets roughing cut, and is made slightly larger in diameter than bore of wheel, and, as already stated, the wheels are pressed on into position by hydraulic pressure, and are not keyed on.

Journal Bearings.—There are many forms and patents in connection with the axle boxes and bearings, but probably the best results for bearings are obtained from a mixture 7 of best copper to 1 of tin; although in others the brass is cast hollow and filled with "Babbitt metal;" others again, in order to secure the best results without incurring the expense of much nicety of fitting, face the bearings with about 1-16th inch thickness of lead, which readily adjusts itself to the journal, and thereby prevents overheating, as is the case unless great care is taken in fitting.

Axle Boxes.—The modifications in the arrangement of oil boxes have all a family resemblance—all aiming at protecting the front and rear more securely, so that the lubricant may give due effect, such as the "Hewitt" door, which, as shown, slides into a wedge-shaped groove, and whilst making a secure fixture, is easily removed, and the rear has a carefully-fitted vulcanite dust guard. The bearing has a packing or adjustable wedge piece, which enables a more perfect bearing to be easily obtained.

Springs.—The desire to produce the best form and arrangement of spring has resulted in many kinds of patents and much trouble and expense; but the round steel bar, coiled one coil within the other, has come into very general use, and gives good results for drawbar and buffer springs. For bolster and carrying springs the elliptic gives satisfaction when made of such proportions as give the desired motion, the usual size for a 20 tons car, having five leaves of 4½ inches by ⅜-inch steel, coupled together, in what are termed "doublets," thus making a spring 24 inches long, 10½ inches wide, and about 9 inches high, giving a motion of 2½ inches to 2¾ inches. The cost of these, however, exclude them from general use.

The round and rectangular coiled bars are more common, and are coiled in every variety, to get a cluster of coils equal to the work required. Sometimes twelve single coils of ⅝ inch bar—in some cases with rubber centre—are grouped in one case, and four of these are used to a car; or other groups will be made thus:—An outside coil 5½ inches or 6 inches diameter of 1 inch or 1⅛ inch round bar, having an inner coil of ⅝ inch or ¾ inch bar, and those set in cast cases, top and bottom.

Another much used is composed of a single coil, made from a rectangular bar 2 inches by 1⅛ inches, coiled to a diameter of 9 inches, and a height of 5½ inches; outside of this are placed two coils—one on either side—made from a bar ¾ inch by ⅝ inch, and coiled to a diameter of 4½ inches, and a height of 6 inches (or ½ inch higher than large coil), the three are set in cast or malleable cases, the difference in height being made to carry the light or empty car without the assistance of the large central coil, and gives a very satisfactory motion.

Trucks.—The trucks or bogies on which the car body rests has been a problem much discussed in trying to decide the best form, and this is still far from a final settlement. There are two kinds principally in use—one known as the rigid or "Cleveland" truck, and the other the "swing motion" truck. The Cleveland has some good points, and after many years' experience has been preferred for its simplicity of construction and non liability to get out of order. This truck is constructed of a top and bottom bolster (or spring plank and bolster), the top plank being usually 13 inches by 8 inches trussed, with 2 ⅞ inch rods, as the half weight of car comes on centre plates screwed to centre of this timber, the bottom or spring plank is 13 inches by 5 inches, connected to the axle boxes by two arch bars of 3 inches by 1 inch iron and 3 inches by ½ inch tiebar, which are secured to the timbers by cast-iron columns and guides, the cases with group of coil springs being placed between said timbers.

The swing motion truck is of somewhat similar construction to the foregoing, so far as the arch and tiebars are concerned, but the spring plank and bolster are hung by links and pins from the upper edge of two 13 inch by 5 inch timbers set on edge, and having a distance casting placed at each end between these timbers, and well secured to them. The swing motion is on account of the bolster and spring planks hanging, with a little spare room between these two vertical timbers, and altogether make a truck more adapted to toughly constructed or poorly ballasted roads, and is much easier on the body of car as well as on journals and bearings, owing to the swing motion accommodating itself to inequalities and irregular strains. The greater number of pieces, the difficulty of easy inspection, the expense of renewal and increased first cost, are some objections to this truck.

Timber has been mentioned as the constructive material, and because it is so plentiful; but now, instead of the two timbers enclosing the swing beam, there is an extensive use of iron channel bars, 10½ inches by 3 inches, and iron is gradually working into use for other parts as well.

A feature lately introduced in heavy cars for carrying ore or stone, has been to apply a *third* truck to the car—that is, placing a truck under the centre of the car. To do this the car has T iron rails secured to the sills crosswise, and rollers on suitable axles are placed on the truck, and arranged to allow a lateral motion when going round curves. These are said to give good results where a continuous service in one locality is found practicable, otherwise one would assume that a shorter car, capable of being safely carried by two trucks, would be much preferable.

Body of Car.—The body of car is usually 33 feet long by 9 feet wide—a very great departure, indeed, from our short cars or trucks, as we call them; and for coal cars have shallow sides about 18 inches to 24 inches deep, and for grain and other traffic have side framing about 7 feet high, with roof over all.

The transoms or body bolsters on which the trucks pivot, are now often made of 7-in. by ⅞-in. iron, although oak transoms 13 in. by 5 in., trussed by two 1-in. rods through and over the sills are still much used. The main or floor framing consists of six longitudinal timbers and two end timbers, with two intermediate timbers across under sills for trussing longitudinals; also the two transom timbers about 5 feet from ends. The timber used is mostly pine, although the two centre longitudinals to which the draw timbers are secured are oak.

Eight inches by 4½ inches is the scantling used for this framing, and those are well tenoned and secured by bolts and plates to each other, and have four longitudinal truss rods 1 inch or 1⅛ inch diameter running through end timbers, over transoms and under the intermediate truss timbers, thus stiffening framework.

Drawbars.—Of all the questions in car construction, this is meanwhile the most perplexing, the drawbar acting both as coupler and buffer in most of the freight cars. There are several thousand patent couplers, many being automatic and having good points, and all desirous of having their patents introduced. This important matter is now being made a subject for legislation, and several are being tested and a general discussion is being carried on amongst railway managers, car builders and others, which must result in ultimate good. A very common drawbar is that with single coupling, link and pin in cast iron drawbar, secured by draw and buffing plates with volute spring to relieve the shock when starting or stopping car. A similar drawbar is arranged to receive three coupling links, so that in the event of a breakdown owing to a link giving way, another can at once be available. There are also "continuous" drawbars which give good results provided every part is reliable, which is not always the case. One with some points of excellence consists of an abutting plate 7 inches by 1¼ inches, let into the draft timbers, and secured by two bolts running across close behind it. The volute spring is placed between this plate and the end of the cast-iron drawbar. A tail pin passing through both, and with cottar in the end outside the plate, but which is not brought into service unless some other part fails. A wrought-iron bar 24 inches long, 5 inches by 1 inch, passes through drawbar, and projects beyond both draft timbers, with room to move longitudinally in the timber equal to the motion of the spring, those slots in the timber are protected by corner irons, and when drawing or buffing, this 5 inches by 1 inch bar should act about the time the spring motion is exhausted. There is also two 1¼ diameter rods having a loop on each end that fits over those 5 inches by 1 inch bars, and thus form a continuous pull on rear end of car. Should these rods give way, then the 5 inches by 1 inch bars passing through the draft timbers will act and the drawbars still be in operation, and if these give way, the tail pin will continue to do service. The weak point, however, is the cast-iron drawbar, which has too long held its position, but is now giving place to drawbars made of malleable iron.

We will now pass to the body framing, which in coal cars consists of short wooden posts set vertically in cast-iron sockets, and to which the timber forming sides and ends is secured.

Grain and covered cars have, however, corner and intermediate posts about 7 feet high, and bracings 4½ by 2½ in., pine being usual, and so framed and secured by iron plates and rods as to make a strong framing, which is lined outside by ⅞ inch boarding, and inside up 3 feet to 4 feet for grain. The doors for those cars usually slide outside on an iron rail, and there are many patent fixtures arranged for holding door close to body of car so as to prevent sparks from getting in. There is also an inner door 3 feet to 4 feet high to prevent the grain from pressing against the outer door, or from leaking out, and some most ingenious devices have been patented for easy fixing and removal of those doors when not in use. Roofs for those grain and covered cars are usually made with about one foot of rise at centre, and are often made up of cross rafters about 2 feet apart, and with longitudinal purlins, on which two thicknesses of ⅝ inch lining, having feather and grooved joints, are fixed and all well painted and watertight.

Galvanised iron sheeting, No. 24 B.W.G., is, however, being extensively used for roof covering, and does good service. A very important equipment of a car is an efficient brake, and the usual practice for freight cars is to apply a brake shoe to each wheel, those being acted on by rods and levers attached to ⅜ inch chain which simply winds round a vertical rod having hand wheel and pawl attachment. Some of the best roads are now equipping their entire freight stock with good automatic brakes, notably the "Westinghouse," which seems to take and keep the lead for efficiency in such appliances. The American Brake

Coy. of St. Louis have, however, a goodly number of their brakes in use, which device is operated only by the pressure of the drawbar, which, upon a stoppage or sudden reversal of the engine sets all the brakes instantly in action. There is one great advantage, viz., that there is no connection between the cars, and any car may be operated on independently in any part of the train, which is not the case in the Westinghouse, as it is connected with the engine, and operated by the engineer. We must not, however, overlook the fact that where a train breaks in two from any cause, then the Westinghouse at once operates on every brake automatically.

And now, to sum up, it may be asked, Why do they not build their cars of iron, and with the superior fittings such as are common on the Indian State and similar railway waggons? To which we reply that timber being cheap and plentiful, and the car works furnished with excellent wood-working machinery for planing, tenoning, morticing, boring, &c., such cars can be turned out very quickly, and above all cheaply—which is a great desideratum with many of the Railway Companies—compared with anything that could be made in iron. Such works as the Missouri Car and Foundry Coy. turn out as many as twelve fully equipped cars, of the kind we have been considering, *each day*, casting their own wheels and doing their own smith work.

In addition to some of the railway companies that build their own cars, there are many private companies engaged in the car building business, and which turn out the enormous requirements for the multitude of railways throughout the country.

And now, is there anything to learn from those long American Railway Freight Cars? probably they are more the special design required for such long journeys as many of them require to take continuously. Still, could there not be such a truck as the "Cleveland" introduced under say our long six-wheeled rail waggons with advantage, for at present it is impossible to get round the curves in many of our workshop yards with such waggons; even the 16 feet and 18 feet waggons with 9 feet wheel base do not take the curves so comfortably as the long American cars. Certainly, those long cars do strike a stranger as something very different from our short dumpy waggons—or trucks as we term them—and the chilled wheels and pivotting trucks do suggest some possible improvements such as are hinted above. In conclusion, it is hoped, that this paper although somewhat hurriedly written, may not be altogether uninteresting to some of our members engaged in rolling stock construction, and may stir them up to favour us with a paper shortly, on home practice specialties.

On Sinking the Cylinders of the Tay Bridge by Pontoons.

By Mr ANDREW S. BIGGART.

(SEE PLATES XXII., XXIII., AND XXIV.)

Received 24th February, and held as read 28th April, 1885.

ONE of the most common forms of foundations now adopted, on which to build the superstructure of a modern bridge is the cylinder. But, as the foundations are often in positions difficult to get at, and when there, to remain at, owing to causes such as the rise and fall of the tide, rapidity of the current, storms, &c., a difficult problem in connection with the building is often, How are the cylinders to be sunk?

One of the easiest, and at the same time most sure methods now in vogue, is to build a wooden stage around the place where each cylinder is to be sunk, and from this as a working platform, lower, dig out, concrete, and carry them up to the desired height.

Some cylinders are of sufficient capacity to float themselves with perfect safety to their respective positions, as well as be made carry all the sinking apparatus and platform necessary to regulate their descent into their final resting place. Or again, if we drop to the smallest form of cylinder, we would instance the screw and the hollow pile, the former of which is sent home by the simple, though sometimes difficult process of screwing into the ground, while the latter is driven.

While work can, and is being done every day, by methods similar to these, it is readily understood that much is done (especially if a stage has been constructed) which requires to be undone with a consequent loss of time and money. To use the old method of staging, for such a work as the New Tay Viaduct would have required almost a forest of timber, for this alone, and owing to the great depth of water, the work would have been both tedious and expensive.

Before proceeding to sketch the novel method which has, however, been adopted, let us look at the primary conditions which must necessarily be fulfilled by whatever form of platform is used.

1st,—There must be a working platform, on which can be placed cranes and other machinery.

2nd,—The platform must be high enough to permit of work being conducted from and upon it at all states of the tide.

3rd,—It should also be capable of being removed speedily from one position to another.

These primary conditions, enhanced by many other advantages, are practically realised in the pontoons designed by Mr Arrol, and now used successfully by him in sinking the cylinders of the New Tay Viaduct. The pontoons used (of which there are four) are all made up of tanks, for the sake of convenience, which are rigidly fastened together in the form of a rectangle ; and they vary in size from 56 feet by 36 feet 6 inches, by 6 feet deep, as in the smallest, to 81 feet by 66 feet, by 7 feet deep, as in that of the largest.

We propose to confine our description to one of these, as all are the same in principle, varying only in some of the details.

Fig. 1, Plate XXII., presents a plan of No. 2 pontoon. You will observe there are two main tanks running the whole length of the platform, connected together by one small tank, and several main cross girders, the full depth of the tanks, as well as, top and bottom outer cross girders. In both of the main tanks there are two rectangular openings, one at each end. Through these the legs are passed, which are used for raising and lowering the platform. To the tanks are fixed at these openings steel plates for carrying the hydraulic cylinders required to perform this action.

Equally from the centre, and at the distance of 26 feet, centre to centre, the large cylinders are lowered (one at a time) through the centre openings in the platform, and this too by special hydraulic machinery, being guided in their descent by the vertical guides G which in their turn are attached to the cross girders H H, fixed at the top and bottom of the tanks. The cross girders are only temporarily fastened, so that in the event of the platform being raised somewhat out of position, they can be shifted, and with them the guides, thus making it practicable with almost a minimum of labour to lower the cylinders in their true position, even although the pontoon has been pitched slightly out of place. On one of the main tanks there is fixed a crane which is used for lifting material on to the platform, and also for excavating by means of mechanical diggers, the sand and earth within the cylinders. In the small connecting tank is placed a boiler and engine, used for driving the hydraulic pumps, working windlass, &c., as may be required. Other machinery and gear, such as portable boiler and engine, centrifugal pumps, capstans, bollards, fairleads, workshops for the men, all find a place on this sometimes floating staging, at other times stationary and high out of the water.

Before this description can be of much practical value it will be necessary to describe more in detail the principal parts of the pontoon, and the mode by which it is wrought. The method of raising and lowering the platform is shown by Fig. 3, Plate XXIII. A is one of the legs, which is 5 feet in diameter, and of a conical shape at the bottom, to prevent the ground on which it rests being scoured from underneath. On it is fixed four heavy steel plates B B, two on each side, about 16 inches apart, having holes C C passing through them, spaced about 6 inches apart. Sliding within these two plates, but fixed to the platform, are other two D D, having holes the same size and pitch as in the outside plates, and carrying between them a hydraulic cylinder E, provided with a piston P, piston rod R, and crosshead I. The action is as follows; suppose the piston P to be at the top of the cylinder through the crosshead I, and outer plates B B, a steel pin is passed, when water is admitted the cylinder E is forced up, because the outer plates B B on which the pin rests are fixed to the leg A which in its turn bears on the ground. The plates D D are thus lifted, and with them the platform. When the cylinder has been raised about six inches, the holes through the inner plates D D and outer plates B B are in line. Into one of these is now passed another steel pin. If the water in the cylinder E is allowed to go free, the platform will now hang on the pin just inserted, and allow the first to be withdrawn. The piston is now forced to the top of the cylinder, and the first pin being again inserted, all is ready for another lift. From this you can readily perceive the only limit to the height to which the platform may be raised is the length of the leg and its accompanying plates. In lowering the platform this action is simply reversed. Both cylinders at each leg are wrought at the same time, and, if convenient, the others at the remaining corners of the platform.

The large foundation cylinders, two of which are in each pier, are lowered into position by the hydraulic apparatus shown by Figs. 1, 3, and 4. Each of these cylinders including an inside brick ring, which must be built before being lowered into position, weighs about 50 tons, varying less or more according to the depth to which it is to be sunk. The hydraulic cylinder and links for lowering these foundation cylinders are shown by Fig. 4, Plate XXIV. Figs. 1, 3, and 4, show the manner in which these are wrought. C is the cylinder to be lowered. A the hydraulic lowering cylinder. P the piston and hollow-trunk, through which is passed the steel links L, these being single and double every alternate length, and through all are cottar-holes about 10 inches apart. B is a bow, fixed on to, and over the hydraulic cylinder A, through it the links are also passed, they being in short lengths and attached to one another by means of bolts. At the bottom of the cylinder C, these links are firmly fixed to a plate which in its turn is securely bolted to the cylinder. The action in lowering is as follows:—Suppose the combined piston and trunk P is almost raised to the top of its stroke, by admitting water through a cock at Q, a cottar is able to be inserted through the hole H. Upon water being again admitted, the links are raised and with them the cylinder C, thus relieving the top cottar (presently resting on the bow), which is then withdrawn and inserted in the first hole higher up. The water being now allowed to escape, the piston P and links L, with the large cylinder C attached, descend till the top cottar again rests on the bow B. The lower cottar is then free to be withdrawn and inserted in the first hole higher up, and this done we are ready to begin a new stroke, and so continuing the cylinder C is gradually lowered till it reaches the river bed. Four of these hydraulic cylinders are employed in lowering one foundation cylinder.

The water for all the hydraulic machinery is obtained from the pumps already mentioned.

The diggers used are of various types, but principally consist of those with hinged automatic doors, which are open when the digger is dropped into the ground, and closed by links in the act of withdrawal. These are used principally in excavating the sand and other soft materials, others having hydraulic cylinders for opening and closing strong toothed doors, in order to tear and bring away the softer rocks and hard clays, have been used, but to no extent. In some cases a centrifugal pump has been utilized to great advantage for taking out the silty sand within the cylinders. The main suction pipe has two inlets, to each of which is attached a flexible rubber pipe. While the nozzle of the one inlet is being held to the sand by a diver, the other is loose and sucking in clean water. By using this precaution the pump seldom gets choked, and with some kinds of deposit this method is found to give excellent results.

After this preliminary description you will readily follow the mode of working the pontoon during the sinking of a pier. The first thing necessary to be done is to float out the pontoon as nearly as possible to its true position, immediately over where the cylinders are to be sunk. It is taken to its place by means of the crane already on it, acting as a windlass, the ropes and chains being fastened to buoys and the piers of the old bridge. Placed in position it is only the work of a few minutes to drive away the temporary supports on which the legs are resting (the pins at this time being all removed) when they gradually sink to the bottom. The hydraulic apparatus used in raising the platform (already described and shown in Fig. 2, Plate XXII.) is now brought into requisition, and made to lift it to the desired height. This is generally attained when the bottom of the pontoon is about two feet under highwater level. The best, and occasionally the only time the pontoon can be brought into position, before being raised, is at high tide. It is the best because the platform is about as high as it requires to be, and occasionally it is the only time, on account of the depth of water required to float it in. Anchors and chains are now called into requisition to assist the legs in keeping the platform steady, which, by the way, is found to be remarkably so, even in the roughest weather. When standing on the platform during a high wind, and carefully watching the movement at high tide, when the waves are dashing against it, the oscillation is found to be very slight, even with both these adverse circumstances to its steadiness in play.

All done we have a fixed platform, above the influence of the tide, and at the same time in the best attainable position relative to the pier at which work is about to be commenced.

Upon the platform is also placed all the necessary apparatus for the lowering, sinking, and building of the cylinders (material of course excepted).

The cylinders are now built over one of the central openings of the platform, being brought in complete rings, for convenience in handling, as part of the fixing together has to be done while they are thus being built in position. As section after section of iron is added (within), on the inner side, is built a ring of brick in cement, thereby increasing the weight, which assists during the process of digging to sink the cylinder and also keep it in form, as well as fulfilling the primary object of its being there, namely, to insure the safety of the structure in the event of the iron being corroded away. While the rings are in course of being added, all at the same time is lowered by the hydraulic apparatus already described till the cylinder reaches the river bed. The digger is now set to work and gradually excavates the material from within the cylinder, and thereby makes a way for it to settle down into the ground, and this is continued until it reaches its proper depth.

Although apparently easy and simple on paper the difficulties in the way preventing the desired end being attained are sometimes enormous; for example, you may come on a bed of boulders (this is found in many piers, being the protecting rubble of the old bridge piers) or even one large one, say one quarter within and the remainder outside of the cylinder, or get into clay so hard that the digger can barely cut into it, and yet so leaky as to make it impossible to pump the cylinder dry. Or there may be difficulties, the causes of which if known, could be as easily counteracted and overcome, as was the case when the sand saddened within the cylinder during the ebbing of the tide, on account of the water being higher within than without; the digger in these circumstances brought up only a small quantity at a time, nothing to be compared to what was done when the water was kept a little lower within than without. This is easily accomplished by the artificial means of pumping, the effect of which is to cause a little water to be constantly leaking through the sand into the cylinder, thereby keeping it loose and consequently making it easy to be dug into.

At other times the diggers are completely useless for excavating the material within the cylinders; a good alternative (if at all possible) in a case of this kind is to force the cylinder down by piling on weights till it becomes practicable to pump it dry, after which it can be dug out by hand. Before this has been accomplished in some cases it has been necessary to add as much as 400 tons of artificial loading to some of the 15-feet diameter cylinders. If the cylinder cannot be made watertight, then in a case of this kind resort has to be had to divers.

When a cylinder has reached the desired depth, and provided the bottom is satisfactory, filling in with concrete is commenced and continued till it reaches the top of the ironwork. The material for making the concrete, (gravel, and cement) is in most cases lowered from the old viaduct, which is only 60 feet to the eastward, and runs parallel with the new, except a short piece at the ends. The gravel is emptied out of the trucks into a shoot resting on the pontoon platform, and is there mixed and afterwards thrown or lowered into the cylinders, as the case may require. The second cylinder having been placed in position in a similar manner to the first, the platform is now lowered and at high tide is floated away over the top of the now sunk cylinders, the tops of which are only visible at extreme low water, thus leading the uninitiated to suppose little has been done because little is seen.

Cast-iron weights are now built on girders above the cylinders for the purpose of testing the sufficiency of the foundation. Sufficient weight is laid on to cause a pressure of five tons per square foot on the whole area under the cylinders. If they sink at all these weights are allowed to remain until all indications of such are stopped, after which they are transferred on to the next set by means of a wire cable or barge. It is here worthy of notice that the test load placed on the piers is 33⅓ per cent. in excess of the weight that would be brought, although the two lines were fully loaded with trains.

On the removal of the weights temporary caissons are fixed to the permanent cylinders by bolts and pumped dry. The remaining blue brick, outer shell, concrete, and stone work above low-water is then executed. Twenty feet down into this are built the holding-down bolts, 16 in number, in each pier, all 2¼ inches diameter. The caissons are removed and afterwards the connecting piece between the cylinders and the remainder of the pier is built up to and under pinned beneath the iron base on which the wrought-iron superstructure rests.

Progress is thus going on at several piers at one and the same time.

1st,—The pontoon, lowering, digging, and concreting.

2nd,—Testing the value of the foundations.

3rd,—Building under high water, within the temporary caissons.

4th,—Finishing remainder of pier, to underside of ironwork.

This again is but the starting point, from which the iron superstructure, as shown in Fig. 5, Plate XXIV., begins to rise (in stages also) to be followed by the placing of the girders and flooring, on which, finally, the track is laid.

Although the advantages gained by using pontoons, such as those described are apparent to all, it is at the same time evident, they could not be used to advantage, except on works of some magnitude where, for instance, there are a goodly number of piers to be put down, and also difficulties to be overcome, for grappling which, they are peculiarly suited.

The new Tay Viaduct furnishes such work and difficulties.

The pontoon on the Dundee side, sunk and concreted, one complete pier (of two cylinders, 10 feet diameter each) per week, for nearly two months on end. The greatest difficulty to contend with being the shallowness of the water in which it had to work. The depth to which each of these cylinders is sunk varies from about 16 feet to 26 feet under the bed of the river.

Such is a very brief resumé of the foundation work, and the mode by which it is being accomplished, at this Viaduct, at the present time. Time alone will tell, when the results are balanced, if the decision was altogether wise, which fixed on this novel method of carrying out a vast undertaking.

Institution of Engineers and Shipbuilders
IN SCOTLAND

(INCORPORATED).

TWENTY-EIGHTH SESSION, 1884-85.

MINUTES OF PROCEEDINGS.

THE FIRST GENERAL MEETING of the TWENTY-EIGHTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 28th October, 1884, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of Annual General Meeting of 22nd April, 1884, was read and approved, and signed by the President.

The PRESIDENT delivered his Inaugural Address.

On the motion of Mr ROBERT MANSEL, a hearty vote of thanks was accorded the President for his address.

The Discussions of the following Papers, read at Annual General Meeting in April, were proceeded with and terminated, and a vote of thanks awarded the Authors :—

On "The Properties of Screw Piles," by Mr WM. MURRAY, C.E.

On "Fog Signalling as Applied to Lighthouses," by Mr GEORGE SLIGHT, Jun

On "Approximation to Curves of Stability from Data for Known Ships," by Messrs F. P. PURVIS and B. KINDERMANN.

Members elected at last General Meeting were presented with Diploma of Membership.

THE SECOND GENERAL MEETING of the TWENTY-EIGHTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 25th November, 1884, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 28th October, 1884, was read and approved, and signed by the President.

A Paper on "Manipulating the Material and Drilling the Great Tubes required in the Forth Bridge," by Mr ANDREW S. BIGGART. was read, a discussion followed and was continued till next General Meeting.

A Paper on "Energy and Entropy and their Applications in the Theories of Air and Steam," by Mr HENRY DYER, C.E., was read, the discussion of which was deferred till next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

AS MEMBERS:—

Mr ANDREW S. BIGGART, Forth Bridge Works, South Queensferry.
Mr E. WALTON FINDLAY, Ardeer, Stevenston.
Mr JOHN WILDRIDGE, Consulting Engineer, Sydney.

AS GRADUATES:—

Mr ARCHIBALD M'BETH, Apprentice Engineer, 111 Govan Road.
Mr THOMAS MILLAR, Ship Draughtsman, 31 Grange Road, West, Jarrow-on-Tyne.

THE THIRD GENERAL MEETING of the TWENTY-EIGHTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 23rd December, 1884, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 25th November, 1884, was read and approved, and signed by the President.

Mr **Henry Dyer** gave notice of the following motion:—"That the Council be requested to consider how the Library may be made more worthy of the Institution, and to report to an early meeting."

The discussion of the following Papers was resumed and terminated:

On "Manipulating the Material and Drilling the Great Tubes required in the Forth Bridge," by Mr **Andrew S. Biggart**.

On "Energy and Entropy and their Applications in the Theories of Air and Steam," by Mr **Henry Dyer**, C.E.

Votes of thanks were awarded the Authors of these Papers.

Mr **William Denny** read his Paper on "Mr Mansel's and the late Mr Froude's Methods of Analysing the Results of Progressive Speed Trials," a discussion followed, and was continued till next General Meeting.

It was agreed that Mr Mansel's "Letter of Reclamation," 1884, and also Mr Froude's letter to Mr Mansel, of date 23rd September, 1876, should be published as forming part of the discussion on this paper.

The reading of the other Papers on "Notice" was deferred till another General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

AS MEMBERS:—

Mr **John W. W. Drysdale**, Engineer, 5 Whitehill Gardens.
Mr **Peter N. Cunningham**, Engineer, 5 North-east Park Street.
Mr **Finlay Finlayson**, Mechl. Engineer, Glengarnock Steel Works
Mr **John M'Beth**, Master Shipwright, 5 Park Street, Kinning Park.
Mr **James M'Lellan**, Mechanical Engineer, 10 West Garden Street.
Mr **John M'Neil**, Mechanical Engineer, Helen Street, Govan.
Mr **Wm. H. Nisbet**, Mechanical Engineer, Mavisbank, Partickhill.
Mr **James Williamson**, Shipbuilder, Barclay, Curle, & Co., Limited Whiteinch.

AS AN ASSOCIATE:—

Mr W. S. C. BLACKLEY, Iron Merct., Blackley Young & Co., Holm St.

AS GRADUATES:—

Mr ARTHUR C. AUDEN, App. Engineer, 9 Carmichael Street, Govan.
Mr D. C. GLEN, Jun., Apprentice Engineer, 14 Annfield Place.
Mr JOHN HOWARTH, App. Mechanical Engineer, 37 Bentinck Street.
Mr ROBERT LOGAN, Ship Draughtsman, 3 Hayburn Cres., Partick.
Mr JAS. M'EWEN M'INTYRE, Engineer Draughtsman, Dalmuir.
Mr W. J. MARSHALL, Engineering Draughtsman, 3 Minerva Street.
Mr R. MANSEL, Jun., App. Ship Draughtsman, 4 Clyde View, Pt'k.
Mr JAMES G. REID, Jun., App. Ship Draughtsman, 4 Holland Place
Mr DAVID W. STURROCK, Engineer Draughtsman, 11 Florence Place.
Mr WM. THOMSON, Engineering Student, 15 Burnbank Gardens.
Mr JOHN THOMSON, Jun., Apprentice Engineer, 15 Burnbank Gardens.

THE FOURTH GENERAL MEETING of the TWENTY-EIGHTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 27th January, 1885, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 23rd December, 1884, was read and approved, and signed by the President.

In accordance with previous notice of motion, Mr HENRY DYER moved as follows:—"That the Council be requested to consider how the Library may be made more worthy of the Institution, and to report to an early meeting." This was seconded by Mr JAMES LANG, and being put to the Meeting was agreed to.

Mr CHARLES C. LINDSAY proposed the following motion:—"That the sum of £20 or thereby shall be expended annually out of the funds of the Institution in the purchase of books for the Library in addition to the ordinary expenditure in binding, &c., all such books to be chosen from the Recommendation Book and approved by the Council." This was seconded by Mr ROBT. DUNCAN, of Whitefield.

As it was explained to the Meeting that the Council had already adopted this resolution, and that it therefore came before the General Meeting under Rule 47 as an alteration of the Bye-Laws, the motion was then put to the Meeting, and was agreed to.

The discussion of Mr WILLIAM DENNY'S Paper on "Mr Mansel's and the late Mr Froude's Methods of Analysing the Results of Progressive Speed Trials," was resumed and occupied the rest of the evening. On the motion of Mr HENRY DYER the discussion was adjourned to next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

AS MEMBERS:—

Mr WILLIAM ARROL, Mechanical Engineer, 10 Oakley Terrace.
Mr PETER FYFE, Mechanical Engineer, 234 Parliamentary Road.
Mr CHARLES A. KNIGHT, Engineer, 107 Hope Street.
Mr JAMES ROWAN, Marine Engineer, 231 Elliot Street.
Mr GEORGE W. THODE, Mechanical Engineer, 107 Hope Street.

AS GRADUATES:—

Mr WM. D. FERGUSON, Assistant Engineer, 63 Finlay Drive.
Mr ALEX. M. GORDON, Ship Draughtsman, 3 Wallace Grove Place.
Mr JOHN M'MILLAN, Engineering Student, 26 Ashton Ter., Hillhead.
Mr JAMES L. PROUDFOOT, App. Civil Engineer, 154 W. George St.

THE FIFTH GENERAL MEETING of the TWENTY-EIGHTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 24th February, 1885, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 27th January, 1885, was read and approved, and signed by the President.

The discussion of Mr WILLIAM DENNY'S Paper on "Mr Mansel's and the late Mr Froude's Methods of Analysing the Results of Progressive Speed Trials," was resumed and terminated, and a vote of thanks awarded Mr Denny for his Paper.

The discussion of Mr ALLAN CLARK'S Paper on "Electrical Navigation," was deferred till next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

AS MEMBERS:—

Professor FRANCIS ELGAR, Naval Architect, 17 University Gardens.
Mr JAMES SAMUEL, Jun., Mechanical Engineer, 238 Berkeley Street.

AS AN ASSOCIATE:—

Mr ROBERT DARLING, Manager, North British Steam Packet Co., 5 Summerside Place, Leith.

AS GRADUATES:—

Mr WM. S. DAWSON, Engineering Draughtsman, 24 Glen St., Paisley.
Mr JOHN INGLIS, Ship Draughtsman, Bonnington Brae, Edinburgh.
Mr JOHN LANG, Draughtsman, 6 Elderslie Street, Anderston.
Mr JOHN T. RAMAGE, Apprentice Engineer, The Hawthorn's, Bonnington, Edinburgh.
Mr CHARLES H. WANNOP, Draughtsman, 12 Derby Street.

THE SIXTH GENERAL MEETING of the TWENTY-EIGHTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 24th March, 1885, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 24th February, 1885, was read and approved, and signed by the President.

The discussion of Mr ALLAN CLARK'S Paper on "Electrical Navigation," was terminated and a vote of thanks awarded Mr CLARK for his Paper.

The following Papers were read :—

On "A Continuous Regenerative Gas Kiln for Burning Fire-bricks, Pottery, &c.," by Mr JOHN MAYER, F.C.S., and on "The Butt Fastenings of Iron Vessels," by Mr STAVELEY TAYLOR.

Discussions followed and were continued to next General Meeting.

Votes of thanks were awarded the authors of the Papers read.

Mr AND. MACLEAN and Mr DAVID KINGHORN were unanimously appointed Auditors of the Annual Financial Accounts.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows :—

AS MEMBERS :—

Mr JOHN B. CAMERON, Engineer, 160 Hope Street.
Mr EDMUND MOTT, Board of Trade Surveyor, 7 York Street.
Mr ALEXANDER THOMSON ORR, Mechanical Engineer, Messrs Hall, Russell, & Co., Aberdeen.
Mr W. CARLILE WALLACE, Mechl. Engineer, Maryland, Dumbarton.

AS AN ASSOCIATE :—

Mr JAMES S. GARDNER, Engineering Lithographer, 52 North Frederick Street.

AS GRADUATES:—

Mr Alexander Bishop, Assistant Engineer, 3 Germiston Street.
Mr Matthew Ritchie Brown, Engine Draughtsman, 6 Hamilton Place, Clydebank.
Mr Robt. Elliot, B.Sc., Engineering Student, 25 St. Vincent Cresct.
Mr William Linton, Ship Draughtsman, 1 Carmichael St., Govan.
Mr Fred. Lobnitz, Engineer Apprentice, 55 Thomson St., Govan.
Mr Frederick William Zucker, Engineering Draughtsman, 139 West Bridgend, Dumbarton.

The Twenty-Eighth Annual General Meeting of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 28th April, 1885, at 8 P.M.

Professor James Thomson, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 24th March, 1885, was read and approved and signed by the President.

The President having to leave on account of another engagement, vacated the Chair in favour of Mr Charles C. Lindsay, C.E., Vice-President.

The Treasurer's Annual Financial Statement was submitted and adopted.

The Library Committee's Report as to Library Books was read and adopted.

The proposal by the Council to alter the Bye-Laws dealing with premiums for Papers read, was, on the motion of Mr Henry Dyer, seconded by Mr John Mayer, unanimously adopted; the alteration is as follows:—"The Council may recommend premiums of books in lieu of, or in addition to, the Gold Medals. The values of such premiums of books to be determined by the Council."

The awards for Papers read Session 1883-84 were then made.

The Institution Medal was awarded to Mr RALPH MOORE for his Paper on "Cable Tramways," and the Marine Engineering Medal to Mr J. H. BILES for his Paper on "The Stability of Ships at Launching"

A premium of Books was awarded Mr R. L. WEIGHTON for his paper on "The Compound Engine Viewed in its Economical Aspect."

A premium of Books was also awarded Messrs F. P. PURVIS and B. KINDERMANN for their Paper on "Approximation to Curves of Stability from Data for Known Ships."

The election of Members of Council then took place.

Mr C. P. HOGG and Mr JOHN INGLIS, Jun., were unanimously elected Vice-Presidents of the Institution.

The following gentlemen were, by a majority of votes, elected Councillors:—Professor ELGAR, LL.D., CHARLES C. LINDSAY, C.E., JOHN HENDERSON, Jun., HENRY DYER, C.E., M.A., and GEORGE RUSSELL.

Mr JAMES M. GALE, C.E., was unanimously re-elected Treasurer.

Mr JOHN THOMSON was unanimously re-elected to represent the Institution at the Council of the College of Science and Arts.

Proposals by Mr GEORGE RUSSELL and Mr HENRY DYER, relating to membership and office-bearers, were remitted to the Council for consideration.

The discussion of Mr JOHN MAYER'S Paper on "A Continuous Regenerative Gas Kiln for Burning Fire-bricks, Pottery, &c.," was resumed and terminated.

The continued discussion of Mr STAVELEY TAYLOR'S paper on "The Butt Fastenings of Iron Vessels" was deferred till First General Meeting of next Session.

The Papers by Mr ALEXANDER FINDLAY on "American Railway Freight Cars," and by Mr ANDREW S. BIGGART on "Sinking the Cylinders by Pontoons for the Tay Bridge," were held as read, the Papers to be printed and the discussion taken at First General Meeting of next Session.

The Chairman announced that the candidates balloted for had been elected, the names of these gentlemen being as follows :—

AS MEMBERS :—

Mr JOHN AULD, Mechanical Engineer, Whitevale Foundry.
Mr WALTER BROWN, Chief Draughtsman, London Works, Renfrew.
Mr PETER TAYLOR, Shipyard Manager, London Works, Renfrew.

TREASURER'S STATEMENT—1884-85.

DR. GENERAL FUND. CR.

To Balance in Union Bank at close of Session 1883-84,				£315	13	10
Subscriptions received :						
Session 1884-85, ..	£660	10	0			
Arrears of Previous Sessions,	57	0	0			
	£717	10	0			
Deduct Entry Money transferred to Building Fund, ..	8	0	0			
				709	10	0
Sales of Transactions, ...				10	10	0
Bank Interest, ...				4	10	10
				£1040	4	8

By Amount paid Treasurer of House Committee as Institution's proportion of Expenditure, for Session 1884-85, ...				£128	19	0
Do. Rent of Room for Lectures on Naval Architecture,				11	1	0
Printing, ...				156	16	5
Lithography, ...				126	1	0
Graduate Section Medal, Session 1883-84, ...				1	7	6
Salary to Secretary, ...				125	0	0
Commission Collection of Arrears of Subscriptions, viz. :—						
For Session 1884-85, ...	£384	0	0			
For Previous Sessions, ...	57	0	0			
	£441	0	0 at 5 %	22	1	0
Postages, and Delivery of Annual Volumes, ...				59	19	7
Stationery, &c., ...				12	13	5
New Books for Library, ...				20	17	5
Book Binding, ...				5	1	0
Cash to New Buildings Account to meet Interest on Loan, ...				16	0	0
Petty Cash, ...				2	9	7
Balance in Union Bank, ...				351	17	9
				£1040	4	8

MARINE ENGINEERING MEDAL FUND.

Dr.		Cr.	
To Balance in Union Bank at close of Session 1883-84, ...	£33 10 10	*By* Balance in Union Bank, ...	£44 1 3
Interest on Capital lent to New Buildings Account, ...	10 0 0		
Bank Interest, ...	0 10 5		
	£44 1 3		£44 1 3

RAILWAY ENGINEERING MEDAL FUND.

Dr.		Cr.	
To Balance in Union Bank at close of Session 1883-84, ...	£27 13 8	*By* Balance in Union Bank, ...	£34 2 2
Interest on Capital lent to New Buildings Account, ...	6 0 0		
Bank Interest, ...	0 8 6		
	£34 2 2		£34 2 2

GRADUATE MEDAL FUND.

Dr.		Cr.	
To Balance in Union Bank at close of Session 1883-84,	£21 2 3	*By* Balance in Union Bank, ...	£21 12 3
Bank Interest, ...	0 10 0		
	£21 12 3		£21 12 3

BUILDING FUND.

Dr.			Cr.		
To Balance in Union Bank at close of Session 1883-84,		£234 6 0	*By* Balance in Union Bank, ...		£246 1 5
Entry Money, ...		8 0 0			
Bank Interest, ...		3 15 5			
		£246 1 5			£246 1 5

NEW BUILDINGS ACCOUNT.

Dr.			Cr.		
To Capital to meet Cost of New Buildings, viz. :—			*By* Paid on New Buildings to date, ...		...£2,047 8 1
From General Fund, ...	£542 15 7		Interest on Loans, viz. :—		
,, Marine Engineering Medal Fund, ...	351 11 2		To Marine Engineering Medal Fund,	£10 0 0	
Railway Engineering Medal Fund, ...	213 13 3		,, Railway Engineering Medal Fund,	6 0 0	
,, Building Fund, ...	939 8 1				16 0 0
		£2,047 8 1			
Cash received from General Fund to meet Interest on Loans, ...		16 0 0			
		£2,063 8 1			£2,063 8 1

Glasgow, *18th April, 1885.*—We have examined the foregoing Annual Financial Statement of Treasurer, the Accounts of the Marine and Railway Engineering Medal Funds, the Graduate Medal Fund, the Building Fund, and the New Buildings Account, and find the same duly vouched and correct, the Amounts in Bank being as stated.

(Signed) ANDw. MACLEAN, } Auditors.
D. KINGHORN, }

SUBSCRIPTION ACCOUNT.

Dr.

To Subscriptions due as per Roll :—			
Arrears due at close of last Session, ...		£139 10 0	
Deduct Irrecoverable, ...		56 0 0	
		£83 10 0	
Add elected at Annual General Meeting, April, 1884, ...		6 10 0	
			£90 0 0
SESSION 1884-85 :—			
371 Members	at £1 10 0 —	£556 10 0	
1 New Member	,, 2 10 0 —	2 10 0	
12 ,,	,, 2 0 0 —	24 0 0	
9 ,,	,, 1 10 0 —	13 10 0	
33 Associates	,, 1 0 0 —	33 0 0	
1 New Associate	,, 1 10 0 —	1 10 0	
2 ,, ,,	,, 1 5 0 —	2 10 0	
174 Graduates	,, 0 10 0 —	87 0 0	
			720 10 0
			£810 10 0

Cr.

By Subscriptions received, as per Cash Book, viz. :—			
Arrears of Sessions previous to Session 1884-85, ...			£57 0 0
SESSION 1884-85 :—			
342 Members	at £1 10 0 —	£513 0 0	
1 New Member	,, 2 10 0 —	2 10 0	
12 ,,	,, 2 0 0 —	24 0 0	
8 ,,	,, 1 10 0 —	12 0 0	
31 Associates	,, 1 0 0 —	31 0 0	
1 New Associate	,, 1 10 0 —	1 10 0	
2 ,, ,,	,, 1 5 0 —	2 10 0	
148 Graduates	,, 0 10 0 —	74 0 0	
			660 10 0
			£717 10 0
Arrears due for Session 1884-85, ...		£60 0 0	
Arrears due for previous Sessions,		33 0 0	
			93 0 0
			£810 10 0

BANK ACCOUNT.

Dr.

To Balances at close of Session 1883-84 :—	
General Fund, ...	£315 13 10
Marine Engineering Medal Fund, ...	33 10 10
Railway Engineering Medal Fund, ...	27 13 8
Graduate Medal Fund, ...	21 2 3
Building Fund, ...	234 6 0
Amounts lodged, Session 1884-85, ...	478 13 1
Interest, Session 1884-85, ...	9 15 2
	£1,120 14 10

Cr.

By Amounts Drawn, Session 1884-85, ...	£423 0 0
Balances in Union Bank, ...	697 14 10
	£1,120 14 10

CAPITAL ACCOUNT.

	£	s.	d.	£	s.	d.
GENERAL FUND.						
Loan to New Buildings Account, ...	£542	15	7			
Cash in Union Bank, ...	351	17	9			
				£894	13	4
MARINE ENGINEERING MEDAL FUND.						
Loan to New Buildings Account, ...	£351	11	2			
Cash in Union Bank, ...	44	1	3			
				395	12	
RAILWAY ENGINEERING MEDAL FUND.						
Loan to New Buildings Account, ...	£213	13	3			
Cash in Union Bank, ...	34	2	2			
				247	15	5
GRADUATE MEDAL FUND.						
Cash in Union Bank, ...				21	12	3
BUILDING FUND.						
Amount to New Buildings Account, ...	£939	8	1			
Cash in Union Bank, ...	246	1	5			
				1,185	9	6
ARREARS OF SUBSCRIPTIONS.						
Arrears due for Session 1884-85, ... —	£60	0	0			
Do. previous Sessions, ...	33	0	0			
				93	0	0
				£2,838	2	11

HOUSE EXPENDITURE ACCOUNT.* (ABSTRACT 1884-85.)

Dr.		
To Rents for Letting Rooms, ...		£53 10 6
Amounts Received by Treasurer to meet Expenses, viz.:—		
From Institution of Engineers and Shipbuilders, ...	£128 19 0	
From Philosophical Society, ...	145 5 2½	
		274 4 2½
Balance due Treasurer, ...		19 2 7
		£346 17 3½

Cr.	
By Balance due Treasurer, ...	£4 1 5
Interest on Bond, ...	132 3 8
Salary to Curator, ...	90 0 0
Expenses of Attendant at Library, Cleaning, &c.,	33 5 6
Taxes, ...	24 14 11½
Feu-duty, ...	0 18 4
Gas, ...	15 0 0
Water, ...	7 13 4
Coals, ...	6 12 0
Insurance, ...	7 15 0
Repairs and Furnishing, ...	24 13 1
	£346 17 3½

*The Account of the House Committee is kept by Mr John Mann, C.A., Treasurer to the Committee, and is periodically audited by the Auditors appointed by the Institution and the Philosophical Society.

W. J. MILLAR, *Secretary to House Committee.*

Deceased Members.

During the Session 1884-85 the Institution has lost from the Roll of Membership the following gentlemen who have been long connected with the Institution and took an interest in its operations. These are:—

Captain WM. BROWN, Glasgow, who joined the Institution as an Associate in 1877.

Mr ARCHIBALD GRAY, Dalry, who joined the Institution as a Member in 1861.

Mr J. I. M'DERMENT, Ayr, who joined the Institution in 1857, the year in which it was founded.

Mr WALTER NEILSON, of Summerlee, who was one of the Original Members of the Institution.

Mr WALTER NEILSON, as an Original Member, has been connected with the Institution since its commencement in 1857. While under his father, Mr John Neilson, in the Oakbank Foundry, he had a varied experience of engineering work, the "Fairy Queen," the first iron steamer plying on the Clyde, having been built at the Oakbank Foundry, and taken from thence to the river and launched in 1831. Mr Neilson afterwards became connected with the pig-iron manufacture, and was one of the partners of the Summerlee Iron Coy.

DONATIONS TO LIBRARY.

Results of Trials made in Her Majesty's Screw Ships and Vessels from 1880-84. From the Lords Commissioners of the Admiralty.

The Forth Bridge, by B. Baker, Esq. From the Author.

Life of Graham, by Dr R. Angus Smith. From J. J. Coleman, Esq., F.C.S.

Inaugural Address—Chair of Naval Architecture and Marine Engineering, Glasgow University—by Professor Elgar, LL.D. From the Author.

Guide Book of Canada. From the Canadian Pacific Railway Co.

Hydraulic Pumping, by D. Johnston, Esq. From the Author.

Chain Cables and Chains, by Thomas W. Traill, Esq., C.E., R.N. From the Author.

Descriptive Sketch of Canada, with Maps, &c. From the Directors of the Geological Survey of Canada.

Map of Canada. From the Directors of the Canadian Pacific Railway Co.

Sketch of Rise and Progress of Lloyd s Register. From the Chairman and Committee of Lloyd's Register of British Shipping.

LIST OF NEW BOOKS RECENTLY ADDED TO THE LIBRARY.

Spon's Dictionary of Engineering, in Eight Divisions.

Supplement to do., in Three Divisions.

Tredgold's Elementary Principles of Carpentery. Ed. by E. W. Tarn.

Sanitary Engineering. Baldwin Latham.

Disposal of Sewage. Henry Robinson.

The Municipal and Sanitary Engineers' Handbook. Percy Boulnois.

Electricity, Its Theory, Sources, and Application. J. T. Sprague.
Fuel and Water. Professor Schwackhöfer and W. R. Browne, M.A.
Civil and Hydraulic Engineering. Henry Law. Ed. by D. K. Clarke.
Manual of Electro-Metallurgy. James Napier.
The Stability of Ships. Sir E. J. Reed.
The Steam Engine. Arthur Rigg.
Cresy's Civil Engineering.
Manual of Telegraph Construction. J. C. Douglas.
Strength of Iron and Steel. Professor Thurston.
Tunnelling. Drinker.
Heat. Thomas Box.
Manual of Geology, Vol. I. Phillips.
Cotterill's Mechanics.

THE INSTITUTION EXCHANGES TRANSACTIONS WITH THE FOLLOWING SOCIETIES:—

Institution of Civil Engineers.
Institution of Civil Engineers of Ireland.
Institution of Mechanical Engineers.
Institution of Naval Architects.
Institute of Mining and Mechanical Engineers.
Institute of Mining, Civil, and Mechanical Engineers.
Iron and Steel Institute.
Liverpool Polytechnic Society.
Literary and Philosophical Society of Manchester.
Mining Institute of Scotland.
Patent Office, London.
Philosophical Society of Glasgow.
Royal Scottish Society of Arts.
Royal Dublin Society.
South Wales Institute of Engineers.
Society of Engineers.
Society of Arts.
Association of Employers, Foremen, and Draughtsmen Manchester.

American Society of Civil Engineers.
Geological Survey of Canada.
Smithsonian Institution, U.S.A.
Stevens Institute of Technology, U.S.A.
Bureau of Steam Engineering, Navy Department, U.S.A.
Royal Society of Tasmania.
Royal Society of Victoria.
Royal Academy of Sciences, Lisbon.
Société des Ingénieurs Civils de France.
Société Industrielle de Mulhouse.
Société d'Encouragement pour l'Industrie Nationale.
Société des Anciens Elèves des Ecôles Nationales d'Arts et Métiers.
Société des Sciences Physiques et Naturelles de Bordeaux.
Austrian Engineers' and Architects' Society, Vienna.
Engineers and Architects' Society of Naples.
The Association of Civil Engineers of Belgium.
Master Car Builders' Association, U.S.A.

PUBLICATIONS RECEIVED PERIODICALLY IN EXCHANGE FOR INSTITUTION TRANSACTIONS:—

Annales Industrielles.
Annales de la Propriété Industrielle.
Colliery Guardian.
Engineering.
Iron.
Iron and Coal Trades' Review.
Journal de L'Ecole Polytechnic.
Mining Journal.
Nature.
Revue Industrielle.
The Engineer.
The Steamship.
The Machinery Market.
The Marine Engineer.
The American Manufacturer and Iron World.
The Contract Journal.

The Library of the Institution, at the Rooms, 207 Bath Street, is open daily from 9-30 a.m. till 8 p.m.; on Meeting Nights of the Institution and Philosophical Society, till 10 p.m.; and on Saturdays till 2 p.m. Books will be lent out on presentation of Membership Card to the Sub-Librarian.

Members have also the privilege of consulting the Books in the Library of the Philosophical Society.

The use of Library and Reading Room is open to Members. Associates, and Graduates.

The Portrait Album lies in the Library for the reception of Members' Portraits.

Members are requested when forwarding Portraits to attach Signature to bottom of Carte.

The Library is open during Summer from 9·30 a.m. till 5 p.m.; and on Saturdays till 2 p.m.

Copies of Catalogue of Books in Library may be had from the Secretary.

Members of this Institution, who may be temporarily resident in Edinburgh, will, on application to the Secretary of the Royal Scottish Society of Arts, at his Office, 117 George Street, be furnished with Billets for attending the Meetings of that Society.

The Meetings of the Royal Scottish Society of Arts are held on the 2nd and 4th Mondays of each Month, from November till April, with the exception of the 4th Monday of December.

LIST

OF

HONORARY MEMBERS, MEMBERS, ASSOCIATES, AND GRADUATES

OF THE

Institution of Engineers and Shipbuilders in Scotland

(INCORPORATED),

SESSION 1884-85.

HONORARY MEMBERS.

James Prescott Joule, LL.D., F.R.S., 12 Wardle Road, Sale, near Manchester.

Professor Charles Piazzi Smyth, F.R.S.E., Astronomer-Royal for Scotland, 15 Royal Terrace, Edinburgh.

Professor Sir William Thomson, A.M., LL.D., D.C.L., F.R.SS.L. and E., Professor of Natural Philosophy in the University of Glasgow.

Professor R. Clausius, the University, Bonn, Prussia.

Sir Joseph Whitworth, Bart., C.E., LL.D., F.R.S., Manchester.

Professor John Tyndall, D.C.L., LL.D., F.R.S., &c., Royal Institution, London.

His Grace the Duke of Sutherland, Trentham, Stoke-upon-Trent.

Sir Wm. G. Armstrong, C.B., LL.D., D.C.L., F.R.S., Newcastle-on-Tyne.

Professor H. Von Helmholtz, Berlin.

MEMBERS.

DATE OF ELECTION.			
1883, Mar. 20:	Geo. A.	Agnew,	3 Gladstone Terrace, Govan.
1859, Jan. 19:	James	*Aitken, jun.,	Shipbuilder, Whiteinch, Glasgow.
1860, Dec 26:	William	Aiton,	Sandford Lodge, Peterhead.
Original:	William	Alexander,	23 India Street, Glasgow.
Original:	Alexander	Allan,	Glen House, The Valley, Scarborough.
1872, Feb. 27:	A. B.	Allan, C.E.,	Burgh Surveyor, Burgh Chambers, Govan.
1869, Jan. 20:	William	Allan,	Scotland House, Sunderland.
1864, Dec. 21:	James B.	Alliott,	The Park, Nottingham.
G. 1865, Feb. 15: M. 1877, Dec. 18:	Wm. M.	Alston,	24 Burnbank Gardens. Glasgow.
1880, Nov. 23:	Thomas	Anderson,	Government Dockyard, Bombay.
G. 1874, Feb. 24: M. 1880, Nov. 23:	James	Anderson,	100 Clyde St., Glasgow.
1860, Nov. 28:	Robert	Angus,	Lugar Ironworks, Cumnock.
1883, Dec. 18:	J. Cameron	Arrol,	18 Blythswood Square, Glasgow.
1875, Dec. 21:	Thomas A. *(Vice-President.)*	Arrol,	18 Blythswood Square, Glasgow.
1885, Jan. 27:	William	Arrol,	10 Oakley Ter., Glasgow.
Original:	David	Auld,	65 Rochester St., Glasgow.
1885, Apr. 28:	John	Auld,	Whitevale Foundry, Glasgow.
1881, Oct. 25:	Allan W.	Baird,	Eastwood Villa, St. Andrew's Drive, Pollokshields.

Names marked thus * were Members of Scottish Shipbuilders Association at Incorporation with Institution, 1865.

Names marked thus † are Life Members.

1880, Feb. 24:	William N.	Bain,	Collingwood, Pollokshields, Glasgow.
1873, Apr. 22:	H. W.	Ball,	Cranstonhill Engine Works, Glasgow.
1858, Dec. 22:	Andrew	Barclay, F.R.A.S.,	Caledonian Foundry, Kilmarnock.
1876, Jan. 25:	James	Barr,	
1882, Mar. 21:	Prof. Archd.	Barr, B.Sc., C.E.,	The Yorkshire College, Leeds.
1868, Apr. 22:	Edward	Barrow,	Rue de la Province, Sud, Antwerp, Belgium.
1881, Mar. 22:	George H.	Baxter,	Ramage & Ferguson, Leith.
1875, Jan. 26:	Charles	Bell,	4 Clifton Place, Glasgow.
	David	*Bell,	Shipbuilder, Yoker, near Glasgow.
1868, Feb. 12:	Edward M.	Bell,	Tinplate Works, Coatbridge.
1880, Mar. 23:	Imrie	Bell, C.E.,	1 Victoria Street, Westminster, London, S.W.
1880, Nov. 2:	Alfred G.	Berry,	33 Carnarvon St., Glasgow.
G. 1883, Mar. 20: M. 1884, Nov. 25:	Andrew S.	Biggart, C.E.,	Forth Bridge Works, South Queensferry.
1884, Mar. 25:	John Harvard	Biles,	Clydebank Shipyard, near Glasgow.
1866, Dec. 26:	Edward	Blackmore,	Eagle Foundry, Greenock.
1864, Oct. 26:	Thomas	Blackwood,	Shipbuilder, Port-Glasgow.
1869, Feb. 17:	Geo. M‘L.	Blair,	127 Trongate, Glasgow.
1867, Mar. 27:	James M.	Blair,	38 Elmbank Cres., Glasgow.
1883, Jan. 23:	Chas. C.	Bone, C.E.,	23 Miller Street, Glasgow.
1883, Oct. 23:	William L.	Bone,	Ant and Bee Works, West Gorton, Manchester.
1874, Jan. 27:	Howard	Bowser,	13 Royal Crescent, W., Glasgow.
1880, Mar. 23:	James	Brand, C.E.,	109 Bath Street, Glasgow.
G. 1873, Dec. 23: M. 1884, Jan. 22:	James	Broadfoot,	55 Finnieston St., Glasgow.

1865, Apr. 26:	Walter	*Brock,	Engine Works, Dumbarton.
1859, Feb. 16:	Andrew	*Brown,	London Works, Renfrew.
1885, Apr. 28:	Walter	Brown,	Castlehill, Renfrew.
1880, Dec. 21:	William	Brown,	Albion Works, Woodville Street, Govan, Glasgow.
G. 1874, Jan. 27: M. 1884, Jan. 22:	William	Brown,	61 Queen Street, Renfrew.
1858, Mar. 17:	James	Brownlee,	23 Burnbank Gardens, Glasgow.
1877, Oct. 30:	Robert	Bruce,	12 KelvingroveSt., Glasgow.
1860, Dec. 26:	James C.	Bunten,	100 Cheapside St., Glasgow.
1866, Apr. 26:	Amedee	Buquet, C.E.,	15 Chemiss, St. Martin, Pontoise, S. O. France.
	Andrew	*Burns,	Hilton of Burleigh, Milnathort.
1880, Dec. 21:	James W.	Burns,	37 Bentinck St., Glasgow.
1881, Mar. 22:	Thomas	Burt,	371 New City Rd., Glasgow.
1884, Jan. 22:	Edward H.	Bushell,	G 19 Exchange Buildings, Liverpool.
1878, Oct. 29:	Edward B.	Caird,	8 Scotland St., Glasgow.
1878, Dec. 17:	James	Caldwell,	130 Elliot Street, Glasgow.
1885, Mar. 24:	John B.	Cameron,	160 Hope Street, Glasgow.
1875, Dec. 21:	J. C.	Cameron,	24 Pollok Street, Glasgow.
1868, Dec. 23:	David	Carmichael,	Ward Foundry, Dundee.
1859, Nov. 23:	Peter	Carmichael.	Dens Works, Dundee.
1862, Jan. 8:	John	Carrick,	6 Park Quadrant, Glasgow
1881, Nov. 22:	John H.	Carruthers,	Craigmore, Queen Mary Avenue, Crosshill, Glasgow.
1859, Oct. 26:	Robert	Cassels,	168 St. Vincent Street, Glasgow.
1867, Jan. 30:	Albert	Castel,	3 Lombard Court, London, E.C.
G. 1873, Dec. 23: M. 1883, Oct. 23:	Walter	Chambers,	24 Ulster Chambers, Belfast.

1883, Jan. 23:	John	Clark,	British India Steam Navigation Co., Calcutta.
1875, Oct. 26:	W. J.	Clark,	Southwick, near Sunderland.
1880, Nov. 2:	James	Clarkson,	Maryhill Engine Works, Maryhill, Glasgow.
1860, Apr. 11:	James	Clinkskill,	1 Holland Place, Glasgow.
1884, Feb. 26:	James T.	Cochran,	Duke Street, Birkenhead.
1881, Oct. 25:	George	Cockburn,	Rhodora Villa, St. Andrew's Drive, Pollokshields, Glasgow.
G. 1876, Dec. 19: M. 1884, Mar. 25:	Charles	Connell,	Whiteinch, Glasgow.
Original:	Robert	Cook,	Woodbine Cottage, Pollokshields, Glasgow.
G. 1876, Jan. 25: M. 1884, Jan. 22:	William M.	Cooke,	Gourlay Bros. & Co., Dundee.
1864. Feb. 17:	James	Copeland,	16 Pulteney St., Glasgow.
1864, Jan. 20:	William R.	Copland, C.E.,	146 West Regent Street, Glasgow.
1868, Mar. 11:	S. G. G.	Copestake,	Glasgow Locomotive Works. Little Govan. Glasgow.
1866, Nov. 28:	M'Taggart	Cowan, C.E.,	109 Bath Street, Glasgow.
1868, Apr. 22:	David	Cowan, C.E.,	Mount Gerald House, Falkirk.
1861. Dec. 11:	William	Cowan,	46 Skene Terrace, Aberdeen.
1883, Dec. 18:	Samuel	Crawford,	Clydebank, near Glasgow.
1881, Mar. 22:	William	Crockatt,	2 Marjory Place, Pollokshields, Glasgow.
1866, Dec. 26:	James L.	Cunliff,	Plewlands House, Merchiston, Edinburgh.
1872, Nov. 26:	David	Cunningham, C.E.,	Harbour Chambers, Dundee.
1884, Dec. 23:	Peter N.	Cunningham,	5 North East Park Street, Glasgow.
1869, Jan. 20:	James	Currie,	16 Bernard Street, Leith.

G. 1874, Feb. 24: M. 1882, Dec. 19:	James	Davie,	234 Cathcart Road, Crosshill, Glasgow.
1861, Dec. 11:	Thomas	Davison,	248 Bath Street, Glasgow.
1864, Feb. 17:	St. J. V.	Day, C.E.,	115 St. Vincent St., Glasgow.
1869, Feb. 17:	James	Deas, C.E..	Engineer, Clyde Trust, 7 Crown Gardens, Glasgow.
1882, Dec. 19:	J. H. L. Van	Deinse,	85 de Ruyterkade, Amsterdam.
1883, Nov. 21:	James	Denholm,	360 Dumbarton Road, Glasgow.
1866, Feb. 14:	A. C. H.	Dekke,	Shipbuilder, Bergen, Norway.
	Peter	*Denny,	Helenslee, Dumbarton.
1873, Feb. 18:	William	Denny,	Leven Shipy'd, Dumbarton.
G. 1878, Dec. 23: M. 1884, Jan. 22:	Peter	Dewar,	25 North Street, Glasgow.
1878, Mar. 19:	Frank W.	Dick, (*Member of Council.*)	405 Eglinton Street, Glasgow.
G. 1873, Dec. 24: M. 1878, Jan. 22:	James S.	Dixon,	170 Hope Street, Glasgow.
1882, Nov. 28:	John G.	Dobbie,	British India Steam Navigation Co., Mazagon Dockyard, Bombay.
1871, Jan. 17:	William	Dobson,	The Chesters, Jesmond, Newcastle-on-Tyne
1864, Jan. 20:	James	Donald,	Abbey Works, Paisley.
1876, Jan. 25:	James	Donaldson,	Fulbar Street, Renfrew.
1863, Nov. 25:	Robert	Douglas,	Dunnikier Foundry, Kirkcaldy.
1884, Dec. 23:	John W. W.	Drysdale,	5 Whitehill Gardens, G'gow.
1882, Oct. 24:	Chas. R.	Dubs,	Glasgow Locomotive Works, Glasgow.
1864, Oct. 26:	Robert	*Duncan. (*Past President; Vice President.*)	Shipbuilder, Port-Glasgow.
1881, Jan. 25:	Robert	Duncan, (*Member of Council.*)	Whitefield Engine Works, Govan, Glasgow.

1873, Apr. 22: Robert Dundas, C.E., 3 Germiston Street, Glasgow.
(*Member of Council.*)

1869, Nov. 23: David Jno. Dunlop, Inch Works, Port-Glasgow.

1877, Jan. 23: John G. Dunlop, 17 Goulton Road, Lower Clapton, London.

1880, Mar. 23: Hugh S. Dunn, Earlston Villa, Caprington, Kilmarnock.

1879, Dec. 23: Wm. T. Courtier Dutton 30 Gordon Street, Glasgow.

1883, Oct. 23: Henry Dyer, C.E., 8 Highburgh Terrace, Dowanhill, Glasgow.

1876, Oct. 24: Jn. Marshall Easton, Redholm, Helensburgh.

1885, Feb. 24: Prof. Francis Elgar, LL.D., F.R.S.E., 17 University Gardens, Glasgow.

1875, Oct. 26: James G. Fairweather, C.E., B.Sc., 8 Findhorn Place, Grange, Edinburgh.

John *Ferguson, Shipbuilder, Whiteinch, Glasgow.

G. 1869, Nov. 23: } M. 1878, Mar. 19: } John Ferguson, jun., Shipbuilder, Leith.

1874, Feb. 24: Immer Fielden, 6 Lorne Terrace, Holderness Road, Hull.

1880, Jan. 27: Alexander Findlay, Hamilton Road, Motherwell.

G. 1873, Dec. 23: } M. 1884, Nov. 25: } E. Walton Findlay, Ardeer, Stevenston.

1884, Dec. 23: Finlay Finlayson, Glengarnock Steel Works, Glengarnock.

Original: William Forrest, 77 Renfield St., Glasgow.

1872, Nov. 26: Thomas Forrest, M.E., Dumfries Ironworks, Dumfries.

1883, Dec. 18: Lawson Forsyth, 10 Grafton Sq., Glasgow.

1870, Jan. 18: William Foulis, Engineer, Corporation Gas Works, 42 Virginia St., Glasgow.

1880, Nov. 2:	Samson	Fox,	Leeds Forge, Leeds.
1862, Nov. 26:	Alexander	Fullarton,	Vulcan Works, Paisley.
1879, Nov. 25:	John	Frazer,	P. Henderson & Co., 15 St. Vincent Place, Glasgow.
1885, Jan. 27:	Peter	Fyfe,	234 Parliamentary Road, Glasgow.
1858, Nov. 24:	James M.	Gale, C.E., (*Past President; Member of Council, and Treasurer.*)	Engineer, Corporation Water Works, 23 Miller Street, Glasgow.
1862, Jan. 8:	Andrew	Galloway, C.E.,	St. Enoch Station, Glasgow.
1883, Oct. 23:	Gilbert H.	Garrett,	47 West Cumberland St., Glasgow.
1873, Dec. 23:	Bernard	Gatow,	Veritas Office, 29 Waterloo Street, Glasgow.
G. 1873, Dec. 23: M. 1882, Mar. 21:	Andrew	Gibb,	Rait & Gardiner, Millwall Docks, London.
1859, Nov. 23:	Archibald	*Gilchrist,	11 Sandyford Pl., Glasgow.
G. 1866, Dec. 26: M. 1878, Oct. 29:	James	Gilchrist, (*Member of Council.*)	Stobcross Engine Works, Finnieston Quay, Glasgow.
1859, Dec. 21:	David C.	Glen,	14 Annfield Place, Dennistoun, Glasgow.
1868, Nov. 25:	Thomas	Goldie,	Waverley Mills, Ceres Road, Cape of Good Hope.
1864, Feb. 17:	James	Goodwin,	Ironfounder, Ardrossan.
1866, Mar. 28:	Gilbert S.	Goodwin,	Alexandra Buildings, James Street, Liverpool.
1868, Mar. 11:	Joseph	Goodfellow,	136 Sackville Place, Stirling Road, Glasgow.
1858, Dec. 22:	Henry	*Gourlay,	Dundee Foundry, Dundee.
1882, Apr. 25:	H. Garrett	Gourlay,	Dundee Foundry, Dundee.
	Edwin	*Graham,	Osbourne, Graham, & Co., Hylton, Sunderland.
1858, Mar. 12:	George	Graham, C.E.,	Engineer, Caledonian Railway, Glasgow.

1876, Jan. 25:	Thomas M.	Grant,	4 Clayton Terrace, Dennistoun, Glasgow.
1871, Mar. 28:	Thomas	Gray,	Chapel Colliery, Newmains.
1862, Jan. 8:	James	Gray,	Pathhead Colliery, Cumnock, Ayrshire.
1870, Feb. 22:	P. B. W.	Gross, M.E.,	4 Albion Place, Cumberland Road, Bristol.
1881, Dec. 20:	L. John	Groves,	131 Hope Street, Glasgow.
1879, Nov. 25:	Robert	†Hadfield,	Hadfield Steel Foundry Co., Attercliffe, Sheffield.
1872, Feb. 27:	A. A.	Haddin, C.E.,	131 West Regent Street, Glasgow.
1881, Jan. 25:	William	Hall, jun.,	Shipbuilder, Aberdeen.
1876, Oct. 24:	David	Halley,	Burmeister & Wain, Copenhagen, Denmark.
G. 1873, Dec. 23: M. 1881, Nov. 22:	David C.	Hamilton, (*Member of Council.*)	Clyde Shipping Co., 21 Carlton Place, Glasgow.
G. 1866, Dec. 26: M. 1873, Mar. 18:	James	Hamilton, jr.,	Ardedynn, Kelvinside, Glasgow.
	John	*Hamilton.	22 Athole Gardens, Gl'gow.
G. 1869, Nov. 23: M. 1875, Feb. 23:	J. B.	Hamond,	The Victoria Engineering Coy., Victoria Works, Stockport.
1876, Feb. 22:	Walter	Hannah,	Board of Trade Surveyor, 7 York Street, Glasgow.
G. 1880, Nov. 2: M. 1884, Jan. 22:	Bruce	Harman,	R. Napier & Sons, Govan, Glasgow.
1878, Mar. 19:	Timothy	Harrington,	61 Gracechurch Street, London, E.C.
1875, Jan. 26:	Peter T.	Harris,	19 West St. (S.S.), Glasgow.
G. 1874, Feb. 24: M. 1880, Nov. 23:	C. R.	Harvey,	166 Renfrew St., Glasgow.
1864, Nov. 23:	John	Hastie,	Kilblain Engine Works, Greenock.

1871, Jan. 17:	William	Hastie,	Kilblain Engine Works, Greenock.
1879, Nov. 25:	A. P.	†Henderson,	30 Lancefield Quay, Glasgow.
1877, Feb. 20:	David	*Henderson,	Meadowside, Partick, Glasgow.
1873, Jan. 21:	John	†Henderson, jr.,	Meadowside, Partick, Glasgow.
1879, Nov. 25:	John L.	†Henderson,	Westbank House, Partick, Glasgow.
1878, Dec. 17:	William	Henderson,	Meadowside, Partick, Glasgow.
1880, Nov. 2:	William	Henderson, C.E.,	121 W. Regent St., Gl'gow.
1870, May 31:	Richard	Henigan, C.E.,	Alma Terrace, Avenue Road, Southampton.
1877, Feb. 20:	George	Herriot,	7 York Street, Glasgow.
	Laurence	*Hill, C.E.,	5 Doon Gardens, Hillhead, Glasgow.
1880, Nov. 2:	C. P.	Hogg, C.E., *(Member of Council.)*	175 Hope Street, Glasgow.
1883, Mar. 20:	John	Hogg,	Victoria Engine Works, Airdrie.
1880, Mar. 23:	F. G.	Holmes, C.E.,	103 Bath Street, Glasgow.
1883, Mar. 20:	Matthew	Holmes, *(Member of Council.)*	551 Sauchiehall St., Glasgow.
Original:	James	Howden,	8 Scotland Street, Glasgow.
1884, Apr. 22:	John G.	Hudson,	18 Aytoun Road., Pollokshields, Glasgow.
Original:	Edmund	*Hunt,	87 St. Vincent St., Glasgow.
1860, Nov. 28:	James	Hunter,	Coltness Iron Works, by Newmains.
1881, Jan. 25:	James	Hunter,	Aberdeen Iron Works, Aberdeen.
1857, Dec. 23:	John	Hunter,	Dalmellington Iron Works, near Ayr.

1873, Dec. 23: 1877, Feb. 20:	P. S.	Hyslop,	Buenos Ayres.
Original:	John	*Inglis,	64 Warroch Street, Glasgow.
1861, May 1:	John	Inglis, jun.,	Point House Shipyard, Glasgow.
1879, Jan. 21:	Thos. F.	Irwin,	2A Tower Chambers, Old Churchyard, Liverpool.
1880, Nov. 2:	Lawrence N.	Jackson,	Colombo, Ceylon.
1875, Dec. 21:	William	Jackson,	Govan Engine Works, Govan, Glasgow.
1884, Jan. 22:	J. Yate	Johnson, C.E.,	115 St. Vincent Street, Glasgow.
1879, Feb. 25:	David	Johnston,	6 Osborne Place, Copeland Road, Govan, Glasgow.
1870, Dec. 20:	David	Jones,	Highland Rlwy., Inverness.
1883, Jan. 23:	F. C.	Kelson,	Angra Bank, Waterloo Park, Waterloo, Liverpool.
1872, Mar. 26:	Ebenezer	Kemp,	Linthouse Engine Works, Govan, Glasgow.
1875, Nov. 23:	William	Kemp,	Ellen St. Engineering Works, Govan, Glasgow.
1878, Mar. 19:	Hugh	Kennedy,	Redclyffe, Partickhill, Glasgow.
1877, Jan. 23:	John	Kennedy,	R. M'Andrew & Co., Suffolk House, Laurence Pountney Hill, London, E.C.
1876, Feb. 22:	Thomas	Kennedy,	Water Meter Works, Kilmarnock.
1876, Oct. 24:	Andrew	Kerr, C.E.,	Town Surveyor's Office, Warrnambool, Victoria, Australia.

		David	*Kinghorn,	172 Lancefield St., Glasgow.
	1879, Dec. 23:	John G.	Kinghorn,	2 Alexandra Terrace, Rock Ferry, Cheshire.
	1864, Oct. 26:	Alex. C.	Kirk,	19 Athole Gardens, Hillhead, Glasgow.
	Original:	David	*Kirkaldy,	Testing and Experimenting Works, 99 Southwark Street, London, S.E.
	1885, Jan. 27:	Charles A.	Knight,	107 Hope Street, Glasgow.
	1880, Mar. 23:	Frederick	Krebs,	M. B. M. S. S. Co., Tokio, Japan.
	1875, Oct. 26:	William	Laing,	17 M'Alpine St., Glasgow.
	1858, Apr. 14:	David	Laidlaw,	Chaseley, Skelmorlie, by Glasgow.
	1884, Mar. 25:	John	Laidlaw,	98 Dundas St., S.S., Gl'gow.
	1862, Nov. 26:	Robert	Laidlaw,	147 E. Milton St., Glasgow.
	1880, Feb. 24:	James	Lang,	c/o John Lang, 552 St. Vincent St., Glasgow.
	1884, Feb. 26:	John	Lang, Jun.,	Church Street, Johnstone.
	Original:	James G.	*Lawrie, *(Past President.)*	2 Westbourne Terrace, Glasgow.
	1882, Mar 21:	Henry A.	Lawson,	Craigenly Cottage, Lenzie, near Glasgow.
	1880, Mar. 23:	Allison	Lennox,	131 W. Regent St., Glasgow.
	1878, Mar. 19:	John	Lennox,	131 W. Regent St., Glasgow.
G. M.	1873, Dec. 23: 1876, Oct. 24:	Charles C.	Lindsay, C.E., *(Vice-President.)*	167 St. Vincent St., Glasgow.
	1884, Feb. 26:	John	List,	Messrs D. Currie & Co., London.
	1862, Apr. 2:	H. C.	Lobnitz,	Renfrew.
	1865, Dec. 20:	John L.	Lumsden,	Alex. Jack & Coy., Seacombe, Cheshire.
	1873, Jan. 21:	James M.	Lyon, M.E.,	Engineer and Contractor, Singapore.

1862, Oct. 29: John M'Andrew, 17 Park St. East, Glasgow.
1884, Dec. 23: John M'Beth, 5 Park St., S.S., Glasgow.
1858, Feb. 17: David M'Call, C.E., 160 Hope Street, Glasgow.
1874, Mar. 24: Hector MacColl, Jas. Jack & Co., Engineers, Liverpool.
Hugh *MacColl, Manager, Wear Dock Yard, Sunderland.
1883, Oct. 23: James M'Creath, C.E., 95 Bath Street, Glasgow.
1871, Jan. 17: David M'Culloch, Vulcan Works, Kilmarnock.
1884, Feb. 26: James M'Ewan, Cyclops Foundry, 50 Peel Street, London Road, Glasgow.
1880, Nov. 2: James W. Macfarlane, Valeview House, Overlee, Busby.
Original: Walter MacFarlane, Possil Park, Glasgow.
Andrew *M'Geachan, Newark Shipbuilding Yard, Port-Glasgow.
1880, Apr. 27: Wm. Rae M'Kaig, 17 Water St., Liverpool.
1881, Mar. 22: William A. Mackie, 3 Broomhill Terrace, Partick, Glasgow.
1873, Jan. 21: J. B. Affleck M'Kinnel, Dumfries Iron Works, Dumfries.
1859, Dec. 21: Robert M'Laren, 22 Canal St., S.S., Glasgow.
Andrew *Maclean, Viewfield House, Partick, Glasgow.
1884, Dec. 23: James M'Lellan, 10 W. Garden Street, Glasgow.
1858, Nov. 24: Walter M'Lellan. 127 Trongate, Glasgow.
John *M'Millan, Shipbuilder, Dumbarton.
William *MacMillan, 19 Elgin Terrace, Partick, Glasgow.
(*Member of Council.*)
1884, Dec. 23: John M'Neil, Helen St., Govan, Glasgow.
Original: Andrew M'Onie, 1 Scotland Street, Glasgow.
1883, Jan. 23: William M'Onie, Jr., 128 West Street, Glasgow.
1883, Jan. 23: James M'Ritchie, C.E., Singapore.

1864, Oct. 26:	Robert	*Mansel, (*Past President.*)	Shipbuilder, Whiteinch, Glasgow.
1875, Dec. 21:	George	Mathewson,	Bothwell W'ks, Dunfermline.
1884, Apr. 22:	Henry A.	Mavor,	140 Douglas St., Glasgow
1876, Jan. 25:	William W.	May,	142 Fountain Road, Walton, Liverpool.
1883, Feb. 20:	James	Meek,	10 Clarence Road, Devonshire Park, Birkenhead.
G. 1876, Oct. 24: M. 1882, Nov. 28:	James	Meldrum, C.E.,	3 Elmbank Street, Glasgow.
1883, Jan. 23:	William	Melville, C.E.,	Caledonian Ry., Buchanan Street, Glasgow.
1881, Mar. 22:	William	Menzies,	7 Dean Street, Newcastle-on-Tyne.
1861, Dec. 11:	Daniel	Miller, C.E.,	204 St. Vincent St., Glasgow.
	James	*Miller,	Kelvin Forge, Partick, Glasgow.
G. 1873, Dec. 23: M. 1881, Nov. 22:	John F.	Miller,	204 Stobcross St., Glasgow.
Original:	James B.	Mirrlees,	45 Scotland St., Glasgow.
1876, Mar. 21:	James	Mollison,	Lloyd's Register, 36 Oswald Street, Glasgow.
1869, Dec. 21:	John	Montgomerie,	Kitson & Co., Airedale Foundry, Leeds.
1883, Nov. 21:	Joseph	Moore,	East Finchley, London.
1862, Nov. 26:	Ralph	Moore, C.E.,	Croft Villa, Rutherglen.
1878, Apr. 23:	Robert H.	Moore,	Mount Blue Works, Camlachie, Glasgow.
1868, Feb. 12:	Alexander	Morton,	241 W. George St., Glasgow.
G. 1878, Dec. 17: M. 1883, Jan. 23:	Robert	Morton,	53 Waterloo St., Glasgow.
1885, Mar. 24:	Edmund	Mott,	Board of Trade Surveyor, 7 York Street, Glasgow.
1864, Feb. 17:	Hugh	Muir,	345 Bath Crescent, Glasgow.
1882, Jan. 24:	John G.	†Muir,	100 Cheapside St., Glasgow.
1870, Mar. 22:	Wm. T.	Mumford,	36 Oswald Street, Glasgow.

1882, Feb. 21: George Munro, 254 Bath Street, Glasgow.
1882, Dec. 19: Robert Munro, 162 Buchanan St., Glasgow.
Original: James Murdoch, Shipbuilder, Port-Glasgow.
1880, Jan, 27: William Murdoch, 20 Carlton Place, Glasgow.
1877, Jan. 23: Robert Murray, 25A Coltman Street, Hull.

1881, Jan. 25: Henry M. Napier, Shipbuilder, Yoker, near Glasgow.
1857, Dec. 23: John †*Napier, 23 Portman Sq., London.
1881, Dec. 20: Robert T. †Napier, Shipbuilder, Yoker, near Glasgow.
Original: Walter M. Neilson, (*Past President.*) Queen's Hill, Kirkcudbrightshire.
1869, Nov. 23: Theod. L. Neish, 78 Finnart St., Greenock.
A. 1865, Apr. 26: }
M. 1879, Oct. 28: } R. S. *Newall, F.R.S., F.R.A.S., &c., Ferndene, Gateshead-on-Tyne.
1883, Dec. 18: Thomas Nicol, Clydebank, near Glasgow.
1884, Dec. 23: Wm. H. Nisbet, Mavisbank, Partickhill, Glasgow.
1876, Dec. 19: Richard Niven, C.E., Dalnottar House, Old Kilpatrick.
1861, Dec. 11: John Norman, 475 New Keppochhill Road, Glasgow.

1882, Jan. 24: Robert S. Oliver, C E., Highland Railway Co., Inverness.
1860, Nov. 28: John W. Ormiston, Shotts Iron Works, by Wishaw.
1885, Mar. 24: Alex. T. Orr, Hall, Russell, & Co., Aberdeen.
1867, Apr. 24: T. R. Oswald, The Southampton Ship building & Engineering Works, Southampton.

1882, Mar. 21: Geo, S Packer, F.I.C, Hallside Steel Works, Newton, near Glasgow.

1864, Oct. 26:	John	Page, C.E.,	1 Kersland Ter., Glasgow.
1876, Apr. 25:	William	Parker,	2 White Lion Court, Cornhill, London.
1883, Nov. 21:	W. L. C.	Paterson,	19 St. Vincent Crescent, Glasgow.
1877, Apr. 24:	Andrew	Paul,	Levenford Works, Dumbarton.
1880, Nov. 2:	James M.	Pearson, C.E.,	8 Duke St., Kilmarnock.
1866, Dec. 26:	William	Pearce,	Fairfield Shipyard, Govan, Glasgow.
1868, Dec. 23:	Eugène	Perignon, C.E.,	105 Rue Faubourg, St. Honoré, Paris.
	John	*Price,	Rose Villa, Gateshead Road, Jarrow-on-Tyne.
1877, Nov. 20:	F. P.	Purvis,	Craig Villa, Dumbarton.
1868, Dec. 23:	Henry M.	Rait,	155 Fenchurch St., London.
1873, Apr. 22:	Richard	Ramage,	Shipbuilder, Leith.
1866, Dec. 26:	Daniel	Rankin,	Eagle Foundry, Greenock.
1872, Oct. 22:	David	Rankine,	75 West Nile Street, Glasgow.
1876, Dec. 19:	Robert	Rankin,	35 Paisley Road, Glasgow.
1881, Jan. 25:	Charles	Reid,	Lilymount, Kilmarnock.
1883, Nov. 21:	George W.	Reid,	Highland Rly., Inverness.
1868, Mar. 11:	James (*Past President.*)	Reid,	Locomotive Works, Springburn, Glasgow.
1869, Mar. 17:	James	Reid,	Shipbuilder, Port Glasgow.
	John	*Reid,	Shipbuilder, Port-Glasgow.
1880, Apr. 27:	John	Rennie,	Ardrossan Shipbuilding Co., Ardrossan.
G. 1873, Dec. 23: M. 1881, Nov. 22:	Charles H.	Reynolds,	Cuprum House, Hamilton Ter., Partick, Glasgow.
1876, Oct. 24:	Duncan	Robertson,	8 Brighton Place, Govan, Glasgow.
Original:	James	Robertson,	21 Gower Street, Paisley Road, Glasgow.

1873, Jan. 21:	John	Robertson,	Grange Knowe, Pollokshields, Glasgow.
1863, Nov. 25:	William	Robertson, C.E.,	123 St. Vincent Street, Glasgow.
1884, Apr. 22:	R. A.	Robertson,	42 Aytoun Road, Pollokshields, Glasgow.
Original:	Hazltn. R.	*Robson, (*Past President.*)	14 Royal Cresct., Glasgow.
1877, Feb. 20:	Jno. MacDonald	Ross,	11 Queen's Cres., Glasgow.
1861, Dec. 11:	Richard G.	Ross,	21 Greenhead St., Glasgow.
G. 1864, Nov. 23: M. 1870, Jan. 18:	Alex.	Ross, C.E.,	Lynnwood, Alva.
Original:	David	*Rowan, (*Past President.*)	231 Elliot Street, Glasgow.
G. 1875, Dec. 21: M. 1885, Jan. 27:	James	Rowan,	231 Elliot Street, Glasgow.
1877, Oct. 30:	Alexander	Russell,	186 North Street, Glasgow.
G. 1858, Dec. 22: M. 1863, Mar. 4:	George	Russell,	Engineer, Motherwell.
1881, Feb. 22:	Joseph	Russell,	Shipbuilder, Port-Glasgow.
1859, Dec. 21:	Thomas	*Russell,	Albyn Lodge, Bridge of Allan.
1876, Oct. 24:	Peter	Samson,	Board of Trade Offices, Downing Street, London, S.W.
1885, Feb. 24:	James	Samuel, jun.,	238 Berkeley St., Glasgow.
1883, Feb. 20:	John	Sanderson,	Lloyd's Registry, 36 Oswald Street, Glasgow.
1882, Dec. 19:	Prof. Jas.	Scorgie, F.C.S.,	Civil Engineering College Poona, India.
1884, Apr. 22:	Andrew	Scott,	56 Græme Street, Glasgow.
1872, Jan. 30:	James E.	Scott,	13 Rood Lane, London.
1881, Jan. 25:	John	Scott,	Whitebank Engine Works, Kirkcaldy.
1860, Nov. 28:	Thos. B.	*Seath,	42 Broomielaw, Glasgow.

1875, Jan. 26:	Alexander	Shanks,	Belgrade, Ayton Road, Pollokshields, Glasgow.
1858, Nov. 24:	William	Simons,	Renfrew.
1862, Jan. 22:	Alexander	Simpson, C.E.,	175 Hope Street, Glasgow.
1871, Mar. 28:	Hugh	Smellie,	Belmont Grange Terrace, Kilmarnock.
Original:	Alexander	Smith,	57 Cook Street, Glasgow.
1880, Nov. 2:	Alexander	Smith,	1 Braeside Terrace, Maxwell Rd., Pollokshields, Glasgow.
1869, Mar. 17:	David S.	Smith,	Hellenic Steam Navigation Co., Syra, Greece.
1859, Jan. 19:	George	Smith,	Kennedy Street, Parliamentary Road, Glasgow.
1871, Dec. 11:	Hugh	Smith,	9 Kelvinside Terrace, North Kelvinside, Glasgow.
G. 1868, Dec. 23: M. 1874, Oct. 27:	Hugh	Smith,	97 Wellington Street, Glasgow.
1878, May 14:	James	Smith,	40 Margaret St., Greenock.
1870, Feb. 22:	Edward	Snowball,	Engineer, Hyde Park Locomotive Works, Springburn, Glasgow.
1883, Oct. 23:	Andrew	Sproul,	Palmerston Blds., Greenock.
1883, Dec. 18:	Alex. E.	†Stephen,	12 Park Terrace, Glasgow.
	John	†*Stephen,	Linthouse, Govan, Glasgow.
1881, Nov. 22:	Alex. (*Member of Council.*)	Steven,	Provanside, Glasgow.
1867, Jan. 30:	Duncan	Stewart,	47 Summer Street, Glasgow.
1874, Oct. 27:	Peter (*Member of Council.*)	Stewart,	53 Renfield Street, Glasgow.
G. 1873, Dec. 23: M. 1882, Oct. 24:	W. B.	Stewart,	1 Scotland Street, Glasgow.
1866, Nov. 28:	James	Stirling,	Loco. Engineer, S. Eastern Ry., Ashford, Kent.
Original:	Patrick	Stirling,	The Great Northern Railway, Doncaster.

1881, Jan. 25: Walter Stoddart, Caledonian Railway, Carstairs.

1864, Nov. 23: Edward Strong, °/o Kent Cottage, Queen's Cres., Southsea, Hants.

1877, Jan. 23: James Syme, 8 Glenavon Ter., Partick, Glasgow.

1879, Oct. 28: James Tait, C.E., Wishaw.

1882, Apr. 25: Alex. M. Taylor, Java Cottage, Lenzie.

1885, Apr. 28: Peter Taylor, 59 Queen Street, Renfrew.

1879, Mar. 25: Staveley Taylor, Russell & Co., Shipbuilders, Greenock.

1873, Dec. 23: E. L. Tessier, Veritas Office, 29 Waterloo Street, Glasgow.

1885, Jan. 27: George W. Thode, 107 Hope Street, Glasgow.

1882, Apr. 25: Geo. P. Thomson, Clydebank Shipbuilding Yard, Glasgow.

1883, Dec. 18: George Thomson, 9 Buckingham Ter., Partick, Glasgow.

1874, Nov. 24: Prof. James Thomson, C.E., LL.D., F.R.SS.L. & E., (*President.*) 2 Florentine Gardens, Hillhead Street, Glasgow.

1868, Feb. 12: James M. Thomson, 36 Finnieston St., Glasgow.

1882, Mar. 21: James R. Thomson, Clydebank Foundry, Glasgow.

1868, May 20: John Thomson, 36 Finnieston St., Glasgow.

1876, Feb. 22: John Thomson, 147 East Milton Street, Glasgow.

1875, Jan. 26: Robert S. Thomson, 3 Melrose Street, Queen's Crescent, Glasgow.

1864, Feb. 17: W. R. M. Thomson, 96 Buchanan Street, Glasgow.

1878, May 14: W. B. Thompson, Tay View, Broughty Ferry.

Original: Thomas C. Thorburn, 35 Hamilton Square, Birkenhead.

1874, Oct. 27:	Prof. R. H.	Thurston, M.E., C.E.,	Sibley College, Cornell University, Ithaca, U.S.A.
1875, Nov. 23:	John	Turnbull, jun.,	Consulting Engineer, 255 Bath Street, Glasgow.
1876, Nov. 21:	Alexander	Turnbull,	15 Whitehill Terrace, Dennistoun, Glasgow.
1876, Jan. 25:	Henry	Turner,	Managing Engineer, Canada Works, Birkenhead.
1880, Apr. 27:	John	Tweedy,	Neptune Works, Newcastle-on-Tyne.
1880, Nov. 2:	Ralph H.	Tweddell,	14 Delahay Street, Westminster, London.
1865, Apr. 26:	W. W.	Urquhart,	Blackness Foundry, Dundee.
1883, Jan. 23:	Peter	Wallace,	25 Argyle Place, Partick, Glasgow.
1885, Mar. 24:	W. Carlile	Wallace,	Maryland, Dumbarton.
1875, Mar. 23:	G. L.	Watson,	108 W. Regent St., Glasgow.
1864, Mar. 16:	W. R.	Watson,	16 Woodlands Ter., Glasgow.
1883, Jan. 23:	D. W.	Watt,	58 Union Street, Glasgow.
	John	*Weild,	Underwriter, Exchange. Glasgow.
1874, Dec. 22:	George	Weir, M.E.,	18 Millbrae Cres., Langside, Glasgow.
1874, Dec. 22:	James	Weir, M.E.,	Silver Bank, Cambuslang, near Glasgow.
G. 1876, Dec. 19: M. 1884, Feb. 26:	Thomas D.	Weir, C.E.,	97 W. Regent St., Glasgow.
1869, Feb. 17:	Thomas M.	Welsh,	63 St. Vincent Cres., Glasgow.
1868, Dec. 28:	Henry H.	West,	13A Exchange Buildings, Liverpool.

1883, Feb. 20:	Richard S.	White,	Shipbuilder, Sir Wm. Armstrong, Mitchell, & Co., Newcastle-on-Tyne.
1884, Nov. 25:	John	Wildridge,	Consulting Engineer, Sydney.
1876, Oct. 24:	Francis W.	Willcox,	45 West Sunniside, Sunderland.
1884, Dec. 23:	James	Williamson,	Barclay, Curle, & Co., Whiteinch.
1883, Feb. 20:	Robert	Williamson,	Lang & Williamson, Engineers, &c., Newport, Mon.
1878, Oct. 29:	Thomas	Williamson,	Netherton House, Wishaw.
	Alex. H.	*Wilson,	Aberdeen Iron Works, Aberdeen.
1868, Dec. 23:	James	Wilson, C.E.,	Water Works, Greenock.
1870, Feb. 22:	John	Wilson,	Wellfield House, Springburn, Glasgow.
1858, Jan. 20:	Thomas	†*Wingate,	Viewfield, Partick.
G. 1873, Dec. 23: M. 1884, Jan. 22:	Robert	Wyllie,	Hartlepool Engine Works, Hartlepool.
1879, Oct. 28:	John	Young,	Phœnix Iron Works, Glasgow.
1867, Nov. 27:	John	Young,	Galbraith Street, Stobcross, Glasgow.

ASSOCIATES.

Thomas	*Aitken,	8 Commercial Street, Leith.
Andrew	*Armour,	68 Anderston Quay, Glasgow.

Names marked thus * were Associates of Scottish Shipbuilders' Association at ncorporation with Institution, 1865.

1883, Oct. 28: John	Barr,	Secretary to Glenfield Co., Kilmarnock.
1882, Dec. 19: Wm.	Begg,	47 West Cumberland Street, Glasgow.
1882, Jan. 24: John	Black,	4 Alexandra Terrace, Govan, Glasgow.
1884, Dec. 23: W. S. C.	Blackley,	10 Hamilton Crescent, Partick.
1876, Jan. 25: John	Brown, B.Sc.,	11 Somerset Place, Glasgow.
1865, Jan. 18: John	Bryce,	Sweethope Cottage, N. Milton Road, Dunoon.
1880, Dec. 21: John	Cassells	56 Cook Street, Glasgow.
1870, Dec. 20: Joseph J.	Coleman, F.C.S.,	Ardarroch, Bearsden, by Glasgow.
1885, Feb. 24: Robert	Darling,	5 Summerside Place, Leith.
1859, Nov. 23: Arch. Orr	Ewing, M.P.,	2 W. Regent St., Glasgow.
1863, Mar. 18: Robert	Gardner,	52 North Frederick Street, Glasgow.
1885. Mar. 24: James S.	Gardner,	52 North Frederick Street, Glasgow.
1860, Jan. 18: George T.	Hendry,	79 Gt. Clyde St., Glasgow.
1882, Oct. 24: Wm. A.	Kinghorn,	6 Colebrooke St., Hillhead, Glasgow.
1864, Dec. 21: Anderson	Kirkwood, LL.D.,	7 Melville Ter., Stirling.
1878, Oct. 29: John	Langlands,	88 Gt. Clyde St., Glasgow.
1884, Feb. 26: C. R.	Lemkes,	198 Hope Street, Glasgow.
1873, Feb. 18: John	Mayer, F.C.S.	2 Clarinda Terrace, Pollokshields, Glasgow.
1874, Mar. 24: James B.	Mercer,	Broughton Copper Works, Manchester.

	George	*Miller	1 Wellesley Place, Glasgow.
1865, Dec. 20:	John	Morgan	Springfield House, Bishopbriggs, Glasgow.
1883. Dec. 18:	W. M'Ivor	Morison,	Mayfield, Marine Place, Rothesay.
	James S.	*Napier,	33 Oswald Street, Glasgow.
	John	Phillips,	17 Anderston Quay, Glasgow.
1869, Nov. 23:	Capt. John	Rankine,	31 Airlie Terrace, Pollokshields, Glasgow.
1867, Dec. 11:	William H.	Richardson,	19 Kyle Street, Glasgow.
1882, Dec. 19:	Colin Wm.	Scott,	30 Buchanan St., Glasgow.
1876, Jan. 25:	George	Smith,	45 West Nile St., Glasgow.
	John	*Smith,	Aberdeen Steam Navigation Co., Aberdeen.
	Malcolm M'N.	*Walker,	45 Clyde Place, Glasgow
	H. J.	*Watson,	5 Oswald Street, Glasgow.
	T.	*Westhorp,	West India Road, London.
1882, Dec. 19:	John D.	Young,	141 Buchanan St., Glasgow
	William	*Young,	Galbraith Street, Stobcross, Glasgow.

GRADUATES.

1884, Dec. 23:	Arthur C.	Auden,	9 Carmichael St., Govan.
1882, Nov. 28:	William H.	Agnew,	70 Grant Street, Glasgow.
1880, Nov. 2:	James	Aitken,	142 Cromwell Rd., Patricroft, Manchester.

1880, Feb. 24: George	Almond,	Belmont, Bolton-le-Moors, Lancashire.
1877, Nov. 20: James T.	Baxter,	9 Brighton Terrace, Copeland Road, Govan.
1883, Dec. 18: Seymour H.	Beale,	Banbury, Oxon.
1871, Feb. 21: W. S.	Beck,	246 Bath Street, Glasgow.
1883, Dec. 18: Ludwig	Benjamin,	11 Normandy Street, Upper, Parliament St., Liverpool.
1882, Feb. 21: Alfred G.	Berry, jun.,	33 Carnarvon St., Glasgow.
1885, Mar. 24: Alexander	Bishop,	3 Germiston St., Glasgow.
1883, Dec. 18: David	Blair,	Allan Line Works, Mavisbank Quay, Glasgow.
1884, Jan. 22: George	Blair, jun.,	6 Alfred Terrace, Hillhead, Glasgow.
1884, Jan. 22: Henry	Blair,	Clutha Ironworks, Glasgow.
1880, Mar. 23: Alexander	Bowie,	M. Langlands & Sons, Porter Street, Liverpool.
1878, Dec. 17: Rowland	Brittain,	11 Mount Pleasant Road Stroud Green, London, N.
1883, Apr. 24: Arthur R.	Brown,	9 Brighton Place, Govan Glasgow.
1876, Jan. 25: A. M'N.	Brown,	Castlehill House, Renfrew.
1879. Feb. 25: Alex. T.	Brown,	6 Olrig Terrace, Glencairn Drive, Pollokshields, Glasgow.
1883, Dec. 18: Eben. H.	Brown,	2 Carmichael Street, Govan.
1885, Mar. 24: Matthew R.	Brown,	6 Hamilton Place, Clydebank.
1881, Jan. 25: Matthew T.	Brown, B.Sc.,	33 Hope Street, Glasgow.
1872, Oct. 22: Hartvig	Burmeister,	°/o Rahr & Raundrap, 14 Brown St., Manchester.
1876, Dec. 19: Lindsay	Burnet,	Moore Park Boiler Works. Govan, Glasgow.

1882, Dec. 19: William T. Calderwood, 6 Smith Street, Hillhead, Glasgow.
1882, Dec. 19: Hugh Campbell, Leeds.
1884, Feb. 26: John Cleland, B.Sc., Woodhead Cottage, Old Monkland.
1884, Feb. 26: Arthur S. Clerk, 9 Carmichael St., Govan, Glasgow.
1881, Nov. 22: Alfred A. R. Clinkskill, 1 Holland Place, Glasgow.
1884, Feb. 26: Alexander Conner, 9 Scott Street, Glasgow.
1877, Dec. 18: James Conner, Isle of Wight Railway, Sandown, England.
1884, Jan. 22: Alex. M. Copeland, Bellahouston Farm, Paisley Road, Glasgow.
1874, Feb. 24: Andrew Corbett, Glasgow.
1880, Dec. 21: Sinclair Couper, 2 White Hill Gardens, Dennistoun, Glasgow.
1880, Nov. 23: James M. Croom, Earle's Shipbuilding and Engineering Co., Hull.
1882, Feb. 21: Wm. S. Cumming, Blackhill, by Parkhead, Glasgow.
1882, Mar 21: Alex. Cunningham, Glasgow.

1884, Jan. 22: James Dalziel, 119 Sandyford Street, Glasgow.
1883, Apr. 24: Alexander Darling, Upper Assam Tea Coy., Maijen Dilbrugdah, Upper Assam, India.
1881, Mar. 22: David Davidson, 24 Dixon Avenue, Crosshill, Glasgow.
1885, Feb. 24: William S. Dawson, Broomhill Ironwks., Glasgow
1879, Oct. 28: Jonathan L. Dean, 5 Pembroke Square, Kensington, London.
1883, Dec. 18: William Denholm, Glasgow.
1883, Feb. 10: Lewis M. T. Deveria, Mansfield Cot., Kilwinning.
1882, Oct. 24: Daniel Douglas, Earle's Shipbuilding Co., Hull.

1880, Nov. 2:	Geo. C.	Douglas,	Douglas Foundry, Dundee.
1882, Oct. 24:	John F.	Douglas,	18 Meadowpark Street, Dennistoun.
1883, Oct. 23:	Harry W.	Downes,	Claremont House, Alpha Road, New Cross, London, S.E.
1884, Jan. 22:	William	Dunlop,	961 Govan Road, Glasgow.

1882, Dec. 19:	A. Von	Eckermann,	91 Pollok Street, Glasgow.
1885, Mar. 24:	Robert	Elliot, B.Sc.,	The Engineers' Club, 10 Hare Street, Calcutta

1878, Jan. 22:	James R.	Faill,	Craig-en-Callie, Ayr.
1882, Feb. 21:	Albert E.	Fairman,	21 St. Bede's Terrace, Sunderland.
1880, Dec. 21:	Henry M.	Fellows,	Westbourne Lodge, Great Yarmouth.
1883, Dec. 18:	John James	Ferguson,	8 Walworth Ter., Glasgow.
1884, Jan. 22:	Thomas G.	Ferguson,	14 Queen's Cres., Glasgow.
1881, Feb. 22:	William	Ferguson,	Larkfield, Partick, Glasgow.
1885, Jan. 27:	Wm. D.	Ferguson,	63 Finlay Drive, Glasgow.
1881, Nov. 22:	Charles J.	Findlay,	10 Belmont Cres., Hillhead, Glasgow.
1883, Oct. 23:	Duncan	Finlayson,	1 Osborne Place, Govan, Glasgow.
1869, Oct. 26:	F. P.	Fletcher,	South Russell St., Falkirk.

1874, Feb. 24:	James	Gillespie,	21 Minerva St., Glasgow.
1884, Dec. 23:	D. C.	Glen, Jun.,	14 Annfield Pl., Glasgow.
1885, Jan. 27:	Alex. M.	Gordon,	3 Wallace Grove Place, Paisley Road, Glasgow.
1882, Jan. 24:	Arthur B.	Gowan,	3 Octavia St., Port-Glasgow.
1884, Feb 26:	Alexander	Gracie,	9 Great George Street, Hillhead, Glasgow.

1881, Dec. 20: Andrew Hamilton, 2 Belmar Terrace, Pollokshields, Glasgow.
1874, Feb. 24: Archibald Hamilton, New Dock Works, Govan, Glasgow.
1881, Feb. 22: James Harvey, Park Grove Iron Works Paisley Road, Glasgow.
1883, Feb. 20: David Henderson, 11 Hayburn Crescent, Partickhill, Glasgow.
1882, Nov. 28: F. N. Henderson, 11 Princes Terrace, Dowanhill, Glasgow.
1881, Oct. 25: Charles G. Hepburn, Ben Boyd Road, Neutral Bay, North Shore, Sydney, N.S.W.
1882, Feb. 21: Wm. S. Herriot, Leonora, Demerara.
1881, Jan. 25: A. C. Holms, jun., Hope Park, Partick, Glasgow.
1884, Dec. 23: John Howarth, 37 Bentinck St., Glasgow.
1873, Dec. 23: Guybon Hutson, Kelvinhaugh Engine Works, Glasgow.

1883, Jan. 23: John A. Inglis, 23 Park Circus, Glasgow.
1885, Feb. 24: John Inglis, Bonnington Brae, Edinburgh.

1873, Dec. 23: David Johnston, 12 York Street, Glasgow.

1883, Feb. 20: Eben. E. Kemp, Overbridge, Govan Glasgow.

1885, Feb. 24: John Lang, 6 Elderslie St., Glasgow.
1882, Jan. 24: Andrew Laing, Glenavon Ter., Crow Road, Partick, Glasgow.
1883, Nov. 21: William R. Lester, 2 Doune Terrace, North Woodside, Glasgow.
1885, Mar. 24: William Linton, 1 Carmichael St., Govan.

1885, Mar. 24:	Fred.	Lobnitz,	2 Park Terrace, Glasgow.
1884, Dec. 23:	Robert	Logan,	3 Hayburn Cres., Partick.
1884, Nov. 25:	Archd.	M‘Beth,	111 Govan Road, Glasgow.
1880, Nov. 2:	Patrick F.	M‘Callum,	Fairbank Cottage, Helensburgh.
1881, Dec. 20:	H.	M‘Coll, jun.,	3 Dalmeny Terrace, Pollokshields, Glasgow.
1883, Dec. 18:	Peter	M‘Coll,	Stewartville Place, Partick. Glasgow.
1876, Oct. 24:	Jno. M.	M‘Currich, M.A.,	Dock Engineer's Office, Cumberland Basin, Bristol.
1883, Dec. 18:	John	MacDonald,	293 New City Road, Glasgow.
1874, Feb. 24:	George	M‘Farlane,	65 Gt. Clyde St., Glasgow.
1882, Oct. 24:	James L.	Macfarlane,	Meadowbank, Torrance.
1883, Dec. 18:	John Bow	M‘Gregor,	22 Church Street, Partick, Glasgow.
1882, Dec. 19:	Allan	M‘Keand,	Glasgow.
1880, Feb. 24:	Neil	M‘Kechnie,	31 Bank Street, Hillhead. Glasgow.
1881, Oct. 25:	James	Mackenzie,	16 Kelvinhaugh Street, Glasgow.
1883, Jan. 23:	Thos. B.	Mackenzie,	342 Duke Street, Glasgow.
1884, Dec. 23:	Jas. M‘E.	M‘Intyre,	The Crescent, Dalmuir.
1883, Feb. 26:	Robert	M‘Kinnell,	56 Dundas Street, S.S., Glasgow.
1876, Dec. 19:	John	M‘Kirdy,	21 St. James Square, Edinburgh.
1883, Dec. 19:	Colin D.,	M‘Lachlan,	5 Ibrox Place, Ibrox.
1875, Dec. 21:	Hugh	M‘Lachlan,	5 Dowanhill Place, Partick, Glasgow.
1880, Nov. 2:	Robert	M‘Laren, jr.,	Eglinton Foundry, Glasgow.
1874, Feb. 24:	Andrew	Maclean, jun.,	Viewfield House, Partick, Glasgow.

1882, Dec. 19: Peter	M‘Lean,	Trafalgar Cottage, South Queensferry.
1874, Feb. 24: William	Maclean,	Viewfield House, Partick, Glasgow.
1885, Jan. 27: John	M‘Millan,	26 Ashton Ter., Glasgow.
1875, Dec. 21: Allister	M‘Niven,	Clutha Iron Works, Vermont Street, Glasgow.
1879, Oct. 28: Donald	M‘Taggart,	48 Overnewton St., Glasgow.
18 4, Dec. 23: Robert	Mansel, jun.,	4 Clyde View, Partick.
1884, Dec. 23: W. J.	Marshall,	3 Minerva Street, Glasgow.
1880, Nov. 2: Ivan	Mavor,	Wincomlee, Low Walker-on-Tyne.
1882, Jan. 24: Robt. Alex.	Middleton,	20 Merryland St., Govan, Glasgow.
1884, Nov. 25: Thomas	Millar,	8 Wilberforce Street, Wallsend-on-Tyne.
1880, Feb. 24: Robert	Miller,	13 Park Grove Terrace, W., Glasgow.
1883, Dec. 18: Charles W.	Milne,	7 Carmichael Street, Govan.
1880, Feb. 24: James F.	Mitchell,	Glasgow.
1881, Jan. 25: Ernest W.	Moir,	Forth Bridge Works, South Queensferry.
1881, Oct. 25: John	Moir,	26 St. Hilda Street, Hartlepool.
1882, Feb. 21: C. J.	Morch,	Horten, Norway.
1882, Nov. 28: M. J.	Morrison,	8 Annfield Terrace, Partick, Glasgow.
1884, Feb. 26: Andrew	Munro,	629 Govan Road, Govan Glasgow.
1878, May 14: Angus	Murray,	47 Kelvinhaugh Street, Glasgow.
1883, Dec. 18: James L.	Napier,	22 Salisbury Pl., Hillhead, Glasgow.

1884, Feb. 26:	D. J.	Nevill,	352 St. Vincent Street, Glasgow.
1879, Nov. 25:	Alex. R.	Paton,	Redthorn, Partick, Glasgow.
1884, Feb. 26:	Matthew	Paul, Jun.,	Levenford Works, Dumbarton.
1873, Dec. 23:	Edward C.	Peck,	Yarrow & Co., Poplar, London, E.
1881, Oct. 25:	William T.	Philp,	284 Bath Street, Glasgow.
1885, Jan. 27:	James L.	Proudfoot,	154 West George Street, Glasgow.
1885, Feb. 24:	John T.	Ramage,	The Hawthorn's, Bonnington, Edinburgh.
1883, Nov. 21:	Hugh	Reid,	10 Woodside Terrace, Glasgow.
1884, Dec. 23:	James G.	Reid, jun.,	4 Holland Place, Glasgow.
1884, Feb. 26:	Walter	Reid,	90 Bellgrove St., Glasgow.
1882, Nov. 28:	J. M‘E.	Ross,	Ravensleigh, Dowanhill Gardens, Glasgow.
1884, Mar. 25:	J. B.	Sanderson,	15 India Street, Glasgow.
1879, Mar. 25:	John	Scobie,	Samana Railway, Samana. St. Domingo.
1880, Apr. 27:	Archibald	Sharp,	31 Morrison St., Glasgow.
1882, Oct. 24:	John	Sharp,	461 St. Vincent St., Glasgow.
1883, Jan. 23:	Adolph U.	Sheldon,	91 Pollok Street, Glasgow.
1883, Dec. 18:	George	Simpson,	13 Maxwell Street, Partick, Glasgow.
1877, Mar 20:	Nisbet	Sinclair, jun.,	43 Park Road, Glasgow.
1884, Mar. 25:	Russell	Sinclair,	49 Stanley St., W. North Shields.
1882, Nov. 28:	Geo. H.,	Slight, jun.,	413 East India Road, London, E.
1881, Nov. 22:	John A.	Steven,	12 Royal Crescent, Glasgow.

1881, Jan. 25:	William	Stevenson,	R. & J. Hawthorn, St. Peter's Works, Newcastle-on-Tyne.
1873, Dec. 23:	John	Stewart,	270 New City Road, Glasgow.
1875, Dec. 21:	Andrew	Stirling,	62 Bothwell Circ., Glasgow.
1884, Dec 23:	David W.	Sturrock,	11 Florence Pl., Glasgow.
1878, Jan. 22:	Benjamin B.	Sykes,	Caledonian Railway Works, St. Rollox, Glasgow.
1880, Dec. 21:	Stanley	Tatham,	2 Cambridge Gate, Regent's Park, London, N.W.
1883, Dec. 18:	Lewis	Taylor,	2 Hillsborough Terrace, Hillhead, Glasgow.
1882, Nov. 28:	William	Taylor,	57 St. Vincent Cres., Glasgow.
1883, Apr. 24:	Wm. R.	Taylor,	Lennox, Lang, & Co., 131 W. Regent St., Glasgow.
1880, Nov. 23:	George	Thomson,	64 Sycamore Road, Handsworth, near Birmingham.
1874, Feb. 24:	George C.	Thomson,	39 Kersland Terrace, Hillhead, Glasgow.
1884, Dec. 23:	John	Thomson, jun.,	15 Burnbank Gardens, Glasgow.
1883, Dec. 18:	Nicol	Thomson,	39 Kelvinhaugh Street, Glasgow.
1884, Dec 23:	William	Thomson,	15 Burnbank Gardens, Glasgow.
1885, Feb 24:	Charles H.	Wannop,	12 Derby Street, Glasgow.
1884, Feb. 26:	William	Warrington,	23 Miller Street, Glasgow.
1881, Mar. 22:	Robert	Watson,	1 Glencairn Drive, Pollokshields, Glasgow.
1880, Apr. 27:	Robert D.	Watt,	Butterfield, Swire, & Co., Shanghai.

1875, Dec. 21:	Richard G.	Webb,	60 Warwick Gardens, Kensington, London.
1878, Dec. 17:	Robert L.	Weighton, M.A.,	R. & J. Hawthorn, St. Peter's, Newcastle-on-Tyne.
1884, Apr. 22:	John	Weir,	Ramage & Ferguson, Shipbuilders, Leith.
1882, Nov. 28:	Geo. B.	Wemyss,	Glasgow.
1883, Dec. 18:	John	Whitehead,	71 Scott Street, Garnethill, Glasgow.
1877, Jan. 23:	Robt. John	Wight,	7 Berlin Place, Pollokshields, Glasgow.
1879, Oct. 28:	William	Willox, M.A.,	27 Albert Terrace, Aberdeen.
1883, Jan. 23:	John	Wilson,	175 North Street, Glasgow.
1883, Dec. 18:	David	Wood,	124 West Nile Street, Glasgow.
1885, Mar. 24:	Fred. W.	Zucker,	Dumbarton.

INDEX.

WILLIAM MUNRO, General Printer, 80 Gordon Street, Glasgow.

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PLATE XIV

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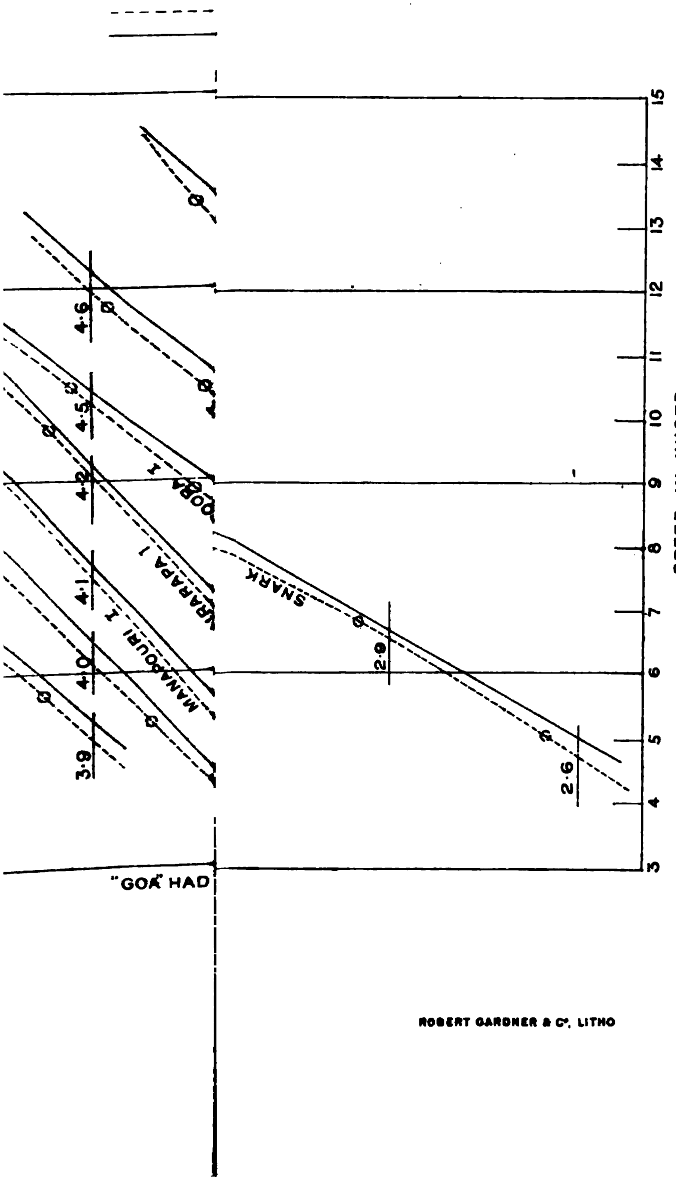

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Fig. 15.

STEAMERS BUILT BY MES

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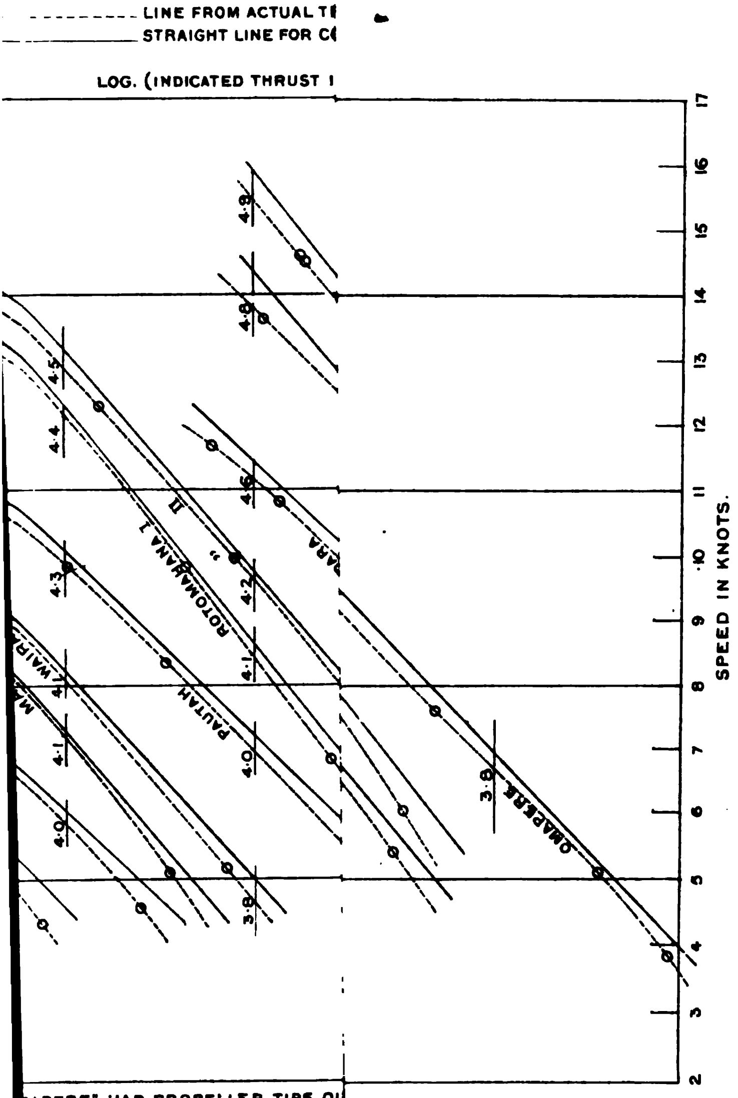

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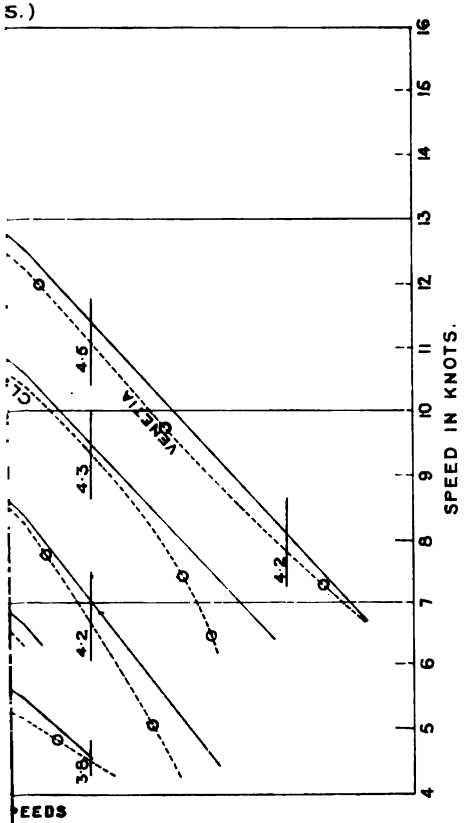

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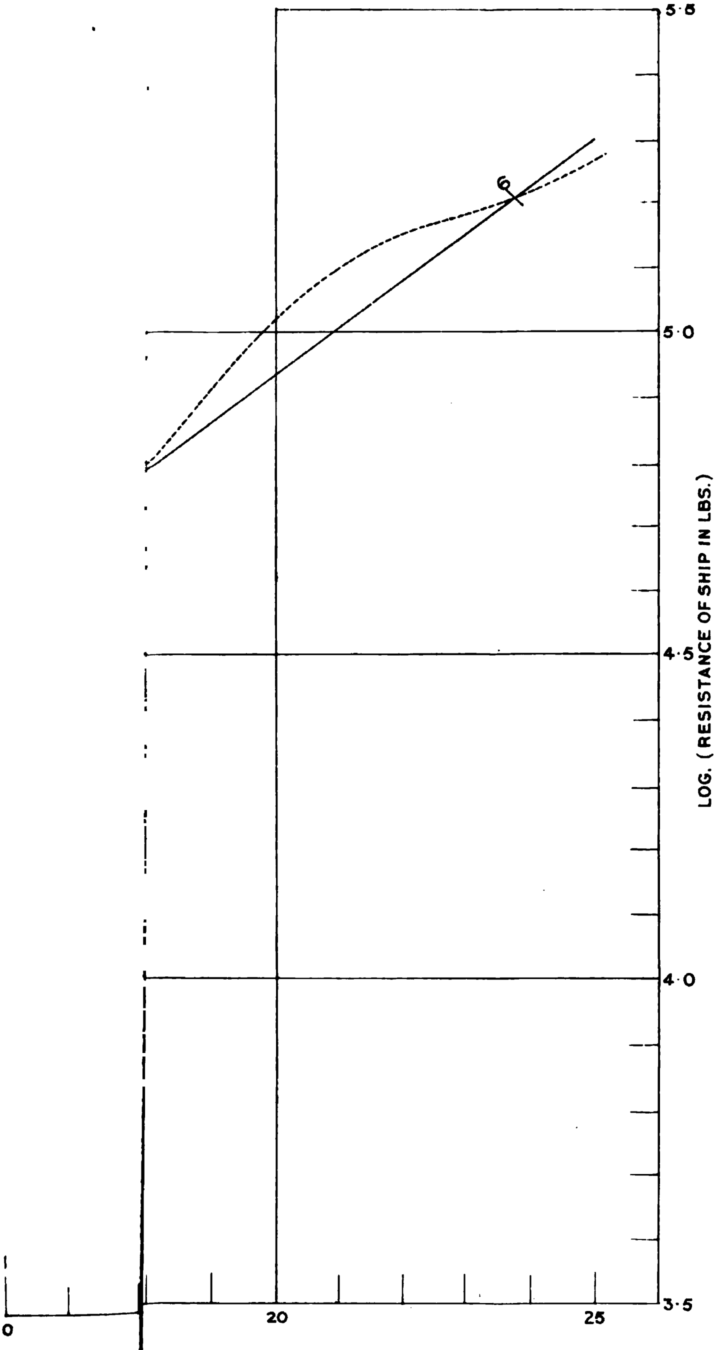

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