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IN SCOTLAND.

(INCORPORATED.)

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IN SCOTLAND.
(INCORPORATED.)

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INSTITUTION OF ENGINEERS & SHIPBUILDERS
IN SCOTLAND.

(INCORPORATED.)

NINETEENTH SESSION—1875-76.

Introductory Address. By Mr H. R. **Robson**, President.

Read 26th October, 1875.

Gentlemen,—The duty again devolves upon me of delivering to you the opening address of another Session of the Institution of Engineers and Shipbuilders in Scotland, an Institution whose importance and influence are now acknowledged, I am proud to say, wherever our professional literature is read, both at home and abroad.

I do not speak in terms of false modesty when I say that I certainly would have preferred that some other, and more able member of this Institution had occupied this position. As it is by your unanimous vote that I am so placed, it only remains for me to use my best endeavours to discharge the duties of the office, soliciting your indulgence to overlook all shortcomings on my part, while I rely upon you for such cordial support and assistance as are absolutely necessary for carrying out the aims and interests of the Institution.

Permit me, in the first place, to direct your attention to one or two matters in which the Institution itself is more immediately concerned.

Our present Membership reaches 309, and Associates 40, while the

Graduate Section numbers 69. Last Session 17 new Members and 2 new Associates were admitted, while from death and other causes we lost 19 Members, 1 Associate, and 5 Graduates. Our balance-sheet shows an increase over the previous Session of £117 8s 9d, of which increase the sum of £11 2s 11d belongs to the Marine Engineering Medal Fund, and £6 12s 8d to the Railway Engineering Medal Fund. These deducted, leave us with a net balance of £99 13s 5d in favour of the funds of the Institution.

This, as a whole, I conceive to be a very satisfactory state of affairs. But I regret that we have during the past year lost an unusually great number of our members by death, amongst whom we find Mr Thomas Kennedy, of Kilmarnock, who was so well known for his water meter; Mr Peter Wilson, the respected Engineer of the Highland Railway; Mr George Harvey, of the firm of G. & A. Harvey, engineers and toolmakers, of this city, a firm alike famous for the elegance in the design of the tools they make, and the perfection of the workmanship thereon. Mr Harvey's extensive knowledge of chemistry, botany, and other scientific subjects, together with his many estimable qualities, endeared him to a large circle of friends. The only other deceased member that I shall mention is Mr John Downie, who was one of the original members of this Institution, and who, during a number of years, displayed an active interest in its welfare, both by taking part in the discussions, and also by contributing several valuable papers on various subjects. For a paper which he read during the Session 1863-64, on "Removing the Substructures of Railway Bridges and Viaducts without Stopping the Traffic," the Institution awarded him the Railway Gold Medal. As many of you are aware, Mr Downie had attained a prominent position in his profession as a mechanical engineer long prior to becoming connected with the dynamite industry, with which he was prominently identified at the time of his death.

In the year 1868 Mr Alfred Nobel, the originator of the manufacture of nitroglycerine and dynamite as industrial agents, made his first public experiments in Glasgow with dynamite, his new explosive, at which time Mr Downie was introduced to that gentleman, and, being convinced of the value of the patent, became connected with him in forming a limited liability company to take up the manufacture of dynamite in this country, under the name of the British Dynamite Company. Extensive works for the manufacture of the explosive were erected amongst the sandhills at Ardeer, near Stevenston, Ayrshire. I am informed that within the last few weeks the thousandth ton-charge was manufactured and worked up into cartridges ready for use in blasting operations. Doubtless much more might have been manufactured by this time, had it not been for the refusal of some of the leading railway companies to carry it into those districts where it was in great demand for railway cutting, quarrying, &c. Incidentally, I may mention that under Mr Nobel's patents there have grown up no fewer than fourteen dynamite factories, the total production of which during last year was 3120 tons, whereas the amount made in 1867 was only 11 tons, and in the following year only 78 tons.

The engineering abilities of our deceased colleague were well displayed in the many happy and ready methods which he suggested for overcoming difficulties, and otherwise facilitating all work in which he was interested. As with his relatives, so amongst his personal and professional friends, his lamented death created a profound regret.

Having had occasion to speak of the new blasting agent, in connection with the foregoing brief sketch of Mr Downie's professional career, I am naturally led to make a few remarks upon the extraordinary development that has taken place during the last few years in rock-boring and rock-drilling machinery. Owing to the scarcity of workmen, and the necessity for energetically pushing forward shaft-sinking, mining, railway tunnelling, and similar operations, engineers and other inventors have most anxiously exercised themselves in devising such machinery during recent years. The result of this activity in invention is, that there are at the present time before the public somewhere about a dozen or fifteen different kinds of rock-boring or rock-drilling machines, every one of which has some special feature for which it may be commended. Did time permit, I might say something regarding each of those machines; but it does not, and therefore I shall not require even to name them. There are two, however, which seem to me to be especially worthy of a brief notice at my hands on account of the ingenious and substantial character of their construction, their effectiveness in operation, and the extent to which they are employed, while in the case of one of them there is the fact that it is exclusively manufactured in this city, and in great numbers, by a well-known firm which is largely represented upon the membership of this Institution. Through the kindness of the firm in question, Messrs P. & W. M'Lellan, and of the Diamond Rock Boring Company, London, I am enabled to place before you an interesting series of specimens, including (1) one of McKean's Rock Drilling Machines and Tools; (2) a number of cores of different kinds of rock that have been taken, most of them from very great depths, by the Diamond Rock Boring Machine; (3) the crown of one of the boring tools, which is set with pebbles. These specimens will doubtless impart additional interest to my remarks on the subject in hand.

First, let me direct your attention to a few facts regarding the McKean Rock Drill, of which doubtless a number of the members present may have some acquaintance, from having seen it at work at the Highland and Agricultural Society's Show on Glasgow Green a few months ago. This most excellent machine is now attaining very general use throughout the world for mining, tunnelling, quarrying, and submarine boring. As many as eight different types and sizes of the drills are now made, so that it is easy to select that one which is most suitable for any special kind of work that is to be executed. The smallest of them weighs not more than 70lbs. The McKean drills are powerful, portable, compact, and durable, while they have comparatively few working parts, can be worked at very great extremes of pressure, and can be made to work safely at 1500 strokes per minute. Their effectiveness may be judged of when I inform you that I have myself seen one of these drills penetrate into a block of Aberdeen granite at the rate of one foot in three minutes. Upwards of 500 of these machines have already been made, of which at least 60 are in use in piercing Mont St. Gothard, in the Alps, for the railway in course of construction.

It would be difficult to even estimate the value of the Diamond Rock Boring Machine, the invention of which is due to Major Beaumont, R.E. I need not attempt to describe the machine itself, but may simply say that the crown, or lowest termination of the boring tool, is merely a piece of steel tube set with a series of rough uncrystallised diamonds, to which the name carbonite is applied. The boring rod is hollow, and on the top end of it is placed a water union, joined up to a force pump by means of a flexible hose and wrought-iron pipes. Water is freely used while the crown is at work, to answer the double purpose of keeping the crown cool while boring, and washing the debris resulting from the boring to the surface of the ground. The pressure, depending upon the nature of the rock to be cut, varies from 400lbs. to 800lbs. per square inch, when the drill should penetrate at speeds ranging from 2″ to 4″ per minute—granite and the hardest limestones being easily cut at 2″ to 3″ per minute, sandstone at 4″, and quartz at 1″ per minute. These speeds may be obtained when the drill is making 250 revolutions per minute, beyond which it is not desirable to go, unless under very special circumstances. For speed of boring through the hardest rocks, and giving a true and reliable sample of the strata passed through, this boring machine far outstrips any that have been hitherto invented. By its means a solid core is produced, and brought to the surface, in the following manner: The boring rod and crown being tubular, it follows that only an annular space is cut out of the strata in passing through, leaving a piece in the centre uncut, which passes up the inside of the boring rod in the form of a cylinder, and by means of a projecting ring, or sliding wedges, attached to the crown, it is jammed inside the boring rod, and is removed when the rod is drawn up to the surface. When you look at the core specimens which are exposed on the table, you cannot help concluding that for mineral prospecting the Diamond Rock Borer is one of the most valuable inventions of the present day. At this point I ought to mention that the action of the Diamond Rock Borer differs from that of all others, in the fact that the tool revolves with its face continually in contact with the work, similar to the practicability of the undertaking. I understand that Mr P. W. Barlow, C.E., lately sailed for Brazil, and is commissioned to conduct the survey, and prepare the necessary plans and estimates. Lastly, it may suffice if I refer to the fact of the Mersey Tunnel scheme being again under consideration, and of which there seems to be some prospect that it will be brought to a practical issue. Arrangements are being made for holding a public meeting on the subject in Liverpool, at which Mr Gladstone has given a conditional promise to be present, for the furtherance of the project. The object of this proposed tunnel is not merely to facilitate local traffic between Liverpool and the Cheshire side of the Mersey, but to open a more direct railway connection with North Wales. In passing from this subject, I would express a hope that the Clyde Subway scheme may still receive some consideration in the proper quarter, and that it will be an accomplished fact in our day.

An Institution such as this, which embraces in its membership many gentlemen who are prominently identified with the most advanced practice in locomotive engineering and the construction of railways, can scarcely remain uninterested in respect of the remarkable event which took place at Darlington a month ago. I refer to the celebration of the Jubilee of the first passenger railway— the "mother of railways," in fact—the one which was opened between Stockton and Darlington on 27th Sept., 1825, and whose existence was due to the co-operation of two of the most distinguished men in the whole records of engineering and social progress. A Tyneside man myself, I can scarcely venture to speak as I might feel inclined to do on the character and labours of the modest engine-wright of Killingworth Colliery, an industrial spot which George Stephenson has rendered so famous; neither would I trust myself to speak at any length of that other north countryman, Edward Pease of Darlington, who was Stephenson's most worthy colleague in the inception, and in providing the pecuniary means for carrying out the idea, of the Stockton and Darlington Passenger Railway. Each was necessary to the other for the full development of the idea; and together, and with the assistance of their co-labourers, they have conferred a lasting benefit on mankind. The benefit is not limited to the people of this country, or even to the people of Europe and America. It is experienced in all quarters of the globe. Even "strange Japan" has, in the matter of railways, cast in her lot with the Western civilisation, and, still more extraordinary, the "Celestial Empire" is about to "go and do likewise." If one circumstance more than another could contribute to the interest of the Jubilee rejoicings at Darlington, it surely must have been the announcement made by the Chairman who presided at the great banquet given on the evening of the Jubilee day by the Directors of the North-Eastern Railway Company, in whose system of 220 miles the original 25 miles of the Stockton and Darlington Railway are now embraced. It was to the effect that a Stockton rail manufacturing firm had signed a contract for the construction of the first Chinese Railway, and that on the night of the banquet the first rails would be rolled at Stockton. The length of the proposed line is only 10 miles, the aim being to connect the city of Shanghai with the town of Woosung; but short as it is, it may certainly be regarded as the "thin end of the wedge," and it will doubtless be of much service in opening up the interior of China to the influence of the outer civilisation. Let us hope that it will eventually lead to the construction of such an iron network as that with which we have become so familiar during the past half century in this country. Already workmen have been sent out to China from the North of England to break the ground, and the intention of the contractors is to have the work finished by the 1st of May, 1876.

Probably most of you have observed that of late years there has been an extraordinary degree of anxiety among many communities throughout Scotland to secure an abundant supply of good wholesome water, free from the sewage contamination which so frequently causes the water of wells to be an important agent in the propagation of epidemic disease. This desire for wholesome water is a wholesome sign of the times, and it forms a subject that may most suitably call for a few general remarks in such an address as the present. Within reservoir itself having an area of 396 acres and a maximum depth of 68 feet, and contents 272 million cubic feet, the top water being 890 feet above Ordnance datum; (2) the Edgelaw Compensation Reservoir, having a natural catchment of 7782 acres; (3) the Rosebery Reservoir, on the South Esk, which has a further catchment of 1620 acres. There are also a brick culvert, 3017 yards in length; a distributing reservoir at Alnwick Hill, about 2¼ miles from Edinburgh; and the necessary filters and tank, which are to be at the level of 400 feet above Ordnance datum. Messrs J. & A. Leslie are the engineers; and our townsmen, Messrs Laidlaw & Son, are supplying the necessary cast-iron pipes, the total length of which will be about 12½ miles, and the weight about 6000 tons, and will cost about £40,000—the total cost of the works being estimated at £325,000.

Within the last few years the Town Council of Dundee secured Parliamentary powers to take over and greatly increase the water supply of that town; and, in their capacity as Water Commissioners, they have practically completed a scheme, the magnitude of which is not excelled by that of any other town in Scotland, if we except the magnificent Loch Katrine water supply of this city, which we cannot too highly prize. Having been constituted a Water Commission by Act of Parliament in 1869, when they took over the works of the Water Company, the Town Council of Dundee forthwith employed Mr J. F. Bateman, C.E., F.R.S., to prepare plans for greatly extending the water supply of the town itself, as also that of Lochee and Broughty Ferry. Powers were obtained from Parliament in 1871 to carry out those plans, the chief features of which were to resort to the Loch of Lintrathen, which is about 18½ miles from the town, impounding its two feeding streams, the Melgam and the Inzion, and, by raising the level of the Loch, increase the area of the latter from 180 to 412 acres. The available supply thus yielded would have been 20 million gallons per day. But Mr Bateman's scheme was not destined to be carried out in its entirety. Owing to a division of opinion among the Water Commissioners, it was resolved to adopt what was termed the "direct route," and Messrs J. & A. Leslie and Mr J. W. Stewart, of Edinburgh, were employed to devise plans for it, and Parliamentary sanction was obtained for it in the year 1872. The scheme of Messrs Leslie & Stewart was simply to convey the water from the Loch of Lintrathen by a 27-inch cast-iron pipe direct to Dundee. In 1874, however, a supplementary Act was obtained giving powers for the construction of a reservoir at Clatto, on the line of this pipe, to hold ten days' supply of eight million gallons per day. This reservoir is now in course of construction, but the works at Lintrathen and the piping are completed, the supplying capacity being 12 million gallons per day. So far as the Loch works are concerned, Mr Bateman's design has been carried out under the superintendance of the Messrs Leslie; 12 miles of piping have been laid under the superintendence of Mr Stewart, the contractors, both for the pipes and the laying, track-cutting, &c., being Messrs Edington & Sons and D. Y. Stewart & Co., of this city. The remainder of the piping, with the bridges, culverts, &c., and the laying out of the grounds, have been executed under the direction of Mr James Watson, the Manager and Resident Engineer for the Dundee Water Commissioners. Messrs Leslie have in hand the construction of the reservoir at Clatto, which is about four miles from Dundee, and is at at an elevation of 495 feet above Ordnance datum. The full distance from Lintrathen to the reservoir is 14½ miles, and, as already indicated, the Loch and the reservoir are connected by a pipe 27 inches in diameter. From Clatto to Clepington Road there are two lines of pipes, respectively 21 and 15 inches in diameter; and from Clepington Road there are three branches—18, 15, and 13 inches in diameter—which convey the water to the various service reservoirs. Lastly, there is a 15-inch direct main from Clatto to supply the higher parts of the town.

The purchase price of the Water Company's works, including transference, was £375,354. On the existing and new works, up till last May, there had been expended no less than £632,722. The new works when completed will cost, it is estimated, about £320,000; and the whole undertaking of the Dundee Water Commissioners may be put down as having cost three quarters of a million sterling. The population at present supplied is about 160,000, including Dundee

Various watering-places on the Firth of Clyde, as well as populous places inland, have lately had to extend their water supply powers. For example, the Burgh Commissioners of Gourock have at present in course of construction a new reservoir and filter, at a probable cost of about £3500.

At Dunoon, also, there are still more important works in progress, indeed they are nearly completed. They consist of a reservoir on the Balgay Burn, to the west of the town, and a filter in conjunction, together with the necessary distributing pipes. The total cost will be about £18,000.

Since becoming a Burgh under the Police and Improvement Act of 1862, Callander has also provided itself with an excellent water supply. It is obtained from the River Leny, at a point high enough above the town to carry the water to the highest part of it. The main pipe is led right out into the middle of the river, and as there is always abundance of water even in the driest weather. no reservoir is required. On this account the works only cost about £3000. They were finished in 1872.

I may mention that important sewage irrigation works have also been carried out recently at Callander, at a cost of about £2000, and that in the course of last year a very complete system of drainage was laid down through the ancient town of Alloa, the expense of which was about £9000. The most of the above works have been designed by Mr Copland.

Through the kindness of Mr R. D. Oliver, C.E., Inverness, I am enabled to mention a few facts regarding the water scheme which has recently been devised for that town, in accordance with the provisions of an Act of Parliament, passed last session, and which transferred the supply of water and gas from a private company to the Town Council. The new water supply is to be taken from Loch Ashie, 6½ miles from, and 600 feet above the level of the highest part of the town within the Parliamentary boundary, or 716 feet above Ordnance datum. The works will consist (1), of an embankment, with waste weir, &c., for impounding, to the extent of two feet, the level of the loch; (2), of a line of pipes from the loch to

(3), the service reservoir, which is to be 2½ miles from the town, and will contain seven million gallons. This reservoir will be formed partly by excavation and partly by embanking, the top water level being 240 feet above Ordnance datum. Lastly, there will be a line of twelve inch pipes from the reservoir to the outskirts of the town, where the new works will be connected with the existing main. Exclusive of Parliamentary costs, land, engineering, and law expenses, these new works will cost about £15,000.

Messrs Leslie of Edinburgh are at present engaged in the survey of a scheme for Dunfermline, which aims at giving a daily supply of one million gallons, at an estimated cost of from £60,000 to £70,000, the proposed source of supply being the "dark winding Devon" of the poet. Works are now in progress for giving an additional supply to Cupar, at a cost of about £13,000. Messrs Bell & Miller, of this city, are providing Grangemouth with an additional supply, at a cost of about £9000. A scheme is nearly finished at Dalry, the cost of which will probably reach £12,000. Thurso is about to go into a scheme which will also cost about £12,000. The manufacturers in the valley of the Leven, in Fifeshire, are proposing to get powers to raise the loch of that name two feet, so as to provide themselves with an additional storage for summer use. The estimated cost is set down at £25,000.

The cost of the water-works which I have mentioned amounts to the large sum of £1,119,500.

In the same way I might go on to mention what many other places are doing in the way of supplying themselves with water, but enough has been said to indicate that there is throughout the land a real progress showing itself in sanitary matters.

Such undertakings as those of which I have just spoken as going on in Scotland, together with others in various parts of the United Kingdom, and also abroad, have caused within the last few years a large industry to spring up in Glasgow; and it is highly gratifying for us to know that, notwithstanding the great depression that is, and has for a considerable time past been felt amongst the engineers and consequently iron-founders of this city and locality generally, there is at least one department of foundry work that has been and is still well employed, namely, that of pipe-founding. You may not be a little astonished when I state that on inquiry, I find that three firms in this city, namely, Messrs Edington & Son, Messrs D. Y. Stewart & Co., and Messrs Robert Laidlaw & Son, have turned out during the last twelve months from 70,000 to 80,000 tons of cast-iron pipes, thus giving employment to several thousands of artizans, and in many other ways aiding and assisting in the prosperity of Glasgow.

To all shipbuilders, mechanical engineers, and others who use wrought-iron upon a large scale, the production of that material at a cheap rate has become a matter of the utmost importance. On this account much inventive talent has been spent in recent years with a view of devising some thoroughly effective means for converting pig-iron into wrought-iron without depending so much upon manual labour. Several mechanical puddling furnaces have been brought under notice from time to time, but the general opinion now is, that, to be really serviceable and economical, the puddling furnace of the future must itself rotate, so that while the iron of the charge is in the liquid state, and "coming to nature," it may be as completely and intimately exposed to the chemical influence of the oxygen of the air as is possible in the most perfect hand puddling. Chiefly through the exertions of the Iron and Steel Institute, and by the efforts and commercial enterprise of several English ironmasters, there seems now to be good reason for believing that mechanical puddling has become a commercial success, or is upon the eve of entering the successful stage. The history of the subject during the last three or four years is invested with a great amount of interest alike to ironmasters and engineers.

At the Dudley meeting of the Iron and Steel Institute in 1871, a paper was read by Mr Samuel Danks, of Cincinnati, on the revolving puddling furnace which he had invented and brought into successful use in America. Himself a South Staffordshire man originally, there was a peculiar fitness in his describing the invention in a district that has a world-wide fame for its finished iron industry. Some of the best practical ironmasters in the kingdom took part in the discussion which followed the reading of the paper, and such a high opinion was formed of the merits of the invention that the Institute sent out a Commission of Inquiry, consisting of two practical iron manufacturers, one from Middlesbrough and the other from South Staffordshire, and a highly skilled metallurgical chemist from Dowlais, in South Wales, for the purpose of making most rigid experimental investigations at the American ironworks where the Danks furnace was in operation. The Commissioners reported most favourably regarding what they had seen, and the result was that several Danks puddling furnaces were forthwith erected and set to work in the Middlesbrough district. After a time, however, they seem to have failed, chiefly from defects in mechanical construction. The difficulties that were met with naturally caused some disappointment. Messrs Hopkins, Gilkes, & Co., who were the first to commence the use of the Danks furnace, gave it up; but the Erimus Iron Works Co., whose managing director is Mr John A. Jones, one of the Commissioners sent out to America, although they were not by any means satisfied with the results which they had obtained with the new furnace, resolved to continue, but to devote their attention in the first instance to the removal of the obstacles which they considered to be the chief drawbacks to the success of rotatory puddling. Amongst those drawbacks there were the educating of the workmen, and the removal of prejudice from amongst them, the difficulty with the "fettling" of the furnace, and the mechanical weakness of the Danks machine. In a letter addressed to the President of the Iron and Steel Institute about six months ago, Mr Jones stated that the difficulties with the workmen and in respect of the fettling of the furnace had disappeared; and in speaking of the improvements which had been made on the mechanical details of the rotary puddling furnace, he said that the directors were so satisfied with the work done by the modified machine that they had ordered five more, and the necessary engines to drive them. That, certainly, may be regarded as a hopeful sign of progress. Mr Jones has recently been good enough to give me some still fresher information on the subject, in reply to a letter which I had addressed to him asking how the question then stood. He says that the changes indicated in his letter to the President of the Iron and Steel Institute are being made, and that up to the date of his letter to me the Company were quite satisfied with the results. Within the next week or two the whole of the contemplated changes will have been made, I believe, throughout the rotary puddling department at the Erimus Works, when, doubtless, the iron trade in all parts of the country will look anxiously for information on the subject in question.

Mr Robert Heath, of the Ravensdale Iron Works, North Staffordshire, very early resolved on giving the Danks system of mechanical puddling a full and fair trial. His remarkable earnestness and enterprise seem to bid fair to receive their due reward. He started with six furnaces, and a few months ago he erected other four, making ten in all. Mr Heath's efforts to make the Danks system a manufacturing and commercial success had excited such an amount of interest amongst practical ironmasters, that the members of the Iron and Steel Institute most gladly availed themselves of an invitation to visit the Ravensdale Iron Works on the occasion of the meeting of that body in Manchester a few weeks ago. Unfortunately, I have not been able to accept of the kind invitation lately made to me by Mr Heath to visit his works, but I am happy to say that a gentleman who made a very careful personal inspection of the Danks furnaces and the mode of working, on the occasion referred to, has favoured me with the impressions of what he saw. Though not actually engaged in the iron manufacture, my informant has made himself familiar with ironworks generally. He says:—

"I can assure you that I looked forward with great pleasure to our visit to Ravensdale, and my anticipations were fully realised. Mr Heath has embarked in this new phase of the iron manufacture quite enthusiastically. He has faith in the Danks system, and he seems to have determined to make it succeed. We found ten furnaces in full operation, in two parallel rows under one roof; and no sooner had we entered the works than the members of our very large party distributed themselves around the various furnaces, into which they peered most anxiously, at the same time minutely questioning Mr Heath, his managers, and the workmen regarding the working of the furnaces, the weight of the charges, the quality of the pig-iron used, the nature of the fettling, the durability of the internal lining, and the external casing, the number of heats per day, the yield per furnace, &c. Presently, one of the furnaces was opened by drawing aside the moveable mouthpiece, and in a few seconds there was drawn forth a puddled ball of some 8 or 9 cwt. upon a gigantic prong or fork, worked by gearing overhead; and by means of it the plastic mass of iron was carried into a Danks squeezer, in which it was subjected alternately to powerful blows from a horizontal hammer, and to the squeezing operation of a pair of curiously formed rolls. It was then passed to a 10-ton steam hammer, under which it was worked into a bloom or slab, and afterwards passed through the roughing rolls, and subsequently drawn out to a length of 16 or 18 feet, fully 12 inches broad and nearly 2 inches thick. All this was the work of a very few minutes; but it was most interesting, if not even exciting, to the onlookers. Other charges were drawn and operated on during our stay in the works in the same way.

"We learned that Mr Heath had made very important modifications and improvements upon the mechanism of the Danks furnace, which he had got to work in a surprisingly effective manner, and that in his hands the Danks system had become a decided success. Even six months ago he was rolling Dank's blooms, in the ordinary forge rolls, 16-inch bars 24 feet long, more cheaply than by the old puddling process, to say nothing of the saving in waste in cutting up long bars as compared with bars one-fourth the length. We also learned that arrangements were in progress for turning out puddled balls weighing at least 13 cwts. Mr Heath has not contented himself with rolling heavy plates from the Danks furnace, but has now begun to roll a great variety of the smaller sizes of merchant iron; and, judging from what we saw at Ravensdale, I feel satisfied that very marked success is the result."

local industry in all its departments. They should at once be historical, scientific, and statistical; and as there are still persons living who can furnish the necessary data for making a correct estimate of the position of the Clyde in steam navigation thirty, forty, or fifty years ago, and comparing it with the stage of progress which we have arrived at, and are severally assisting to work out, there is every reason to believe that the papers which I contemplate may be made quite an interesting feature of the meeting of the British Association, as well as a landmark which some future Scott Russel may refer to with perfect confidence when treating of the history of steam navigation. If the suggestion which I have now thrown out for your consideration meets with your approval, I doubt not that your Council will do their best to secure the services of gentlemen to take in hand the subject with which they are most familiar.

Before closing this address, there is one subject which, as it appears upon the notice of this meeting for consideration this evening, I may say a few words by way of information to the members.

Several gentlemen, members of this Institution, who have long taken a great interest in its prosperity, believing that a Course of Lectures upon Theory applied to Mechanical Subjects would be of great service to the young men connected with the Institution as Graduates, proposed to the Council that a course of such lectures might be arranged to be delivered during this Session. Several meetings of Council, and of the Committee appointed by the Council, have been held, at which the matter was fully discussed. A meeting, also, with the representatives from the Council of the Graduate Section was held. The result of the latter meeting was considered quite satisfactory by the Committee, the representatives of the Section having intimated their willingness to co-operate with the Committee in promoting the movement.

It is desirable, however, that the members express their opinions upon the steps which the Council have taken in the matter.

The Lectures proposed would deal with mechanical principles and their application to engineering subjects, under such heads as—

Matter and Force—Representation and Measurement of Forces—Centre of Gravity—Principles of the Lever, Pulley, and Inclined Plane, and their applications. Strength of Materials—Stress and Strain—Factors of Safety. Strength of Structures—Beams, Girders, Arches, Boiler Shells, &c. Stability of Structures—Retaining Walls, Towers, and Chimneys. Energy and Work—Motions of Bodies—Centrifugal Action, Fly-wheel. Friction of Surfaces—Prime Movers Water Wheels, Windmills, Steam and other Engines.

It is proposed that the Lectures be delivered on Thursday evenings, commencing at 8 P.M., in this hall, each Lecture to occupy an hour.

The Fees proposed by the Committee for the course of Lectures are —For Graduates, 5s; for Members and Associates, 7s 6d; for those not connected with the Institution who may wish to attend, 10s.

I may add that the promoters propose that any deficiency that might arise in the necessary funds for carrying on the Lectures should be met by subscription, so that the funds of the Institution would not be affected by the movement.

I now leave the matter in the hands of the meeting, trusting that success will follow this and every other movement connected with our Institution.

Mr D. Rowan begged to propose a vote of thanks to the President for his able address, to prepare which a large amount of time must have been occupied. The President had treated in the address of a number of subjects, all of which were interesting to them as engineers and shipbuilders. Especially had he called to mind, in his remarks regarding increased water supply, that most excellent paper which had been contributed by Mr Gale a few years ago, descriptive of the Loch Katrine Water Works, the value of which paper was indicated by the fact that the volume of their Transactions which contained it was the only volume that had been long out of print. He was very glad to see Mr Gale present, as he was aware that he had not been so strong lately, and that his duties in connection with the water supply of Glasgow were very arduous, preventing him attending their meetings so regularly as he had for-

for their using safety valves loaded with springs. It was not only for the worth of the paper that the medal had been awarded, but for the position the President occupied upon the whole subject of Spring-loaded Safety Valves. He had, therefore, very much pleasure in presenting the medal.

The PRESIDENT said—While he esteemed it a very high honour to receive a medal from the Institution, yet as the award of the medal had been connected by Mr Rowan with a marked fact in marine engineering practice, it was very gratifying to him to think that he had been the instrument in obtaining the removal of restrictions on the use of Spring-loaded Safety Valves on marine boilers. The removal of this restriction was of great commercial importance in connection with steam navigation. In writing the paper referred to, he had not the slightest intention of gaining a medal; but while he appreciated the honour the Institution had conferred upon him, he must confess that he had a far higher feeling in the knowledge that there was something more than that which he had achieved. He, however, regretted to say that some of the Board of Trade officials still continued to throw unnecessary obstacles in the way of spring-loading for Government Safety-valves which, he might add, were now admitted and advocated by all the Marine Engineering firms in the kingdom to be the best and safest method for loading Safety-valves of boilers on ship-board. He, therefore, trusted that they would each, as engineers desirous of the advancement in their practice, "put their shoulder to the wheel," and give a determined resistance to all such unnecessary interference, both with regard to springs and also other matters connected with the survey of passenger steamers which were from time to time thrust upon them in most objectionable forms.

Mr ROWAN then intimated that the lectures would be delivered every Thursday evening during the Session, and that Mr Millar was quite prepared to give the opening lecture on Thursday of next week. In the interim a syllabus would be printed and distributed, stating the time of meeting, and everything in connection with the course of lectures, The proposal of the Council as to the institution of the course of lectures was then formally put to the meeting and unanimously agreed to, the President expressing a desire that as many of the members of the Institution would give their presence and assistance to the course as possible.

Without entering upon the many causes of this waste, or into the many systems of valves, and the nearly perfect action of some ot them, the comparison may be made between the valves and the various pistons now in use. The valves hold much the same relation to the steam engine as the hands hold to the human body. They are the servants which convey the food to the mouth. But unless healthy digestion, and the power of a proper retention of the food in the stomach is contained in the body, it is certain that much of the food will be wasted. Just so with the steam engine. For in many, if not in all cases, if the fuel consumed, and the water evaporated by that fuel, be measured, after allowing for radiation, friction, and the difference between the initial pressure of the steam on the piston and the pressure at the end of its traverse, a very great loss has somewhere taken place which cannot be accounted for, except by extra friction, or that it has leaked past the piston during its passage from one end of the cylinder to the other, and in many instances it would be far more economical to replace the old packing rings and packing than to attempt to make them steam-tight.

It may be truly said that the majority of pistons are far from being practically steam-tight. Howsoever they may have been when first constructed, their continual use, and the want of elasticity in the packing, soon bring about a leaky condition, the shocks that they are subject to at sea often fracturing the springs and leaving them in an imperfect state for the remainder of the voyage. If these defects could be as clearly defined by the diagram indicator as the defects and derangements of the valves, we should naturally think that some remedy would before now have been found. But, speaking from thirty years' experience, on shore, the writer thinks that there has not been that consideration given to the steam-engine piston which its importance deserves.

There are several principles to take into consideration in constructing a good and efficient piston. The two of most importance being the ensuring of steam-tightness and a minimum amount of friction, the latter generally ensuring durability. The great majority of pistons which have come under the notice of the writer have been far too rigid, and although they may have been moderately steam-tight for a short time, the friction, and consequently the wear, have been so great that their efficiency is very much to be questioned. One defect in many of the modern pistons cannot be too forcibly expressed, viz., there being no means provided for re-adjustment; forgetting the advice given by Æsop, "that if the bow is always strung it must become fixed in that position or break." Another great cause of failure, which has no doubt been observed by many, is the want of proportion between the substance contained in the packing rings and that contained in the spring packing. It appears to the writer that these should hold some relation to each other, or, so soon as the packing rings have worn and require holding up to the face of the cylinder, the spring packing, if weaker than the rings, will not have the power to do this without overstraining, which not only endangers the breaking of the springs, but causes them to lose their elasticity, this latter cause being often erroneously attributed to the softening of the springs by the heat of the steam.

During the last few years the writer has had occasion to examine almost all the various kinds of pistons now in use, the makers of which it would be invidious to name; but any one placed in the same position must have been forcibly reminded of the great necessity of some standard proportion of parts. There is as great a want of this proportion of parts in the steam engine piston as there was some years back for Mr Whitworth's standard pitch of thread for screws. In many instances the breadth or depth of the packing rings are out of all proportion to the diameter of the cylinders, and varying with each maker. In many cases the spring packing is so weak that there is no relation to the strength or substance of the packing rings, while in other cases the spring packing is quadruple the strength one would think necessary; yet this being considered insufficient, the wedge and screw—the two greatest mechanical effects—are both brought to bear on the spring packing to set up the packing rings. If for one moment we think of these powers in the hands of an inexperienced man, when re-adjusting such pistons, we need scarcely wonder how some of the coal consumed is expended. An upon the packing rings, so that no part of the rings is without a spring; still by being connected they equalise themselves until an equal pressure is obtained on all parts of the circle of the cylinder, and this equal pressure cannot be disturbed by re-adjusting. It is almost impossible to overstrain the spring packing, the elasticity being distributed over such a great number of points. In small pistons of ten inches diameter, no coil is deflected more than 1-32nd part of an inch, while in pistons of forty inches diameter, the deflection of each coil will not be more than from the 1-100th to the 1-110th part of an inch, the deflection decreasing as the diameter of the pistons increase—thus, if possible, giving an advantage to the larger diameters.

In re-adjusting the spring packing, which is not required oftener than once in two years, and that only when the engines have been continuously at work, all that is required is to place a washer the same size as the steel and the same section as the spring on the *dowel* at the loose ends of the spring packing. This, by increasing the diameter of the spring, thereby causes it to press with renewed energy upon the packing rings in the three directions already named, without in the least interfering with its equal action. It cannot be said from practice how long the spring packing will bear this re-adjusting, further than that one has been in continual work, night and day (Sundays excepted), for five years without failing in the least, or in any way losing its elasticity. The friction on this piston is very small, and its durability as a natural consequence is great in proportion; and if the instructions given for re-adjusting be followed, perfect steam tightness is obtained with a minimum amount of friction. But in no case, from the construction of the spring, can a sufficient amount of friction be put upon the packing rings to cause them to cut or groove the cylinder even when it is made of soft metal, as the spring closes into a smaller circle before this can be done.

The tongue-pieces are fitted into a double recess cut into the edges of the packing rings. They are angular in section, having a broad and narrow side, the broad side of the angle being level wit the edge of the ring, a recess being first cut in the ring to receive the same, after which another recess is cut at the bottom of the first to receive the narrow side of the angle. The tongue-pieces having the same radius as the packing rings, fit into these recesses, and the packing rings being always pressing up to the junk ring and on to the flange of the piston, thus keep them in their proper places. These provisions prevent them from wearing away quicker than the packing rings, yet at the same time give perfect freedom for expansion to the face of the cylinder.

These combined qualities adapt this piston to all classes of cylinders, and, as has been proved in practice, unless the cylinders are very badly worn, re-boring is quite unnecessary. One piston has been for the last two years working in a cylinder which is three-eighths of an inch taper, being thirty inches diameter and having four feet stroke. Numerous instances could be given where other makers have refused to apply their pistons without re-boring the cylinders, where these pistons have been applied with the greatest success. Neither is it necessary in all cases to have new pistons, as the rings and packing may be adapted to all existing pistons which have sufficient room to receive them, the space required from the periphery of the piston to the body of the same being two-thirds the depth or breadth of the packing rings—thus supposing the two rings to be six inches, then the space required would be four inches. In all cases of application to existing pistons, it is requisite that the junk ring and the flange of the piston should be re-faced to ensure a perfect fit, and to prevent the steam from passing round the back of the packing rings. Afterwards the lateral motion of the piston and the treble action of the spring packing keep the faces true, Where this has been done in land engines, and the difference in the consumption of the fuel has been noted, the cost of the alteration has been recouped in three months. This is taking the average and not the extreme cases, new pistons bearing the same proportion.

The PRESIDENT said he was sure every engineer present must feel great interest in the subject of the paper read. They all knew the prevention of waste of steam by leakage past the back of the packing, or by leakage into and out of the space at the back, than upon the question of the pressure at the back as affecting the expenditure of power in friction between the rubbing faces of the metallic packing and the cylinder. Whether much or little steam leaked past there would still be, he thought, very variable effects from the pressure at the back of the packing, unless there was some arrangement, which had not been explained, why the pressure of the steam or the vacuum on the back of the rings was not a variable pressure. He would be very glad if any of the members present could give information as to whether means were, by any makers, used to settle what pressure or the vacuum should be at the back. If there was an arrangement to let the pressure of the steam in at the back without allowing waste, that would give a more definite pressure than any springs that could be put in. The other question he had put was whether there was any reason at all for making the breadth of the face of the rings greater in a large piston than in a small one.

The PRESIDENT said he thought that in the Buckley piston there would be a leakage of steam to the back of the rings, as he observed that the rings were cut right through, and as there is no cod fitted behind the cuts, but merely a thin face-piece fitted on the edges of the rings where they cut, which to his mind gave very little surface to maintain the necessary tightness, and prevent the steam passing to the back of the rings and destroying the springs.

Mr A. MORTON reminded Professor Thomson that an ordinary ring and piston acted like a valve and valve seat, closing the steam side and opening the exhaust every alternate stroke, thus causing a partial vacuum behind the rings in condensing engines; hence the use of springs to keep them out against the cylinder with the necessary pressure to overcome that of the steam between the rings' outward surface and the cylinder.

Mr PRIOR said with regard to the breadth of the packing rings, they had given the increase to their scale more for obtaining a sufficient strength in the pistons of stationary engines than any necessity there is for such broad rings; the general custom in making pistons for stationary engines being to make the junk ring and the bottom of the piston parallel to each other, and not convex, as in marine pistons, which gives a sufficient strength in the piston to receive the cone of the piston rod without requiring the piston to be as deep on the periphery; for if they had two pistons of equal diameters to fit with rings that were not of equal breadth or depth, they increased the strength of the spring packing in proportion to the greater depth of the packing rings. For example, take two pistons of equal diameter, the one having packing rings four inches deep and the other having packing rings five inches deep, they would give one-fifth more pressure to those rings that were five inches deep than to those that were four inches; therefore, he did not see that the wear would be more in the one case than in the other, yet in each case the pistons would be equally steam-tight. So far as the question was concerned of the steam getting into the piston, he did not say that they kept all vapour out, but they prevented the steam from escaping by the back of the packing rings, as well as preventing the piston from being charged with steam each time the cylinder was charged. They considered this subject of such great importance, that in applying the packing rings and spring packing to existing pistons, they recommended that brass washers should be placed under the heads of the piston bolts to prevent the steam from entering the piston by that means. He could not say what pressure they put upon the packing rings. The strength of the steel for the spring packing was tabulated to the various sizes, and they varied to the 1-64th part of an inch.

In answer to Mr Morton,

Mr **Prior** said that he did not know that Buckley's pistons were more expensive than others now in use. New pistons sold at £1 per inch, complete with rings and springs. The rings and springs were sold separately at 10s per inch in diameter. If they applied them to existing pistons, and made them ready for working, they charged 12s. These were the prices up to 40 inches in diameter.

Mr **Morton** thought one of the objections stated was a valid one.

The PRESIDENT might reply to one of Professor Thomson's questions, about the pressure of the steam or the vacuum on the back of the rings. Steam had been tried instead of springs, and he believed it did not succeed. He did not know that it had been tried in any large size of engine, but in locomotives it had.

Mr PRIOR, in answer to questions, said that the disproportion of parts alluded to in the paper was in some instances extraordinary. In one instance where Buckley's piston had been applied, the cylinder being 30 inches in diameter, the old piston was 16⅝ inches deep. In another, a 12-inch cylinder had a piston 12 inches deep. With regard to the friction of the Buckley pistons, it was acknowledged by all who had tried them, that they had less friction than any other to be steam-tight. He believed there was very little pressure needed to keep a piston steam-tight, provided that pressure was given by an elastic substance.

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

The following additional remarks have been received from Mr Prior:—

"The remarks made by Professor Thomson with regard to the steam or the vacuum acting at the back of the packing rings, was of great importance. But if we take for example a vertical cylinder with an ordinary piston, and supposing the packing rings to have worn on the edges only the 1-64th part of an inch, we shall find that in some cases—and these not a few—that the pressure of the steam is acting at the back of the packing rings during one part of the traverse of the piston, and the vacuum during the remainder part. For instance, as remarked by Mr Morton, 'a slack packing ring acted like a valve,' and the pressure of the steam when on the top side of the piston would force such a ring on to the piston flange, and *vice versa.* The whole steam pressure would be acting at the back of the packing rings, except what was escaping past the opposite edge of the rings. For it is well known that such rings are never steam tight. This is the action of slack packing rings. Now, if we take an instance where the packing rings are pressing upon the cylinder with sufficient friction for the initial pressure of the charge of steam to overcome, then, as in the first instance, the packing ring will be forced on to the piston flange, or the junk ring, as the case may be, and the pressure of the steam will be acting at the back; but as soon as the pressure of the steam becomes, by expansion, less than what is necessary to hold the packing ring on to the piston flange, or the junk ring, the packing ring will then be pressed on the opposite edge and the vacuum will be acting on the back; thus proving that the steam is acting at the back of the ring during one part of the traverse of the piston and the vacuum during the other part. This I believe to be the cause of cylinders being generally of less diameter in the middle of the traverse after they have worked for a time. Again, if we take a very tight packing ring, or the packing rings of the condensing pistons of compound engines, where the pressure of the steam seldom exceeds the atmospheric pressure, we may be assured that the back of the rings are at all times exposed to the exhaust side of the piston, or in other words, that the vacuum is acting at the back. Thus it will be seen that the only safe way is to keep both steam and vacuum out as much as possible, as is done by the Buckley's piston. To prove that I have not spoken extravagantly in my paper of the merits of this piston, I may state that all the engineers in Sheffield are using them, including the extensive firms of Messrs Davy Brothers, Limited, and Messrs Pigott and Farrar of Barnsley."

fractures occurred more or less *removed from* centre of span. Further observation has shown this to be uniformly the case, and also that the *form* of curve varies with the *distance* of fracture from centre of span.

TABLE I.

POSITION OF FRACTURE.	NUMBER OF BARS.		BREAKING WEIGHTS AND DEFLECTIONS.			
	Straight Fracture.	Curved Fracture.	Straight Fractures.		Curved Fractures.	
			Lbs.	Inch.	Lbs.	Inch.
At Centre of Span	29		3584	·393		
„ ⅛" „	4		3615	·393		
„ ¼" „	6		3532	·394		
„ ⅜" „	7		3675	·394		
„ ½" „	9		3538	·394		
„ ⅝" „	1	3	3350	·378	3507	·384
„ ¾" „		7			3600	·388
„ ⅞" „		1			3890	·425
„ 1" „		2			3620	·388
„ 1¼" „		1			3900	·450
„ 1⅜" „		3			3716	·395
„ 1½" „		1			3560	·400
„ 1⅝" „		1			3450	·377
„ 2" „		1			3390	·375
„ 2¼" „		1			3640	·405
„ 2½" „		1			3660	·400
„ 2¾" „		2			3024	·305
„ 3½" „		1			3136	·380
			6)21294	6)2·346	13)46093	13)5·072
			3549	·391	3545	·390
Total Breaking Weight,			200512	...	88784	
No. of Bars, 56			3580	No. of Bars 25	3551	
Deflections,			·393	and	·398	

Table No. I. shows the positions of fracture, from which it will be seen that, of the straight fractures, the majority broke at or near centre; the limit of straight fractures being at ½-inch from centre, the solitary exception at ⅝-inch from centre having a slightly sloped fracture. The curved fractures, on the other hand, vary from ⅝-inch from centre to 3½ inch from centre.

From an examination of the specimens of fractures exhibited, it will be seen that the curved form *increases* with distance of fracture from centre.

The fractured parts in all cases fit exactly together, no portion having been observed to be thrown out during fracture. The curves, it will be seen, are often approximately hyperbolical.

From these experiments, then, it would appear that the form of fracture is determined by the *position* at which fracture *begins*, and is not affected by the strength of the metal.

By considering the conditions of stress existing in a bar undergoing testing as above described, we may see how it is that the *position* of fracture should determine the *form* of fracture.

Let A B C (see Plate IV. Fig. 1) be a bar supported at the points A and C, and loaded at the middle of the span, B. Let the bar be supposed to bend in the arc of a circle; the lower parts will then be extended and the upper parts compressed. Consider a layer of the bar whose surface is A C, and of a depth *b* D—the section at *b* D will be under tension, so that the forces acting at that surface will be equal and opposite, as represented by the horizontal arrows. If the action of the load at B be sufficient to overcome the tenacity of the metal at this section, fracture will take place; and since this fracture is due to the action of two equal and opposite forces acting normally to the section *b* D, a straight fracture will take place along that section, and in the direction D *b*. Successive layers will be affected in turn, until the fracture terminates at B.

It is evident that, as each layer is ruptured, there will be a tendency for the curved surfaces, A D and C D, to become straight, the forces so acting being at right angles to the curved surface; the ver-

Table II. shows deflections, and set at different loads.

Nos. 1, 2, 3, and 4 show deflections *increasing* with increase of load.

No. 5 shows deflections *decreasing* with increase of load.

This decrease in deflection for additional load appears also to take place, in some cases, in malleable iron bars. From some experiments made by Mr David Rowan, it was found that a forged bar, 2 inches deep and $\frac{17}{16}$ inches broad, with span of 3 feet, gave the following results:—

Loads.............Lbs.	4144	4144	4144
Deflections......Inch	·230	·200	·180

It will be observed that the sets at high loads, that is, near the ultimate or breaking load, *decrease*, whilst at low loads an *increase* takes place—showing that the set seems to increase, attain a maximum, and then decrease.

The force due to straightening is very well shown by the springing out of the broken portions from the testing machine, the two parts being flung out to a distance sometimes of 32 inches from each other, the weight of the bar being about 21 lbs.

The "work" principally done, then, in bending the bar to its ultimate deflection will be spent in producing fracture, and in causing the motion of the fractured parts.

Since the work will be = deflection × half the load, we have

$$\text{Work} = \frac{\cdot 392 \times 3571}{2} = 58\tfrac{1}{3} \text{ foot pounds.}$$

The appearance of surface of fracture is in general a good test of the quality of the iron—softer qualities being distinguished by a metallic lustre and rounded particles; harder qualities, again, have less metallic lustre, and are fine grained. In many cases the surface of the fracture has a flakey or pointed appearance, with a purply grey colour. This generally indicates strong metal, having a good deflection.

The breaking weights of bars of 1 inch broad, 1 inch deep, and of 36 inch span, appear to be about *one-fourth* that for bars measuring

2 inch deep, but of similar dimensions otherwise; the bar of 1 square inch section giving, in some cases, higher results than that due to one-fourth of the bar of 2 square inch section.

The average breaking weight of 72 bars of 1 square inch section was found to be 805 lbs., the deflection being ·626 inch. The average breaking weight of 81 bars of 2 square inch section, as already given, was 3571 lbs.; deflection, ·392 in. The tensile strength of links cast of same metal as the 1 square inch section bars was found to be, for 84 links of 1 square inch section, 20,981 lbs. The ratio, then, of tensile to transverse strength in the 1 inch square section bars referred to is as 26 is to 1.

We may determine a "constant" for transverse strength from the foregoing experiments by considering that the breaking weight varies inversely as the span, and directly as the breadth and square of the depth; or $\frac{W \times S}{b d^2} = C$.

$$\text{Ex.—}C = \frac{3571 \times 36}{1 \times 4} = 32{,}139;$$

or say C = 23, if load be taken in *cwts.* and span in *feet.*

We shall then have $W = \frac{23 \times bd^2}{S}$.

The tenacity may also be expressed in terms of the transverse strength, as follows:—

$$T = \frac{\cdot 72 \times W \times S}{b d^2}$$

where T = tenacity in lbs. per square inch,
W = transverse strength in lbs., and
S = span in inches.

And conversely, $W = \frac{T \times b d^2}{\cdot 72\ S.}$

The PRESIDENT said the subject of the paper just read was of great interest to many of the members. It was a subject that he had taken considerable interest in, and had some experience of, a few very difficult, or even impossible, to imagine any way in which the two sides could have been commenced and propagated in a symmetrical manner. They could not both begin at the outside simultaneously, and then advance exactly simultaneously, so as to meet exactly at the middle. The moment that either side would commence to break, all symmetrical disposition of stresses would instantaneously cease. In many cases in broken glass, in discussing how the cracks were made, we could point out how they were formed by the manner in which they branched out, and by numerous markings which they exhibited. Looking at the cracks in an old delf plate, or in the varnish on a terrestrial globe, we could often read the order in which the cracks had occurred. He thought these matters would repay further scrutiny and examination.

Mr ALEX. MORTON thought that the skin had something to do with the fracture. Mr Millar had not spoken of the breaking of bars whose breadth was greater than their depth. If only the fracture of the surface was horn-shaped, the small castings used in the experiment, as also the sealing wax, plates, &c., referred to, might explain this; as coarse wax, and large castings, such as side levers, generally broke square across.

The PRESIDENT said there was one rather peculiar thing in connection with this subject. It appeared on Table No. II., examples No. 5, that some of these bars had deflected less, with greater weight. He could not tell how that should arise—could Mr Millar give them any information on that point?

Mr MILLAR said that he was unable to give any explanation of this further than that the bars referred to were exceptionally strong, not being broken by the machine. There was also a peculiarity in the action of the "set." So far as he had been able to ascertain, the set, as obtained from successive loads, increasing from light loads upwards, seemed to increase, attain a maximum, and then decrease.

Mr J. M. GALE said that, in the specimens shown by Mr Millar, the horn always extended towards the centre of the bar. Had Mr Millar found in any case that the horn extended away from the centre of the bar? He understood Mr Millar to say that the horn, or curved fracture, always pointed towards the centre of the bar. Was this the case, or did it sometimes extend away from the centre?

Mr **Millar** said that, with reference to Mr Morton's remark, the bars were greensand castings, and there appeared to be very little skin on them. He had in several cases filed portions of the bar to ascertain what effect the breaking of the skin might have, but was unable to find any appreciable difference. The tensile links and the bars for cross-breaking had been treated in this manner. In some cases—where the file draft was made pretty deep in both the one-inch and two-inch section bars for cross-breaking—fracture did not take place at that point. In answer to Mr Gale's question, he might say that in no case had he observed the curve of fracture to extend away *from* the centre; it invariably pointed *towards* the centre and point of application of the load. The averages shown on the tables were derived from 85 bars. These experiments were carefully made, the form and position of fracture being steadily noted for each bar consecutively as it was broken. A considerable number of such fractures had been observed at different times besides the above, and in no case had a curved fracture been observed extending away from the centre. As a very good proof of the invariable nature of this action, he had here a specimen of a curved fracture which belonged to a bar which, when loaded in the ordinary manner, refused to break; the bar was then reversed, and finally broke with a curved fracture, which pointed towards the centre or point of application of the load, as in the other cases.

Mr J. Z. **Kay** said that he could bear witness to the very great care taken by Mr Millar in making those experiments, and to the enthusiastic manner in which he had gone into them. There was a peculiarity which he had noticed—viz., that in some cases where two test bars were cast from iron run out of the same ladle, it would be found they would not bear the same strain, and that the one would break with much less weight than the other, although the bars seemed in every respect the same. Another curious thing was what Mr Millar alluded to in reference to a blown hole being in the oar, extending through several inches. He had found that a faulty or, as we may say, to the downward force which they receive from gravitation.

For the first of the two kinds of cases just referred to, a comprehensive but simple and easily intelligible principle, which it is proposed now to establish and illustrate, may be stated as follows :—

General Principle.—Similar structures, if strained similarly within limits of elasticity from their forms when free from applied forces, must have their systems of applied forces, similar in arrangement and of amounts, at homologous places, proportional to the squares of their homologous linear dimensions.

To establish this we have only to build up, in imagination, both structures out of similar small elements or blocks, alike strained, with the same intensity and direction of stress in each new pair of homologous elements built into the pair of objects. (See figures on opposite page.) To aid our conception, we may first imagine both structures to be free from forces applied to them from without, and we may imagine the two to be similarly divided into a great number of small similar elements, or blocks, similarly situated. Thus we may notice that since the similar elementary blocks, unstrained, would fit together in both cases, so as to produce two similar unstrained structures, the blocks, or elements, if alike stressed in homologous pairs in the two cases, so as to be similarly deformed from their free

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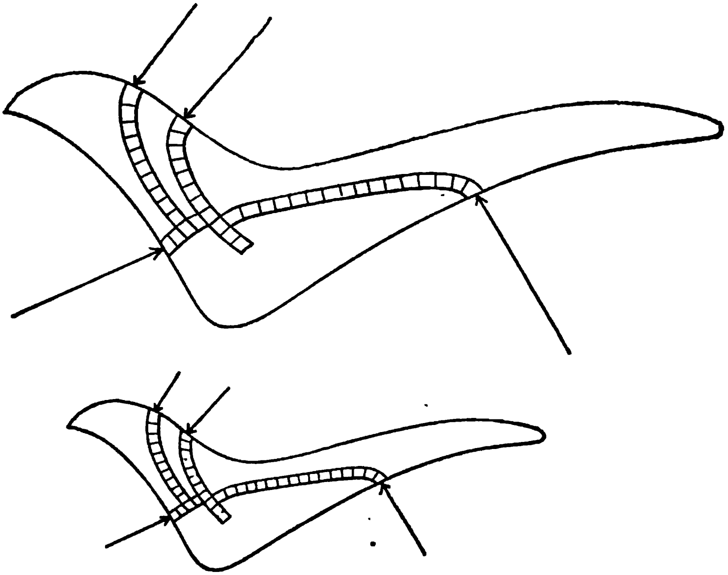
sures, it is enacted that a certain piece of platinum referred to as a "weight of platinum," deposited in the Office of the Exchequer, shall be denominated the IMPERIAL STANDARD POUND AVOIRDUPOIS, and shall be deemed to be the only standard of weight from which all other weights, and other measures having reference to weight, shall be derived, computed, and ascertained. And secondly, the same word "weight" is often used to signify the downward force which a piece of matter exerts on whatever supports or suspends it ; as when we speak of the *weight* of a piece of matter hung to a spring-balance as being the *force* which draws out the spring. The word MASS is employed to express distinctly, in scientific language, one of the two meanings of the word "weight"—that, namely, in which it signifies *quantity of matter ;* but for the other meaning, in which it signifies a *force*, we have no established name as yet ; it may, however, very well be called the *gravity* of the piece of matter.

condition, would also go together so as to produce two similar structures similarly strained from their free condition.*



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* A case liable to occur in practice, but which is very commonly, and sometimes erroneously, neglected or left unnoticed in investigations on strength and elasticity of materials and structures, is here, for simplicity, left out of consideration—the case, namely, in which a piece of material, or a structure, is subject to stresses in its substance, arising from mutual action of its parts when free from externally applied forces. The principles discussed in the present paper are, however, easily applied to such cases, but, for avoidance of any extra complication, the detailed discussion of such cases is not here entered on. It may suffice to say that, so long as the utmost stresses do not strain any part of the substance of either structure beyond limits of elasticity, and provided that the ordinarily existing condition of elasticity (known as Hooke's law), and which may be briefly described as *stress proportional to strain*, or more precisely, as *unital stress proportional to unital strain*, applies truly to the material of the structures, the conclusions arrived at here as to the similarly applied forces and the similarity of the strains and stresses produced by them, will be in no degree affected by the antecedent existence or non-existence of internal stresses in the structures when free from forces applied from without. In respect to this subject, reference may be made to a paper by the author, published in the *Cambridge and Dublin Mathematical Journal*, November, 1848, *On the Strength of Materials, as influenced by the Existence or Non-Existence of certain Mutual Strains among the Particles composing them.*

Numberless other examples or illustrations might be offered, but a few will suffice.

We may consider the case of the comparison of two similar girders or horizontal beams, rectangular in their cross sections (like flooring joists), and each loaded only with its own weight; and we may suppose the similar forms and the dimensions to be such that, in neither case, do the utmost stresses anywhere introduced exceed the limit of elasticity of the material; and that the beams do not bend, in either case, to any such considerable curvature as to vitiate importantly the application of the ordinary formula which will be used.

For the elastic deflection of rectangular beams of uniform cross-section, and loaded uniformly along their length, while placed horizontally and supported at both ends, we may use the well-known formula,—

$$\delta = \frac{5}{32} \frac{p\, l^3}{\mathrm{E}\, b\, d^3}; \text{ where}$$

l denotes the length between supports,
b the breadth of the beam,
d its depth,
E the modulus of elasticity,
p the load applied, and
δ the deflection or depression at middle due to the load.

Now, putting u to denote any chosen homologous linear dimension in the larger and smaller beam, we see that $p \propto u^3$, $l \propto u$, $b \propto u$, and $d \propto u$; and hence from the formula we get

$$\delta \propto \frac{u^3}{u} \frac{u^3}{u^3}, \quad \text{or} \quad \delta \propto u^2.$$

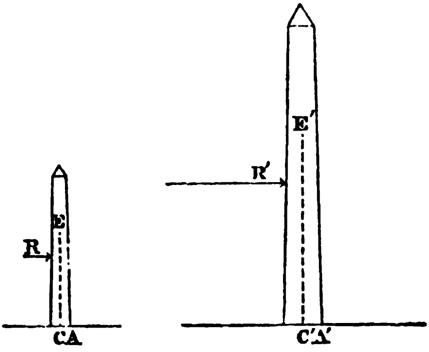
That is, the deflections of similar rectangular beams supported as has been stated and loaded with their own weights, would increase proportionally to the squares of the linear dimensions. But, obviously, for the production of equally severe stresses, the deflections ought to be proportional only to the linear dimensions, and consequently the larger beam will be more severely stressed by the load of its own weight than the smaller one will be in like manner by the load of its own weight.

Again, as another example, let us suppose that a tank of boiler plate, or of cast iron plates, for holding water, is arranged, for simplicity, to stand on three columns placed triangularly on the ground thus .·., so that the tank is supported simply at three points of itself, and that it has been found of sufficient strength for bearing the load of water which fills it; and let us suppose that it is judged to be of good design, neither too strong nor too weak. Let it be proposed to construct another tank like this one, of exactly the same proportions in all respects, but having all its linear dimensions exactly double of the corresponding linear dimensions of the one already successfully in use. The question arises, What inference is to be drawn, as to whether the proposed larger tank would be of suitable strength, or needlessly too strong or too weak, for bearing to be filled with water, from the information that the similar, but smaller, tank is of just suitable and proper strength for what it has to bear? The answer, which in this case would not be attainable in any such easy ways as were available in the two very simple cases previously adduced, is obtainable at once from the general principles which have been demonstrated. Thus we see, in comparing the loads of water pressure that can be borne on homologous areas in the smaller and the larger tank, that in order to introduce equal intensities of stress at corresponding places in the metal of both tanks, and therefore to produce similar deformations in both, the loads of water pressure on homologous areas must be proportional to the squares of the linear dimensions; or, in the case supposed, must be four times as much in the larger tank as in the smaller, but that the actual loads would be eight times as much in the larger as in the smaller tank. Hence the larger tank would be insufficient in strength for its load, or would have a less margin of superabundant strength for safety than the smaller one has.

Now, from the particular cases which have been adduced, and from the general principles which have been demonstrated, we may see that it would be wrong to judge from the sufficiency of strength of a model beam bridge, or of a water tank or other structure, in circumstances such as have been under consideration, that the larger corresponding structure would be likewise sufficient. In fact, we may when that is mainly or essentially due to their gravity (commonly named as their *weight*)—we may see that, if we regard the cases to be dealt with as being such that in them the strength of the material is amply sufficient to prevent the occurrence of any such failure of strength at any part of either structure as would be in any important degree influential on the relative stabilities of the structures of different sizes, the similar structures will be brought alike to be just ready to fail in stability (that is, alike ready to be overturned) by the application on their surfaces of similar force-systems, the magnitudes of the forces applied on homologous areas being in the ratio of the cubes of homologous linear dimensions of the different structures; or, in other words, the intensities of the forces applied at similarly situated places being in the ratio of the homologous linear dimensions.

In reference to this, we may first consider the case of two similar obelisks, or two similar pinnacles, supposing them to be acted on by wind pressing on all corresponding patches of surface of the two objects, similarly in direction, and *with the same intensity of force;* and we may suppose the material to have sufficient strength to prevent the introduction of any important influence on their relative stabilities, from crushing or failing of the material in either case, at any place where it would become severely pressed at the edge of the base round which rotation would occur if overturning were effected.

Obviously, the resultants of the forces of the wind, acting as here



supposed, on the two obelisks will be similarly situated, as shown by R and R′ in the figure: and their magnitudes will be as the squares of homologous linear dimensions; and their arms or leverages for their overturning moments round A and A′ will be as the linear dimensions. Hence the moments of the overturning motives will be as the cubes of the linear dimensions. But if the obelisks were both on the point of overturning, the resisting moments would be as the fourth powers of the linear dimensions, because the gravities of the two obelisks are as the cubes of the linear dimensions, and the arms or leverages at which they act round the edges A and A′ are C A and C′A′ respectively, which obviously are as the linear dimensions. Thus we see that the larger obelisk would resist a force of wind which would blow down the smaller one; and we can readily see, further, that to make both be just on the point of overturning by the action of similar systems of forces applied by wind, the forces per unit of area at corresponding places would be in the ratio of the linear dimensions. If, for instance, one of them is of linear dimensions double of those of the other, it will be able to bear twice as much force of wind per square foot as the smaller one can bear; it will be able to bear eight times as much force on every element of its surface as the smaller one can bear on every corresponding element of its surface; and, in the aggregate, the resultant of all the wind forces which it will be able to bear will be eight times as great as the resultant of all the wind forces which the smaller one can bear; and, further, the moment of the overturning motive, and that of the equal and opposite resisting turning motive, will each be sixteen times as great in the larger structure as in the smaller one.

The consideration of the relations between factory chimneys or other chimneys, similar in form but differing in dimensions, may be aided, though not fully elucidated, by consideration, in connection with them, of the principles just adduced for the supposed cases of obelisks and pinnacles. In the case of large chimneys, the yielding of the material by crushing at the severely compressed edge of the base, or by crushing, splitting, or otherwise failing through deficiency of strength at any part, would be decidedly influential, and in some cases might be largely influential, on the results, and therefore must not be left out of account in any very complete consideration of the damage; and if another ship were made, of larger size, and were similarly loaded, all linear dimensions—including thicknesses of plates, diameters and lengths of rivets, linear dimensions of angle irons, and all other linear dimensions, both of the ship and of its load—being increased in one and the same ratio, we would not be entitled to judge that the larger ship would be capable of bearing the same usage. In order to its being capable of standing supported on rocks at its ends, with the same margin of superabundant strength for safety as in the smaller ship, the materials in the larger ship would all require to have their elastic rigidities and their strengths per unit of area of section increased in the ratio in which the linear dimensions were increased. But our materials do not afford any increase in rigidity and strength per unit of area, on being made in larger pieces; and thus, the larger ship will be insufficient in strength, or at least will have a less margin of extra strength for safety than the smaller ship has. In reference to the ductile or malleable bending of iron bars or plates of different sizes, Professor Thomson explained that if the material were of quite the same quality in the various cases of different sizes, the pieces ought all to bear similar bendings equally well; but that very often the larger sized materials, as made by men, are of qualities inferior to those which are easily attainable in the smaller pieces, and that this accounts for our so often finding the larger pieces more liable to break than the smaller pieces in being subjected to the same kinds of deformations.

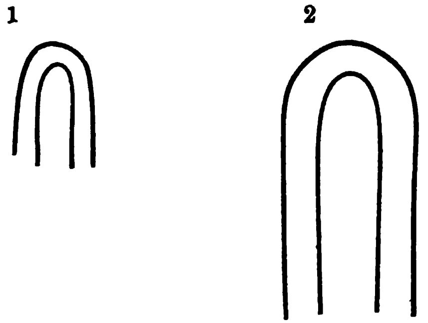
Mr A. MORTON said he was very glad that Professor Thomson had said that the weight varied as the cube, and the pressure as the square. In speaking of the breaking and bending of larger and smaller pieces of iron the Professor had said he thought the difference of the material had all to do with it. Suppose he took a flat malleable plate, or flat bar, which would bend flatways without breaking, then he would like to know whether it would bend equally on edge?

Mr J. E. WILSON said that he understood the Professor warned them as to the comparing of quantities. In the bending of iron they had a greater leverage if it were thick than if thin.

In bending a one inch bar they had to stretch the outside to a half circle of a certain diameter; and a thicker bar to a greater diameter. It was the same thing which Mr Morton had asked about —comparing the surface of the weight and the surface of the rod. The Professor meant to say that in that they were wrong; that they should take the square of the rod against the cube of the weight.

Professor THOMSON replied, giving explanations in accordance with the principles he had previously put forward.

The PRESIDENT drew a diagram on the black board and referred to it saying:—Suppose that 1 was half the size of 2, when they came to bend them down he thought that 2 would break very much easier than 1 at the top (as he had marked by the lines), in so much as the surface to be stretched round the top of the arch was the same in both instances, atom for atom, while the structure was twice the thickness. He thought in the case of wire drawing, it was quite clear.



Professor THOMSON, by aid of sketches on the black board, showed how it is that, in the thicker bar or plate bent double, although there is a greater amount of stretching of the material round the convex side, yet there is in like proportion a greater length of material to receive this greater stretch, and that the severity of the deformation is really the same in the large as in the small bar or plate.

On the motion of the PRESIDENT, a vote of thanks was awarded to Professor Thomson for his paper.

steam within the engines possesses, and must exert, in the low pressure cylinder before being exhausted into the condenser.

Considering the extremes of effort engines on board ship have to develope when a vessel is pitching heavily in a head sea—from their greatest power when the propeller is covered by the water to the minimum or power little more than necessary to overcome the friction of the engines when working without load when the propeller is revolving in the air—it would appear that an instrument becomes necessary, possessing independent action from the engines, or from whatever other source it may be derived, should have the power of anticipating by its movements the races of the main engines.

Such a power may be gained from the variations of the wave level at the stern of the vessel, and transmitted pneumatically to the engine-room in the manner which will now be explained, by the aid of the model and drawings of the governor before you.

The governor may be said to be composed of two separate parts, represented by the Figures Nos. 1 and 2. (See Plate VII.) Fig. 1 is an air-chamber, placed where convenient, in the after part of the vessel if in screwsteamer, or alongside the paddles of a paddle steamer. By opening the sea-cock B, which is secured by the flange D to the shell-plating of the vessel's hull, the inside of the chamber will be put in communication with the sea, and the air it contains be compressed to the pressure due to the height of the water outside. By means of the air-pipe H, which leads from the top of the chamber A through the screw-shaft tunnel, or otherwise as convenient, into the engine-room, this pressure is transmitted to the apparatus represented by Fig. 2, which has its lower part A made perfectly air-tight by the India-rubber diaphragm C, which is securely bolted between the flanges of the parts A and B. In the upper part is placed the spring E, which can be compressed or relieved by the adjusting nut F to suit the water the vessel is drawing. Having filled the air-vessel, pipes, &c., with air, by means of the drain-cock C on Fig. 1 (and which should always be left open wh n the Governor is not in use), and opened the sea-cock, the spring in Fig. 2 is compressed, and will remain so as long as the stern of the vessel preserves a uniform draught, or, in other words, as long as the screw-propeller remains immersed in the water. As the propeller emerges from the sea, and consequently the engines have less work to perform, the pressure decreases in the air-chamber, pipes, &c., and the India-rubber diaphragm C in Fig. 2 is forced down by the spring E, and the steam valve acted upon through the connecting-link K (the length of which is adjustable through a right and left hand nut in the centre), and the lever J, which may be placed direct upon the throttle valve.

The Governor may be fitted to work the common throttle valve or the equilibrium valve of the largest engines, there being no practical limit to the power it may be made to exert.

In answer to questions,

Mr Dunlop said the disc was made of indiarubber, and was pressed up by the air. The water never left the chamber. The communication between the governor and chamber was entirely made by air. The action of the air was found to be very quick. For instance, with as much as 250 feet of a 1-inch bore brass pipe, there was practically instantaneous communication with the steel springs. Forty-nine vessels had been fitted with these governors. The largest vessel supplied was of 600 horse-power. The effect of the governor, over 24 hours in which it had been in use, was an average increase of speed per minute of two revolutions with the same power of steam. They acted as well with vessels of light draught, such as six feet.

Professor Thomson asked whether there was any provision for keeping up a uniform proper quantity of air in the air-vessel—would there be no liability to lose all the air by leakage or by its absorption in the sea water? Or was there any provision to prevent any too great increase of air in the air-vessel, or anything to indicate to persons looking at the outside whether or not there was a proper quantity of air in it?

Mr Dunlop said the quantity of air in the air-vessel could not affect the action of the governor. If it was ten times as large as at present made, it might affect the action of the governor, but a small be shut, one or two, or even seven or eight revolutions in anticipation of the race of the engines, so that when the throttle-valve was closed, and the propeller out of the water, there was only enough of steam passing into the engines to keep them going. The governor was regulated very simply, with a nut on the top of the spring shown in Fig. 2.

The **President** said that, if it was necessary to cut off the steam seven or eight revolutions in anticipation of the racing, there must necessarily be a loss of power and speed of the ship to that extent; and, also, that in orderto have steam through the high-pressure engine ready to act upon the low-pressure piston as soon as the propeller was immersed, it would be necessary to anticipate that action also, otherwise there would be a further loss of power and speed.

Mr **Morton** asked if, with this governor, there was any difference in the action with waves going along the vessel or those rolling on the broadside? In another form of governor the action of a suspended weight was tried for the same purpose, but it did not act with the rolling motion. It was hung in the stern, and free to oscillate fore and aft only, therefore it could only act with a pitching motion.

Mr **Dunlop** answered that the action of this governor was not affected either by a rolling or a pitching motion. It could work in any sea.

On the motion of the **President**, a vote of thanks was awarded Mr Dunlop for his Paper.

On the Action of Water and Frictional Resistance or Loss of Energy when Flowing at various Velocities through a Nozzle with a Converging Entrance and Diverging Outlet.

By MR JAMES BROWNLEE.

(SEE PLATES VIII. AND IX.)

Received and read on 25th January, 1876.

1. There is, perhaps, no engineering question which has received more consideration or been submitted to experimental determination with so much persistence as that of the flow of water. Any experiments, however, which have been made with the view of determining the discharge or velocity with which water flows through a nozzle with a diverging outlet appear not only to have been very limited, but there are no experiments on record by which the velocity has been determined when the water is discharged into a space in which the pressure is above that of the atmosphere.

2. It is generally conceived that the velocity depends upon the difference between the internal and external pressures only, and that an augmentation of the external resistance or outside pressure has the same effect in reducing the flow as a similar lowering of the inside head or pressure. It is only, however, within restricted limits that this is true. When, for example, water under a pressure of four atmospheres flows through the nozzle (Fig. 2, Plate IX.) into a pressure of three atmospheres, the velocity is double of that which would be generated by a pressure of one atmosphere when flowing into the best possible vacuum.

3. In some old works on hydraulics, it is mentioned that, when the discharge is not into the atmosphere, but into a vacuous space, the expanding outlet has no influence in either increasing or diminishing the discharge. This is obviously correct. As this mouthpiece is only a means of utilising the energy of the current, and causing the acquired momentum of the water to be expended in repelling the external resistance, and thereby reducing the pressure at the throat of the nozzle, this adjunct can have no influence when there is no external atmospheric pressure or other resistance to be repelled.

4. The Jet Pump of Professor James Thomson is perhaps the first practical and successful application of the diverging mouth-piece to any useful purpose.

In describing his invention to the meeting of the British Association at Belfast, over twenty years since, Professor Thomson said:—"The action of the Jet Pump depended upon two principles. One is the same as that of the steam blast used in locomotive engines and in the ventilation of mines; the other is one which was known to the ancient Romans, and was used sometimes by them for drawing off more water from the public pipes than they paid for." The same fraudulent artifice is said by Mr James R. Napier to be at present practised in New York, where the price charged for water is in proportion to the area of the delivery pipe.

There is, however, no instance, unless it be Mr Robertson's Excavator, in which the action of the diverging outlet has been so advantageously and successfully utilised as it is in the Ejector-Condenser of Mr Alex. Morton.

5. In the course of last winter some very instructive and interesting experiments were brought before the Philosophical Society by the inventor. In these experiments it was shown by Mr Morton, that a vacuum little inferior to that in the barometer was produced at the throat of the ejector by the passage of water under a pressure of 2½ lbs. per square inch, which corresponds to a flow from 17½ lbs. absolute pressure, into 15 lbs., with a vacuum between those pressures. The pressure which first falls from 17½ lbs. to nothing—

when the water acquires a velocity of about 50 feet per second—should again rise to 17½ lbs. before this current could be deprived of motion, provided no loss of motion was occasioned by friction; but as the pressure re-ascends to 15 lbs. only, 2½ lbs. represents the frictional loss of energy in passing water through the tube at a velocity of about 50 feet per second, being one-seventh of the initial absolute head or pressure of 17½ lbs.

6. If, then, the frictional loss is in proportion to the square of the velocity, the flow of water from 35 lbs. absolute pressure into 30 lbs., should—through the tube (Fig. 2)—be the same as when discharged into a vacuous space; or the velocity from 70 lbs. into 60 would be about 100 feet per second, and could not move faster although this 60 lbs. of back pressure were entirely removed. Those statements are, however, only deductions, which were thought consistent with Mr Morton's experiment, and not discordant with mechanical science; but not finding that similar views had ever before been propounded, the hypothesis suggested required confirmation.

7. A series of experiments were, therefore, commenced in April last, and continued at intervals up to August. The arrangements made for those experiments are represented in Plate VIII., Fig. 1.

By opening the tap A, Loch Katrine water was admitted into the pipe P. The pressure of this water at the City Saw Mills, where those experiments were made, was found to be about 35 lbs. per square inch above the atmosphere. In now partially opening the tap B, water was admitted into chamber C, whence it passed through the tube (a full-sized section of which tube is shown in Fig. 2) into chamber D, thence through the tap E (when open), and finally escaped into the atmosphere at the end of the pipe O. Pressure gauges Nos. 1 and 2 communicated with chambers C and D, and indicated the pressure of the water before and after passing through the nozzle. A small brass tube, *t*, with stop-cock and coupling, *c*, as shown, communicated with the throat of the nozzle and the top of a mercurial glass tube, V, by which the pressure or vacumm at the throat of the nozzle could also be observed.

8. With a pressure in chamber C of 30 lbs. above the atmosphere,

as indicated by gauge No. 1, and the outlet tap E, opened so far only as to allow the pressure in chamber D, to fall to 25·5 lbs., as indicated by pressure gauge No. 2, it was observed that the mercury stood at the same level in both limbs of the vacuum glass tube V, thereby showing that the pressure at the throat of the nozzle was neither above nor below the pressure of the atmosphere.

We have thus at the inlet end of the tube, in chamber C, a pressure of 30 lbs. Then forward 1½ inches, at the throat of the tube, neither pressure nor vacuum; but onward another 5¾ inches, at the outlet end of the tube, when motion is arrested, the pressure again rises to 25·5 lbs. above the atmosphere in chamber D.

9. This, it will be observed on referring to Table I., is entered as Experiment No. 1.

As 30 lbs. per square inch corresponds to a head of 69·24 feet of water, those figures are both entered in the second column of the Table as the pressure above the atmosphere in chamber C. In the third column is entered the pressure in chamber D in lbs. per square inch, and also in feet of water, 58·85 feet of water corresponding to 25·5 lbs. In the fourth column is entered the difference between the inside and outside pressure, which, as noted in the first experiment, is 4·5 lbs., corresponding to 10·39 feet of water.

In the ninth column is entered the vacuum at the throat of the nozzle in inches of mercury, and also in *feet* of water, 1 inch of mercury corresponding to 13·6 inches of water; but as in this experiment there was no vacuum, the figures are here 0·00. While the water was thus flowing through the nozzle from 30 lbs. pressure into 25·5 lbs., and showing neither pressure nor vacuum at the throat, the narrow-mouthed bucket F was, at a determined instant of time, put under the outflowing current, and was found to fill to the lip in exactly 54 seconds, as entered in the fifth column of the Table. Without describing the method, it may suffice to say that the time occupied in filling the bucket could readily be determined to within less than one-fourth of a second. The bucket (at 60° Fah.) held 47⅝ lbs. of water, which corresponds to the weight of a prism of water 2·308 × 47⅝ = 109·92 ft. in length and 1 in. square. Then, as the diameter of the nozzle at the throat, as carefully measured by an accurate instrument constructed by Mr J. White, Union Street, was ·1982 inch, or $\frac{1}{32·41}$ square inch in area, the water contained in the bucket would correspond to a prism 32·41 × 109·92 = 3562·5 feet in length, with a diameter or area of that of the throat of the nozzle.

To find the velocity of the water in feet per second, we have, therefore, only to divide this constant number (3562·5) by the time in seconds required to fill the bucket, and in dividing 3562·5 by 54 we have 65·97 ft. per second (entered in Col. 6 of the Table) as the velocity with which in this experiment the water passed the throat of the nozzle. The figures 25·86, as entered in Col. 7, is the velocity in feet per second due to a pressure of 4½ lbs. or fall of 10·39 feet, and this velocity could not be exceeded if the nozzle was deprived of the diverging outlet.

The velocity has, therefore, by utilising the energy of the issuing current, been increased from 25·86 to 65·97 feet per second, or to 2·55 times that which is (without loss from friction) due to a fall of 10·39 feet, as entered in Col. 8. By actual experiment, however, with the same head of 10·39 feet, the velocity of the water when passed through the orifice, Fig. 3, with no diverging outlet, was only 25 feet per second—the difference (·86 foot per second) being lost in friction.

In Col. 9 is entered the observed vacuum; but as in this experiment the pressure at the throat was precisely the same as that of the atmosphere, the figures here are 0·00.

We pass now to Col. 10, in which is entered the head in feet of water, as in Col. 2, plus the vacuum in feet of water; but as we have nothing here to add for vacuum, the figures 69·24 are the same as in Col. 2. The figures 66.78 in Col. 11 is the velocity due to a pressure of 30 lbs., or 69·24 feet of head. In looking back, however, to Col. 6, we find that a velocity of 66·78 feet per second was not obtained, but only 65·97, and dividing this by 66·78 we obtain the figures ·988, as entered in Col. 12, thereby showing that the water had lost from friction 1 — ·988 = ·012, or 1¼ per cent. of the velocity due to a pressure of 30 lbs., before entering the diverging outlet.

The experimental velocity, as entered in Col. 6, is 65·97 feet per second, and that velocity should be generated by a head of $65{\cdot}97^2 \div 64{\cdot}4 = 67{\cdot}58$ feet, as entered in Col. 13, provided there was no loss from friction; but as $h + V = 69{\cdot}24$ feet of head was required, we have $67{\cdot}58 \div 69{\cdot}24 = {\cdot}976$ (as entered in Col. 14) of the head $h + V$ effective in generating motion. The residue, $1 - {\cdot}976 = {\cdot}024$, or $2\frac{4}{10}$ per cent. of the head $h + V$, being lost in friction before entering the expanding outlet.

10. We now come to Experiment No. 2, in which the tap E is so much further opened as to allow the pressure in chamber D to fall from 25·5 lbs. to 23·5 lbs., and the tap B is at the same time opened as much further as was required to maintain the same pressure as before of 30 lbs. in chamber C.

In thus lowering the outside pressure from 25·5 lbs. to 23·5 lbs., the mercury in the vacuum tube V, which before stood at the same level in both limbs of the glass tube, now rose to a difference of level of 27·8 inches, corresponding to a column of 31·5 feet of water, as entered in Col. 9 of the Table. It was now found that the bucket filled in 45 seconds, which corresponds to a velocity of 79·17 feet per second; whereas the velocity due to the difference of pressure of 6·5 lbs., or 15 feet of fall, is only $8{\cdot}025 \sqrt{15} = 31{\cdot}08$ feet per second, as entered in Col. 7.

This experiment may, however, be better illustrated by supposing two water cisterns standing side by side with the nozzle communicating with both, as represented in Fig. 4. The altitude of the water in cistern R being 69·24 feet, and in cistern S 54·24 feet, let a small tube now descend from the throat of the nozzle, and dip into water in a well at a depth of 31·5 feet. The water would then flow from cistern R into and over the top of cistern S, while at the same time water would ascend 31·5 feet from the well up the small tube to the throat of the nozzle. The velocity (excluding friction) with which the water would flow through the nozzle would be that which a body would acquire in falling from the surface of the water in the cistern R to that of the water in the well—that is, through $(h_1 + V) = 100{\cdot}74$ feet, which is

$$8{\cdot}025 \sqrt{100{\cdot}74} = 80{\cdot}55 \text{ feet}$$

per second, as entered in Col. 11 of the Table. The experimental velocity, however, was not 80·55, but only 79·17 feet per second, as entered in Col. 6. By raising the level of the water in cistern R, the velocity through the nozzle would be increased, and water from the well would then slowly ascend the tube, enter the throat of the nozzle, and flow over the top of cistern S at an altitude 85·75 feet above the surface level of the water in the well.

It is upon this principle that the operations of the Excavator and Elevator of Mr James Robertson depends. Two pipes furnished with proper nozzles pointing towards each other are lowered to the bottom of a river. Water is forced down one, and rises above the river surface in the other, bringing with it mud, sand, and gravel from the bottom of the river.

11. Passing on to experiment No. 4, the outside pressure in chamber D is here lowered to 20 lbs., which corresponds to bringing the level of the water in cistern S down to 23·08 feet below the level of the water in cistern R. The mercury in the vacuum tube V now stands 29·6 inches higher in the limb communicating with the throat than it does in the other limb open to the atmosphere, the barometer at the same time standing at 30 inches, and temperature of water 60 deg. Faht. The bucket now filled in in 43¾ seconds, corresponding to a velocity of 81·43 feet per second, and as will be observed in the 5th experiment, did not flow any faster when the tap E was full open and the 20 lbs. of outside pressure entirely removed. It will, therefore, be observed that the flow from cistern R, with hydrostatic head of 69·24 feet, into the cistern S, under a head of 54·24 feet of water, is very nearly the same as if that cistern were entirely void of both air and water, the difference being only 81·43 — 79·17 = 2·26 feet per second.

12. As the inside and outside pressures in those experiments were taken with pressure gauges which are more or less uncertain, and as

the pressure of Loch Katrine water was also very unsteady, the figures in columns 2, 3, and 4 of Table I. may err to the extent of half a pound, but not more. A more accurate method was, however, adopted in making the experiments which are noted in the succeeding Tables.

13. In those experiments, the Loch Katrine water was shut off by closing tap A (Fig. 1), and water from a cistern, which forms the roof of the boiler-house, was admitted by opening tap H. The water in this cistern—which is 40 feet in length and 19 feet in breadth—was maintained at a level of 12½ feet above the throat of the nozzle, as indicated by two vertical glass tubes which were substituted for the pressure gauges. One of these tubes communicated with chamber C and the other with chamber D, and thereby indicating by the height of the water in those tubes the pressure before and after passing through the nozzle. The tubes being open at the top, rose to a height of about 13 feet, or a little above the level of the water in the cistern. When the taps H and B were open, and tap E shut, the water stood in both glass tubes at the same level as that of the water in the cistern—12½ feet above the throat of the nozzle, as indexed on a scale divided into feet and inches, measured from the throat of the nozzle upwards, as shown on the right hand side of Fig. 1.

14. The taps H and B being full open, and the water in both glass tubes standing, with tap E shut, on the same level as that of the water in the cistern—12½ feet above the throat of the nozzle—the discharge tap E was now slowly opened until the water in the outside glass tube fell one foot, or from 12½ to 11½ feet, as noted in Experiment No. 6, Table II. The bucket was now found to fill in 218 seconds, which corresponds to a velocity of 16·34 feet per second, as entered in Cols. 5 and 6. Then, as the velocity due to a fall of one foot is only 8·025 feet per second, the quantity discharged in this experiment is 16·34 ÷ 8·025 = 2·04 times that which could possibly be obtained without the diverging outlet, as entered in Col. 8.

15. Passing on to Experiment No. 8, the tap E was here opened

until the water in the outside glass tube fell to 10·15 feet. The difference of level in the two glass tubes being now 2·35 feet, when (as entered 0·00 in Col. 9 of the Table) there was neither vacuum nor pressure above the atmosphere at the throat of the nozzle. The bucket now filled in 130 seconds, corresponding to a velocity of 27·4 feet per second, which velocity is 27·4 ÷ 12·29 = 2·23 times that which is due to a fall of 2·35 feet; but as the level of the water in the cistern, as indicated by the inside glass tube, was 12½ feet above the throat of the nozzle, and as there was in this experiment no pressure above the atmosphere at the throat, the velocity should correspond to that which is due to a fall of 12½ feet, or 28·37 feet per second, provided there was no loss from friction. The velocity obtained was therefore only 27·4 ÷ 28·37 = ·966, or say $96\frac{6}{10}$ per cent. of that which is due to a fall of 12½ feet, as entered in the 12th column of the Table.

This velocity of 27·4 ft. per second generated from an initial head or difference of level of 2·35 feet is, it may here be observed, precisely the same as was obtained from 12½ feet of head when the water passed through the nozzle, Fig. 3, without the diverging outlet, as noted in the 43rd Experiment, Table IV. With the former nozzle it, therefore, appears that 2·35 feet of head causes the passage of precisely the same quantity of water that 12½ feet of head does with the latter—both orifices being exactly of the same size.

In this experiment the water first descends 12½ feet, when it flows through the tube with a velocity of 27·4 feet per second, and then re-ascends 10·15 feet, or, = (10·15 ÷ 12·5) = ·812 feet for each foot of fall. The loss from friction in passing through the nozzle at a velocity of 27·4 feet per second is, therefore, 1 – ·812 = ·188, or $18\frac{8}{10}$ per cent. of the initial head of 12½ feet. This frictional loss it may, however, be observed, is much less in entering than in leaving the nozzle. In descending 12½ feet the water only acquires a velocity due to a fall of $27·4^2$ ÷ 64·4 = 11·65 feet, when it re-ascends to a height of 10·15 feet only. While, therefore, 12·5 – 11·65 = ·85 feet, or $6\frac{8}{10}$ per cent., is lost in entering and generating motion, 11·66 — 10·25 = 1·5 feet, or 12 per cent. of the initial head, is lost in arresting that motion. The last loss being nearly double that of the first.

The length of the outlet, or diverging part of the tube, is 30 times the diameter of the throat; but a greater length would likely reduce the frictional loss. That loss is, however, relatively greater at lower than at higher velocities, in a ratio which has hereafter to be noticed.

16. Again, passing to Experiment No. 14, in which the tap E is further opened, until the water in the outside glass tube falls to an altitude of 5·5 feet above the throat of the nozzle. The difference of level in the two glass tubes is now 12·5 — 5·5 = 7 feet. The velocity is 53·17 feet per second, being 2·5 times that (21·23) which is due to a fall of 7 feet.

The mercury in the vacuum tube V now rises to a height equivalent to 32·5 feet of water; and adding this 32·5 feet of vacuum, or pressure below the atmosphere at the throat, to the head in the cistern of 12½ feet above the throat, the pressure by which the water is now forced into the nozzle, is equivalent to $h_1 + V = 12·5 + 32·5 = 45$ feet head of water, as entered in Col. 10. The velocity due to that head is $8·025 \surd 45 = 53·83$ feet per second, as entered in Col. 11. The experimental velocity is, however, only $53·17 \div 53·83 = ·988$ of this (as entered in Col. 12), which corresponds to a loss from friction in entering the nozzle of $1 - ·988^2 = ·024$, or $2\frac{4}{10}$ per cent. of the 45 feet of head. After descending 45 feet, the water re-ascends 38 feet, or ·844 feet for each foot of fall. The whole frictional loss in passing water through the nozzle at a velocity of 53·17 feet per second is 7 feet of head, which corresponds to $7 \div 45 = ·155$, or 15½ per cent. of the 45 feet of pressure required to generate that velocity. It will be observed that at this higher velocity the per centage of frictional loss has decreased, while entering the nozzle and generating motion; but in arresting that motion, after passing the throat, and restoring pressure that loss has increased. Both losses together are, however, relatively less at the higher than at the lower velocities.

17. In further lowering the outside pressure from 5·5 to 5 feet, as in Experiment No. 15, the mercury in the vacuum tube V rose (when the barometer stood at 30 inches) to a height of 29·6 inches,

which corresponds to 33·5 feet of water, as entered in Col. 9. The height of the column of mercury in the vacuum tube being now only 4-10ths of an inch less than in the barometer, did not, with water in the cistern at 60° Fah., rise any higher with any further diminution of the outside head or pressure. The velocity, however, increased slightly, until (as shown in Experiment No. 17) the outside head was lowered to 3·5 feet, when the velocity increased to 54·6 feet per second, but did not further increase when the discharge tap E was opened in full, and all outside head or pressure above the atmosphere thereby removed.

A velocity of 54·6 feet per second is that which is due to a fall of $(54{\cdot}6^2 \div 64{\cdot}4) = 46{\cdot}3$ feet, whereas the actual *fall* could not exceed 46·5 feet, although the vacuum at the throat of the nozzle had been as good as in the barometer, since 30 inches of mercury corresponds to 34 feet head of water, and adding this to the 12½ feet of head in the cistern gives the maximum head $= 34 + 12{\cdot}5 = 46{\cdot}5$ feet.

I have said that the mercury in the vacuum tube, at its highest point, was 4-10ths of an inch less than in the barometer. There was, however, reason to believe that the vacuum at the throat of the nozzle was better than as indicated by the mercurial vacuum tube V. On the top of the mercury in that tube there always rested more or less water, and that water, it could be observed, was in an incipient state of boiling. Numerous small globules of vapour could be seen forming and rising up through the water at the top of the tube, and flowing into the throat of the nozzle.

It is, however, rather remarkable, after all pressure at the throat had nearly vanished, that the frictional loss in generating motion should also disappear, as evinced in the four last experiments of this Table II., where the velocity is 54·6 feet per second, since it will be observed that this velocity corresponds very nearly to that which is theoretically due to the absolute pressure or head above zero of $12{\cdot}5 + 34 = 46{\cdot}5$ feet.

18. In Tables I. and II. the inside pressures were constant and outside pressures variable; but in Table III., to which we have now to refer, the inside pressures vary, beginning with 3 inches, or ·25 feet

head of water, and ending at 12⅓ feet; while the outside pressure is constantly that of the atmosphere with barometer at 30 inches.

In Experiment No. 21, being the first in this Table, the tap B was opened so far only as was sufficient to maintain the water in the inside glass tube at an altitude of 3 inches, or ·25 feet above the level of the throat of the nozzle, while at the same time the water in the outside glass tube was, by so adjusting the opening of the discharge tap E, maintained at the same level as the throat, as indexed O on the scale. The bucket now filled in 535 seconds, corresponding to a velocity of 6·66 feet per second, being only 1·66 times that which is due to a fall of ·25 feet.

In this and the two succeeding experiments the mercury was taken out of the vacuum tube V and water substituted, and the difference of level of this water in the two limbs of the vacuum gauge was ·52 feet, as entered in Col. 7. The difference between the inside pressure and the pressure at the throat is, therefore, as entered in Col. 8, $h_1 + \mathrm{V} = \cdot 25 + \cdot 52 = \cdot 77$ feet, and the velocity due to that head is $8\cdot 025 + \surd\ \cdot 77 = 7\cdot 04$ feet per second; whereas the experimental velocity, v_1 is 6·66, or $\frac{v_1}{v_3} = 6\cdot 66 \div 7\cdot 04 = \cdot 946$ of that which is due to a fall of ·77 feet, as entered in Col. 10. In entering the nozzle the pressure first falls $h_1 + \mathrm{V} = \cdot 25 + 52 = \cdot 77$ feet, but re-ascends only ·52 feet. The fraction of pressure or head restored is, therefore, only $\frac{\mathrm{V}}{h_1 + \mathrm{V}} = \cdot 52 \div \cdot 77 = \cdot 675$, or 67½ per cent., as entered in Col. 11; while the residue, 32½ per cent. or ·325, as entered in Col. 12, is entirely lost in friction while passing water through the nozzle at a velocity of 6·66 feet per second.

This frictional loss is separated into two quantities, as entered in Cols. 13 and 14. The first is the loss while entering the nozzle and acquiring motion; while the pressure due to that velocity, but which is partly unrecovered, constitutes the second and much the largest loss. In looking down these two last columns of figures, it will be observed that both losses diminish with increase of velocity until coming to Experiment No. 27, where the difference between the inside pressure and that at the throat of the nozzle is $h_1 + V = 23·8$ feet of water; but as 19·8 feet only of that pressure is restored, the remaining 4 feet corresponds to a total loss of $4 \div 23·8 = ·168$, or $16\frac{8}{10}$ per cent. of the 23·8 feet of pressure, while passing water through the nozzle at a velocity of 37·9 feet per second. The loss on entering the nozzle being $1 — ·968^2 = ·063$, or $6\frac{3}{10}$ per cent., and that unrestored on leaving is ·105, or $10\frac{1}{2}$ per cent. of the 23·8 feet of pressure requisite to produce that velocity.

19. In raising the inside pressure to 6·5 feet (Experiment No. 30), the mercury in the vacuum gauge rose to 29·6 inches, corresponding to 33·5 feet of water. The difference between the inside pressure and that at the throat is therefore now $h_1 + V = 6·5 + 33·5 = 40$ ft., and the velocity due to a fall of 40 feet is $v_3 = 8·025 \sqrt{40} = 50·75$ ft. per second, but the experimental velocity is only 49·83, or $49·83 \div 50·75 = ·982$ of the theoretical. For each foot of fall the water reascends ·838, as in Col. 11, and (as entered in Col. 12) $6·5 \div 40 = ·162$ is the fraction of pressure lost in first generating a velocity of 49·83 feet per second, and then arresting that motion—the frictional loss in generating motion apparently here diminishing to $1—·982^2=·036$, while the loss in impeding and arresting that motion rises to ·126, as entered in Col. 14.

20. The figures in Cols. 11, 12, and 14 are not continued in the Table beyond the 30th Experiment, because the pressure at the throat is, with 6·5 feet of inside head, reduced to nearly nothing, so that there is not then any further resistance or pressure at the throat to be repelled. A head of 6·5 feet, or velocity of 49·83 feet per second, being sufficient to overcome or balance the atmospheric pressure, and so prevent any part of that pressure from acting upon the water at the throat, any additional head or velocity would only exhaust itself in agitating or churning the water in the expanding outlet.

21. In Experiment No. 36, the velocity with a head of $12\frac{1}{2}$ feet is again nearly that which is due to the absolute pressure or head of $12·5 + 34 = 46·5$ feet, as previously noticed in the 20th Experiment of Table II.

22. Not knowing of any series of experiments through a nozzle with a diverging outlet with which to compare those here noted, it was thought desirable to try an orifice with rounded entrance only, but of precisely the same (·1982 inch) diameter, so that the flow through so small an orifice might be compared with experiments made by others with orifices of the same form but of much larger area. The orifice (Fig. 3) was therefore substituted for the nozzle (Fig. 2), and in the results which are noted in Table IV., it will be observed in Col. 6 that the co-efficient of discharge or velocity gradually increases with the head, beginning at ·941, with a head of 1 foot, and rising to ·966, with a head of 12½ feet.

The co-efficients given by Weisbach for an orifice 9-10ths of an inch diameter, and for heads of 1, 5, and 10 feet, are ·958, ·969, and ·975.

From those experiments it therefore appears that the *velocity* through the smaller orifice is from 1 to fully 1½ per cent. less than through the larger, having an area of more than twenty times that of the smaller.

23. When water flows through an orifice with rounded entrance, such as Fig. 3, it will be observed (as in Table IV.) that the quantity discharged is rather *more* than doubled with a quadruple head; but it has been found otherwise when the flow is through an orifice in a thin plate with sharp edges, as the quantity discharged under a quadruple pressure is then *less* than doubled. This, very probably, arises from the jet contracting more as the pressure rises

24. After these Tables had been completed, it became an easy question to find whether or not the velocity with which the water passed through the compound nozzle (Fig. 2) corresponded with any uniform power of the pressure, or difference of head at the opposite ends of the tube.

On making this investigation, it was found that the experimental velocities agreed very nearly with the following equation :—

$$v_1 = 16{\cdot}02\,(h_1 - h_2)^{\frac{1}{1\cdot81}}$$

Or the converse of which is—

$$(h_1-h_2)=\left(\frac{v_1}{16\cdot02}\right)^{1\cdot61}=v_1{}^{1\cdot61}\div 87$$

The symbols h_1 and h_2 denoting the respective pressures at the extreme ends of the nozzle in feet of water, and v_1 the velocity in feet per second.

25. For the purpose of comparing the experimental velocities with those calculated from the above formula, Table V. has been constructed, in which the figures are placed in parallel columns; and on the inspection of those figures it will be observed that the velocities so calculated agree very nearly with the experimental, the extreme difference in no case rising to as much as 2 per cent.

26. The experiments in Tables II. and III. were made with very great care, and were many times repeated, the results seldom differing by as much as two per cent. And as the proposed formula agrees very well with those experiments, it appears that a double pressure drives the water through the compound nozzle with $2^{\frac{1}{1\cdot61}}=1\cdot538$ times the speed; and a double velocity does not, as might have been expected, require four times, but only $2^{1\cdot61}=3\cdot05$ times the pressure.

27. In these experiments the velocities through this small compound tube are, with respect to the pressures, greatly in excess of those obtained in any similar trials which are recorded in the best standard works. This augmented flow may be attributed to the greater length of the diverging outlet as compared to the area of the throat, the water thereby being brought more slowly to rest.

28. Should not, then, the resistance to a ship's motion be similarly reduced by lengthening the after-run, thereby also allowing the water more time to close in, and so contribute to the maintenance of a greater pressure or higher level towards the stern?

Those experiments seem to indicate that this lengthening of the after-run would have more influence in reducing the resistance than a long attenuated bow.

29. There is surely considerable similarity in the action of water moving through this tube and that of a ship moving through water.

In both cases the water is first put in motion at one end of the tube or ship, and brought nearly to rest at the other. Why, then, should the resistance not increase with the speed in the same ratio in the one case as in the other?

The screw, which constantly draws water away from the after part of the ship, always tends to depress the water level and reduce the pressure around the stern, where it is most wanted, thereby causing the vessel (which, when in motion, always settles down at the stern) to droop more than it otherwise would, and thus augments the resistance. The screw has, therefore, to some extent a retrogressive action upon the ship, and this action must at the higher speeds be greatly increased by a bluff stern or short after-run. If, however, a ship was drawn through smooth water by a cable, is it not likely that the resistance would only increase with the speed in the same ratio as that of the water through the tube? Has it not been found that the end thrust upon the screw shaft at the higher velocities is frequently much greater than the strain upon a rope when towing the vessel at the same speed?

Have any reliable experiments been made in the way of towing a ship at a wide variety of speeds, with the view of determining the rate at which the resistance varies with the velocity?

Those are questions which some of the many eminent shipbuilders belonging to this Institution are better able to answer than one unexperienced as I am in the matter.

It is only necessary further to add, that I have, at the suggestion of a member, constructed a curve to the equation in Table V., which curve is represented in Plate IX., Fig. 5. The vertical ordinates denote the resistance or hydrostatic head in feet requisite to generate the speeds which are figured along the horizontal base line in knots per hour, and also in feet per second.

As the speed generated by a head of one foot of water is 16·02 feet per second, which corresponds to $\frac{16{\cdot}02}{1{\cdot}688} = 9.49$ knots per hour, the velocity, when expressed in knots, is—

$$V = 9{\cdot}49\ (h_1 - h_2)^{\frac{1}{1{\cdot}61}}$$

$$\text{Or,}\quad h_1 - h_2 = \left(\frac{V}{9{\cdot}49}\right)^{1{\cdot}61}$$

TABLE I.—Flow of Water through a compound nozzle $\frac{1}{32\cdot41}$ sq. in. area at throat, under a constant inside pressure of 30 lbs. per sq. in. above atmosphere, which corresponds to a head of 69·24 feet into various outside pressures; the atmospheric pressure as indicated by barometer being 30 in. of mercury, which corresponds to 34 feet of water.

1	2	3	4	5	6	7	8	9	10	11	12	13	14
No. of Experiment.	Inside Pressure above Atmosphere in lbs. per sq in. and in feet of water.	Outside Pressure above Atmosphere in lbs. per sq. in. and in feet of water.	Difference of Pressure in lbs. per sq. in. and in feet of water.	Time of Filling Bucket in seconds.	Experimental Velocity in feet per second.	Velocity due to Difference of Pressures.	Velocities in Col. 6 divided by those in Col. 7.	Vacuum in inches of Mercury, and in ft. of water at throat of nozzle.	Pressure above Atmosphere in feet of water plus Vacuum in feet of water.	Velocity in feet per second due to Pressure plus Vacuum.	Velocities in Col. 6 divided by those in Col. 11.	Head in feet requisite to generate Experimental velocity v_1 in Col. 6.	Figures in Col. 13 divided by those in Col. 10.
	h_1	h_2	$h_1 - h_2$	t	v_1	v_2	$\frac{v_1}{v_2}$	V	$h_1 + V$	v_3	$\frac{v_1}{v_3}$	h_3	$\frac{h_3}{(h_1 + V)}$
1	30 69·24	25·5 58·85	4·5 10·39	54	65·97	25·86	2·55	0·00	69·24	66·78	·988	67·58	·976
2	30 69·24	23·5 54·24	6·5 15·00	45	79·17	31·08	2·55	27·8 31·5	100·74	80·55	·983	97·33	·966
3	30 69·24	22 50·78	8 18·46	44	80·97	34·89	2·32	29·6 33·5	102·74	81·34	·995	101·8	·991
4	30 69·24	20 46·16	10 23·08	43¾	81·43	38·55	2·11	29·6 33·5	102·74	81·34	1·001	102·96	1·002
5.	30 69·24	0 0	30 69·24	43¾	81·43	66·78	1·219	29·6 33·5	102·74	81·34	1·001	102·96	1·002

TABLE II.—Flow of Water through the same nozzle as in Table I., but under a constant head of 12½ feet, which corresponds to nearly 5·2 lbs. per sq. in. above atmospheric pressure, into outside pressures varying from 11½ feet down to nothing above the external atmospheric pressure of 30 in. mercury, or 34 feet of water.

1	2	3	4	5	6	7	8	9	10	11	12
Number of Experiment.	Inside Pressure in feet of water.	Outside Pressure in feet of water.	Difference of Inside and Outside Pressure.	Time of filling Bucket in seconds.	Experimental Velocity in feet per second.	Velocity due to difference of Pressure.	Velocities in Col. 6 divided by those in Col. 7.	Vacuum in feet of Water at throat of Nozzle.	Inside Pressure plus Vacuum.	Velocity due to Inside Pressure plus Vacuum.	Velocities in Col. 6 divided by those in Col. 11.
	h_1	h_2	$h_1 - h_2$	t	v_1	v_2	$\frac{v_1}{v_2}$	V	$h_1 + V$	v_3	$\frac{v_1}{v_3}$
6	12·5	11·5	1	218	16·34	8·025	2·04				
7	12·5	10·5	2	144	24·74	11·35	2·18				
8	12·5	10·15	2·35	130	27·4	12·29	2·23	0·0	12·5	28·37	·966
9	12·5	9·5	3	111½	31·95	13·9	2·30	4·5	17	33·09	·966
10	12·5	8·5	4	94	37·9	16·05	2·36	11·3	23·8	38·84	·968
11	12·5	7·5	5	82	43·45	17·94	2·42	18·5	31	44·69	·972
12	12·5	6·5	6	74	48·14	19·66	2·45	25	37	49·14	·980
13	12·5	6·	6·5	70	50·9	20·66	2·46	29	41·5	51·76	·983
14	12·5	5·5	7	67	53·17	21·23	2·50	32·5	45	53·83	·988
15	12·5	5·	7·5	66	53·98	21·98	2·45	33·5	46	54·43	·996
16	12·5	4·5	8	65½	54·39	22·76	2·39	33·5	46	54·43	·999
17	12·5	3·5	9	65¼	54·6	24·07	2·27	33·5	46	54·43	1·003
18	12·5	2·5	10	65¼	54·6	25·37	2·15	33·5	46	54·43	1·003
19	12·5	1·5	11	65¼	54·6	26·61	2·05	33·5	46	54·43	1·003
20	12·5	0·0	12½	65¼	54·6	28·37	1·92	33·5	46	54·43	1·003

TABLE III.—Flow of Water through the same Nozzle, with rounded inlet and diverging outlet, as in Tables I. and II., under various pressures, increasing from 3 in. up to 12½ feet head of water, but into the uniform outside pressure of the atmosphere, with barometer at 30 inches.

1	2	3	4	5	6	7	8	9	10	11	12	13	14
Number of Experiment.	Head of Water in feet.	Time of Filling Bucket in seconds.	Experimental Velocity in feet per second.	Velocity due to Head in feet per second.	Velocities in Col. 4 divided by those in Col. 5.	Vacuum at throat of Nozzle in feet of water.	Head plus Vacuum in feet of water.	Velocity due to Head plus Vacuum.	Velocities in Col. 4 divided by those in Col. 9.	Fraction of Initial Pressure $h_1 + V$ restored.	Fraction of Initial Pressure $h_1 + V$ lost by Friction.	Frictional Loss in entering Nozzle.	Frictional Loss of Head $h + V$ in leaving Nozzle.
	h_1	t	v_1	v_2	$\frac{v_1}{v_2}$	V	$h_1 + V$	v_3	$\frac{v_1}{v_3}$	$\frac{V}{h_1 + V}$	$\frac{h_1}{h_1 + V}$	$1-\left(\frac{v_1}{v_3}\right)^2$	
21	·25	535	6·66	4·012	1·66	·52	·77	7·04	·946	·675	·325	·105	·220
22	·5	348	10·23	5·674	1·80	1·3	1·8	10·76	·951	·722	·278	·096	·182
23	·75	262	13·6	6·95	1·96	2·4	3·15	14·24	·955	·762	·238	·088	·150
24	1	218	16·34	8·025	2·04	3·5	4·5	17·02	·960	·778	·222	·078	·144
25	2	144	24·74	11·35	2·18	8·2	10·2	25·63	·965	·804	·196	·069	·127
26	3	111½	31·95	13·9	2·3	14	17	33·09	·966	·824	·176	·067	·109
27	4	94	37·9	16·05	2·36	19·8	23·8	38·84	·968	·832	·168	·063	·105
28	5	82	43·45	17·94	2·42	26	31	44·69	·972	·839	·161	·055	·106
29	6	74	48·14	19·66	2·45	31·1	37·1	48·88	·985	·839	·161	·035	·126
30	6·5	71½	49·83	20·66	2·44	33·5	40	50·75	·982	·838	·162	·036	·126
31	7	70	50·9	21·23	2·40	33·5	40·5	51·07	·997			·016	
32	8	69	51·67	22·76	2·31	33·5	41·5	51·7	·999			·003	
33	9	68	52·39	24·07	2·17	33·5	42·5	52·31	1·000			·000	
34	10	67	53·17	25·37	2·09	33·5	43·5	53·00	1·003				
35	11	66½	53·57	26·61	2·01	33·5	44·5	53·53	1·000				
36	12·5	65¼	54·6	28·37	1·92	33·5	46	54·43	1·003				

Table IV.—Flow of Water through orifice $\frac{1}{32 \cdot 41}$ square inch in area, with rounded entrance—(See Fig. 3)—under various pressures, rising from 1 foot up to 12½ feet head of water.

1	2	3	4	5	6
Number of Experiment.	Head of Water in Ft. on centre	Time of Filling Bucket in Seconds.	Experimental Velocity in Feet per Second.	Velocity due to Head.	Co-efficient of Discharge or Velocity.
	h_1	t	v_1	v_2	$\frac{v_1}{v_2}$
37	1	472	7·55	8·025	·941
38	2	332	10·73	11·35	·946
39	4	231½	15·39	16·05	·959
40	6	188½	18·9	19·66	·961
41	8	163	21·85	22·7	·963
42	10	145½	24·49	25·38	·965
43	12½	130	27·4	28·37	·966

TABLE V.—Recapitulation of Experimental Velocities, v_1 as entered in Col. 4 of Table III., with the view of being compared with the Velocities expressed by the Equation $v_1 = 16{\cdot}02\ (h_1 - h_2)^{\frac{1}{1\cdot61}}$

1	2	3	4
Number of Experiment.	Difference of Inside and Outside Pressure in Feet of Water.	Experimental Velocity.	Velocity by Formula.
	$h_1 - h_2$	v_1	$16{\cdot}02\ (h_1 - h_2)^{\frac{1}{1\cdot61}}$
21	·25	6·66	6·77
22	·5	10·23	10·42
23	·75	13·6	13·39
24	1	16·34	16·02
25	2	24·74	24·64
26	3	31·95	31·7
27	4	37·9	37·9
28	5	43·45	43·52
29	6	48·14	48·74

The PRESIDENT said they would agree with him that Mr Brownlee must have spent much time upon the subject he had brought under their notice. The experiments detailed in the paper had been the labour of many months; and while they, as an Institution, must feel very much indebted to him, they must all be very much benefited in many ways by these experiments. He thought it beyond doubt that science generally would be advanced through this set of experiments, the like of which had never before been published. If any of the members had any remarks to make on the paper, or any questions to ask, he would be pleased to hear them: he had no doubt Mr Brownlee would give any further explanations in his power.

Professor JAMES THOMSON said he felt great confidence that these experiments given by Mr Brownlee would be of very high value. They had been gone about evidently with much care, and very important precautions had been taken for insuring the utmost accuracy. He hoped to have an opportunity for examining into them carefully, when time permitted consideration; but at the present time he would not like to enter upon a discussion of such elaborate tables of results.

Mr R. D. NAPIER said if they would permit one invited to be present, though not a member of the Institution, to express an opinion on the paper, he would say that instead of an hour's consideration it was worthy of a month's thought. It was impossible for them to enter upon its discussion at present. He rose simply to put on record a formula, which he had never seen published, for the theoretical limit of vacuum produceable in the throat of a nozzle, nothing being allowed for friction. Mr Napier then wrote the formula on the black board, and promised to produce the proof of it at the next meeting. This formula makes the theoretical limit to the vacuum in the throat equal to $(n^2 - 1)$ times the head above the nozzle, where n is the number of times that the area of the mouth is larger than the cross section of the throat. Or, $(n^2 - 1)\ h = V$, in which h is the height of the liquid in a cistern above the nozzle, n is the number of times that the area of the mouth or outermost part of the nozzle is greater than the cross-sectional area of the throat, and V is the maximum limit to the vacuum in the throat, on the supposition of there being no friction. The vacuum represented by V is the height to which the vacuum in the throat would raise the effluent liquid.

Mr Brownlee said he believed Mr Napier's formula would be found to be correct, but the difficulty was to find out the amount of friction.

In answer to Mr Brown,

Mr Brownlee said that the diameter of the bore of the pipe leading from the water cistern to the nozzle was 2¼ inches, or about 128 times the area of the throat of the nozzle, and the area of the extreme end of the nozzle at the outlet was 16 times that of the throat. The water would therefore issue from the tube with $\frac{1}{16}$th of the momentum, or $\frac{1}{256}$th part of the *vis viva*, or energy, with which it passed the throat.

Mr Alex. Morton said some twelve years ago he had made many experiments such as those in the paper, and now and again since. He was glad to see that Mr Brownlee had followed up the subject, as he had no doubt that it would bring credit to himself as well as to this Institution. He was pleased to see that Mr Brownlee had produced a full vacuum with a head pressure of only 6½ feet of water; and he was glad to see it put down in figures, as it confirmed the statement he (Mr Morton) had given them on a former occasion, and more so, as this fact was questioned at the Philosophical Society's meeting referred to by Mr Brownlee. The arrangements for experimenting seemed excellent, and his water and mercury glass gauges were much preferable to any spring pressure gauge. There was a great deal in the proportion of the length of the diverging tube. He had tried many lengths, and he thought any practical formula would have to include the temperature of the liquid as well as the proportions of the tube; as also the density of the liquid passing through, such as oil or mercury, would require widely different proportions to give the best results. There was more in the diverging tube than they were yet aware of; but to discuss the paper from merely hearing it read would be useless, as he had no doubt it represented the work of a year, instead of a week or a month; and that they would satisfy themselves that night with merely voting him hearty thanks for the labour he had been at, and resume the discussion on another occasion.

Mr R. D. **Napier** explained that the formula he had drawn on the board was entirely theoretical, friction being excluded.

The **President** said they would now adjourn the discussion of the paper until it could be printed and in the hands of the members; and moved a vote of thanks to Mr Brownlee for his valuable paper. He trusted that Mr Napier would be present on that occasion, as it was a question he was well versed in.

The discussion on this paper was resumed on the 22nd February, 1876, when

Mr R. D. **Napier** read the following remarks, entitled, "On the Theory of the subject of Mr Brownlee's experiments :"—

I believe it is customary in the navy for a commander first to ask the opinion of his junior officers and afterwards that of his more experienced ones as to the state of affairs; and I suppose it must have been on some such principle that I, having little practical experience on the subject, was asked at your last meeting to open the discussion to-night of Mr Brownlee's most interesting paper.

The observations which I propose to make will relate less to the paper itself than to its subject generally—that is to say, I intend my remarks to be mainly auxiliary to Mr Brownlee's paper rather than a criticism of it; for the series of experiments he has described has been so exhaustive, has been so ably and carefully conducted, and the experiments themselves have been so lucidly described that it seems next to impossible to say anything but praise of the experiments, the experimentalist, or his paper. Yet it seems to me that there is a deficiency in the last, arising from the very fact of the author's great intimacy with the subject of his investigations. What I mean is, that he appears to have thought that most of us were much farther advanced in the matter than I imagine we are; and from that cause he has given us a paper which almost requires an introductory or supplementary part, giving a rudimentary account of the origin of the vacuum in the throat, and explaining how it is possible for it to assume such proportions as to be in some cases more than five times the pressure due to the primary head, I call it the primary head, to distinguish it from the total head which produces the velocity through the throat, and which is the primary head, plus the vacuum or negative pressure in the throat.

Now, what I propose chiefly to myself this evening is to supply as well as I can the rudimentary part that I have referred to of the subject we are about to discuss; and I trust that those who have given the matter so much attention as to understand most of its bearings will excuse me for taking up a little of their time while trying to explain the A B C work of the question, for I suppose that this society exists not only for the benefit of the advanced thinkers but also for those belonging to it who are anxious to learn.

When Mr Morton, at the Philosophical Society last winter, described the experiment referred to in Paragraph 5 of Mr Brownlee's paper—an experiment by which he said, speaking from memory, he had got nearly a perfect vacuum in the throat by the use of a primary head of about six feet of water—he remarked that he thought the action of diverging mouth-pieces was not well understood. With that remark I quite agreed; but at the same time I stated that theoretically an indefinitely small head should be capable of producing a perfect vacuum in the throat, and that therefore, in a theoretical point of view, the results he had obtained were not at all surprising, though they were, by a long way, the best I had ever heard of being actually got.

Some scientific persons at the same meeting, while asserting positively that there was no such thing as any theory tending towards the conclusions that I had arrived at, maintained at the same time that they perfectly understood the action of the diverging mouthpiece, and immediately afterwards indulged in remarks implying that Mr Morton must have been radically in error in his statements, and that it was quite impossible he could have got anything like the results he said he did. Now, it seemed to me then, as it does now, that a theory which is supported by experiments is far more likely to be right than one which is in direct opposition to them, and I am very glad that Mr Brownlee has now removed the question from the domain of speculation to that of ascertained facts. At the same time, it appears to me to be none the less important that we should as far as possible understand the theory, and not merely fancy we do so, because we happen to know some of the facts to which it relates, which is unfortunately a very common form of self-delusion.

Referring again to Mr. Brownlee's paper, it seems to assume without actually saying so, that the view that it is theoretically possible to produce a perfect vacuum by means of an indefinitely small primary head, has not only been adopted by himself but that it is a generally accepted view; instead of which I am of opinion that there are very few to whom it is not a matter of great surprise that it is possible to produce a vacuum in the throat of such proportions to the primary head, as Mr. Brownlee and Mr. Morton have shown us, it sometimes assumes, and that there are many who have no clear notion why there should be a vacuum at all there.

By the aid of a couple of diagrams, I hope to be able to explain first, how there is a vacuum at all produced in the throat, and next, how it is possible to assume the magnitude it sometimes does.

Fig. 1 (see Plate X.) represents an imaginary case in which A and B are supposed to be two rectangular tubes jointed together, both tubes being of equal width, but the depth of A being 1 unit and of B being 2 units.

Now I wish you to suppose that a succession of rectangular pieces D_3, D_4, D_5 &c., are being projected at a given velocity, through the tube A into B, and that these projectiles after entering B gradually alter their shape, becoming shorter and deeper till they assume the shape D_2 and ultimately that of D_1; then supposing that the spaces CC are left open, let us see what will take place.

The alteration of the shape of a projectile does not interfere with the velocity of its centre, and, therefore, on the supposition of friction being absent, the distance from centre to centre of the projectiles in the tube B would remain what it was before leaving the tube A—namely, the length of the projectile—provided that nothing interfered with their motion.

As shown in the diagram, D_4 is just entering the tube B, and D_1 has extended in depth so as to fill B from top to bottom, so that if both are, as by supposition, travelling at the same velocity, and the depth of D_1 is double that of D_4, we have the space between D_1 and the tube A being increased by D_1 at twice the rate that it is being decreased by D_4, and therefore that space is being increased at half the rate that D_1 is travelling, and therefore air must enter at the spaces CC; in fact, D_1 is acting as a piston and D_4 as a piston-rod half the cross sectioned area of the piston surface. Under these suppositions we should ultimately have the tube filled with alternate equal spaces of projectile and air. But let us now stop up the spaces C C, and then of necessity a vacuum will begin to be formed, which will tend to stop D_1, but at the same time to increase the velocity of D_4, and when D_4 comes to take the place of D_1 it will have more momentum, and consequently greater power to produce a vacuum, which again will react to produce greater velocity at the entrance of B, and so on.

I may be asked what this has to do with the question of water flowing. Well, it has this to do with it, that the attraction of cohesion performs the function I have asked you to suppose inherent in the imaginary projectiles—that is to say, the tendency which the particles of water have to adhere to each other, and to most surfaces they touch, drags the sides of the stream out to the periphery of an expanding mouthpiece, and the outer particles drag the next ones, and so on; but if I am asked what the attraction of cohesion is, I can only say I know no more about it than I do about what the friction of a fluid is, and that, I might almost say, is less than nothing.

I shall now endeavour to explain why we are entitled to say that, theoretically, a very small primary head should be able to produce a perfect vacuum.

Diagram 2 will assist us to understand this. Two cisterns, A and B, are connected by a tube, E F, which is parallel in the parts E and F, converges from C_5 to a parallel throat, C_1 D_1, and then diverges from D_1 to D_5, where it is the same size as at C_5.

The throat, C_1 D_1 in the diagram, is half the diameter, and therefore one-fourth of the cross-sectional area of the tube at E and F, The cistern A is supposed to be filled with water to the height H I, above the level K in the cistern B, when of course the water will flow from the cistern A to the cistern B, passing first through E at any velocity V, then increasing in velocity till it passes through the throat C_1 D_1 at velocity 4 V, then it begins to decrease in velocity till it passes through F at the original velocity V.

I have no doubt it will surprise some to learn that, theoretically, the contraction of the tube to a throat as shown should be no obstruction to the flow; that is to say that as much water should flow in a given time through a tube of the form shown as if a parallel tube having the diameter at E and F were used.

To show that this is so, will be the next step in my argument.

As already noticed, we are indebted to the principle known as the attraction of cohesion for expanding the stream in the diverging part, D_1 D_5; but for that the stream would rush through the middle of the diverging part, leaving the water in the outer parts of it stationary: on the other hand, the converging of the streams is effected by direct statical pressure. From these causes we are obliged to make the diverging piece much longer, to get the best results, than the converging part requires to be for the same object, for the attraction of cohesion is comparatively a very limited power; but for certain reasons both parts are shown exactly alike in the diagram, so that at the equal distances, C_1 and D_1, C_2 and D_2, &c., from G, the diameters are alike; from which it follows that the velocities are equal at C_1 and D_1, at C_2 and D_2, &c.

Now what is the reason that the velocity is greater at C_1 than at C_2, and at C_2 than at C_3, &c.? Some may say that clearly enough it is because it is smaller at C_1 than at C_2, &c., which in one sense is quite true; but we require the further reason that the pressure is greater at C_2 than at C_1, greater at C_3 than at C_2, &c., without which the increased velocity would be impossible. On the other hand, what causes the decrease in the speed from D_1 to D_2, D_3, &c.? Simply the smaller pressure at D_1 than at D_2, at D_2 than at D_3, &c.; or, putting it the other way, the greater pressure at D_2 than at D_1, at D_3 than at D_2, &c.

Again, the force and time required to stop a moving weight is exactly equal to the force and time required to give it its velocity, so that it follows that if the whole of each section of the stream in the diverging part of the tube is moving at a uniform velocity, then the vacuum produced at D_1, due to the slowing of the velocity from D_1 to D_2, will be equal to the amount that the pressure at C_1 is less than C_2, and so of all the other similar positions; whence we get the result that the reduction of the momentum in passing from D_1 to D_5 will produce a vacuum (that is, a lower pressure at D_1 than at D_5) which is exactly equal to the difference of pressure required between C_1 and C_5 to give the increased velocity at C_1 as compared with that at C_5; that is to say, the diminution of the pressure produced at D_1, by reducing the momentum of a given quantity passing D_1 to that of the same quantity passing D_5, will be exactly equal to the reduction of pressure required at C_1, as compared with that at C_5, to give the increase of velocity at C_1, as compared with that at C_5; from which it follows that the contraction in the tube, with proper shaped converging and diverging parts, will not theoretically reduce the amount of discharge; but, as already said, this is on the supposition that there is no friction, and that the necessary vacuum in the throat is not in excess of a perfect one.

Taking a similarly shaped tube to that in diagram 2, let the cross section of the throat be 1-nth of that of each of the two ends of the tube, and let h equal the primary head, and $v =$ the velocity due to h; then with a proper entrance to the part E of the tube, the velocity through the two ends of the tube will be v, and the velocity through the throat will be $n\,v$: but the velocity through the throat cannot be greater than what is due to the total head, that is to say, to the sum of the primary head plus the vacuum in the throat, and this total head will be equal to $n^2\,h$, from which, if we deduct the primary head h, we get the theoretical limit of the vacuum in the throat, equal to $(n^2-1)\,h$, which is the formula I gave at your last meeting, and promised to explain at this, but I had no idea of running into such a long story about it.

If the outlet part of the tube remains the same, it is evident that we shall get the best result by using the best form of inlet part, and we know that such an inlet part as is shown in diagram 2, which was only employed for the purpose of argument, is not the best form; we know, on the other hand, that a short conoidal frustum can be made which is practically perfect, and if that form of inlet is used we may make n in the formula represent the ratio of the section of the outer end to that of the throat, making $(n^2-1)\ h = v$, where n is equal to the area of the mouth divided by the cross-section of the throat.

To take an example. Let the cross-section of the throat be 1, and that of the mouth be 10, then the theoretical maximum limit to the vacuum in the throat will be 99 times the primary head; and as n may be indefinitely large, we may theoretically have the vacuum indefinitely greater than the primary head.

I had hoped to say something about the friction in the nozzle; but patience, like the attraction of cohesion, is a limited quantity and must not be too much trifled with.

One point I am very glad to see settled by Mr Brownlee's experiments, as it confirms a set of experiments made by myself on the flow of steam, and may be useful to know in other cases. The point I refer to is this, that there is no such thing as currents being induced, as is generally supposed, at right angles to each other; that is to say, if a current is flowing past a surface, and a hole is made in that surface, such, for instance, as a hole in a parallel pipe through which water is flowing, then a pressure gauge placed on that hole, with proper care to see that it is at exactly right angles to the current, will indicate exactly the pressure on the side of the surface or pipe. This is not generally supposed to be the case, but whatever experiments I have tried have gone towards the conclusion that it is so.

Without referring to any other experiment, let us look at Mr Brownlee's No. 8 and No. 3, as compared on page 9 of his paper. We find that he got the same discharge from 12½ feet into the air, with a converging nozzle, as he got from 12½ feet into 10·15 feet with the compound nozzle, showing neither pressure nor vacuum in the throat, or what I would prefer to call 10·15 feet vacuum in the throat. This shows that there was nothing of what is called an induced current due to the water flowing through the throat of the nozzle, and all Mr Brownlee's other experiments go towards corroborating this view.

One more remark I wish to make, and that is in reference to Mr Brownlee's 28th and 29th paragraphs, where he draws a comparison between the effect of long diverging outlets and long after lines in a ship.

I wish to point out that the two cases are not only not analogous, but in some respects diametrically opposed to each other. For instance, in the one case we want to get the largest vacuum possible, and in the other we want to get the least. But there is another difference, which I shall only indicate, but not stay to inquire into.

Mr Froude, of Torquay, read a paper a little while ago in London, of which I have only seen extracts; but he, while no doubt taking the same ground as I do as to the contraction of a pipe with suitable entrance and delivery parts not being theoretically a hindrance to the discharge, goes further, and says that theoretically the power required to drive a proper shaped vessel through the water would be nothing if there were no friction. Now this I entirely disagree with, and think I see pretty well where the fallacy lies; but it does not concern our discussions to-night further than as being referred to in Mr Brownlee's paper entitles us also to refer to it.

Mr ALEX. MORTON said as this was a paper on a subject he had long taken a special interest in, he had prepared some sketches (see Plate XI.) the better to illustrate his meaning in describing what he considered constituted a good diverging tube. In discussing the paper, he could not go into it very minutely, as he thought that Mr Brownlee had been rather hurried, having performed his experiments with one especial tube which he considered faulty. He would like to suggest to Mr. Brownlee that, seeing he had all the apparatus at hand, he might try a number of experiments with water heated by say a steam jet, up to 100 degrees and upwards, and that he might inform the Institution of the results which he found; for he was satisfied that Mr. Brownlee would find, as he himself had found, great differences of results from differences of temperature, more especially with little differences of levels. He had been for the last twelve years experimenting more or less with diverging tubes of different lengths and with water at different temperatures, but never had been able to come to any satisfactory results which he considered sufficiently valuable to give to the Institution. He would, however, show them what he thought was the best shape and proportions for a diverging tube to give the best results. In the paper referred to by Mr. Brownlee, which he (Mr Morton) had read before the Philosophical Society, he was not certain that he had been definite enough in describing the proportions at the throat of the diverging tube, although clearly shown on the accompanying drawing; but he would now take this opportunity of describing it particularly, so that there would be no mistake in making it act at little differences of the levels, such as Mr Brownlee had put in Table II., where, down to 2·35 feet difference of levels, he got no vacuum at all, as shown by putting down 0·0 in column 9. Now, with a well-made tube, so soon as water passes through the throat, some vacuum should be produced, even with a few inches difference in levels, and in every case a comparative greater vacuum with little difference of levels: hence his opinion that the experiments have been performed with a faulty tube. It is very difficult to bore a hole through a deep flange on the side of any tube that would meet exactly below the throat. In this apparatus the hole for the vacuum tube, in his opinion, was a little *above* the actual throat instead of being *below* it, and it was almost impossible on such a small scale to measure this, and therefore he advised making the tube in two parts, if to measure the vacuum be the object of the experiment. To measure the flow of water the tube should be in one piece from beginning to end, without a speck or flaw on any part of the interior curve; then the water will find the exact throat itself. He had prepared a large-sized sketch from Mr. Brownlee's actual tube; and he would try and show them how he would alter it. He would bore out the mouth of the tube, and screw in a separate converging nozzle, as shown at *a*. He would make this nozzle so that a truly turned mandrill would just stick in the nozzle at *a*, and drop freely through the tube at *b*, which is sufficient difference if the tube widens continually toward the mouth; but in no case must it ever be the reverse. By making it of two parts you can examine the throat carefully that no rag exists, and you have the means of adjusting the throat as closely as to, say, the thickness of a piece of paper; in fact, it could be actually closed, and yet not be perfectly air-tight. When the apparatus is so crude, as in Mr Brownlee's experiments, as to have the vacuum tube acting all on one side of the stream, it is apt to draw the water back on the top of the mercury. He had not been able to come to any conclusion as to the *cause* of this diverging stream producing a vacuum, or to give a set of tables that he had any faith in, for the simple reason that no two tubes would give the same results, and even a slight difference in temperature would make a variation. He found also that the distance apart at the throat made a variation; for instance, the tube communicating with the vacuum gauge, such as Mr Brownlee had used, if of double the diameter, would make a great change in the effects produced. With the tube he had described, they would observe that the distance at the throat can be any thing they please, from "0" upwards. He preferred this form of tube in comparing the differences of velocity, because he used the same converging nozzle whether he had the diverging tube or not. But if two nozzles were used, then the question of measurement came in, which was very difficult to get mathematically correct on such a small scale as that. He found the best results when the diverging tube was from 10 to 12 times the diameter of the throat in length, and from 10 to 12 times the area of the throat at the outlet end. He had never been able to discover the motion of the water in the diverging tube. It was very strange that it was able to produce a full vacuum with only a very few feet of differences of levels. Give it but a slight bend and the whole action is gone, the water taking the round side of the bend, and the air acting all the way back right up to the throat as shewn in the sketch, so that by bending it destroyed the action altogether. He had made a cross of very thin sheet brass, and fitted it to the inside of the tube, to ascertain if the stream revolved on its own axis; but when he put a similar cross at the entrance side so as to give the stream a straightforward motion, he found that this cross fixed in the diverging tube had no effect whatever in diminishing the vacuum. He had again fixed the cross free to revolve on a centre, to ascertain if the water revolved on its own axis, and he found that it did not of necessity require to revolve on its own axis. He had tried many a scheme to ascertain this motion, but had hitherto been beat. He considered the motion to be somewhat like the exhaust steam coming from a steam hammer, turning outside in all the way. On referring again to the paper now under discussion, he found Mr. Brownlee had measured the *inside* pressure from the throat, and to this he had added the vacuum he found there, and by the well-known rule of gravity got what he has called the theoretical velocity; but this is adding theory to practice. If we take the inside pressure plus the vacuum, we have then nothing whatever to do with the outside pressure, whether level with the throat, or some feet above it; because we have taken the vacuum found for the measure of the outside pressure. In calculating the velocity he differed from Mr Brownlee, as he took the head down to the wide lower end of the diverging tube when vertical, as in his opinion it acted as a syphon, and the results he got compared very well with the actual velocities observed, instead of as Mr Brownlee had got it, more than possible, he found it always less. When the tube was well made, as he had pointed out, he found that small difference of levels produced a partial vacuum in a certain ratio, and that wherever the tube was placed, either above, below, or between, certain differences of levels always gave practically the same results. He thought if Mr Brownlee would alter the tube as he had pointed out, that he would find better results to follow. There was a remark in the paper which he thought deserved attention; that was in regard to the form of vessels, whether the curve should be as Mr Brownlee had suggested or not. He had here turned the tube outside in and placed the two curves together, to illustrate his meaning, and they would find how near it resembled the form of a swift fish; but some of the shipbuilders present would be able to say whether this was likely to be a good idea or not.

Professor JAMES THOMSON said—With reference to Mr Napier's explanations respecting the flow of water in the pipe with the narrow throat and subsequent gradually widening duct, he thought—if he understood rightly—that Mr Napier had said, and was quite right in saying, that the water theoretically, that is, theoretically upon the supposition of its being a perfect fluid, having no friction, would flow through the pipe from the one cistern to the other in the diagram as if there were no contraction at the middle, and as if the pipe were of the same width throughout. Some statements had been made as to the reason why there is a diminution of pressure, or a suction produced at the narrow throat, and Mr Morton seemed to think that we know nothing about the reason of it. He did not agree with that opinion. A great deal is very well known about it. We do not know the whole of the influences which depend upon fluid friction; but the reason why there is this suction produced, Mr Napier had shown pretty distinctly in explaining nearly to the effect that the water, having acquired a high velocity in shooting through the narrow throat, tends, while passing forward through the gradually widening duct, to retain its rapid motion at each instant; but that, while filling the cross-section of the widening duct with its forward flowing stream, as it actually does, it would have to leave vacuous spaces behind—or, rather, spaces filled only with gaseous water-substance, exerting almost no pressure—if it were to retain its high velocity; and that, in the cases of unbroken flow under discussion, instead of the pressure being so much diminished behind as to allow of the breaking up of the stream with the formation of such vacuous spaces, there is actually produced a greatly diminished pressure behind, at, and near the throat, comparatively to that in front at the wide end of the divergent duct.* If we suppose the

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* The statement in last clause here as to the change of pressure which occurs in any particle of the water during its passage from the throat to the wide end of the divergent duct may be more clearly put forward—and put forward

water to be spouting from the outlet F in Mr Napier's diagram into the vessel B, and carrying away the velocity that it had in leaving that outlet, theoretically the velocity it should have at the outlet F into the lower cistern is just that due to the fall from the free-level in the upper cistern to the free-level in the lower one. That height of fall from the one free-level to the other is often spoken of as the "head"—a name, however, which is subject to much ambiguity of signification, and deficiency of explicitness among many of its common applications. The free-level for each cistern, and for any water in statical communication with the water of that cistern, is the level at which there is, or there can be, a level surface of the continuous statical water, freely exposed to the atmosphere. Now, if the water were flowing through this pipe, it should be expected, on the theoretical supposition already stated, to leave the outlet into the lower cistern with the velocity due to the fall of free level, from H to K, and then it must have a much higher velocity at the contracted part. The velocity there at the throat must be to the velocity at the wide outlet, and in the inverse ratio of the area at the throat to the area at the outlet. Now, as to something Mr Napier had said about the "attraction of cohesion," he must assert that it was not to any influence of cohesion that they had to refer the spreading out of the water in the widening pipe. It had been stated in old writings on Hydraulics, by Venturi and others, that there is what is called a "lateral communication of motion in fluids," which, indeed, may be regarded as just another name for the friction of fluids. If at any moment there were a jet of rapidly flowing water shooting from the throat along the centre of the divergent duct, with approximately statical or retrogressive water surrounding

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in a form applicable also to cases when the flow is not along a level divergent duct, but vertically downwards, or upwards, or in any other direction—by saying that for a particle flowing from the upper cistern, through the throat, and forward through the widening duct, there occurs, during the flow in the convergent nozzle to the throat, a remarkable fall of free-level (or of level of the atmospheric end of the pressure column for the particle), and during the flow forward in the divergent duct there is again a remarkable rise of free-level up to that of the statical water in the lower cistern.—J. T it, between it and the interior face of the duct, it would, by the lateral communication of motion, very quickly scour out that surrounding water, and then the forward flowing stream would occupy the whole cross-section of the divergent duct. Now, the "attraction of cohesion" would be that which would account for a well known fact—that water specially cleared of the presence of air, or treated with other precautions for getting rid of causes facilitating the formation of cavities which may be called vacuous, or more strictly may be spoken of as occupied only with gaseous water substance, could be made to exist in a condition of having less than no pressure, or of exerting pull in all directions instead of push—that is, in what may be called a condition of being subject to, or of exerting, a negative pressure. If in a water barometer, when the atmosphere has a certain pressure, the water will stand with its upper surface 34 feet above the free level in the open cup at bottom, and if in another barometric tube of the same kind, filled to the very top, without any vacuous cavity whatever, the water will continue occupying the tube to the top, situated at a level more than 34 feet above the free level in the open cup or cistern at bottom, the fact of the contiguous parts of the water, or of the water and glass, holding together, and not separating so as to leave vacuous cavities, may properly be attributed to what may be called "*attraction of cohesion.*" He thought, on the other hand, that the widening of the stream of water in flowing from the throat along the divergent duct was not to be attributed to attraction of cohesion; but that, in fact, in hydraulic experiments on the flow of water through a divergent duct a negative pressure, maintained by attraction of cohesion, is not ordinarily produced at all. He thought that in fact in such experiments as those of Mr Brownlee, when the flow of the water would be such as would produce a negative pressure in the case of the water holding together, and not opening to form cavities, it actually does break open and forms cavities occupied by gaseous water substance. But it is to be noticed that in the cases chiefly under discussion there is neither a negative pressure at the throat, nor a breaking up of the water to form cavities; but there is a positive pressure at the throat, even though that, in some cases, is less than the atmospheric pressure, and so the attraction of cohesion is not acting for keeping the water together and in contact with the internal face of the duct. As to the manner in which the water, flowing forward from the throat through the divergent duct, is made to diverge and not to shoot as a jet through the centre of the duct with comparatively statical water (or in some cases with air) around it, he had already explained that if there was water around a central flowing jet this water would quickly be removed by the lateral communication of motion in fluids. Also, if air was present around this central jet, it might or might or might not under different circumstances be scoured out as bubbles. When, however, the full bore flow is once established, the divergence of the stream lines, which Mr Napier had attributed to attraction of cohesion, he (Prof. Thomson) would attribute simply to a very slight excess of pressure along the central part of the stream, as compared with the pressure in the stream lines at or near the circumference. This excess of pressure diverts those outer stream lines from the straight paths which they would otherwise tend to assume; and the hydraulic principles already adverted to amply show how it is that, in a very gradually widening tube occupied with water, no stagnant nor retrograding water will be allowed to remain around a centrally flowing jet. The origin of the slight abatement of pressure at the outer parts as compared with the central part of the stream, necessary for causing the divergence of the outer stream lines, is likewise not difficult to be understood. Professor Thomson gave some further explanations on this subject by reference to diagrams, and explained how, with a jet pump discharging into the air, it may happen that the water may flow in the divergent duct either full bore or not full bore according to the manner in which it has been set into action, and that a slight temporary change applied may sometimes alter the flow from either way to the other when the discharging end is not immersed in water. He thought Mr Brownlee's experiments had yielded very carefully observed and clearly recorded facts, and were likely to be very valuable in many ways.

Mr R. D. Napier said—With regard to the attraction of cohesion he understood that when Mr Morton tried pipes that were greased, he found that no vacuum was formed which showed that the attraction of cohesion had the whole to do with the action.

Mr **Alex. Morton** said these explanations of the action of the water in the trumpet mouth seemed to point to the same difficulty as he had felt—the one speaker called it the attraction of cohesion, the other lateral action—but the question was, Why did this action not take place when the tube was bent as well as when it was straight? If the attraction of cohesion was so powerful as to be able to make a perfect vacuum at the throat, why did it fail with a single slight bend? As to the sketch which Professor Thomson had made on the black board, of his jet pump, the inaction which he had just described was much more simple and easily explained. For instance, in this case the throat was considerably wider than the nozzle itself. The nozzle might be only an inch, whereas the throat must be considerably larger to pump any water at all—say 1½ or 2 inches, and it was easy to see how water from an inch nozzle could spout through a throat an inch and half or two inches diameter without touching it. This was the form that was adopted by all the old experimentalists; but he found, in order to get the best vacuum, that instead of the straight cone, the inner curve of the tube had to be a parabola—widening very slowly at first, and increasing as it went on. He thought he had been the first to adopt that particular form, and by adopting it, and adjusting the proportions as to relative sizes, he had been led to the good results and high vacuum that he had got from a very low head.

Professor **Thomson**—in reference to the case brought forward by Mr Morton, of the failure of the bent divergent duct to produce a vacuum at the throat, when a straight one would succeed—thought there need be no puzzle nor any mystery in the matter. Mr Morton's own diagram showed the reason clearly enough, exhibiting as it did the water flowing along one side of the pipe—that is to say, along the outer side of the bend, and being made to flow along that side in virtue of its own centrifugal force—while the side of the pipe next the centre of the bend was left without any of the stream flowing along it, and was left to contain either air or comparatively statical or eddying water. The case was somewhat like that of a slanting pipe with water flowing down along the lower side of it, with air above this stream in the pipe. The water, in such a case, will scarcely scour out the air; and even if at any part air were to get mixed as foam in this stream, it would be apt soon to rise to the surface as bubbles, and to escape out of the water. But a vertical pipe with a stream, or a shower of water descending through it, affords much more favourable conditions for the carrying forward of air by the water, either in bubbles in a stream of water, or by drops of water falling as a heavy shower through the air. From such considerations, he thought there was obvious reason to be seen for the failure of the bent divergent duct to produce a vacuum when a straight one would succeed.

Mr NAPIER inquired why Mr. Brownlee had to make such a long pipe, and said it was because the attraction of cohesion was not stronger.

Mr A. MORTON said he was not satisfied with the explanation given, and he never was able to come to any satisfactory conclusion on the matter. The centrifugal force from only a foot head of water could not be great; therefore the attraction of cohesion must be very small; but, again, it was able to produce a full vacuum with only 6 feet head of water—certainly very accommodating. He had no doubt that the attraction of cohesion had something to do with the matter, and would admit that it acted as the piston rings to make the fluid piston air-tight, but that it was the sum total of the force he did not believe.

Mr NAPIER said that but for the attraction of cohesion there would be no result.

Mr EBEN. KEMP said that supposing the water with which the experiments had been made was at 50 degrees of heat, then if its temperature were increased to 60 degrees, he understood Mr. Morton to say that so small a difference as 10 degrees would make the action quite different.

Mr A. MORTON could not tell to what extent the action would be different. When the temperature was increased to 100 degrees it made a great difference, and if Mr Brownlee could give them the comparative results at different temperatures it would be of great consequence. He might mention for the benefit of some of the younger members why the vacuum as given in the tables remained constant beyond a certain point, simply because water at 60 degrees boiled under a less vacuum than given. It could not therefore increase.

Professor **Thomson** remarked that Mr Morton had said that he found grease on the inside of a pipe spoiled its action altogether. He would like to know whether he had found that to be the case when the discharge end of the pipe was immersed in a cistern of water, or only when the pipe was discharging into the air or any other gaseous substance. Referring to Mr Morton's drawing exhibited at the meeting, he said—Suppose there was a cistern of water down at this discharge mouth, would a greasy inside destroy its action. He would think not—that is, supposing that the grease was not roughly applied, but applied and rubbed away so as to leave a finely polished unctuous surface.

Mr **Morton** said that the pipe was polished and oiled, and when the end was out of water it spouted out to a distance of many yards, and he did not detect any difference when in water.

Professor **Thomson** thought that a jet pump when the discharge end was immersed in water, would not fail in consequence of its interior being greasy, and if Mr Morton found this not to be the case, it would be an interesting fact to make known and to examine into; but he was not quite sure whether in Mr Morton's experiments air was present. He thought Mr Morton was right in saying that the attraction of cohesion had something to do with it; for it had a great deal to do with forming bubbles and getting air to mix with the water; and if air were present it was extremely likely that the greasy interior would have the effect which Mr Morton said; but if no air was present he did not think the action would be damaged by a greasy condition of the polished interior.

Mr **Napier** said that if the grease had an effect when discharging into the air it would also have an effect when discharging into the water. The stream would rush through if it were not affected by the attraction of cohesion.

Mr COLEMAN thought that common sense would teach us that amongst others two causes contribute to the form of the issuing jet: viz., 1st, the adhesive attraction of the water to the interior surface of the trumpet-shaped orifice in cases where the metal is not greased; and, 2ndly, the lateral action alluded to by Professor Thomson (substituted by centrifugal action in the case of a bent tube). It appeared to him that from general experience a surface adhesion exists between a metal and water, modified by the condition of surface of the metal, and attributed by some writers to a force called "attraction of adhesion," to distinguish it from the force which regulates the liquid or solid condition of the homogeneous masses, usually described as "attraction of cohesion."

Mr NAPIER was quite satisfied to accept the phrase "attraction of adhesion" in place of "cohesion."

Professor THOMSON remarked that Mr Napier said that if the interior of the pipe was greasy even when the pipe discharges into water, the jet of water would rush through the tube without diverging, and without scouring out the surrounding water. It was to be recollected, however, that there was no film of grease between the two waters—that is to say, between the jet and the surrounding water, and the grease upon the interior surface of the pipe would not prevent the spouting water from entangling and dragging forward the surrounding water, and then there must be an eddy to keep up the supply as long as the jet does not diverge to fill the tube, and then, obviously, there is less pressure in this comparatively dead but eddying water.

Mr NAPIER could not see how there was greater pressure in the centre than in the sides of the jet.

Mr D. JOHNSTONE asked whether the grease affected the flow of water from the exhaust of an engine or the vacuum in a Morton's condenser, and if greasy water was prejudicial to its effective working.

Mr Morton answered that the grease from an engine was so burned before it reached the "Ejector Condenser," that no bad effects followed; if any, they must be momentary, as the friction of the water rushing through at a high velocity very shortly cleansed a well-greased tube.

Mr Kemp said Mr Napier had stated that the flow in the throat would not be altered if contracted to any extent. Suppose that it were made one-fourth, would it remain the same?

Mr Napier said that provided the pressure of air was sufficient, or as Mr Brownlee had put it, if both heads were raised together, so as to enable us to get a sufficient vacuum in the throat, or if the barometer indicated 60 or 90 instead of 30 inches.

Mr Brownlee said that as he concurred in much that had been said, there was little that called for any reply. While agreeing with Mr Napier's formula in the sense and within the limits to which it is intended to apply, it may be observed that by actual experiment 6·5 feet of head was required to produce a vacuum of 29·6 inches of mercury, or 33·5 feet of water; whereas by his formula the same vacuum should have been produced, excluding friction, with a head of

$$33{\cdot}5 \div (16^2 - 1) = {\cdot}131 \text{ feet,}$$

or a little over 1½ inch of head. As some misapprehension appeared to have arisen on the meaning which he had attached to the word "vacuum," he need only in explanation take Experiment No. 8, to which experiment Mr Napier and Mr Morton had both referred. In this experiment it is stated that the inside pressure, or head of water, was 12½ feet above the throat of the nozzle, while the outside pressure, or head, was 10·15 feet above the throat, and the pressure *at* the throat was neither above nor below the pressure of the atmosphere. The vacuum is therefore here entered as 0·0. Now, while Mr Morton, in referring to this experiment, thinks there must be some mistake in not finding any vacuum at the throat with a free fall or difference of level of 12.5 — 10·15 = 2·35 feet, Mr Napier says he would "prefer to call this 10·15 feet of vacuum," signifying that the pressure at the throat was 10·15 feet less than the outside pressure, which would have been quite correct if so stated; and there is no doubt the vacuum, or in other words the pressure at the throat would have been 10·15 ft. less than the pressure of the atmosphere, provided the outside and inside pressures had both been lowered 10·15 feet. In that case the outside water would stand on a level with the throat of the nozzle, while the inside water would stand on a level 2·35 feet above the throat, and the pressure *at* the throat would then be 10·15 feet below the atmospheric pressure. On referring to Experiment No. 25, in which the outside water was on the same level as the throat of the nozzle, and the inside water 2 feet higher, the vacuum or pressure at the throat was then 8·2 feet less than the atmospheric pressure. Mr Morton has said that he thought it very desirable that experiments should be made with water heated up to 100 degs. Fah. He (Mr Brownlee) might therefore say that he had made many very careful experiments with water at a temperature of 80 degs., and found a decrease of vacuum, or, in other words, the absolute pressure at the throat corresponded very nearly with that of the vapour of water at the given temperature. The pressure of the vapour of water at 60 degrees is fully half an inch of mercury, while at 80 degrees it is fully one inch, and the lowest absolute pressures at the top of the vacuum tube was never found to be less than 4-10ths and ⅞ths inch of pressures at the above respective temperatures; or, as may be otherwise stated, the mercury in the vacuum tube rose to a height of 29·6 inches (when the barometer stood at 30 inches), with water at 60 degrees, but never ascended to a greater height than 29⅛ inches with water passing through the tube at a temperature of 80 degrees. If steam or air entered the same tube at a pressure of 1 to 3 lbs. above the atmosphere, and was discharged from the outlet into the atmosphere, it is not only probable but almost certain that the pressure at the throat would be less than that of the external atmosphere, or, in other words, a partial vacuum would be found at the throat; and so also with water at a temperature of 212 degrees, a partial vacuum would also be found at the throat, provided the water passed through the tube with sufficient velocity.

Mr **Morton** said he found in every case that difference of levels, whether above or below the diverging tube, always gave practically the same results: if they added the inside head to the vacuum, they had nothing whatever to do with the outside pressure. There is a ratio which in every case would give the exact vacuum; but the velocity of the jet at the throat, when they added the vacuum to the head, had nothing to do with the outside pressure.

The **President** then proposed a hearty vote of thanks to Mr Napier for his addendum to Mr Brownlee's very valuable paper.

Note received from Mr Brownlee, 14th March, 1876.

I have thought it desirable to mention that the experiments brought before the Institution were not *commenced* with any other view than that of simply proving that water would flow through a properly formed tube from 100 lbs. into 80 lbs. absolute pressure, with about the same velocity as it would into the atmosphere, or even into a vacuous space. I am not aware of this fact having ever before been stated, and until I had made the experiments, and found the results to go even rather beyond those for which I had before contended, my adversaries insisted that such views were not only improbable, but opposed to nature and reason.

EXCERPT MINUTE OF COUNCIL MEETING OF 22ND FEBRUARY, 1876.

On the motion of the PRESIDENT, seconded by Mr JOHN Z. KAY, it was unanimously resolved that there be recorded in the minutes the expression of the Council's deep sense of the loss sustained by the Institution through the death of Mr Benjamin Conner, who, from the commencement of the Institution, of which he was one of the original members, had always shown the warmest interest in its prosperity.

Mr Conner had frequently held office in the Council of the Institution, and was one of the Councillors holding office during the first Session of the Institution.

The Council feel more deeply this loss sustained by the Institution, as they would now lose the support and encouragement which Mr Conner had given in furthering the efficient working of the Graduate Section—the Course of Lectures now being delivered to that Section having been largely promoted by Mr Conner.

The Council trust that the copy of this minute, which will be transmitted to Mr Conner's relatives, will be received by them as being an expression of the Council's heartfelt sympathy with them in their trying bereavement, as well as in testimony of the high esteem in which Mr Conner was held by the members of the Institution.

On a Proposed Cellular Ship of War.

By Mr H. J. BOOLDS.

(SEE PLATES XII. AND XIII.)

Received 15th, and read 22nd February, 1876.

ELEVEN years ago, in February, 1865, I read a paper before the Scottish Shipbuilders' Association on "Cellular Ships of War," in which I advocated the subdivision of such ships into a greater number of water-tight compartments or "cells" than was then usual, or, so far as I was aware, had ever been contemplated in naval construcion in this or any other country.

My object was to make the ships of our navy unsinkable either by shot, ram, or torpedo, and considering the excessively expensive character of a modern war ship, and what I may call the excessive facility with which ships could be sunk by either means, I believed I was doing the country some service by offering to our naval constructors a cheap and simple solution of the difficulty of keeping an iron-clad afloat and efficient in spite of all the accidents of naval warfare.

Such a vessel I will now attempt to describe.

In order to be more explicit, I have assumed dimensions as represented in the diagram. The vessel there represented is 320 feet long, 45 feet broad, and 25 feet deep in hold to upper deck. The builder's tonnage is about 3160; the register tonnage and weight of the vessel about 2160; the displacement to 19 feet, about 4400 tons; with engines of 500 nominal horse-power, indicating 3000. I calculate that a mean speed of 15 knots could be easily maintained. I propose to use two screws, one on each quarter, for greater facility in manœuvring.

The hull of the vessel is a combination of iron and wood. The frames are of iron, spaced 18 inches apart in the centre, and 2 feet the event of battle; her hull unsinkable and uninflammable; stronger than any iron ship of war now afloat, with less material, the whole of the inner skin and bulkheads being constructed of the material saved on the thickness of the outer skin, the keelsons, stringers, and beams, as already stated, and the additional sheer and bilge strakes, and diagonals now required for ordinary composite ships.

The two ends of the ship are formed as nearly as possible alike. The sides above water are made thin to permit shot or shell to pass through as easily as possible—the easier the better. And as I have great faith in guns, the armament would be of the heaviest practicable calibre on the upper deck in the centre line, in cupolas or open, as might be considered advisable, the bulwarks to fold down in board in line of fire, the masts of iron, light polacca rig, as, being fitted with double screws, sails would be of comparatively little consequence.

The boilers and machinery for each screw would be separated at the centre line by a double armour bulkhead, and transversely by bulkheads through which the pipes and passages could be made water-tight if required, using one, two, or three sets of boilers, according to the power required for the occasion: the coal bunkers being placed where practicable, and also made water-tight with the iron skin and deck if necessary.

Notwithstanding the important changes that have taken place since then in the power and size of guns, thickness of armour, and the science, if not the practice, of torpedo warfare, no attempt has been made, either on my plan or any other, to give our ships of war greater safeguards from sinking; and the internal hold arrangements of the war ships of this country are still—like merchant ships, which are intended only for facility in stowing and discharging cargo—constructed in compartments large enough singly to sink any ship, in the event of perforation from any cause. This has long appeared to me an anomalous state of matters for which there is no accounting.

As a matter of calculation and fact the whole business of a war ship is to keep herself afloat while she sinks or otherwise destroys her antagonist; but while the science of naval warefare has been directed to destruction by every means, and protection only from shot and shell, nothing whatever has been done for protection from the ram or torpedo. The power of the gun has been so much increased that the thickest armour that can be made and carried afloat is comparatively valueless for protection from shot, and worse than valueless for protection from sinking.

If, therefore, a ship, even without armour, could carry her guns and fight without risk of being sunk, she would be a much more formidable antagonist than any ship of war now in existence. There is no reason that I am aware of why all our ships of war should not be of that character. All are now built chiefly of iron and propelled by steam; their stores of every description are calculated in proportion to their armament and steam power; and the spaces in the hold are allotted and partitioned in some irregular way for the respective details of the equipment.

Now, with the exception of the machinery, which necessarily occupies large spaces, and perhaps of coals, which may require to be frequently taken in and consumed in large quantities, there is no part of the stores of a ship of war that may not be stowed in the smallest separate compartments into which I propose to divide such ships without being in any way inconvenient to get at in emergency; even the machinery may be so divided that it may be in several water-tight compartments, instead of as at present usually all in one, and there is no particular reason why coals may not be stowed in smaller separate compartments than customary, which may be emptied in detail, and closed water-tight when empty.

The recent sinking of the "Vanguard" by the ram of the "Iron Duke" has induced me to call public attention again to the system of cellular subdivision, which I believe to be the only adequate safeguard against underwater damage.

All iron ships have some bulkheads, usually limited in sailing ships to one collision bulkhead at the bow; screw steamers have, in addition, one or two whole or partial bulkheads at the stern to protect the body of the ship from danger through accidents to the screw, shaft, or pipe; and two bulkheads generally enclosing the machinery, boilers, and coals. These are all that are considered absolutely necessary between the two skins to that space, with one watertight opening in the inner skin for access to the frame spaces for cleaning and painting.

The engine-room—from the design as a twin screw steamer—occupies one 24 feet space each side of centre line, the next space before that being four coal cells each 12 feet square, then a 24 feet boiler space, then four coal cells, then 24 feet boiler space, and then 12 feet cells from that to the bow, those next the forward boilers being used for coals; each cell will hold 50 tons of coals, and each group of four will hold 200 tons. Three groups on each side will hold 600 tons, or 1200 tons of coal in all, without very little more inconvenience in trimming than ordinary bunker arrangements for such ships.

The after end of the ship would be subdivided in the same way, the cells through which the screw shafts pass being made tight around the shafts by stuffing boxes; the lower part of the cells through which the shafts pass can also be floored over the shaft, with access from the cells. All the cellular divisions are large enough for store-rooms for all purposes, and, being watertight, will serve as tanks for water, and can be filled in groups wherever required for that purpose.

The whole of the bulkhead cellular divisions in such a ship as the "Vanguard" would not exceed 500 tons weight, and as the weight of armour above necessitates as large an amount of ballast in such ships, the whole of this ballast would be saved, and the strength and safety of the ship would be immensely increased, without adding anything to the draft of water or displacement.

The point A on the plan is assumed to be the place where the "Vanguard" was struck by the ram of the "Iron Duke," as the worst possible place on which she could be struck to do the greatest injury, upon or so near a principal bulkhead that the space before and abaft would fill with water.

As a Cellular Ship, according to the drawing, the water would be confined to the one engine-room and one or two cells adjoining; but even assuming that all the bulkheads of the adjoining cells were so much started as to make them leak and fill, the whole of the water in 48 feet of length, half the breadth, and the height of the orlop deck, would not exceed 650 tons, and would not sink her more than two feet deeper in the water; while, with one engine and all her boilers intact, she would be perfectly safe to go anywhere for repair, and even had the "Iron Duke" been an enemy, would have been able to continue the contest without immediate fear for safety or victory.

500 tons of iron judiciously expended as I have shown, on each of the ships of our present Navy, at an expense of £20,000 each, would put twenty-five of them beyond the risk of such summary accidents either in peace or war, at a cost not exceeding the price of the "Vanguard," which, for want of this small addition, now lies at the bottom of the Irish Channel.

Without having any interest whatever in pressing this matter upon the attention of the Government and the people of this country, beyond a desire which I only share, in common with every British subject, that the British Navy should maintain its ancient supremacy, I would most earnestly recommend the immediate adaptation to our present iron-clad fleet of the "Cellular System"—either as here illustrated, or in such modification as our naval constructors may consider advisable—as the only system upon which any approach to safety or protection can be obtained against the underwater dangers of the Ram or Torpedo, the use of which are now inevitable in future warfare.

The President said that at that late hour of the evening, it would only be consulting the convenience of the members present for him to propose that the discussion of Mr Boolds' paper should be postponed until their next meeting, when they would have the paper in their hands. Meantime, he would propose a vote of thanks to Mr Boolds for his interesting paper.

The discussion on this paper was resumed on 21st March, 1876.

The President said that he did not think that any question could arise as to the advantages to be derived from cutting up a ship of satisfied that such a course would lead to good results. He was of opinion that the Admiralty should adopt Mr Boold's excellent plan of construction.

Mr W. R. M. **Thomson** thought there would be considerable difficulty in ventilating such a vessel as Mr Boolds proposed. Some of the present vessels, as reported by navy officers, even with their large sectional compartments right across the ship, were found very deficient in this respect, which he looked on as a serious matter, where so many men were crowded together, and had to live, work, and fight in all kinds of weather and climates; and he would like to know how Mr Boolds proposed to effect this proper ventilation in navy ships with such small and numerous well-like compartments. Safety from sinking in battle is not everything in a war-ship; and one thing is certain, our present mode and appliances for the purpose of ventilation would not answer in these proposed new ships.

Mr **Boolds**, in reply, stated it was quite possible to ventilate his ship as easily as one of any other construction. The original paper explained how every part of the vessel could be ventilated by pipes from the engine; but ventilation, though very important, was not the most important point in a war vessel—his principal object had been to prevent sinking. The crew were supposed to live in the 'tween decks—on the second deck between the main and the lower decks, and the water would require to come up to the main deck before the vessel could sink. That was the great point in the cellular system. In this vessel he proposed to have no openings in the bulkheads under the orlop deck, because if doors were fitted there is the liability of their being left open, and so the water would be allowed to rush into the vessel.

The **President** hoped that the effect of Mr Boolds again bringing the cellular system before the Institution would be to attract the attention of the Government, seeing it was backed up by the unhappy experience of the loss of the "Vanguard," and that something would now be done to make our navy unsinkable.

Propositions on the Motion of Steam Vessels.

By Mr ROBERT MANSEL.

(SEE PLATES XIV., XV., AND XVI.)

Read 21st March, 1876.

INTRODUCTORY.

I could not introduce the subject of Motion of Steam Vessels by a more appropriate remark than the one made by me to one branch of this Institution fifteen years ago. "Most people who have had to do with steam ship propulsion, at some time or another, have been painfully reminded of the final deduction of this and other branches of human knowledge—namely, 'how very little we do know;' and although the mode of advancing our knowledge may give rise to the greatest diversity of opinion, still, the slightest step in advance is worthy of our best efforts; and if made in the guarded, conscientious, and unselfish spirit which ought to characterise any scientific inquiry, may not fail, in the end, to elicit some important and useful truths.

"There are two ways of approaching the subject of steam-ship propulsion. The fluid resistance to the motion of a wedge-ended solid has been viewed on mathematical principles as a fraction of the resistance which would be experienced by a plane of the same area as the mid area of the solid; this fraction varying according to some deduced law dependent on the form of the ends. To this again, in some cases, has been added the direct drag due to the frictional resistance upon the surface; but, in most cases, this latter element has been supposed involved in the former. Such formulas labour under the disadvantage of failing to notice elements to which there are strong reasons for assigning a very important part. For example, the nature of the motions produced in the water, the manner in which they are affected by the actual dimensions of the vessel, and the relation between size and fineness for an assumed speed.

"The second or mechanical mode treats these motions with greater generality and capability of adaptation to the phenomena actually observed to take place. The germ of this method is to be found in the *Principia*, with a further development by Daniel Bernouilli; it then met with a searching exposition at the hands of Dubuat; and still more recently,* Poncelet has treated the subject with his characteristic ability and simplicity."

I gave a rough illustration of Poncelet's method, as follows: Let us consider a solid body, say, for example, a sphere placed at the surface of a deep sheet of water; and being of the same specific gravity as the water, let power be expended in pulling it downward at a uniform rate. In any space of descent, it displaces a body of water whose volume is the section of the sphere multiplied by the space described. This water has flowed round the sphere in an annular sectioned cylinder, of which the one boundary is the surface of the sphere; and since the velocity varies at every point as we proceed outwards, from the sphere, and at a short distance out practically vanishes, we may consider the other boundary to be at this place, and that the power is expended in forcing the displaced water through this channel with a certain mean velocity, which experiment has shown to be considerably less than the velocity of the sphere, but in very nearly constant proportion to that velocity. On a disc of equal area under the same conditions, the velocity of the displaced current would be so much increased that about 3½ times the power would be required at the same velocity. If the body be floating at the surface, and only partially immersed when drawn horizontally, the same general explanation holds good although some of the circumstances are altered. The displaced water flows astern in an annular canal, of which the immerged surface of the body forms the one boundary, and we may suppose the other placed at the distance where the motion astern is practically insensible. Simul-

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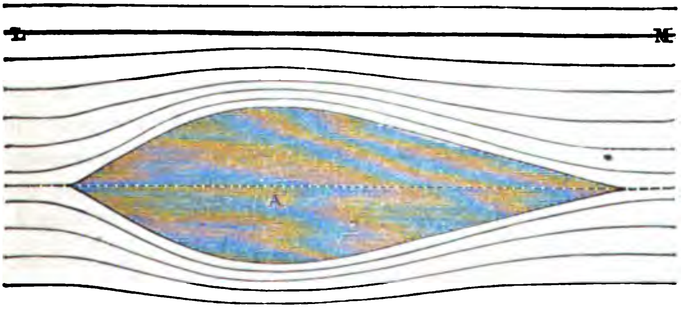
* Say about 1840.

taneously we have a constantly renewed accumulation, or wave, thrown up forward, which may be looked upon as the source or hydraulic head, maintaining the retrograde current, and indicating by its appearance the wasted power when the size and form of the bow is not proportioned to the speed at which the vessel is driven, as in that case the water composing the wave breaks, curls over, is dispersed, and dashed into foam, instead of a long smooth swell and steady movements of the displaced water past the midsection. The same waste of power, and in an aggravated form, is indicated by the great falling of in speed when a vessel enters a contracted channel; for, this limiting the section of the retrograde current, involves an increase of its mean velocity, with a greater accumulation forward, and losses of power from secondary action upon the wave and alteration in the trim.*

Quoting thus far from "Note on Steam Ship Propulsion," March, 1861, I will now read an extract from Poncelet's "Mecanique Industrielle," Vol. I., page 440.

"*Appreciation of the Influence Exercised by Sterns.*—In admitting that the addition of a stern influences in very small degree the rectilinear direction of the imaginary partition lines, LM, L,M (Fig. 82), we perceive, in consequence, that the phenomena of move-



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* If, in the case of a body moving in a fluid, and in the direction of motion, we suppose a number of fine, perfectly flexible filaments or threads extended

ment which take place in the after region of the canal limited by these boundaries, ought to offer the greatest analogy with those of *divergent conical adjutages,* long ago submitted to experiment by Bernouilli and Venturi. Thus the pressure there becomes *negative,* that is to say, inferior to the statical pressure. . . . The mean velocity preserved by these (fluid) threads on their leaving the contraction, or in quitting the body, diminish nearly in the inverse ratio of the area of the sections, and that velocity, when the stern is sufficiently sweetened and elongated, as is represented by the Fig. 82, may finally reduce itself to that of the surrounding medium; or, this tends to make to disappear in the equation of *vis viva,* relative to this case, the term which involves the velocity of onflow of the fluid on the portion forward, and, in consequence, to diminish the resistance."

You may remember, at the close of his interesting experimental inquiry into the flow of water through such adjutages, Mr Brownlee modestly advanced some suggestive remarks in the line of thought of this quotation from a work, which, I have reason to know, was quite unknown to him.

Again, taking Poncelet's Fig. No. 82, which seems to represent somewhat closely the old orthodox, "cod's head and mackerel tail" form, let us suppose it to be divided in the longitudinal middle line and the one-half to be placed outside of the other at a distance such that the imaginary partition lines come together; we obviously have a most excellent representation of the section of a large divergent conical adjutage. Let us now imagine it placed in a *perfect* fluid, which neither exercises friction or adhesion upon it, nor amongst its

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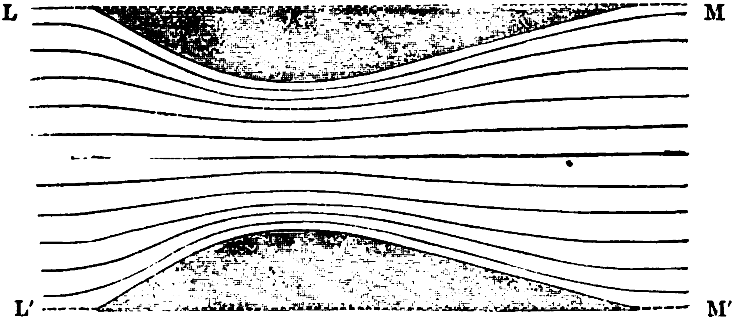
through the fluid in parallel and equidistant lines; on consideration it will be seen at any instant, according to the nature of the changes of velocity throughout the fluid, due to the passage of the body, we should have these lines changing their relative distances, at any point, converging where velocity is increased, and diverging again when the velocity is becoming less; in fact, giving a graphical representation of the actual motions taking place. The French investigators named these filaments "*filets,*" which we may translate (fluid) threads. In this country they are known as *stream lines,* an expressive name, I believe due to the late Dr Rankine.

own particles, and that this twin ship is moving at a uniform rate. Obviously, it will pass the centre (fluid) thread or stream line at the



uniform rate, and any other intermediate one, in some proportion to its distance from the centre, will first converge to the centre till the throat is passed, and then diverge to its original position. Convergence is necessarily accompanied by retrograde motion of the fluid aft, divergence by movement in the direction of the ship's motion. It is a mere truism to assert, what is perfectly obvious, if the particles of fluid are at rest forward of LL′, and are again at rest aft of MM′, no power has been expended in the vessel's motion, and consequently, if we discard the notion of friction and molecular action in a fluid, a proper shaped solid once put in motion would continue in motion without further expenditure of power. It seems to me, however, *no form of solid* would prevent the formation of a wave forward and a retrograde motion of the water in the section MM′, which must be accompanied by a continued expenditure of power in order to maintain the motion. For this reason: convergence of the stream lines forward are not wholly accounted for in retrograde motion; having a free surface the fluid is forced upward as well, and we have precisely the phenomena, which in a more marked degree occur when we take a fluid endowed with the properties found in nature.* A *perfect* fluid exists

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* Mathematical conditions supposed to necessitate the idea of no power being required for movement of a perfect-shaped solid in a perfect fluid, are equally satisfied in the supposition of a constant development of power to maintain the motion, and accounted for in the *vis viva* of a steady stream aft in the opposite direction to the motion.

from, I propose to state formally in three propositions which are founded on no theory, but merely express the result of experiment on well designed vessels, according to the practice of our best constructors.

GENERAL PROPOSITIONS ON THE DEVELOPMENT OF POWER IN THE MOTION OF STEAM VESSELS.

PROPOSITION NO. I.—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.

Notation and Definitions.—The speed of the vessel being V nautical miles per hour, and assuming the engine to have two cylinders of unequal area but the same stroke; let the mean diagram pressures upon a unit area of the small and great pistons, and the ratio of the areas of the latter, be denoted by P, p and r respectively. Defining *piston pressures* as the sum of P, and the product r p; this implies the pressure on the large piston to be increased in the ratio that its area is diminished, so as to be equal to the small piston, and upon these equal pistons, we suppose, mean pressures of P and r p pounds per square inch, respectively, acting.

Again, the tangent of the angle of inclination of the line of logarithmic pressures to the axis being denoted by α, and the *piston pressures* at the origin, when the statical friction and resistance to motion of the machinery are just balanced and motion about to begin, being denoted by f lbs. (divided in some way between the two pistons), the foregoing proposition in the form of an equation becomes—

$$\text{I.}\quad \text{Log.}\,(P + r\,p) = \alpha\,V + \text{log.}\,f.$$

PROPOSITION NO. II.—EXPERIMENTAL LAW OF THE *Slip Ratio.*—If a steam vessel be tried at various speeds, and, if along an axis at points representing these speeds, lines be laid off representing the logarithmic values of the ratios of the retrograde motion of the propeller to the forward motion of the vessel; the ends of these lines will range in a straight line, parallel to or very slightly inclined to the axis, and having its ordinate at the origin, equal to the logarithm of the space unit, increased or diminished by a small quantity.

Notation and Definitions.—During one minute, the retrograde motion of one revolution of the propeller, H feet, multiplied by N, the number of revolutions, gives HN feet as the retrograde motion of the propeller, and meanwhile 101·3 V feet is the direct motion of the vessel. The ratio of these quantities, according to Proposition II., may be constant, or vary with the speed, according to the law of a straight line, the tangent of whose angle of inclination to the axis we denote by β.

Again: suppose the propeller working in a slight current in the direction of motion of the vessel (such as occurs in the wake of a well formed vessel, and due to the frictional drag of its immerged surface), the propeller acting against this, arrests its motion with a resultant effect equivalent to an increase of the numerator of the ratio, or the addition of a small quantity to the normal value, which addition we denote by log. c, where c is slightly greater than unity. On the other hand, suppose the propeller working in a retrograde current (such as occurs with the floats of a paddle-wheel immersed in the current of water displaced by the bow flowing past the mid section), this is equivalent to a diminution of the value of the numerator, and is similarly represented by the addition of log. c to the normal value—only in this case c is less than unity. This last case has marked significance with full formed screw vessels; both occur in all vessels, and it depends upon the circumstances as to which will predominate, and, as a consequence, c be greater or less than unity. With this explanation, the general form of Proposition II. stated as an equation is—

$$\text{II.}\quad \text{Log.}\ \frac{HN}{101{\cdot}3V} = \beta V + \text{log. c,}$$

advisable to take, at least, twice the horizontal scale—(in the accompanying diagrams it is taken at two and a half times the horizontal scale). The ordinates of the 5 and 10 mile speed are to be continued upwards and drawn permanently. Mark off on the speed axis the various speeds at which trials have been made, and let light construction lines be drawn through them from the curved base line upwards.

2. Having the number of revolutions of the propeller for each speed, and the corresponding calculated *piston pressures* $(P + r p)$, from the curved base line lay off upwards the values of the logarithms of the revolutions, log. N, and again, from the ends of these latter the values of log. $(P + r p)$; it will be found that a straight line will pass through the N points, and also through the upper ends. Note, that we take the line which best averages the low and moderate speeds; as explained, at high speeds, as a general rule, the observation spots will be above the line thus determined.

The upper line thus drawn we may call the log. e line; it represents the logarithms of the foot pounds of power which would be developed by a *unit* high pressure cylinder, one inch area and one-half foot stroke, under the circumstances of revolutions and *piston pressures* which exist in the vessel tried.

3. The gross developed power in *indicated horses* is derived from the log. e line by drawing, parallel to it, another line at the distance log. $\frac{d^2 s}{21010}$. this latter quantity being the number of unit cylinders in the cylinder of the vessel tried, divided by 33,000, the divisor for indicated horses. Noticing that this quantity being generally less than unity, the line for gross indicated power, which we denoted by log. E, is lower than the line log. e, and for any speed the value of log. E is found by measuring from the curved base to this line. On the other hand, measuring from the log. e line downwards to the log. N line, we have the logarithms of the *piston pressures* for the same speed.

Again, suppose the case of gross indicated power and revolutions given, it will be readily seen we have only to make a slight difference in the mode of procedure. We should lay off the logarithms of the revolutions, and also the logarithms of the gross indicated power, from the curved base. Then, parallel to the line drawn through the log. E points, we should draw a line, outside, at the distance of log. $\frac{d^2 s}{21010}$; and, measuring from this log. e line to the log. N line, we have the values of the logs. $(P + r p)$. That is to say, any two of the quantities E, N, and $(P + r p)$ given, the third is determined.

4. The analytical character of the straight lines thus drawn can be determined in the following manner:—

Measuring from the log. e line, downwards, to the log. N line, on the 5 and 10 mile ordinate, these, as explained, being the values of the logarithms of the *piston pressures* at these speeds, which we denote by P_5 and P_{10}, respectively, we have in the equation log. $(P + r p) = \alpha V + \log. f$:—

> Tangent of angle of inclination, $\alpha = \frac{1}{5} (P_{10} - P_5)$, and to find Piston pressures equal to friction, $\log f = (2 P_5 - P_{10})$.

Again, measuring from the speed axis to the line log. N, on the 5 and 10 mile ordinate, the values denoted by n_5 and n_{10}, respectively, in the equation $\log. N = \log. V + \beta V + \log. \frac{101{\cdot}3\, c}{H}$ we have :—

> Tangent of angle of inclination, $\beta = \frac{1}{5} (n_{10} - n_5)$, and
>
> Value of $\log. \frac{101{\cdot}3\, c}{H} = (2 n_5 - n_{10})$.

Whence we can easily find the value of log. c.

Progressive speed trials on the same vessel, having the definite object of obtaining the experimental relation between the power and speed of steam vessels, seem to have been first proposed, and carried out, by Mr William Denny, of Dumbarton; and the intelligence and earnest care in order to obtain accuracy, which this gentleman has brought to bear on a most difficult and expensive class of experiments, I am

TABLE No. II.

TRIAL DATA FOR CONSTRUCTION OF DIAGRAMS.

VESSEL.	ELEMENT.	No. I.	No. II.	No. III.	No. IV.
	Speed, - -	6·20	9·20	11·09	12·91
S.S.	Revolutions,-	31·15	44·75	54·35	63·25
“A.”	Pressures, -	22·79	38·12	53·55	73·30
	Gross Power,	299	718	1225	1948
	Speed, - -	6·33	9·57	11·78	11·94
S.S.	Revolutions,-	36·00	52·70	65·40	66·67
“B.”	Pressures, -	25·03	46·75	72·46	74·54
	Gross Power,	200	548	1052	1103
	Speed, - -	5·96	10·05	10·62	11·33
S.S.	Revolutions,-	37·50	59·77	63·25	66·50
“C.”	Pressures, -	24·08	54·93	59·20	66·27
	Gross Power,	176	640	730	857
	Speed, - -	4·90	8·20	10·06	12·44
S.S.	Revolutions,-	29·30	50·35	63·40	81·82
“D.”	Pressures, -	19 90	33·49	46·87	75·74
	Gross Power,	102	295	520	1084
	Speed, - -	7·93	10·88	12·20	13·41
P.S.	Revolutions,-	17·88	26·25	31·50	35·00
“E.”	Pressures, -	24·57	41·89	56·84	73·55
	Gross Power,	176	440	717	1030

TABLE No. III.

ORDINATES MEASURED ON DIAGRAMS, AND CALCULATED RESULTS.

	S.S. "A."	S.S. "B."	S.S. "C."	S.S. "D."	P.S. "E."
P_{10} =	1·643	1·702	1·727	1·643	1·556
P_5 =	1·266	1·285	1·300	1·304	1·162
	Tan. angle of log (P + rp) line to log N line. $\alpha = \frac{1}{5}(P_{10} - P_5)$.				
α =	·0755	·0834	·0854	·0678	·0788
	Value of log $f = 2\,P_5 - P_{10}$.				
log f =	·889	·868	·873	·965	·768
f =	7·745	7·380	7·465	9·226	5·865
n_{10} =	·690	·740	·773	·797	·373
n_5 =	·702	·755	·803	·777	·321
	Tan. angle of log N line to speed axis. $\beta = \frac{1}{5}(n_{10} - n_5)$.				
β =	—·0024	—·0030	—·0060	·0040	·0104
	Value of log $\frac{101{\cdot}3\ c}{H} = 2\ n_5 - n_{10}$.				
log $\frac{101{\cdot}3\ c}{H}$ =	·714	·770	·833	·757	·269
log c =	·0508	·0316	·0827	—1·9882	—1·9910
c =	1·124	1·076	1·210	·973	·980
	Tan. angle of power lines to speed axis. $(\alpha + \beta)$ very nearly.				
$(\alpha + \beta)$ =	·0730	·0804	·0794	·0718	·0892
	Ordinate at the origin = log $\frac{101{\cdot}3\ cf}{H}$. for gross power or log E = log $\frac{101{\cdot}3\ cf}{H}$ + log $\frac{d^2S}{21010}$.				
for log e line =	1·603	1·638	1·706	1·722	1·037
Value log $\frac{d^2S}{21010}$ =	—1·624	—1·346	—1·296	—1·243	—1·602
sum for log E line =	1·227	·984	1·002	·965	·639

defined in the proposition by γ, and the ordinate at the origin by log. l, the foregoing proposition, as an equation, is—

$$\log.\ A\ \frac{P + rp - f}{V} = \gamma V + \log.\ l. \qquad (3)$$

Which modified by (2) changes into—

$$\log.\ \frac{E_v}{V^2} = \gamma\ V + \log.\ l.$$

And, consequently—

$$E_v = l V^2.\ 10^{\gamma V}. \qquad (4)$$

The relation between α and γ can be shown as follows. Since, by Prop. I.,

$$\log.\ \frac{P + rp}{f} = \alpha V,\ \text{or,}\ \frac{P + rp}{f} = 10^{\alpha V}$$

subtracting unity from each member of the latter equation, clearing off fractions, and taking the logarithms of both members, we get—

$$\log.\ P + rp - f = \log.\ f\,(10^{\alpha V} - 1).$$

By adding log. $\frac{A}{V}$ to each member, and modifying the first by (3), we get—

$$\gamma V + \log.\ l = \log.\ Af\left(\frac{10^{\alpha V} - 1}{V}\right);$$

and, consequently,

$$\gamma = \frac{1}{V}\ \log \left\{ A\,\frac{f}{l}\,\left(\frac{10^{\alpha V} - 1}{V}\right).\ \right\} \qquad (5)$$

By substituting the preceding value of log. (P + rp — f), and the value of E_v from (4), in equation (2); we get the curious relation,

$$A\,\frac{f}{l}\left(10^{(\alpha - \gamma)\ V} - 10^{-\gamma V}\right) = V. \qquad (6)$$

Value of Log. l.—The ordinate of the resistance line at the origin, which we denote by log. l, is the direct measure of the resistance, as influenced by the actual dimensions of a particular vessel, without taking into account secondary actions, which, at actual experimental speeds, go to diminish the gross amount. These may be specified as follows:—First, diminished resistance due to the action of the stern, referred to by Ponçelet; secondly, action of the propeller on whole or part of the following frictional current; thirdly, effect of the power expended on slip in modifying both the preceding: the resultant effect of all of which, at experimental speeds, is considerable. With a steam vessel the propeller action on the following frictional current is the most obvious of these, and, last year, in the discussion of Mr Denny's paper, in the calculation, for this case, of what I called the recovered power E_R, I gave a short explanation of the following formulas:—

It was stated, in a vessel of length L, immerged mid area M, and mean girth G, at the speed V nautical miles, the power E_f, necessary to overcome the direct fluid friction on the immerged hull, should be, approximately:—

$$E_f = \frac{LG}{68{,}000} V^2 (V + 3{\cdot}5).$$

While for the power E_m, involved in fluid motions in the surrounding medium due to the volume of the vessel,

$$E_m = \frac{L\sqrt{M}}{15{,}400} V^3.$$

Let us calculate the values of the first part of the second numbers of these equations, independent of the part involving the speed, the log. of the sum of these is the value of log. l, and we have—

$$\text{Log. } l = \text{log. } \left\{\frac{LG}{68{,}000} + \frac{L\sqrt{M}}{15{,}400}\right\} = \text{log. } \left\{\frac{L}{68{,}000} (G + 4.42\sqrt{M})\right\}$$

The calculated values for the five vessels referred to are as follows:—

VESSELS.	$\frac{LG}{68{,}000}$	$\frac{L\sqrt{M}}{15{,}400}$	Sum = l.	Log. l.
S.S. A	·2902	·5473	·8375	— 1·9232
„ B	·2054	·3962	·6016	— 1·7796
„ C	·1737	·3342	·5079	— 1·7059
„ D	·1156	·2285	·3441	— 1·5366
P.S. E	·0915	·1859	·2774	— 1·4480

without reference to secondary actions, the logarithms of the sum of these quantities E_f and E_m, for each speed, being laid off by scale from the logarithmic base, will furnish points in a curved line lying, in general, between the effective power curve and the gross power line. The difference of the ordinates of these two curves on the whole range form a curvilinear figure or line bounded by the two curves, and any one difference represents, for that speed, the value of the quantity E_R, or sum of the power recovered in secondary actions : for the necessary reason, the sum of the work done cannot be greater than the effective power doing that work. In other words, we credit the engine for power expended in slip, surface friction, and fluid movements ; if, by subsequent secondary action, these are in any way diminished, the collective difference becomes apparent in more useful work done; which is the meaning and measure of the quantity E_R .*

The PRESIDENT proposed a hearty vote of thanks to Mr Mansel for his paper, and expressed a hope that when the members got the paper into their hands they would closely examine its statements and reasoning, and come prepared to do justice to its discussion at next meeting.

The vote of thanks was accorded.

In opening the discussion on this paper, on April 25th, 1876,

Mr ROBERT MANSEL said—In the paper read, I have endeavoured to investigate an important and intricate subject, involving many

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* In last year's Transactions I gave an example of the application of this peculiar view to the analysis of screw vessels' performances. The formula there given is not the best as regards the co-efficient involving trim and draft. The equation

$$E_R = \left\{ \sqrt{E_f} - \left(\frac{\alpha^{\frac{1}{2}} V}{700} + \frac{(100 - \alpha)\, \Delta^2}{140,000} \right) \left[\frac{E_V}{E_f} \right]^5 \right\}^2$$

In which α and Δ are the angle of trim, in minutes of a degree, and draft of the propeller respectively, is more general and simpler for calculation ; and although an involved equation, not applicable in all cases, it seems to represent the results of experiment with remarkable fidelity.

collateral questions, which, separately, for their full discussion, might have occupied the time devoted to the whole. I can readily understand, to many the method of treatment may be thoroughly obscure, and my explanations inadequate. I will only observe, a small amount of intelligent application from any one so far qualified as to be able, with decent fortitude, to enter a table of logarithms and extract therefrom a properly indexed logarithm true to four places—if the logarithms of the various elements be laid off to scale, in the manner described, I think it will soon become apparent that the results are direct and simple solutions of several important questions, and suggestive indications of the effects of others, hitherto overlooked or treated improperly.

In the course of the paper it was tacitly assumed that motion took place in fluid at rest. If, however, the water and air be not at rest, but in independent motion, the power implied by this forms an element which in many, indeed we may safely say in every case, exerts a noticeable influence on the phenomena, and, although we may neglect this in our reasoning and formulas, on the erroneous notion that they are unimportant or mutually neutralise, the deduction will force itself on our notice : *nature acts differently.*

I do not consider it necessary here to enter into a long investigation, but will merely consider a few simple cases. First, motion of a vessel at the rate of V miles through water and air at rest. The motion of the upper part of the hull through the air requires an expenditure of power, approximately, equal to the power which would be derived from a V mile breeze acting on the exposed hull. Such a breeze would produce some speed, which, however low, involves an appreciable amount of power included in the effective power E_v expended on the vessel, to be separated before we arrive at the true expenditure of power on the immerged hull. Secondly, motion of a vessel at the rate of V miles in water at rest, but with a V mile breeze blowing along the path of the vessel. The power involved in driving the vessel through air varies, approximately, as the cube of the speed, hence, moving at V miles through air at rest, with a V mile breeze, and against the same, would require an expenditure of power in the

For convenience and practical accuracy, having two scales,[*] as shown on diagram (see Plate XVI.), proceed in the following manner:—

First. Draw a speed axis, Ox, and beginning at the origin O, with the mile scale, mark off the various trial speeds, also the 10 mile point: draw strictly parallel ordinate lines through these, perpendicular to the speed axis.

Second. Take out the logarithms of the trial speeds, revolutions, and piston pressures; with the speed of No. 1 trial, slide the zero of the half-inch scale along No. 1 ordinate, below the speed axis, until the logarithm of the speed, on the scale, is over the speed axis. Mark the ordinate opposite the zero of the scale, and also opposite the logarithm of No. 1 revolutions: again set the zero of the scale on this last determined point, and, above it, mark off the point opposite to the logarithms of the pressures. Proceed in like manner with the elements of the remaining trial speeds.

Third. Through the revolution points draw the straight line A B. Measure the distance it lies above the axis at the origin, that is to say, the ordinate at the origin, marked log. n_0; the difference between this and the like measured ordinate at the 10 mile point, divided by 10, is the value of β.

Through the pressure points draw the straight line C D. Measure the distance of this line above the line A B (*considered as axis to the line* C D), the ordinate to the origin, the line marked log. f, being subtracted from the like measured ordinate at the 10 mile point, the difference, divided by 10, is the value of α.

Fourth. Calculate the value of the expression $\log \frac{d\,s}{21010}$, which set off from the piston pressure, or C D line, on the origin and 10 mile ordinates; the straight line E F through these points, obviously parallel to the pressure line, is the gross power line; the ordinates to it, measured from the logarithmic curve drawn under the speed axis, being the logarithms of the gross indicated powers at the various

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[*] I would suggest one, 1½ ins. per unit, for mile distances along the axis, and the other a ½ in. scale, ten divisions of which, or 5 ins., to be taken as the logarithmic unit for laying off ordinates.

speeds. The ordinate of this at the origin with respect to the speed axis, marked log. E_O, subtracted from the like ordinate at the 10 mile point, the difference, divided by 10, is practically equivalent to the sum $(\alpha + \beta)$.

Fifth. From log. f, determined by the third step, we have the value of f, which, subtracted successively from the various trial piston pressures, the logarithms of these residues, being laid off from the points in the logarithmic curve base, under the speed axis, the straight line G H drawn through same gives the line of effective piston pressures. Now, calculate the value of log. $c = \log. \frac{n_o H}{101 \cdot 3}$, and then the value of log. $\frac{d^2 s}{21010} \frac{n^o}{c^2}$, which latter set off from the line G H on the origin and 10 mile ordinates, the straight line J K, through these two points is what has been named the resistance line,* its ordinate at the origin, marked log. l, being subtracted from the like measured ordinate at the 10 mile point, the difference divided by 10 is the value of γ.

Sixth. Above the straight line J K, set off, on each ordinate, the logarithm of the corresponding trial speed, the fair curve drawn through the points thus determined, in respect to the curved logarithmic base, has for its equation $\log. E_V = 2 \log. V + \gamma V + \log. l$, where log. E_V, the ordinate between the curved logarithmic base and

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* These last operations may be determined without calculation in the following manner:—For the first: slide the scale downwards on the origin ordinate, till the value of log. H is over the speed axis, and mark the ordinate opposite 2·0056 on the scale (log. 101·3), the distance of this point from the revolution line, is the value of log. c, positive when it falls under the revolution line, and negative when above. For the second: double the distance of the last determined point from the revolution line, and from this new point set off the value of log $\frac{d^2 s}{21010}$: that is to say, mark a point at the same distance from 2 log. c that the power line is distant from the pressure line: then: whatever distance the point thus determined is from the speed axis, the resistance line is at the same distance from the *effective* pressure line, and parallel to it.

equal times of exposure to them is to be aimed at, not varying times over an equal space.

The shore ground at Rahane being unbuilt upon, and the property of his Grace the Duke of Argyll, were our Clyde shipbuilders to subscribe for proper posts and necessary expenses, they might approach his Grace with a respectful application for leave to erect the same.

The PRESIDENT said he was sure they would all agree with him that Mr Mansel's further explanations and investigations had involved them in another vote of thanks to him. Mr Mansel had said it was very easy to make these calculations, and no doubt for *him* it was so, although he must have had great labour over this interesting paper He thought it was a most difficult subject for them to discuss, and the diagrams displayed were well worthy of study; indeed, it was almost a marvel to find theory and experiment in so exact agreement Thus, in respect to certain lines, Mr Mansel had told them he had put down the lines, and afterwards placing the observation spots had found them to fall upon the lines with nearly perfect regularity, thus proving the correctness of the hypotheses involved He was certain if they all understood the subject as well as Mr Mansel, they would be more delighted with the paper than they were.

Mr BROWNLEE said, in the present diagrams, Mr Mansel having carried out the various lines to the origin, they would be much easier understood than those illustrating the paper, and he would further suggest putting letters on the various lines of the figures for greater ease of reference; indeed, he had done so himself, and after some little difficulty had succeeded in following out the method, laying down the speeds with a scale, and taking two experiments for each line.* He thought if members made a trial of the system of investigation recommended by Mr Mansel they would soon come to see its simplicity, and the remarkable way in which the lines and observations came in. Again, it appeared the law of variation of the power was not in proportion to the cube, but involved the law of the loga-

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* This suggestion has been carried out in lithographing the diagrams.

rithmic curve, and it was remarkable that this had never been seen before. This was doubtless due to Mr Mansel's familiarity with the subject, and from having the results of correct experiments with the same vessel at different speeds placed within his reach. He thought they were all deeply indebted to Mr Wm. Denny in furnishing data for such investigations. With reference to ship propulsion, this was, perhaps, the most important paper he had ever heard read, in this or any other institution. He had no doubt practical shipbuilders would soon come to understand it; it needed little calculation, only a scale and table of logarithms.

Mr ALEX. MORTON said they must all acknowledge that Mr Mansel had been at great labour and expenditure of time in analysing the different vessels he had brought before them. If he understood rightly, the power values were according to the logarithms of the revolutions, the revolution line being parallel to the speed axis. He had observed that Mr Froude was engaged in some similar investigation, and very probably on some of the same vessels; this gentleman had also stated, only about 40 per cent. of the indicated power was expended in useful work. We all admired these investigations, in the hope that they might clear the mist which had hung so long over this difficult subject.

Mr MANSEL said it was necessary to take the pressures along with the revolution line to obtain the power line. The revolution line was not parallel to the speed axis; the angle, however, in many cases was very small, and, at a casual glance, would seem parallel.

The PRESIDENT suggested, with regard to the effect of the weather in varying the quantity *f* in trials, that, making a small diagram when running with and against the wind, in the latter case the pressure and power lines would be more nearly parallel to the speed axis.

Mr E. KEMP made some observations as to the co-efficient of friction in engines of different powers, at the lowest and highest speeds, when developing their full power in the same vessel.

Mr A. BROWN said he did not quite comprehend Mr Mansel's remark about the increase of friction. He did not know when

On the Compounding of Locomotive and other Non-Condensing Engines.

By Mr EBENEZER KEMP.

(SEE PLATES XVII. AND XVIII.)

Received and read 25th April, 1876.

In bringing the subject of Compound Non-Condensing Engines before the Institution, my object is to endeavour to prove, by some experiments on non-condensing engines, that there is a good deal of loss sustained in working non-condensing engines generally on the single cylinder principle, and that a very considerable saving in fuel would be the result of adopting the compound or double cylinder principle. There is a large class of engines for agricultural and other purposes which will in all likelihood continue to be worked on the non-condensing principle, whether they are fixed or portable; but by far the largest class of non-condensing engines in use is the locomotive. Now, there is no doubt whatever, that any means which can be adopted to reduce the cost of railway communication, and of the increasing goods traffic, would be a great public benefit. Therefore it is the locomotive which offers the greatest inducement for some enterprising superintendent to adopt the system, and thereby enable his shareholders and the public to benefit by the economical advantages to be gained by it.

It is possible I may be wrong in speaking of the locomotive as if it had never been compounded; for although a good many non-condensing compound stationary engines have come under my notice, I have not yet heard of a locomotive, but perhaps some of our locomotive friends will be able to put me right on this point.

to allow the exhaust steam to escape to the exhaust pipe. There was no alteration of the slide valves, the governor throttle valve, nor in any other respect. The steam pressure was maintained at 70 lbs., and the experiment lasted six working days, with the result that we burned 18·8 tons of coal.

The diagrams H, Sheet No. 2, represent the first or compound experiment, with both pistons (A and B) in use. The diagram I represents the single cylinder experiment, with the piston B in use and the piston A removed; and the diagram K represents the single cylinder experiment, with the piston A in use and the piston B removed. The diagram G represents the engine working compound when the horse-power was much greater than during these experiments.

The diagrams taken during the experiments show considerable variation in the power, ranging from 15 to 30 horse-power. Even in the same engine there is a considerable range of power, owing to the heavy class of machinery, such as travelling cranes, &c., driven by the engines, which are liable to be put off and on, causing the load to vary very much. Consequently, I do not intend to compare the horse-power of one experiment with that of another, but merely the coals consumed and the water evaporated (which was nearly proportional to the coal burned). The power exerted was equal in all the experiments, as far as could be ascertained, there being no change in the number of machines driven, nor in any other respect.

The first or compound experiment was gone over again, after the others were completed, with the same results as at first. The single cylinder experiments were verified by being gone over a second time also.

The diagrams H, I, K are selected from a number taken during the various experiments, because they give the nearest equal powers which we could get, viz.:—For the compound experiment (Diagram H), 20·3 H.P.; for the large cylinder experiment (Diagram I), 19·66 H.P.; and for the small cylinder experiment (Diagram K), 18·33 H.P. It is necessary, in order to arrive at a fair estimate of the relative efficiency of the various trials, to subtract the coal consumed for other purposes beyond the driving of the engines from the total quantities before comparing the different trials. Some of these are from measurement, others estimated. The coals required to bank up the fires at night, get up steam in the morning, and keep it up during meal hours, was found to be 2·5 tons; and I estimate the coals required for the steam used by the steam hammer, heating stoves, &c., at 2 tons; which, when subtracted from the total coal, reduces the compound experiment to 11·49 tons; and by subtracting the same quantity from the 14-inch experiment, reduces it to 15 6 tons; and from the 10-inch experiment, to 14·3 tons. These weights show the compound to be 26·34 per cent. more economical than the 14-inch experiment, and 19·65 per cent. more economical than the 10-inch experiment; or, taking the total quantities burned for all purposes—viz., 15·99 tons for the compound, 20·1 tons for the 14-inch, and 18 8 tons for the 10-inch experiment—gives the compound 20·5 per cent less coal than the 14-inch, and 15 per cent. less than the 10-inch.

The latter per centage of advantage would be about the proportion for locomotive engines, where it is necessary to stop at night, and at stations, &c.; and for engines used in manufacturing and such like operations, where it is necessary to stop at night, and during meal hours, and when a little additional work is required to be done by the steam from the same boilers as drive the engines. The former per centages would be the proportion for marine, and other engines which were driven for considerable periods at one time without stopping.

The saving stated here, of course, applies to engines with a working pressure of 70 or 80 lbs. The present locomotive practice is probably double that pressure in some of the new engines, and it could easily be increased considerably beyond what it is at present if the compound system were adopted, the strain on the working parts being so much reduced that there would be no necessity for heavier engines although the pressure was put up 50 per cent. If this were done, I have no hesitation in saying that there would be a saving of at least 30 per cent. in the coal bills of our railways.

It is well known that theoretically (that is, if you simply calculate the quantity of work which a given weight of steam will produce by parts is very much less; and although the cylinder capacity may be a little more, the reduced weight of the working parts and size of boiler required, would always give the compound system the balance of advantage in first cost, as well as in the consumption of fuel in every-day work.

As to the lowest pressure at which it would be advantageous to apply the compound system to non-condensing engines, that is partly a question of circumstances. I may say, however, that I know of various cases where it has been carried out under 50 lbs. pressure successfully, and in one case where the boiler pressure was only 35 lbs., which gave a much better result when working compound than when it was worked as a single cylinder. I mention these cases, not so much with the view to recommend such low pressures, as to show in a general way that there would be some gain, at almost any workable pressure, if the sizes of cylinders and cut-off of valves were well proportioned to the power required.

The drawings, Sheets No. 3, 4, and 5, show an arrangement for compounding a locomotive engine, one of the cylinders being enlarged to double the area of the other, the smoke-box, N, and funnel, O, being formed into a receiver for conveying the steam from the high-pressure cylinder, P, to the low-pressure cylinder, Q. The steam from the boiler is taken to the high-pressure cylinder by the pipe R, and exhausted by the pipe S into the steam smoke-box, passing up one side of the funnel and down the other into the low-pressure cylinder by pipe T, and exhausting into funnel by pipe U. A safety valve, loaded to about one-third the boiler pressure, would be required on the receiver, and a hand valve or cock between the boiler and receiver for steam to keep up the pressure when starting the engine. The cranks to be at right angles as usual.

There would be no difficulty in starting the engine if made in this way, there being a great many of the smaller class of marine compound engines started by simply putting some steam into the receiver by a cock, without having hand valves fitted to the cylinder, which is the usual practice in the larger class of engines.

I have adopted a proportional area of cylinders of one to two, which gives very nearly the same power developed in each cylinder when the cut-off of the valves are equal—this being the case approximately even although the power be considerably altered. This you can see by reference to the diagram G and H, Sheet No. 2, the large diagram representing the engine when the work was nearly double the small diagram. The relative powers are—for the smaller diagrams, 11·1 H.-P. for the high pressure, and 9.2 H.-P. the low pressure; and for the larger, 21·9 for the high pressure, and 19·5 H.-P. for the low pressure respectively, the maximum strain on high pressure cylinder being 2512 lbs., and on low pressure cylinder 2616 lbs.. which are practically equal. The capacity of cylinders which will give the best result can only be found by experience. The sizes I have shown are 16 and 23 inches, to replace two of 16 inches, which is equal to one and a half times the total capacity. This, I feel sure, will be found too large; and a total capacity of cylinders equal to that of those now in use for a given size of engine will be found quite large enough, especially if the boiler pressure is slightly increased, which is quite likely to be the case, the steam being so much easier kept up, and the boiler could then be reduced by the per centage of saving in fuel, and the first cost of a compound locomotive would be reduced proportionately.

In the compound system you get a higher working pressure on the piston in proportion to the boiler pressure than you get with the single cylinder system, which practically gives you a higher range of expansion. This result is very well shown in the two diagrams H and K, on Sheet No. 2. In both cases the same pressure of steam and set of valve was used—in fact the conditions were precisely similar, except that the second cylinder was out of action in one case. The pressure of steam on the piston at the beginning of the stroke on the compound was about 43 lbs., and on the single system about 33 lbs. Now, seeing that the one which indicated the higher pressure at the beginning of the stroke used less steam out of the boiler (as is shown by the less quantity of water evaporated and coal consumed), I think this clearly shows that a larger quantity the slide valves between, when there is not room for a larger cylinder; or the position of the slide valves might be altered to allow of the larger cylinder getting in, the valve being put either on the top or the outside of the new cylinder; but it is needless to multiply examples, as each would require to be dealt with according to the merits of the case.

Having now given you my reasons for compounding locomotives, and shown one method of carrying it out, I submit that, if it has been found advantageous and economical to compound condensing engines which exhaust against an absolute steam pressure of, say 2lbs. or 3 lbs, and having a range of about 70 lbs. between the initial and exhaust pressures, this being about the range in marine engineering practice at present, and as I have shown by the experiments on the non-condensing engines referred to, which exhaust against an absolute steam pressure of, say 17 lbs., and having a range of about 53 lbs. between the initial and the exhaust pressures, I think it is reasonable to say that if a locomotive has an equivalent range of pressure between the initial and the exhaust pressure, it would be equally advantageous and economical. The present locomotive practice, which I have taken at 115 lbs. absolute pressure and 20 lbs. exhaust, gives a range of 95 lbs., which is considerably in excess of marine practice, and ought to give proportionately better results.

The PRESIDENT said that as the hour was now so late, it would be necessary to defer the discussion of this paper until next session. He would have liked very much to have overtaken it that night, but he was afraid they could not do it justice if they attempted it. He had much pleasure in moving a hearty vote of thanks to Mr Kemp for his paper, on a subject which was interesting to the country at large, and had in it the elemen[illegible]great economy. Before separating, he would crave their indulgence fo[illegible] with a matter which, although not strictly part of the business of the Institution, yet was intimately connected with it. They were aware that at the commencement of the Session, a course of Lectures on Applied Mechanics had been arranged to be delivered to the Graduates, and such others as chose to attend. The lectures, he was glad to say, had been quite a success, and he felt satisfied that the young men who attended them must have gained a large amount of useful information from them. Having been present himself at several of the lectures, he could assure them that the popular and interesting way that the various subjects were treated reflected the greatest credit upon the lecturer, Mr Millar, their Secretary, who had spared neither time nor trouble to make all things clear and intelligible to his class. At the commencement of the course, he (the President), in the hope of stimulating the young men attending the lectures, offered a prize, to be competed for by a written examination, to take place at the end of the course upon the subjects of the lectures. Mr Millar kindly undertook to conduct the examination, and also offered a prize for the second in the competition. Through an inadvertence of the bookbinder, the prizes were not ready for presentation, as intended, at the concluding lecture on Thursday last; and as this was the last meeting of this session, with their permission, he wished now to present the prizes to the successful competitors. The first prize had been gained by Mr Hazelton R. Robson, jun., and the second prize by Mr Andrew Stirling.

The prizes were then presented to the successful competitors.

The PRESIDENT concluded the session by thanking the members or their kindness towards him during the time he had had the honour of presiding over the Institution. It had been a great pleasure to him, and he expressed the hope that the Institution would continue to hold the prominent position it had done in the country. From many sources he knew that their doings were looked up to, not only with respect, but that the subjects that came before them were considered as deserving of the utmost attention from all the engineering communities in England. He was sure that when those papers read by Mr Brownlee and Mr Mansel went forth to the world in their next volume of Transactions, they would be considered as second to none that had gone before.

The PRESIDENT delivered his Introductory Address; and, on the motion of Mr D. Rowan, a vote of thanks was awarded to the President for his Address.

The scheme drawn up by the Council for a Course of Lectures upon Applied Mechanics, for the benefit principally of the Graduates of the Institution, was submitted to the meeting by the President and Mr D. Rowan, and the approval of the meeting obtained.

The Marine Engineering Medal, awarded the President for his Paper on "The Advantages of Springs for Loading Government Safety Valves of Marine Boilers, instead of Dead Weight," was presented, the President making a suitable reply.

It was then announced that the Course of Lectures to the Graduates would commence on Thursday, the 4th November, and be carried on weekly during the Session.

On the motion of Mr COSTELLOE, a vote of thanks was accorded those gentlemen who had guaranteed the sum necessary for the carrying out of the Lectures.

THE SECOND GENERAL MEETING of the NINETEENTH SESSION of the Institution was held in the Hall, 2 Dalhousie Street, on Tuesday, the 23rd November, 1875, at 7.30 P.M.

Mr HAZELTON R. ROBSON, President, in the Chair.

The Minute of Meeting of 26th October was read, approved, and signed by the President.

The following candidates for admission to the Institution were elected :—

AS MEMBERS :—

Mr WM. KEMP, Mechanical Engineer, Glasgow.
Mr JOHN TURNBULL, Jun., Mechanical Engineer, Glasgow.

AS GRADUATES :—

Mr WM. PARKER, Jun., Apprentice Civil Engineer, Glasgow.
Mr H. R. ROBSON, Jun., Apprentice Civil Engineer, Glasgow.

Mr MATTHEW PRIOR read his Paper on "Buckley's Patent Compensating Metallic Piston;" a Discussion followed, and was terminated.

On the motion of the President, a vote of thanks was awarded Mr PRIOR for his Paper.

The SECRETARY read his Paper on "The Strength and Fracture of Cast Iron;" a Discussion followed, and was terminated.

On the motion of Mr J. Z. KAY, a vote of thanks was awarded Mr MILLAR for his Paper.

THE THIRD GENERAL MEETING of the NINETEENTH SESSION of the Institution was held in the Hall, 2 Dalhousie Street, on Tuesday, the 21st December, 1875, at 7.30 P.M.

Mr HAZELTON R. ROBSON, President, in the Chair.

The Minute of Meeting of 23rd November was read, approved, and signed by the President.

The following candidates for admission to the Institution were elected :—

THE FIFTH GENERAL MEETING of the NINETEENTH SESSION of the Institution was held in the Hall, 2 Dalhousie Street, on Tuesday, the 22nd February, 1876, at 7.30 P.M.

Mr HAZELTON R. ROBSON, President, in the Chair.

The Minute of Meeting of 25th January was read, approved, and signed by the President.

The following candidates for admission to the Institution were elected:—

AS MEMBERS:—

Mr WALTER HANNAH, Board of Trade Surveyor, Glasgow.
Mr THOMAS KENNEDY, Mechanical Engineer, Kilmarnock.
Mr JOHN THOMSON, Mechanical Engineer, Glasgow.

The PRESIDENT referred to the loss which the Institution had sustained through the death of Mr Benjamin Conner, and stated that the Council had prepared a Minute expressive of their sense of the loss which the Institution had sustained. The Council had also agreed that a copy of this Minute should be transmitted to Mr Conner's relatives.

On the motion of the PRESIDENT this was unanimously agreed to, and that this expression on the part of the Institution be recorded in the Transactions of the Institution.

The Discussion of Mr JAMES BROWNLEE'S Paper on "The Action of Water and loss of energy, when flowing at various velocities through a nozzle, with a converging entrance and diverging outlet," was resumed, and was terminated.

On the motion of the PRESIDENT a vote of thanks was awarded Mr R. D. NAPIER for his contribution to the Discussion.

Mr H. J. BOOLDS read his Paper on "A Proposed Cellular Ship of War," the discussion being adjourned to next General Meeting.

The Sixth General Meeting of the **Nineteenth Session** of the Institution was held in the Hall, 2 Dalhousie Street, on Tuesday, the 21st March, 1876, at 7.30 P.M.

Mr **Hazelton R. Robson**, President, in the Chair.

The Minute of Meeting of 22nd February was read, approved, and signed by the President.

The following candidates for admission to the Institution were elected :—

AS MEMBERS :—

Mr **John G. Kincaid**,	Mechanical Engineer,	Greenock.
Mr **James Mollison**,	do.,	do.

Mr **Andrew Maclean** and Mr **David Kinghorn** were appointed Auditors of Annual Financial Accounts.

The Discussion of Mr **Boold's** Paper was proceeded with, and terminated, and a vote of thanks awarded Mr Boolds for his Paper.

Mr **Mansel** read his Paper, entitled "Propositions on the Motion of Steam Vessels." A vote of thanks was awarded Mr Mansel for his Paper; the Discussion being delayed till next General Meeting.

A letter from the American Society of Civil Engineers, addressed to the **Institution**, was read. The letter intimated that the Society had made provision for the reception of Members of kindred Societies who might visit the Centennial Exhibition, Philadelphia, and that the Society invited the Members of this Institution who might visit the Exhibition to make the Society's Offices in New York or in Philadelphia their head quarters during their visit.

It was unanimously agreed that a letter be sent to the Society conveying the thanks of the Institution, and that Members of the Institution should be made aware of the invitation by a note in Circular calling next General Meeting.

TREASURER'S STATEMENT—1875-76.

GENERAL FUND.

Dr.

To Balance in Union Bank at close of Session 1874-5,		£272 8 8
Subscriptions received :		
Session 1875-76, ...	£483 0 0	
Deduct Entry Money transferred to Building Fund, ..	13 0 0	
		470 0 0
Arrears of Previous Sessions, ...		49 10 0
Dividends on Preference Stock, London and North-Western Railway, ...		15 12 4
Sales of Transactions, ...		19 7 6
Bank Interest, ...		2 13 10
		£829 12 4

Cr.

By Rent of Rooms, ...	£100 0 0	
Less Taxes deducted, ...	0 16 8	
		£99 3 4
Printing, ...		146 18 1½
Lithography, ...		103 11 0
Salary to Secretary, ...		100 0 0
Commission to Secretary for Collecting Arrears of Subscriptions, viz. :—		
For Session 1875-76, ...	£238 15 0	
For Previous Sessions, ...	49 10 0	
	£288 5 0 at 5°/ₒ	14 8 3
Salary to Sub-Librarian, ...		25 0 0
Extra to Do. for Sectional Meetings,		2 0 0
Coal, Gas, Cleaning, &c., ...		3 2 4
Police and Poor Rates and Property Taxes, ...		15 0 2½
Stationery, &c., ...		6 11 2
Postages, ...		25 5 8
Carriage of Transactions, ...		0 4 0
Bookbinding, ...		12 0 0
Furnishings, ...		6 1 0
Law Agents' Account, ...		29 10 6
New Medal Die, Press, and Diploma Plate, ...		43 12 6
Petty Cash, ...		1 10 5
Balance in Union Bank at 12th April, ...		195 13 10
		£829 12 4

MARINE ENGINEERING MEDAL FUND.

Dr.

To Balance in Union Bank at close of Session 1874-75, ...	£76 16 4
Dividends on Debenture Stock, Glasgow Corporation Water Works, ...	1 3 11
Dividends on Preference Stock, London and North-Western Railway, ...	8 19 0
Bank Interest, ...	1 3 3
	£88 2 6

Cr.

By Paid for Medal, Session 1873-74, ...	£10 0 0
Balance in Union Bank, ...	78 2 6
	£88 2 6

RAILWAY ENGINEERING MEDAL FUND.

Dr.		Cr.	
alance in Union Bank at close of Session 1874-75, ...	£48 19 6	*By* Balance in Union Bank, ...	£55 13 9
ividends on Debenture Stock, Glasgow Corporation Water Works, ...	0 15 9		
ividends on Preference Stock, London and North-Western Railway, ...	5 3 8		
ank Interest, ...	0 14 10		
	£55 13 9		£55 13 9

BUILDING FUND.

Dr.		Cr.	
alance in Union Bank at close of Session 1874-75, ...	£199 0 0	*By* Balance in Union Bank, ...	£232 17 11
ntry Money, ...	13 0 0		
ividends on Debenture Stock, Glasgow Corporation Water Works, ...	17 17 0		
ank Interest, ...	3 0 11		
	£232 17 11		£232 17 11

GLASGOW, *15th April, 1876.*—WE have examined the foregoing Annual Financial Statement of Treasurer, the Accounts of the ie and Railway Engineering Medal Funds, and the Building Fund, and find the same duly vouched and correct, the Amounts in being as stated.

(Signed) ANDw. MACLEAN, } AUDITORS.
DAVID KINGHORN,

DONATIONS TO LIBRARY.

Report on the Strength of Single-Riveted Lap Joints, by B. B. Stoney, M.A., M.R.I.A., M.Inst.C.E. From the Author.

The Mechanical Engineer: an Address by Professor R. H. Thurston, A.M., C.E. From the Author.

Report on Explorations and Surveys for Ship Canal, Isthmus of Tehuantepec, by R. W. Shufeldt, Captain U.S. Navy. Presented by Professor Nourse, U.S. Naval Observatory, Washington.

On the Necessity of a Mechanical Laboratory. By Professor R. H. Thurston, A.M., C.E., from the Author.

Approximate Free Board. By F. W. Wymer, from the Author.

Die Mechanische Wärmetheorie. By R. Clausius, from the Author.

Marine Engineering News. From the Publisher.

Kirkaldy's "Experimental Inquiry into the Properties of Essen and Yorkshire Wrought Iron Plates."

On Gumpel's "Patent Rudder."

Our Ironclads and Merchant Ships, by Rear-Admiral E. Gardiner, Fishbourne. From the Author.

A Paper on "Recent Arrangements of Continuous Brakes," by St. J. V. Day, C.E. From the Author.

South Australia, by William Harscus, Esq., J.P. From the Author.

LIST

OF

HONORARY MEMBERS, MEMBERS, ASSOCIATES, AND GRADUATES,

OF THE

Institution of Engineers and Shipbuilders in Scotland,

(INCORPORATED),

At the termination of the Nineteenth Session, April, 1876.

HONORARY MEMBERS.

James Prescott Joule, LL.D., F.R.S., Cliff Point, Higher Broughton, Manchester.

Professor **Charles Piazzi Smyth**, F.R.SS.L. and 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.

General **Arthur Morin**, Conservatoire des Arts et Métiers à Paris.

Sir **Joseph Whitworth**, Bart., C.E., LL.D., F.R.S., Manchester.

1864, Dec. 21:	William	Clapperton,	7 Corunna Street, Glasgow.
1860, Apr. 11:	James	Clinkskill,	176 W. Regent St., Glasgow.
1871, Apr. 18:	Jose Maria	da Conceicao,	Rio de Janeiro, °/o D. Rowan, 237 Elliot St., Glasgow.
	Charles	*Connell,	Whiteinch.
1875, Oct. 26:	W. J.	Clark,	Southwick, near Sunderland.
Original:	Robert	Cook,	Woodbine Cottage, Pollokshields.
1864, Feb. 17:	James	Copeland	16 Pulteney Street, Glasgow.
1864, Jan. 20:	William R.	Copland, C.E.,	83 W. Regent St., Glasgow.
1868, Mar. 11:	S. G. G.	Copestake,	Glasgow Locomotive Works, Little Govan, Glasgow.
1871, Jan. 17:	Antonio	Costa,	Fundicion de la Patria, Valparaiso.
	M. R.	*Costelloe,	189 Albert Terrace, Renfrew Street, Glasgow.
1866, Nov. 28:	M'Taggart	Cowan, C.E.,	137 W. Regent St., Glasgow.
1868, Apr. 22:	David	Cowan, C.E.,	The Tharsis Mines, Tharsis, Huelva, Spain.
1861, Dec. 11:	William	Cowan,	Great North of Scotland Railway, Aberdeen.
1866, Dec. 26:	James L.	Cunliff,	Inch Works, Port-Glasgow.
1870, Oct. 25:	J. F.	Couderé,	27 Wet Street, Antwerp.
	Robert	*Curle,	Stobcross Dockyard, Glasgow.
1869, Jan. 20:	James	Currie,	34 Bernard Street, Leith.
1872, Nov. 26:	David	Cunningham,	C.E., Harbour Chambers, Dundee.

1875, Mar. 23:	Alex.	Davidson, M.E.,	5 Easton Terrace, Glasgow.
1861, Dec. 11:	Thomas	*Davison,	248 Bath Street, Glasgow.
1864, Feb. 17:	St. J. V.	Day, C.E.,	166 Buchanan St., Glasgow.
1869, Feb. 17:	James	Deas, C.E.,	Engineer, Clyde Trust, 16 Robertson St., Glasgow.
	Peter	*Denny,	Helenslee, Dumbarton.
1873, Feb. 18:	William	Denny,	Leven Shipyard, Dumbarton,

1866, Feb. 14:	A. C. H.	Dekke,	Shipbuilder, Bergen, Norway
1871, Jan. 17:	William	Dobson,	Shipbuilder, Joy Lodge, Low Walker-on-Tyne.
1864, Jan. 20:	James	Donald,	Abbey Works, Paisley.
1869, Dec. 21:	James	Donald,	Engineer, Johnstone.
1876, Jan. 25:	James	Donaldson,	Fulbar Street, Renfrew.
1863, Nov. 25:	Robert	Douglas,	Dunnikier Foundry. Kirkcaldy.
1864, Oct. 26:	Robert	*Duncan,	(*Past President*) Shipbuilder, Port-Glasgow.
1869, Nov. 23:	David Jno.	Dunlop,	Inch Works, Port-Glasgow.
1873, Apr. 22:	Robert	Dundas, C.E.,	Engineer Joint Lines, 115 St. Vincent St., Glasgow.
1865, Oct. 23:	Mortimer	Evans, C.E.,	97 W. Regent St., Glasgow.
1872, Oct. 22:	William R.	Evans,	Warrnambool, Victoria, Australia.
1875, Oct. 26:	James G.	Fairweather,	23 Kelvingrove St., Glasgow.
1874, Feb. 24:	Immer	Fielden,	12 James Watt St., Glasgow.
	John	*Ferguson,	Shipbuilder, Whiteinch.
1861, Dec. 11:	William	Ferrie,	Monkland Ironworks, Calderbank.
1861, Jan. 23:	J. R.	Forman, C.E.,	160 Hope Street, Glasgow.
1862, Nov. 26:	Alexander	Fullarton,	Vulcan Works, Paisley.
Original:	William	Forrest,	13 Canal St. (N.), Glasgow.
1872, Nov. 26:	Thomas	Forrest, M.E.,	56 Rose Street, Garnethill, Glasgow.
1870, Jan. 18:	William	Foulis,	Engineer, Corporation Gas Works, 42 Virginia St., Glasgow.
1858, Nov. 24:	James M.	Gale, C.E.,	(*Past President*) Engineer, Corporation Water Works, 23 Miller St., Glasgow.

1865, Mar. 29:	William	Inglis,	Soho Iron Works, Bolton.
1875, Dec. 21:	William	Jackson,	237 Elliot Street, Glasgow.
1872, Oct. 22:	William	Jacks,	39 St. Vincent Pl., Glasgow.
	Geo. W.	*Jaffrey,	The Firs, Partick Hill.
1867, Dec. 26:	John L. K.	Jamieson,	Govan.
Original:	William	Johnstone, C.E.,	(*Past President*), Engineer, Glasgow & South-Western Railway, Glasgow.
1870, Dec. 20:	David	Jones,	Highland Rlwy., Inverness.
1875, Nov. 23:	William	Kemp,	Mechanical Engineer, 1 Ibrox Place, Paisley Rd, Glasgow.
1876, Feb. 22:	Thomas	Kennedy,	Water Meter Works, Kilmarnock.
1872, Mar. 26:	Ebenezer	Kemp,	Linthouse Engine Works, Govan.
1869, Feb. 17:	John Z.	Kay,	Phœnix Iron Works, Glasgow.
1876, Mar. 21:	John G.	Kincaid,	Clyde Foundry, Greenock.
	David	*Kinghorn,	172 Lancefield St., Glasgow.
1864. Oct. 26:	Alex. C.	Kirk,	Govan Park, Govan Road.
Original:	David	*Kirkaldy,	Testing and Experimenting Works, Southwark St., London S.E.
1875, Oct. 26:	William	Laing,	72 Gt. Clyde St., Glasgow.
1875, Feb. 23:	Robert	Liddell,	Manager, Carnbroe Iron Works, Coatbridge.
1858, Apr. 14:	David	Laidlaw,	147 E. Milton St., Glasgow.
1862, Nov. 26:	Robert	Laidlaw,	147 E. Milton St., Glasgow.
1864, Oct. 26:	George	Lauder, C.E.,	c/o Messrs Carnegie & Co., Coke Works, nr. Larimer Station, Pa., United States of America.

Original:	James G.	*Lawrie,	(*Past President*), 241 West George Street, Glasgow.
	Andrew	*Leckie,	Surveyor, Commercial Buildings, West Hartlepool.
1862, April 2:	H. C.	Lobnitz,	Renfrew.
1865, Dec. 20:	John L.	Lumsden,	Fairfield Shipyard, Govan.
	James	*Lyall,	37 W. Sunniside, Sunderland.
1873, Jan. 21:	James M.	Lyon, M.E.,	Bangkok, Siam.
1872, Feb. 27:	J. W.	M'Carter,	City Saw Mills, Londonderry.
1862, Oct. 29:	John	M'Andrew,	17 Park St., East, Glasgow.
1858, Feb. 17:	David	M'Call, C.E.,	160 Hope Street, Glasgow.
1874, Mar. 24:	Hector	MacColl,	Consulting Engineer, 65 West Regent Street, Glasgow.
	Hugh	*MacColl,	Abden Yard, Kinghorn, Fife.
1871, Jan. 17:	David	M'Culloch,	Vulcan Works, Kilmarnock.
1857, Dec. 23:	J. I.	M'Derment,	39 Sandgate Street, Ayr.
Original:	Walter	M'Farlane,	Possil Park, Glasgow.
	Andrew	*M'Geachan,	Newark Shipbuilding Yard, Port-Glasgow.
Original:	James	M'Ilwham,	100 Cheapside St., Glasgow.
1858, Apr. 14:	John	Mackenzie, M.E.,	2 Sandown Terrace, Chester.
1867, Feb. 27:	Alexander	M'Kinlay,	Shipwright Surveyor, 12 James Watt St., Glasgow.
1873, Jan. 21.	J.B.Affleck	M'Kinnell,	Palmerston Foundry, Dumfries.
1859, Dec. 21:	Robert	M'Laren,	22 Canal St., S.S., Glasgow.
	Andrew	*M'Lean,	Stobcross Dkyrd., Glasgow.
1858, Nov. 24:	Walter	M'Lellan,	127 Trongate, Glasgow.
	John	*M'Millan,	Shipbuilder, Dumbarton.
	William	*MacMillan,	3 Elgin Terrace, Dowanhill.
1859, Nov. 23	John	M'Nab,	Dumbreck Priory, Paisley Road, Glasgow.
Original:	William	M'Nab,	10 Hamilton Cres., Partick.

	John	*Price,	Rose Villa, Gateshead Road, Jarrow-on-Tyne.
1875, Dec. 21:	Matthew	Prior,	56 Ellesmere Rd., Sheffield.
1873, Apr. 22:	Richard	Ramage,	Rock Villa, Dumbarton.
1868, Dec. 23:	Henry M.	Rait,	Cranstonhill Foundry, Glasgow.
1858, Nov. 24:	Charles	Randolph,	14 Park Terrace, Glasgow.
1866, Dec. 26:	Daniel	Rankin,	Eagle Foundry, Greenock.
1872, Oct. 22:	David	Rankine,	157 St. Vincent St., Glasgow.
1869, Mar. 17:	James	Reid,	Shipbuilder, Port-Glasgow.
1868, Mar. 11:	James	Reid,	Locomotive Works, Springburn.
	John	*Reid,	Shipbuilder, Port-Glasgow.
Original:	James	Robertson,	Stanley St., Kinning Park, Glasgow.
1873, Jan. 21:	John	Robertson,	5 Kier Ter., Pollokshields.
1873, Apr. 22:	M. W.	Robertson,	387 Duke Street, Glasgow.
1867, Nov. 27:	Stewart	Robertson,	Barrow Shipbuilding Co., Barrow-in-Furness, Lancashire.
1863, Nov. 25:	William	Robertson, C.E.,	123 St. Vincent Street, Glasgow.
1869, Dec. 21:	H. O.	Robinson, C.E.,	34 Bishopsgate St. Within, London, E.C.
Original:	Hazltn. R.	*Robson,	(*President*) 14 Royal Cres., Glasgow.
1870, Oct. 25:	W. W. B.	Rodger, C.E.,	Bagatelle, Greenock.
1861, Dec. 11:	Richard G.	Ross,	21 Greenhead St., Glasgow.
1870, Jan. 18:	Alexander	Ross, C.E.,	Alva.
Original:	David	*Rowan,	(*Past President*), 237 Elliot Street, Glasgow.
1863, Mar. 4:	George	Russell,	Engineer, Motherwell.
1859, Dec. 21:	Thomas	*Russell,	Aberdeen Iron Works, Aberdeen.

1872, Jan. 30:	James E.	Scott,	Shipbuilder, Main Street, Greenock.
1860, Nov. 28:	Thos. B.	*Seath,	42 Broomielaw, Glasgow.
1875, Jan. 26:	Alexander	Shanks,	5 Derby Terrace, Glasgow.
Original:	Thomas	Shanks,	Engineer, Johnstone.
1872, Oct. 22:	Henry	Simon, C.E.,	7 St. Peter's Square, Manchester.
1858, Nov. 24:	William	Simons,	Renfrew.
1862, Jan. 22:	Alexander	Simpson, C.E.,	175 Hope Street, Glasgow.
1871, Mar. 28:	Hugh	Smellie,	Locomotive Superintendent, Maryport.
Original:	Alexander	Smith,	57 Cook Street, Glasgow.
1869, Mar. 17:	David S.	Smith,	Hellenic Steam Navigation Co., Syra, Greece.
1859, Jan. 19:	George	Smith,	Kennedy Street, Parliamentary Road, Glasgow.
1861, Dec. 11:	Hugh	Smith,	9 Kelvinside Terrace, N. Glasgow.
1874, Oct. 27:	Hugh	Smith,	57 Cook Street, Glasgow.
1873, Nov. 25:	Henry B.	Smith, C.E.,	Canadian Pacific Ry. Office, Prince Arthur's Landing, Lake Superior, Canada.
1871, Dec. 5:	J. P.	Smith, C.E.,	Haughhead Cottage, Govan Road.
Original:	William	Smith,	57 Cook Street, Glasgow.
1870, Feb. 22:	Edward	Snowball,	Engineer, Hyde Park Locomotive Works, Springburn.
Original:	Robert	*Steele, jun.,	Shipbuilder, Greenock.
	William	*Steele,	Shipbuilder, Greenock.
	John	*Stephen,	Linthouse, Govan.
1859, Jan. 19:	David Y.	Stewart	3 Provan Place, Glasgow.
1867, Jan. 30:	Duncan	Stewart,	47 Summer Street, Glasgow.
1874, Oct. 27:	Peter	Stewart,	53 Renfield Street, Glasgow.
1866, Nov. 28:	James	Stirling,	G. & S.-W. Ry., Kilmarnock.

ASSOCIATES.

	Thomas	*Aitken,	8 Dock Place, Leith.
	Andrew	*Armour,	68 Anderston Quay, Glasgow.
	T. S.	*Begbie,	36 Walbrook, London, E.C.
	Capt. Alex.	*Blackwood,	Leith.
	Capt. Jas.	*Brown,	11 Somerset Place, Glasgow.
1876, Jan. 25:	John	Brown,	11 Somerset Place, Glasgow.
1865, Jan. 18:	John	Bryce,	82 Oswald Street, Glasgow.
	William	*Calder,	Sailmaker, Leith.
1875, Feb. 23:	Daniel	Cocking,	202 Hope Street, Glasgow.
1870, Dec. 20:	Joseph J.	Coleman, F.C.S.,	69 St. George's Place, Glasgow.
	Robert	*Duncan,	Ironfounder, Partick.
1859, Nov. 23:	Arch. Orr	Ewing, M.P.,	2 W. Regent St., Glasgow.
	Alex.	*Ferguson,	5 East Breast, Greenock.
1863, Mar. 18:	Robert	Gardner,	52 N. Frederick St., Glasgow.
1865, Dec. 20:	James	Haddow,	54 W. Regent Lane, Glasgow.
1860, Jan. 18:	George T.	Hendry,	8 Dixon Street, Glasgow.
	Capt. D.	*Henderson,	Meadow Shipbuilding-yard, Partick.
1864, Dec. 21:	Anderson	Kirkwood, LL.D.,	12 High Windsor Terrace, Glasgow.
1865, Dec. 20:	John Jex	Long,	12 Whitevale, Glasgow.

Names marked thus * were Associates of Scottish Shipbuilders' Association at Incorporation with Institution, 1865.

	John	*M'Allister,	Mavisbank Sailwork, Glasgow.
1873, Feb. 18:	John	Mayer, F.C.S.,	6 Linden Terrace, Pollokshields.
1874, Mar. 24:	Peter	Marshall,	6 Park Grove Ter., Glasgow.
1874, Mar. 24:	James B.	Mercer,	Broughton Copper Works, Manchester.
	George	*Miller,	1 Wellesley Place, Glasgow.
	John	*Milne,	62 Marischal St., Aberdeen.
1865, Dec. 20:	John	Morgan,	Springfield House, Bishopbriggs.
	James S.	*Napier,	33 Oswald Street, Glasgow.
	R. S.	*Newall, F.R.A.S.,	Fernedene, Gateshead.
	John	*Phillips,	17 Anderston Quay, Glasgow.
1869, Nov. 23:	Capt. John	Rankine,	10 West Garden Street, Burnbank, Glasgow.
1867, Dec. 11:	William H.	Richardson,	19 Kyle Street, Glasgow.
1876, Jan. 25:	George	Smith, jun.,	200 Argyle St., Glasgow.
1874, Dec. 22:	Gordon	Smith,	133 W. George St., Glasgow.
	John	*Smith,	Aberdeen Steam Navigation Co., Aberdeen.
	John R.	*Stewart,	55 Anderston Quay, Glasgow.
1873, Oct. 28:	Michelangelo	Siciliano,	Via Cavour, Palermo.
	Malcolm M'N	*Walker,	45 Clyde Place, Glasgow.
	H. J.	*Watson,	62 Jamaica Street, Glasgow.
	T.	*Westhorp,	West India Road, London.
1860, Nov. 28:	James	Young,	Kelly, Wemyss Bay.
	William	*Young,	Galbraith Street, Stobcross, Glasgow.

1874, Feb. 24: C. R.	Harvey,	4 South Wellington Place, Glasgow.
1873, Dec. 23: Guybon	Hutson,	4 Woodburn Place, Glasgow.
1873, Dec. 23: Peter S.	Hyslop,	145 St. Vincent St., Glasgow.
1873, Dec. 23: David	Johnstone,	British Sanitary Co., 43 W. Regent Street, Glasgow.
1873, Dec. 23: Charles	Lindsay,	203 St. Vincent St., Glasgow.
1875, Dec. 21: Thomas	Lamb,	17 Woodside Pl., Glasgow.
1875, Dec. 21: Hugh	M'Laughlan,	29 Merkland Street, Partick.
1870, Dec. 20: Robert	Mackintosh,	c/o Jas. Mitchell, 118 Union Street, Glasgow.
1874, Feb. 24: Andrew	Maclean, jun.,	Whiteinch House, Partick.
1874, Feb. 24: William	Maclean,	Whiteinch House, Partick.
1874, Feb. 24: George	M'Farlane,	65 Gt. Clyde St., Glasgow.
1874, Feb. 24: Alexander	Malloch,	Linwood, Renfrewshire.
1875, Dec. 21: Allister	M'Niven,	Clutha Iron Works, Vermont Street, Glasgow.
1874, Feb. 24: Thomas W.	Meikle,	Broomhall, Partick.
1872, Mar. 26: Ebenr. E.	Millar,	Clyde View, Uddingston.
1873, Dec. 23: George	Miller, jun.,	268 Bath Cres., Glasgow.
1873, Dec. 23: John F.	Miller, C.E.,	1 Wellesley Place, Sandyford, Glasgow.
1875, Dec. 21: James S.	Murray,	3 Callander Place, Cathcart Road, Glasgow.
1873, Dec. 23: George A.	Newall,	4 Sardinia Ter., Hillhead.
1876, Jan. 25: Wm. M.	Ogilvie,	347 Temple Bar, Dumbarton Road, Glasgow.
1875, Nov. 23: William	Parker, jun.,	79 South Portland Street, Glasgow.

1876, Jan. 25: Thos. G. F. Palmer, Clutha Iron Works, Vermont Street, Glasgow.
1873, Dec. 23: Edward C. Peck, 33 West Cumberland Street, Glasgow.

1875, Apr. 27: Robert Rankine, 51 Gardner Street, Glasgow.
1873, Dec. 23: Alexander Reid, 12 Ogwen Street, Everton, Liverpool.
1873, Dec. 23: Charles H. Reynolds. Hamilton Terrace, (West), Partick.
1857, Dec. 23: Thomas Roberts, 51 Whitevale, Glasgow.
1875, Nov. 23: Hazn. R. Robson, jun., 14 Royal Crescent, Glasgow.
1875, Dec. 21: James Rowan, 22 Woodside Pl., Glasgow.
1875, Dec. 21: David Rowan, jun., 22 Woodside Pl., Glasgow.

1865, Oct. 25: Thomas Shaw, South Barr Colliery, by Paisley.
1875, Oct. 26: Magnus Sandison, 12 Minerva Street, Glasgow.
1873, Dec. 23: John Stewart, 90 Thistle St., Garnethill, Glasgow.
1873, Dec. 23: W. B. Stewart, 2 Provan Place, Glasgow..
1873, Dec. 23: John Sutherland, 36 Fenchurch St., London, E.C.
1875, Dec. 21: Andrew Stirling, °/₀ Mrs Aberdeen, 98 Dumbarton Road, Glasgow.

1872 Dec. 24: Cornelius Thompson, 17 Albyn Place, Aberdeen.
1874, Feb. 24: George C. Thomson, 77 Hill Street, Garnethill, Glasgow.
1868, Apr. 22: Alfred Thorne, 1 Lawrence Pountney Hill, Cannon St., London, E.C.

1874, Mar. 24: Robert Walker, jun., Lothamhill, Hogganfield, by Glasgow.

William Munro, General Printer, 81 Virginia Street, Glasgow.

PLATE 1

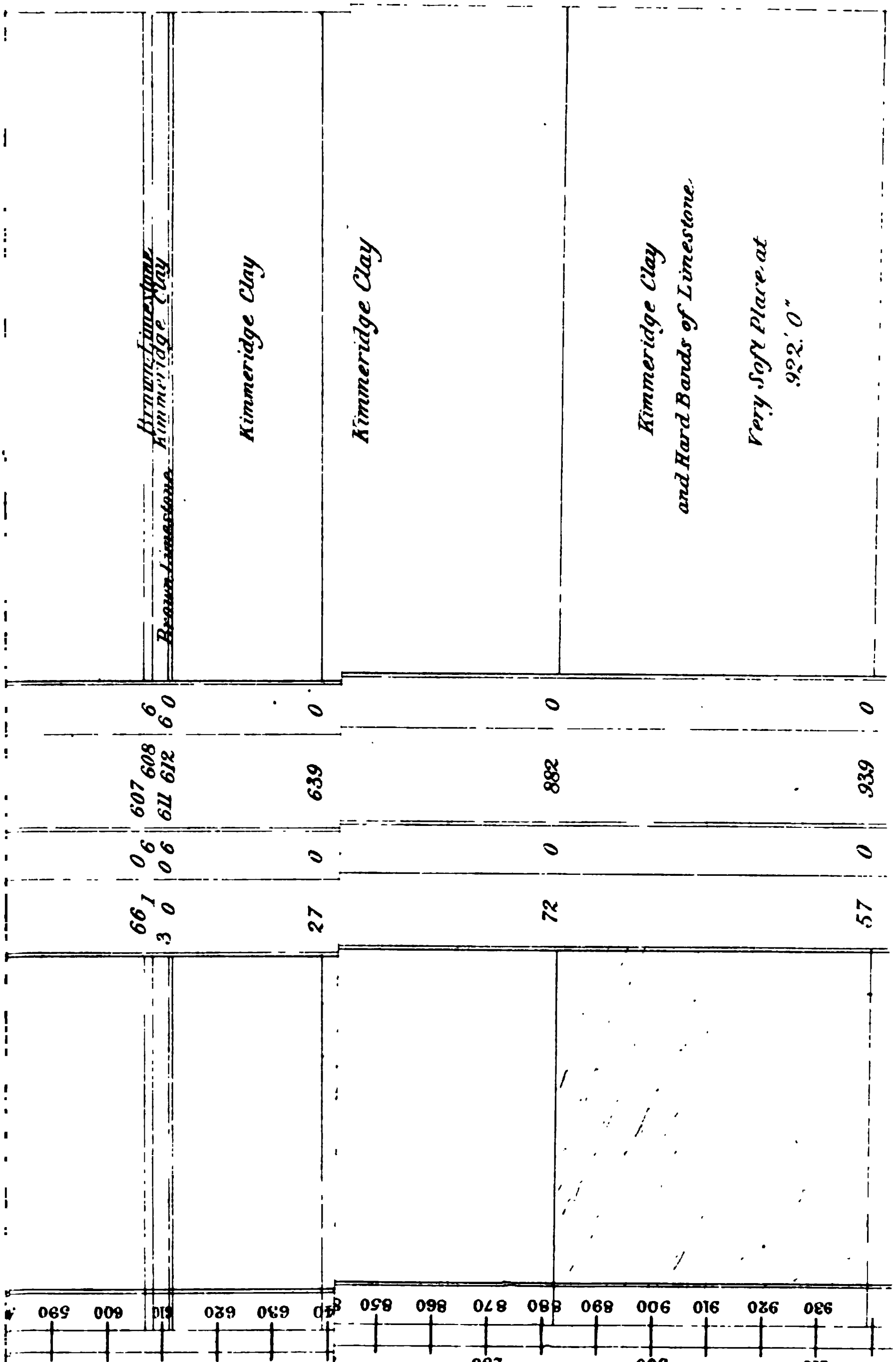
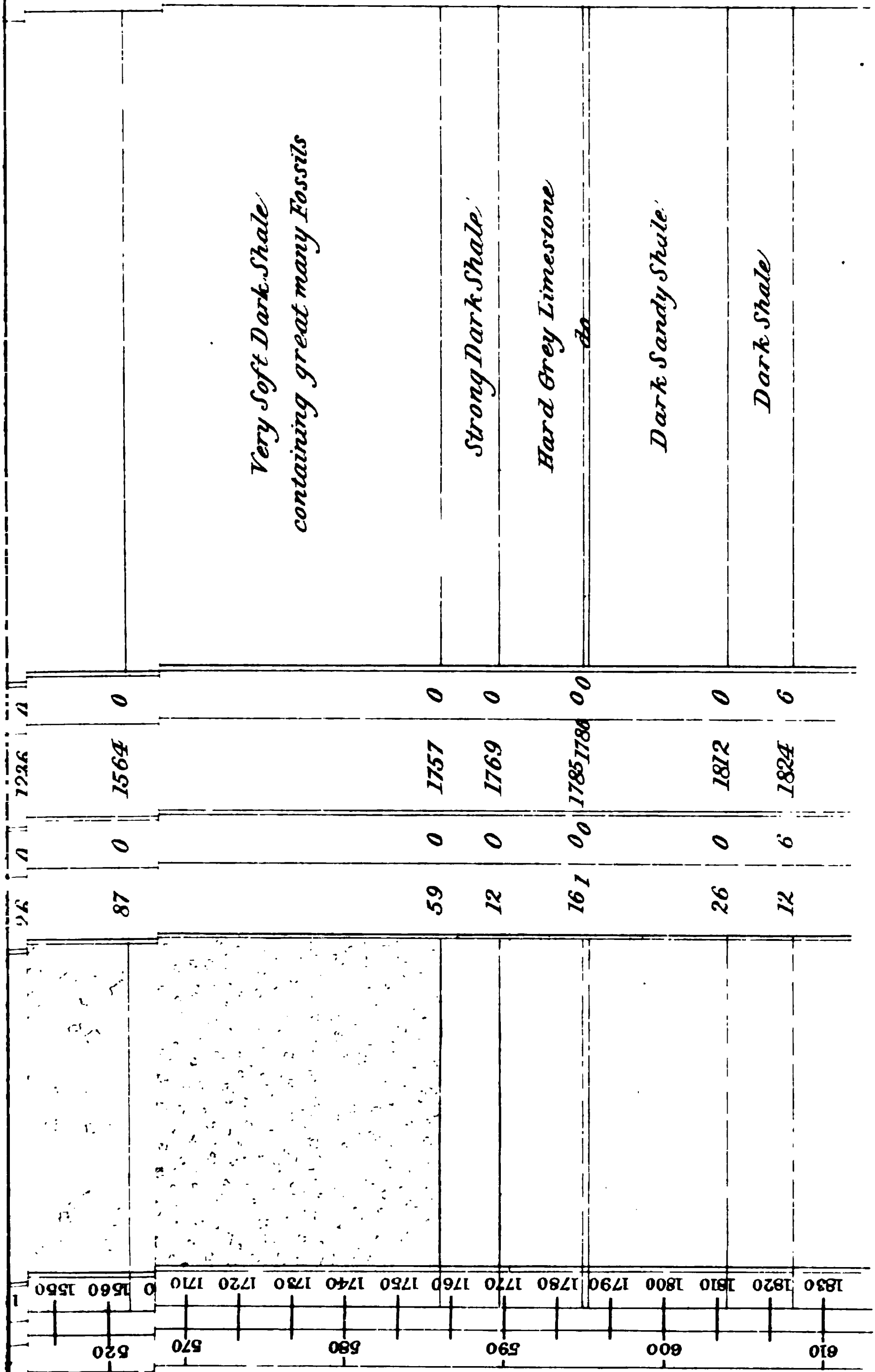

Brown Limestone
Kimmeridge Clay
Brown Limestone
Kimmeridge Clay
Kimmeridge Clay
Kimmeridge Clay
and Hard Bands of Limestone
Very Soft Place at
922' 0"
6 6 0
0
0
0
607 608 611 612
639
882
939
0 6 0 6
0
0
0
66 1 3 0
27
72
57
590
600
610
620
630
640
850
860
870
880
890
900
910
920
930

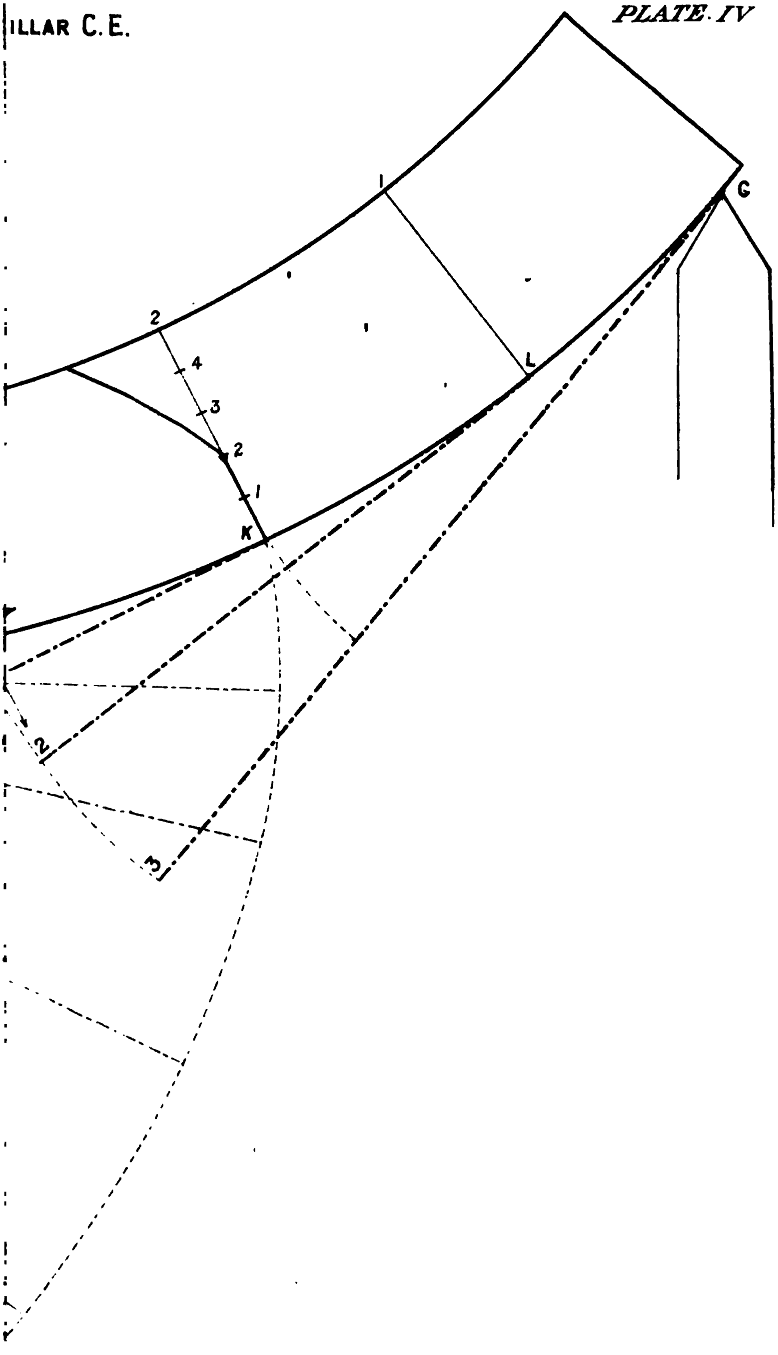
PLATE. II


Very Soft Dark Shale containing great many Fossils
Strong Dark Shale
Hard Grey Limestone
do
Dark Sandy Shale
Dark Shale
1236
87
0
1564
0
59
0
1757
0
12
0
1769
0
16
1
0
1785
1786
0
26
0
1812
0
12
6
1824
6
1550
1560
1710
1720
1730
1740
1750
1760
1770
1780
1790
1800
1810
1820
1830
520
570
580
590
600
610

Mr. M. Prior. *PLATE. III*

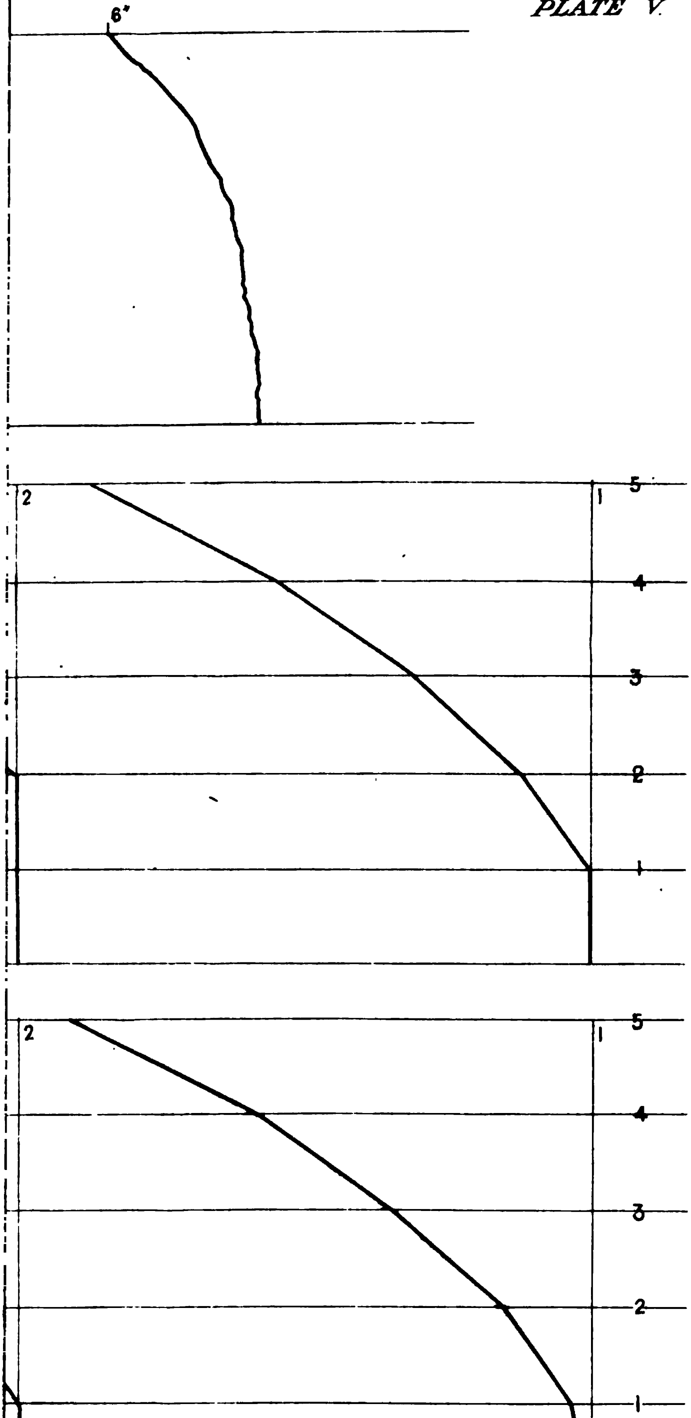
ILLAR C.E.

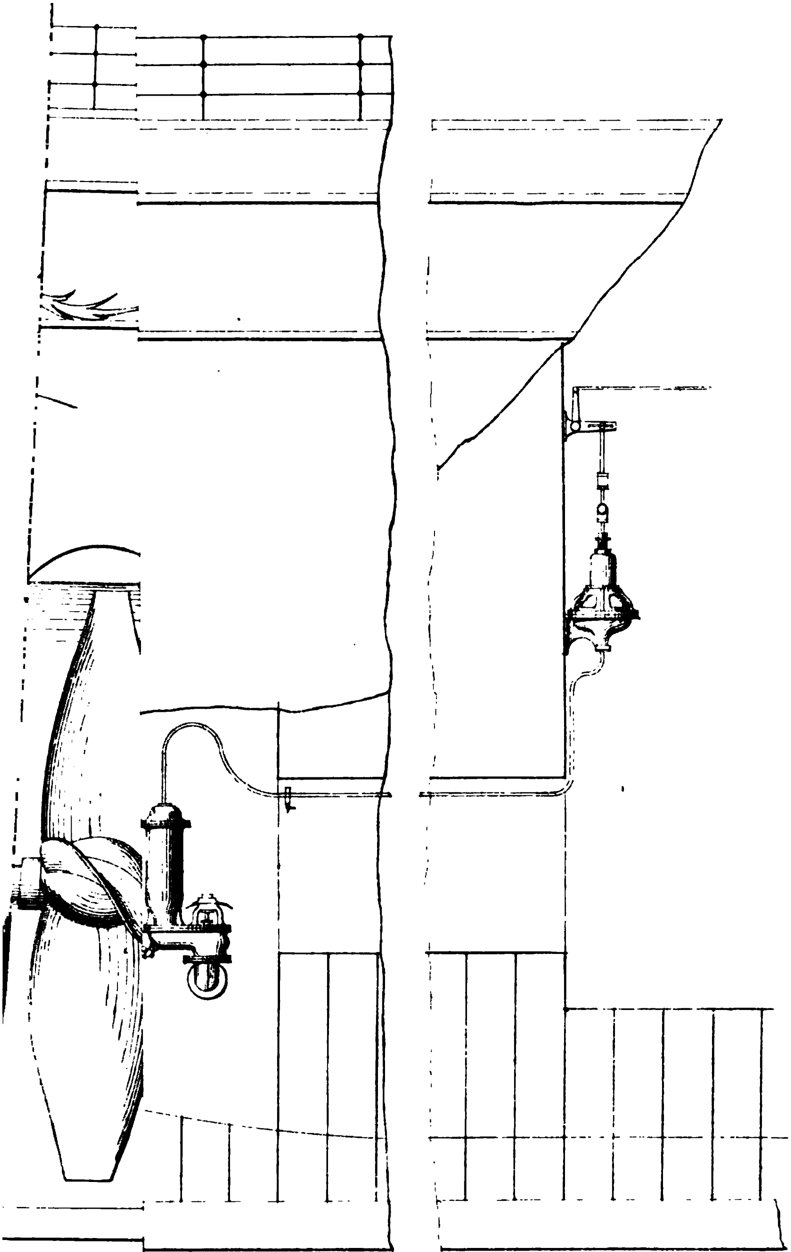
PLATE IV



AR C.E.

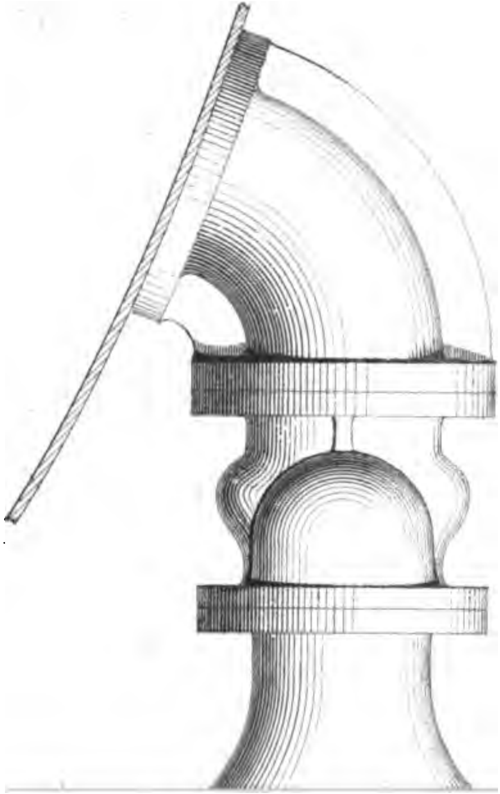
PLATE V.

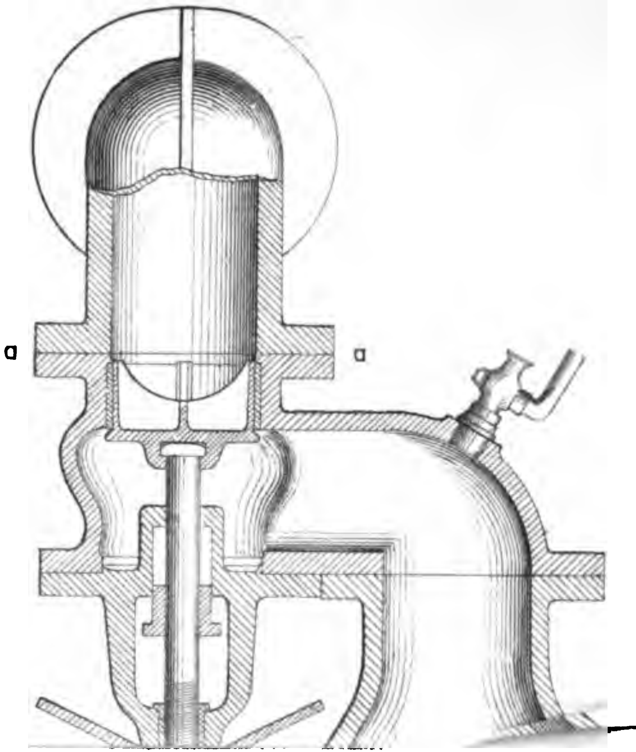




OHN DUNLOP.

PLATE. VII





H A NOZZLE."

PLATE. VIII

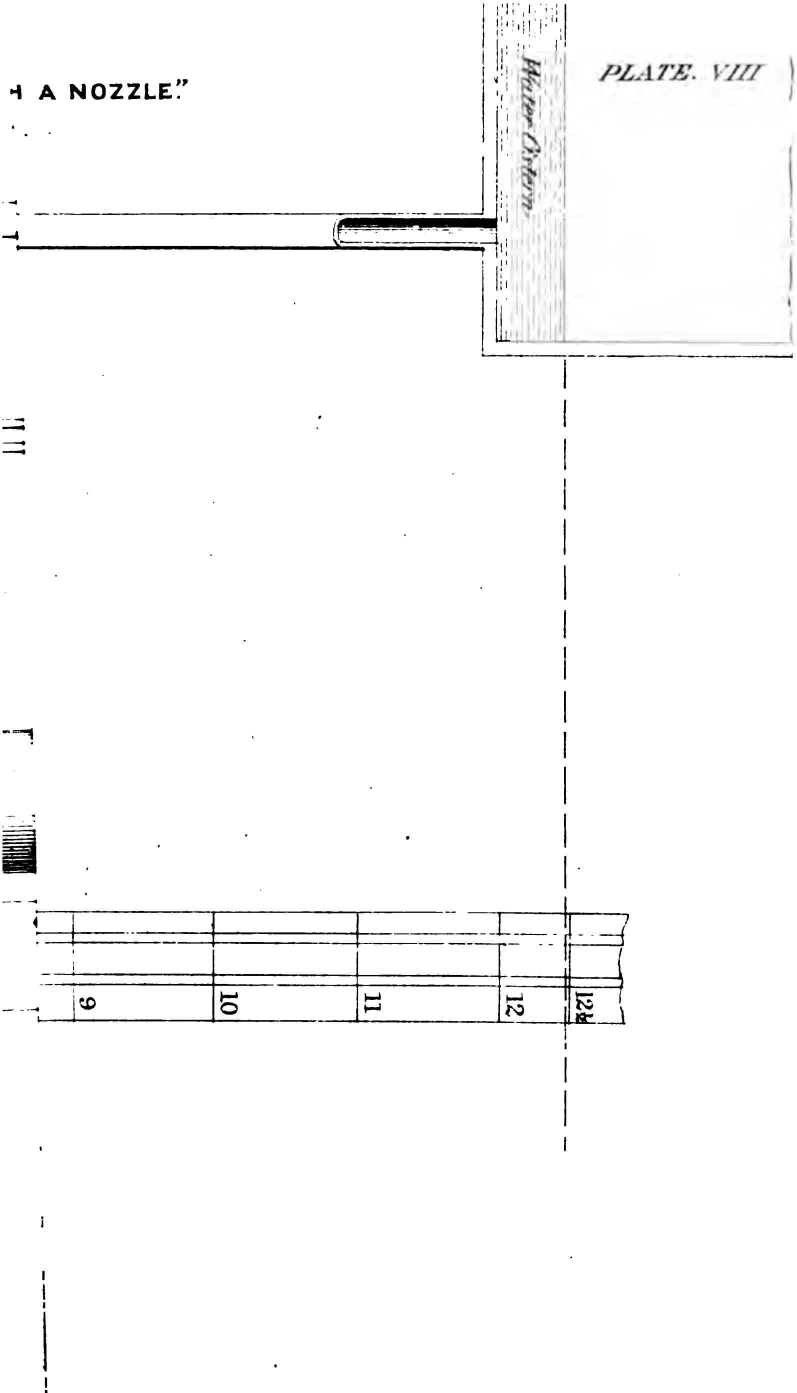
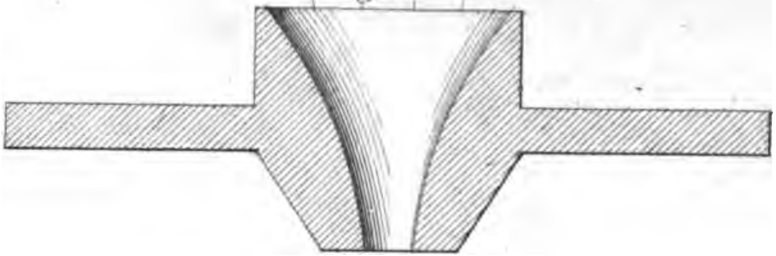


PLATE. IX

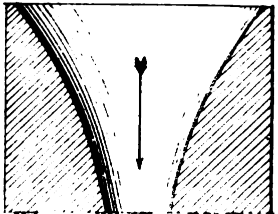
ITIES THROUGH A NOZZLE"

Fig. 3



FULL SIZE

Fig. 2

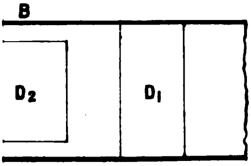


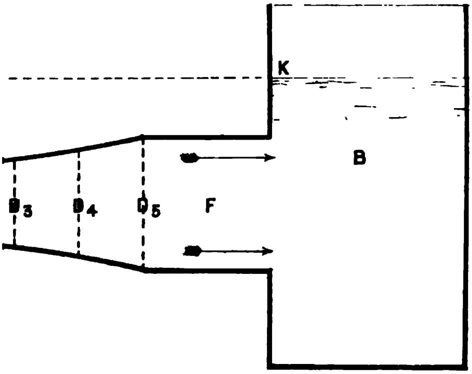
0 3·38 6·75 10·13 13·5 16·88 20·36 23·63 27 30·38 33·76

PLATE. X

S REMARKS

E'S PAPER.





Proceedings Institution of En

Scotland (Incorp

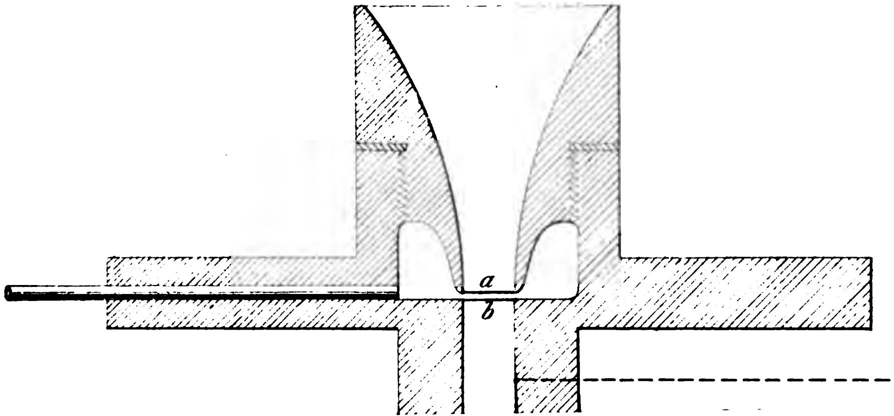
GARDNER & STEVENSON,
ENGINEERING LITHOGRAPHERS.
88 NORTH FREDERICK Sᵀ GLASGOW

To illustrate Mr M

discussion of Mr E

PLATE XI.

Fig. 1.



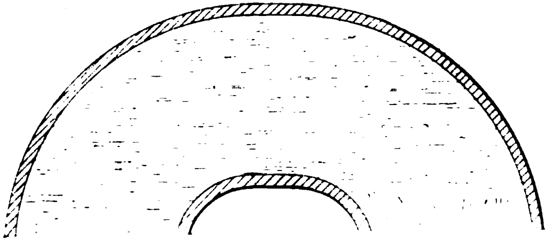
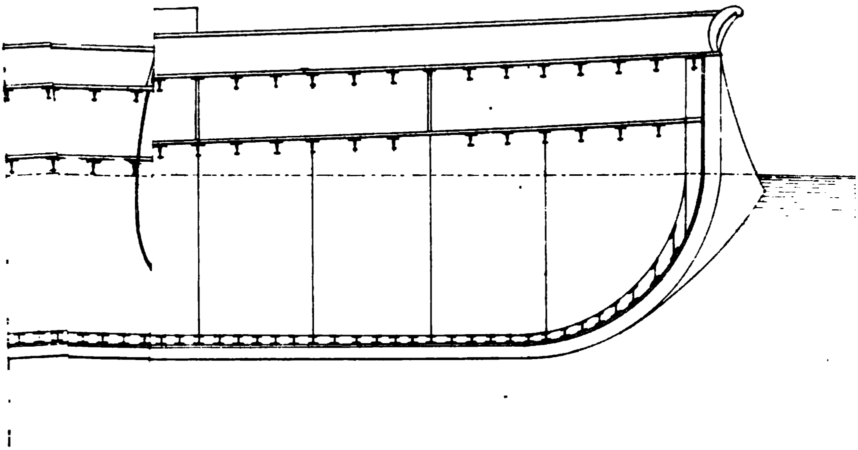


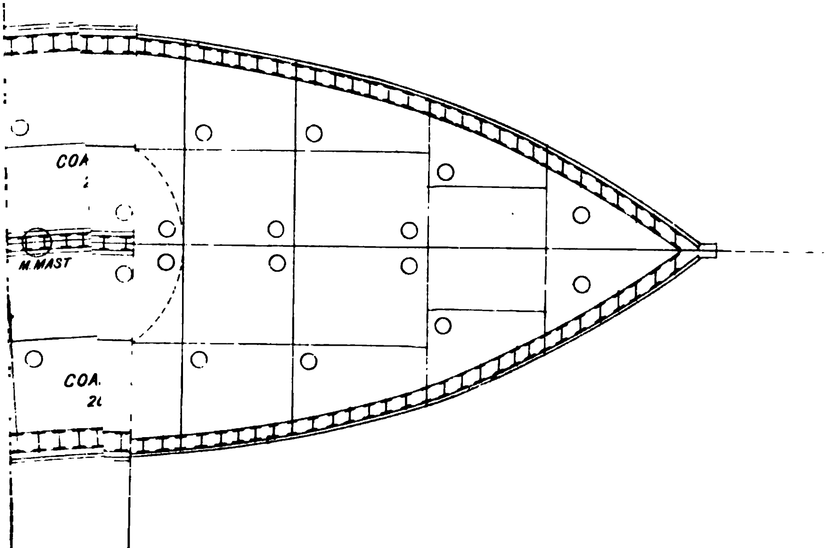
Fig. 3.

IN"A PROP



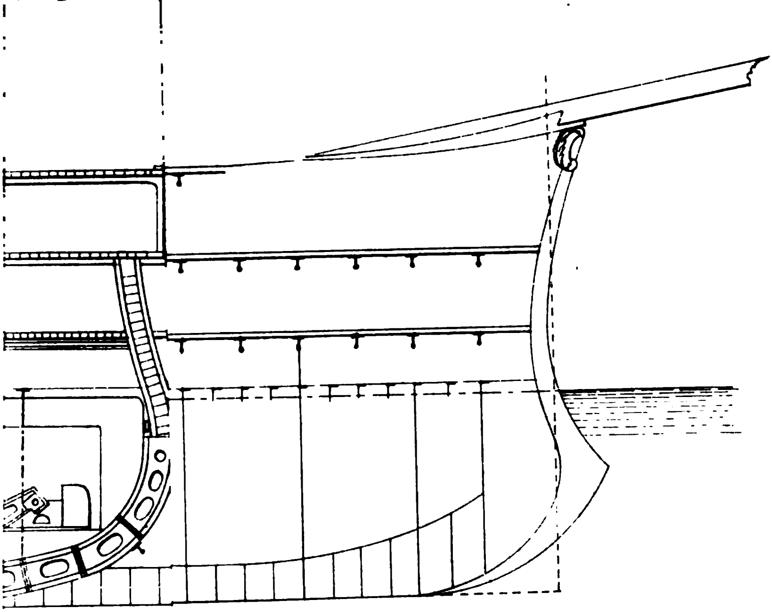
320 x 45 x 25

CALE OF 4 6 8



WAR" BY Mr H

PLATE XIII.

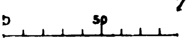


ar Principle.

0 x 54 x 40. 9

p of lower or 3rd deck beams

p of floors, 24' 1"



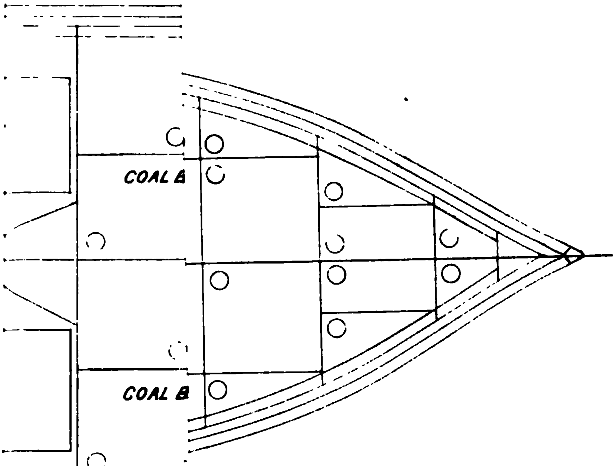
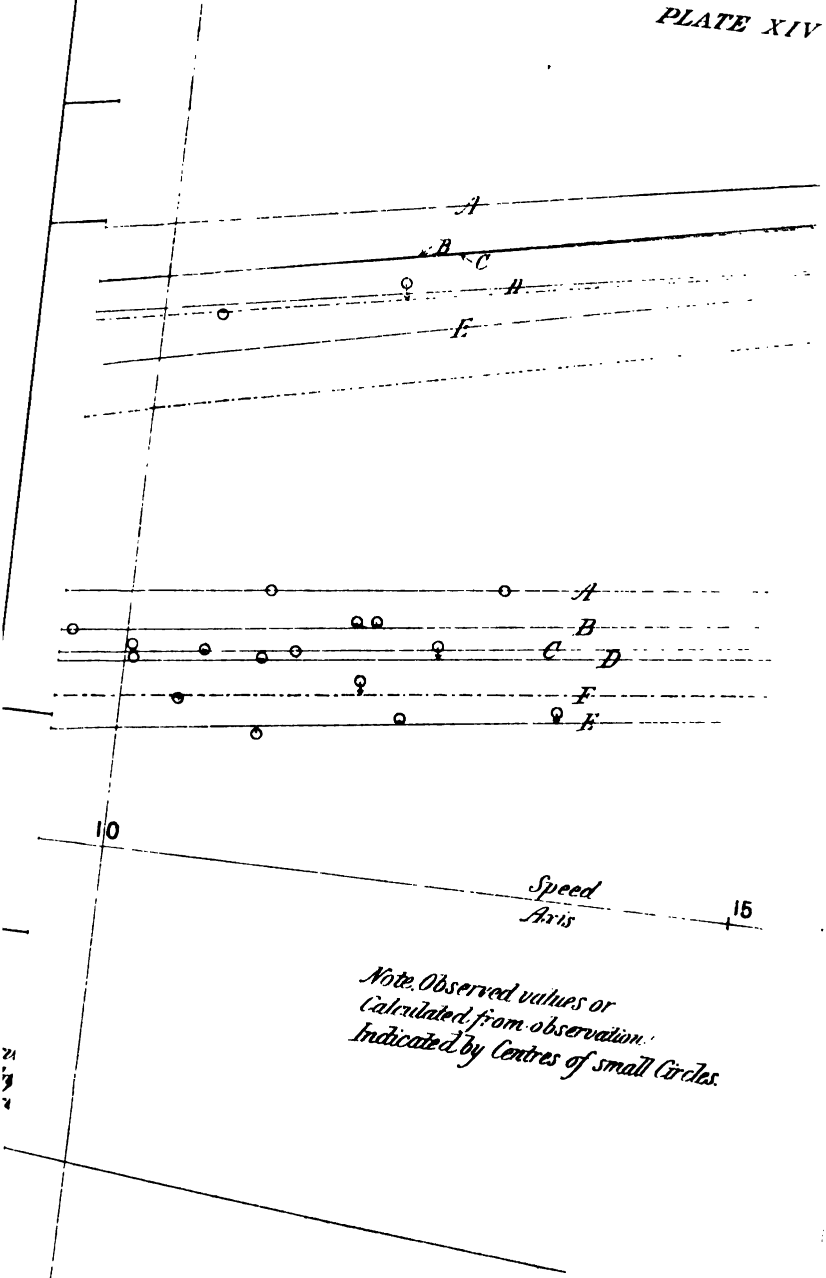
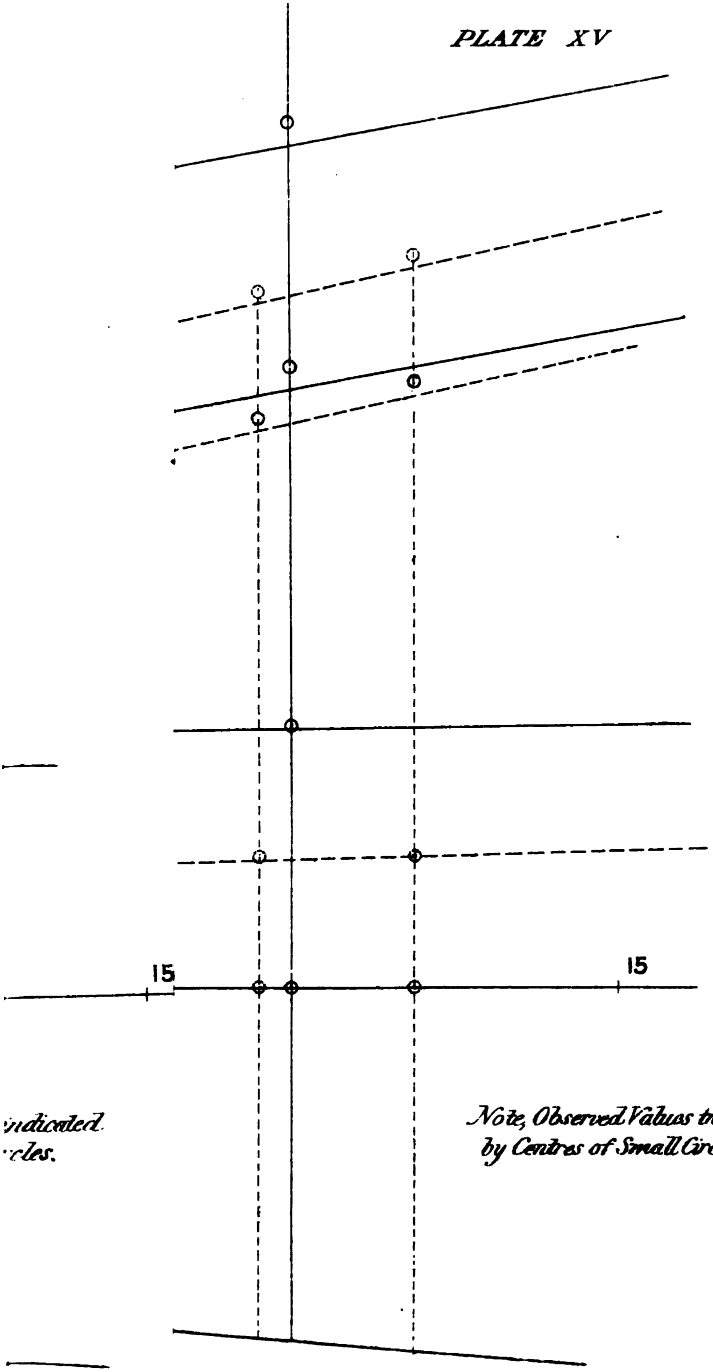


PLATE XIV



Note. Observed values or Calculated from observation. Indicated by Centres of small Circles.

PLATE XV

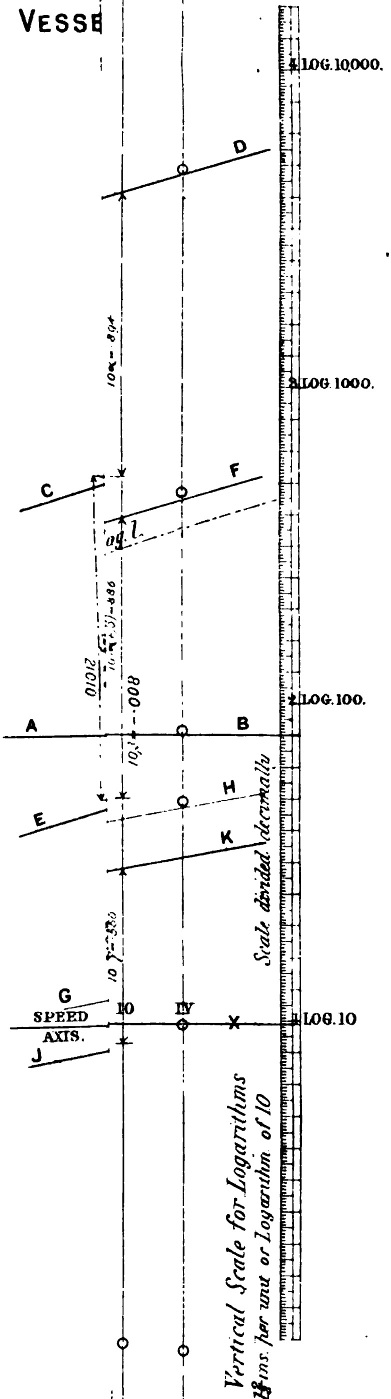


ndicated
cles.

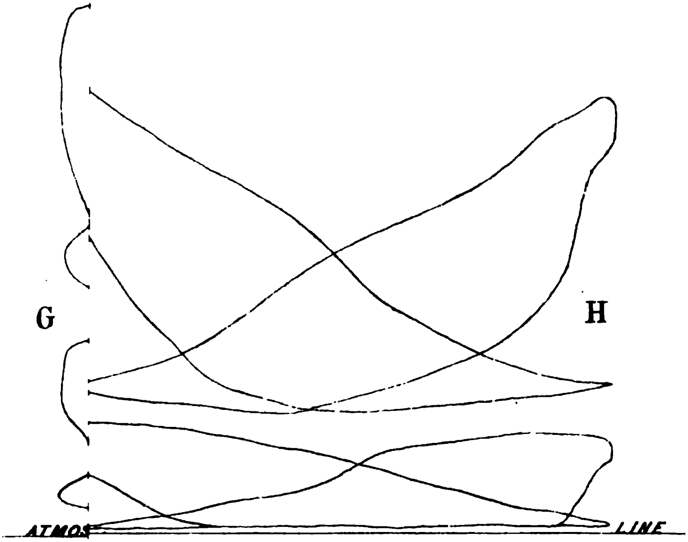
Note, Observed Values indicated by Centres of Small Circles.

OF STEAM VESSE

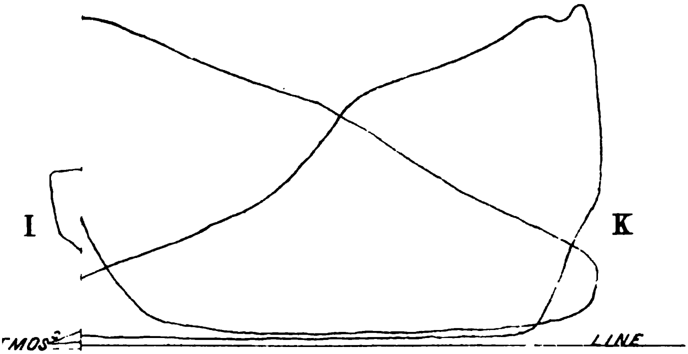
PLATE. XVI.



ms.



ams.



. . . . . *6 days.*
. . . . . . *80 lbs.*
. . . . . . *70 "*
. . . . *15·9 tons.*
. . . . . *20·1 "*

Fig. 5.