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The responsibility of the statements and opinions given in the following Papers and Discussions rests with the individual authors; the Institution, as a body, merely places them on record.

INSTITUTION OF ENGINEERS & SHIPBUILDERS
IN SCOTLAND.

(INCORPORATED.)

TWENTY-NINTH SESSION, 1885-86.

Introductory Address.

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

Read 27th October, 1885.

GENTLEMEN,

In one passage of the address which I had the honour of offering to you at the commencement of our last winter's session, I made some brief remarks in recommendation of a greatly more extensive and more frequent employment than has hitherto been customary, of the method of force-tests for attainment of safety, or for abatement of danger, in various kinds of engineering structures. That important subject, then only briefly touched upon, I have now chosen for the main subject of my address for this evening.

That there is great need of some kind of reform in our modes of procedure for attainment of due security against occurrence of disasters through failure of our engineering structures as to sufficiency of strength, cannot be denied, when the discordances of opinions and usages which prevail in engineering practice are taken into consideration; and when, moreover, the inadequacy of our means of judging of the physical properties and conditions of the materials employed, and the inadequacy, too, of our means of estimating the severities and the variations of the stresses to which they are liable to be exposed are sufficiently noticed and admitted. We have had recently Mr Baker (who is engineer, jointly with Mr Fowler, for the Forth Bridge) speaking in the Mechanical Section of the British Association at Aberdeen to the following effect:—

Taking as a keynote some sagacious words of Professor Rankine of thirty years ago, Mr Baker propounded the consideration of "how far the existing practical rules respecting the strength of metallic bridges are founded on reason, how far on custom, and how far on error." Under this inquiry, then, he had to start with the question: —What are the rules adopted by engineers and Government departments in this country and abroad at the present time?— a question which he had to tell us was by no means easily answered Having had communications for some time past with leading Continental and American engineers as to practices regarding the admissible intensity of stress in iron and steel bridges, he declared as a result that, "at the present time, absolute chaos prevails." The old foundations, he said, are shaken, and engineers have not come to any agreement respecting the rebuilding of the structure. He referred to the well-known fact that nearly all the large railway companies in our country are strengthening their bridges—a result not to be wondered at when he had to tell of his own knowledge of cases where the working stress on the iron has exceeded, by 250 per cent., that considered admissible by leading American and German bridge-builders in like structures. He stated as a practical result that a bridge which would be passed by the English Board of Trade would require to be strengthened 5 per cent. in some parts, and 60 per cent. in others, before it would be accepted by the German government, or by any of the leading railway companies in America.

Thus it would appear to be brought out that the margin of redundant strength for safety, aimed at in our country under our present rules and usages, is very imperfectly defined, and in many cases very small in amount. I have intentionally spoken of the margin *aimed at*, not the margin *actually attained*, because the calculation is necessarily surrounded in most cases with great complications of many uncertainties besides those incidental to the imperfections of commonly prescribed practical rules. To take the case even of one single rivet connecting two or more plates or bars in an iron bridge, I have to assert that it would surpass the powers, whether of our best engineers or of our highest mathematicians, to calculate the distribution of stress throughout the different parts of that one rivet, or the severity of the stress at the most important places—those unknown places where the stress may occur in the greatest severity. The same may be said of the material of the bars or plates in close proximity to the connecting rivets; yet it is in the rivets, and in the material round the rivet holes, that there is commonly more liability to failure, than generally in the larger portions distant from any of the joints.

But now, further, an additional subject of uncertainty and perplexity, formerly and even yet but little attended to in practice, has been coming to the front, at least among those who may be regarded as pioneers in efforts towards the advancement of mechanical science. I allude to the principle, which has come to be very much associated with the name of Wöhler, on account of his elaborate experimental investigations upon it, and his enunciated results; although the principle, indeed, does not appear to be claimed on his behalf as entirely a new discovery, but may be regarded as having been—in part, at least—appreciated by many before. That principle, in so far as I have to refer to it at present, may, I think, be described as being mainly to the effect that:—Fracture may be produced by the frequent alternation of varying stresses, although the utmost stress so applied might be considerably less than what would break the material by a single steady application, whether of brief or long continued duration. Also, that in the alternations of the stresses, the wider the variations be made in diminution from the recurring maximum, the further must that maximum be kept below the stress of the ordinary breaking load, if the varying stress is wanted to be endurable for a vast number of repetitions, such as are liable to occur during many years of ordinary usage of our structures.

I am not myself prepared either to vouch for the truth of the conclusions referred to, or to neglect them as if judged to be entirely devoid of foundation in fact. The subject is very obscure. Already so many remarkable and unexplained or imperfectly correlated phenomena have presented themselves to experimenters, that I must prefer, for the present, on this subject to suspend judgment.

However, it may readily be understood that any such conclusions as those which I have briefly, and, I must add, imperfectly sketched out; in so far as they be admitted as true, and accepted as elements for guidance in the designing of engineering structures, must introduce immensely complicated additional considerations into the requisite investigations. But, furthermore, such conclusions, if reliable, must tend to produce undefined doubts as to whether, in particular cases, there be continual degradation by some occult physical changes supervening in the material of the structure, and altering it for the worse from its original condition, scrutinised perhaps with great care during manufacture. The result must be, in course of time, that the supposition of changes thus accruing must entail great uncertainties and doubts as to the capabilities of the structure for endurance of its working loads and other rough usages, when any such changes can be suspected to be in progress. We cannot scrutinise with any completeness changes towards brittleness or crystallisation in the multitudinous rivets of a bridge, or in the plates between the rivet holes. When weakness begins to be suspected through fears lest such changes may possibly have supervened injuriously in some unknown parts, where are we to seek for any comfort? I answer: that if such doubts be originally well justified, it is by force-tests above all other means, but still conjointly with all manageable aids from inspection and consideration and calculation, that such doubts can be dispelled and security be attained.

The truth as to the supposed physical phenomena, which for brevity we may speak of as the Wöhler changes, is certainly very obscure, or it lies among obscure surroundings; but there are other modes of deterioration which present themselves more obviously to our consideration, and which concur in leading up to force-tests as our most trustworthy guarantee for security.

When the forces to be transmitted through a riveted joint are variable in amount, and still more when they are reversible in direction, any one of the rivets must come to be exerting stresses variable in amount and in kind. But the rivet cannot vary its exertion without variation of its own shape. The like may be said equally of the metallic plates close around the rivet holes, and connected by the rivets. It then obviously follows, in consequence of such changes of form, that slipping in some degree, even though invisible to the eye and imperceptible to the touch, must occur between faces pressing hard in mutual contact. The slipping or creaking and other changing actions, at faces heavily pressed, can scarcely be supposed to occur without producing cumulative effects in the material heavily pressed at the faces of contact during multitudinous repetitions of the changing action. When the forces are too severe, the riveting, if not actually torn, soon becomes loose, and this is no uncommon case in practice. In other cases, the loosening may only show itself after long usage; and often the forces may not suffice to bring it into notice even after long periods of time. Such changes as these, which may be referred to the ordinarily supposed properties of limited range of elasticity and ductile yielding beyond the limits of elasticity, are in many kinds of structures perhaps more to be dreaded than the supposed liability to the Wöhler change in the material. Also we may suppose that the two modes of change can go forward together, mutually increasing one another. In such ways the grounds of confidence may be gradually subsiding that we might tolerably well have relied on when the structure was new and fresh, and when the qualities of the material and the structural adaptations might be supposed to be fairly well in accordance with the calculations for the original design.

The next mode of deterioration to which I have to advert is that of rusting. Now, whatever views may be held on the supposed influences of variations of stress in the material in producing deterioration of structures, there can be no denial of rusting as being a veritable mode of deterioration, fraught with danger of calamitous disasters.

Against the insecurity that may arise through rusting, no satisfactory safeguard is available through skilled calculations in the original designing and watchful scrutinies in the execution of the work. The valuable results of scientific principles, factors of safety, and empirical rules and formulas, used for deciding on proportions and dimensions for the original design, all go to the winds when unknown and unascertainable departures from the original forms and dimensions are irregularly introduced by the corrosion of rusting.

In such circumstances we have one method, and one alone, for attainment of a guarantee for safety in our continued use for some time longer of the structure in case of its being intrinsically strong enough, if we are ignorant as to whether it be sufficient or not. That one method is the method of force-tests conjointly with the utmost available aid by inspections and considerations.

Under some of the prevalent usages, and without any intention of recourse being taken to force-tests, if a bridge shows some progress of rusting, and has come to be doubted as to its sufficiency, the duty may be entrusted to an engineer to examine into the case, and to express an opinion as to the sufficiency of the bridge for traffic. He may then look at the parts that can be exposed to view—he may observe the behaviour of the bridge as to tremors and deflection during the passage of an ordinary heavy train—he may drill small holes at some parts, and find the thickness left by rusting at those parts—and he may have little further means of judgment on which to found his report.

Suppose he has doubted what to advise, and ultimately has decided to condemn the bridge, but could not say that it might not be fit for many years service yet. Now, why not apply suitable force-tests severely, and so save the bridge, and the company's money? In particular cases, indeed, there may be good reason for absolutely condemning the bridge when it has begun to be really doubted. The continuing of it in use until sooner or later it should break down suddenly under a test load, might be judged to entail too great inconvenience by stoppage of the traffic when the event would occur without a new bridge being in readiness, and all other preparations made for quickly replacing the old bridge by a new one. Still, even if this view were dominant, the fact of condemning the old bridge on grounds of supposed insufficient strength, is liable to throw doubts over its safety, even for present use, during the preparation of the new one, and such doubts may often best be allayed by application of force-tests, moderately in excess of the severity of the working traffic.

During the last 35 years wrought-iron bridges have sprung up in countless numbers throughout all parts of the world; and it is an indubitable fact that means and arrangements for their long-continued preservation by painting, or otherwise, have in many cases not been sufficiently provided and promptly and perseveringly used. Very frequently their design and construction have been such as to leave some parts of them inaccessible for inspection and for scraping and painting, but exposed to rusting. Many have, in consequence, been timely removed and replaced by new ones; and some, doubtless, have been retained in use while in an unsafe condition.

On the other hand, great durability may be attainable when sufficient arrangements are provided for protection against rusting by painting or otherwise.

The oldest of the great wrought-iron bridges of riveted work in the world—the Britannia Bridge across the Menai Strait—was very skilfully designed in this respect. The cellular structure of the top and bottom members of the great tubular beams of which that bridge is composed, was, by wise foresight and careful attention, arranged so as to have the interior of the cells accessible for periodical inspection, and for scraping and painting; and I am enabled to state, on the authority of the engineer who has charge of that work, that up to the present time (35 years since it was erected) no deterioration whatever can be detected.

There have been some accidents with cast-iron girders in bridges; and, there being a general consensus of opinion that cast-iron is an unreliable material when subjected to tensile stress, restrictions have been imposed by the Board of Trade on the use of cast-iron for railway under-bridges. As regards wrought-iron bridges, it is satisfactory to find that there is no record of any accident having occurred on British railways through failure of strength under the ordinary working loads of the traffic, whether by original insufficiency, or by rusting.

In respect to the terrible disaster of the Tay Bridge, it is to be recollected that the most critical parts of the bridge—the lofty and slender piers—were not of wrought-iron riveted work, but a combination of wrought-iron and cast-iron work. Still, in respect to that case, it is highly probable that the application of well designed and searching force-tests would have brought down the bridge at the time of testing and averted the real disaster which occurred under the action of a violent storm conjointly with the passage of a train.

In the course of long periods of time, even in well constructed riveted ironwork, maintained with careful attention to painting, it is to be expected that, in the chinks between the overlapping parts at the riveted joints, rusting will in some cases penetrate, and the swelling rust will exert a strong bursting tendency between the pieces connected, and introduce new and uncertain stresses in the rivets.

For many reasons, then, it appears, that the periodical application of judiciously arranged force tests would greatly counteract the dangers and uncertainties as to the sufficiency of bridge structures, on which the lives of the public generally, and large pecuniary interests of the Companies, have to be staked.

The method of force-tests, it must be noticed, is not free from some difficulties and objections of its own, and it is subject to some indeed which affect it in common with modes of procedure in which it forms no part.

One of the worst of such difficulties presents itself in cases where the application either of force in ordinary course of working, or of extra force in testing, when cracking occurs without complete breakage, and the material in its altered form and changed internal condition remains capable of resisting a greater—sometimes it may be a much greater—force than that which began the cracking. The condition so arrived at is generally very insecure. Under varying subsequent applications of force the cracking, when once begun, is very sure to advance, till ultimately complete fracture occurs. This is often exemplified in the testing of chain cables. A link may crack, the pulling force may then be increased, and the crack may open wider, the link still bearing the increased force, and the same may be continued till a great increase of resisting capability is attained in a cracked link or ring included in the chain. I have seen, for instance, a shackle, in connection with a cable, crack with a pull of 26 tons and continue to hold together with increasing cracks until 46 tons of pull was attained without its breaking loose, and I have seen like behaviour in links of chain cables. For such reasons as these the utmost aid available through inspection should be used in connection with testing. Still, however, if I have to sit often under a gasalier hung with pulleys, chains, and weights, I would consider that my position was rendered much safer by my applying occasionally a process of force testing to the chains even without minute inspection of the individual links. The links might, in course of time, without such testing, fail in strength and become cracked, through merely the action of their own suspended weights, or through that action jointly with a gradually arising brittleness which in some brass chains supervenes. If such were the case the complete breakage would most likely occur at a time of working the gasalier up and down with the chains strained by the increased test loading, rather than in ordinary work.

Breakages of the shafting between the engine and the screw propeller in ocean-going ships are very frequent; and the question may be asked whether greater safety could be obtained by employment of some system of force-tests. I would be very glad if such should be found manageable. I shall not discuss the subject on the present occasion, but shall close my address by merely bringing under your notice a case of a broken shaft where the severance seems to have arisen in some unknown way, for the early checking of which probably no sufficient safeguard could have been provided on any system of force-testing.

The PRESIDENT, in concluding, placed before the meeting a piece in form of a circular disk about an inch thick, cut from a propeller shaft, and showing on one side the crack or severance which caused the withdrawal of the shaft from service in the ship. He was indebted to a fellow member, Mr Davison, for the loan of this specimen, and for the information which he was enabled to offer to the meeting, regarding the shaft and its cracking, or severance,

The propeller had been keyed on the piece broken away; and that piece had been subsequently cut up into small pieces for testing as to strength and ductility. The propeller part had not broken off at sea, but, on an occasion of inspection, the crack had been seen extending round the circumference at the termination of the cylindrical brass casing (or liner), and close to the propeller boss: and the shaft was taken out as unfit for use. The shaft was a Siemens Martin steel shaft; 11 inches diameter: and though of steel, it had been made of the usual size that would be required for an iron shaft for the same sized engine. A piece of the material of the shaft had been tested when new, by David Kirkaldy, and a portion of the cracked shaft, close to the severance, was also tested by him after removal from the ship, and complete breakage across, by cross bending forces applied by a hydraulic press, the fissure formed at sea not having gone through. On this breakage being effected, the fissure or set of severances formed at sea was seen extending inwards, all round the circumference, for distances towards the centre varying from about ½ inch to 1½ inches. There was not, in the formation of the shaft, anything of the nature of a circumferential re-entrant angle or groove, such as, when not rounded off, may frequently tend to give origin to cracking and breakage.

There was no practical difference found between the original test results and the test results from the broken piece, so that the material had not become brittle nor deteriorated. The steel was about 50 per cent. stronger than iron, and so soft that under tensile stress it extended 19 per cent. on a length of 10 inches of 1⅛ inch diameter bar, before it broke. The shaft had no outer bearing (no bearing behind the propeller). It had been running about two years. A sister ship built at the same time, had been running during the same two years in the same trade, and its shaft, which had been made alike with this one, continued all right. The breakage occurred about six months ago. Mr Davison attributes the phenomenon "*entirely to some kind of corrosion assimilating to a crack;*" but did not think the severance was of the nature of a crack: and he is of opinion that the means for preventing such damage to propeller shafts is to be sought for through exclusion of the sea water from contact with that part of the shaft. Professor Thomson considered the phenomenon very remarkable, and practically very important, but was not satisfied as to how it was to be accounted for. He invited Mr Davison to bring this case more fully before the Institution conjointly with other observations of his on this and allied subjects.

On the Butt Fastenings of Iron Vessels.

By Mr Staveley Taylor.

The discussion of this paper, which was read on the 24th March, 1885, was resumed on 27th October, 1885.

In reply to the President,

Mr Staveley Taylor said that he had very little to add to what he had ventured to say in the paper. The motive which prompted the paper arose from a conviction that the butt connections ordinarily employed in iron vessels are deficient. The President in his address that evening referred to the primary value of the riveting as a factor governing the strength and durability of bridges or similar structures. He thought his remarks were specially appropriate to the subject of the paper. He believed it was acknowledged that engineers gave greater attention to the fastenings of the structures with which they were concerned than shipbuilders did. He was afraid the latter gave too little attention to the proper connection and fastenings of the plates of a ship, that was to say, the fastenings did not receive due attention in proportion to the strength of the plates, and were too often treated in a mere rule of thumb manner. The rules of the classification societies no doubt governed their practices very largely. His contention was that practices which provide in no instance, irrespective of thickness or section, for more complete connection than that provided by double or treble riveting are in principle wrong. He was pleased to observe that, in the discussion that had already taken place on the paper, the views he had expressed were generally confirmed. What remedy might be best applied to improve the state of affairs he would not venture to define; but he thought that if butts were fitted with double straps a great increase of strength would be the result. From inquiries he had made of cases in which straps were so fitted, he found that the extra resistance looked for in diminished speed was practically *nil.* He thought that if this principle was more generally applied we should hear of fewer suppositious instances of vessels having strained on account of started butts, and should be able to build vessels of perhaps lighter scantlings, but equally capable of doing their work, to the greater profit both of owners, underwriters, and all concerned. He begged to thank those gentlemen who had spoken to the paper for the remarks they had been good enough to make.

Mr J. H. **Biles** said the question of butt fastenings seemed to him to lie at the base of the commercial value of the weight carrying of ships, because for many purposes the strength of the butt was really the strength of the ship. But there were other considerations which in some degree modified that principle. The question of the tensile strength of a butt was not always the most important to be kept in view, for sometimes the thickness of the parts had to be considerably increased to meet strains or work that might result in merely local damage; for instance, a ship that was liable to frequent grounding might have the thickness of the plating quite sufficient for general structural strength, but insufficient for the wear and tear involved in grounding. The question of butt fastening in that case had not the same close relationship to the main structural strength of the ship which it would otherwise have. He therefore thought that the question as treated of in the paper partook more of this main and general character than of the local character to which he had adverted. Giving proper attention to the amount and nature of butt fastenings might make up to a great degree for any deficiencies in the plating itself. But the proper selection of dimensions and form of the vessel was of most importance in securing proper strength. In this way, the amount of stress which had to be resisted might be reduced to a considerable extent; but the question was so intimately mixed up with the proper selection of dimensions and form of ships for the work which they had to do, that he thought the question of butt fastenings had, in the light of these considerations, to be carefully gone into on its own merits in the case of every special type of vessel. He agreed with Mr Taylor that the butt fastenings of vessels were too often determined by rule of thumb; of course, the Classification Registries could not deal with each ship on its own merits, and therefore they had to make rules which would meet the requirements of a fair general average of cases. The question of the best form of butt fastenings—covering as it does the proper and proportionate amount of the fastenings in relation to the plates which had to be fastened and the nature of the material which was employed, by which he referred to the fact that steel was now so much more frequently used than formerly, and consequently involved the question in greater difficulties—was one that had not hitherto received sufficient consideration or proper treatment.

Mr **ALEXANDER C. KIRK** said,—With reference to Mr Taylor's remarks on the use of double butt straps in ship work, the point before the meeting was how to get the maximum duty out of the riveting and the material riveted. Above all, these joints must not work and work slack. When riveted joints were subjected to alternating strains it was only the friction of the plates that held together by the rivets that we had to depend on to effectually resist these strains. This was pointed out long ago by Mr Edwin Clarke. When the strain to which the rivet joint is exposed exceeds the resistance due to this friction the joint will slip more or less. When the strain is reciprocating, first in one direction and then in the other, though the joint works at first but little, as the process goes on this slipping gradually increases. The result eventually is a bad joint, necessitating fresh caulking, and this process goes on without end. Without enough rivets, and sufficient surfaces in contact, the best riveting will not avail, and Mr Taylor was quite right in advocating double butt straps when applicable and the strains severe. For my own part I value a double butt strap not so much because it puts the rivets in double shear, but because it doubles the friction which holds the plates immovably together.

Mr **HENRY M. NAPIER** objected to Mr Taylor's assumption that shipbuilders treated such matters as butt fastenings in a rule of thumb way as compared with engineers, whose work was of a more straight-forward nature, and more easily dealt with. While no doubt double butt straps were good and efficient, yet their adoption, he thought, was open to objection, inasmuch as for vessels coming up rivers and liable to grounding, it would be a serious matter for the outside straps to be rubbed off as a whole—an eventuality which was quite possible. He thought, therefore, there were serious objections to butt straps being fitted on vessels below the water mark.

The PRESIDENT remarked that he had read the paper with pleasure, and had been much interested in the discussion to which it had given rise.

On Sinking the Cylinders of the Tay Bridge by Pontoons.

By Mr ANDREW S. BIGGART.

In closing the discussion, on 27th October, 1885, on this paper, which was presented to the Institution on the 28th April last,

The **PRESIDENT** said that it was a very important paper on a subject which they ought to have in their Transactions, more especially as it had been written by one who had had so much to do with the supervision of the erection of the Tay Bridge.

On the Forth Bridge Great Caissons: their Structure, Building, and Founding.

By Mr ANDREW S. BIGGART, C.E.

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

Received and Read 27th October, 1885.

LARGE cylinders are, in this country, so seldom sunk by means of air pressure, that opportunities for observing the progress of such work rarely occur. Still rarer, anywhere, are caissons of so gigantic dimensions as those which have been employed in the Forth Bridge. Yet we may safely say, never was air pressure used to greater advantage, or with more success, and this principally on account of the plant employed, whether we look at the design of the caissons themselves, or the various appliances used during their state of transition, from their manufacture till they were but a sheath around a block of concrete, weighing in some instances well nigh 15,000 tons.

Owing to the great depth to which some of the caissons had to be sunk, and the nature of the river bed, no ordinary methods were equal to the necessities of this part of the work.

In this paper we propose to confine ourselves mainly to the consideration of the group of four piers on the South Queensferry side of the river, and the two south piers on the island of Inchgarvie, for in these piers alone resort was had to the pneumatic process of founding, and that applied by means of specially constructed caissons.

A word will suffice to dismiss the Fife and remaining Inchgarvie caissons. These are in each case only 60 feet in diameter, and being in comparatively shallow water, were all made within or formed part of the coffer dam into which the pier was built.

Of the Queensferry group of caissons the one sunk to the greatest depth reaches to a point 89 feet under high water level, while that of the shallowest is 71 feet. All in this group are 70 feet diameter at the base, 68 feet at a point 18 feet under the top, while at the top they are only 60 feet.

On the Queensferry side the bed of the river commences with a stratum of soft mud, of varying thickness, and sinking was continued till the caissons were through this layer, and well down into a stiff boulder clay.

Of the two Inchgarvie south caissons, the south-west is the deeper, being 70 feet under high water level. Both are of the same diameter as the Queensferry caissons, and in outward form very similar. The bed of the river is, however, very different from that on the Queensferry side, being the bed rock of the Firth, which here is a hard whin. Passing to the caissons proper, we find the Queensferry group are all to one design, and consist of an outer and inner shell running up the full height. The caissons are provided with an air-tight roof, 7 feet above the cutting or lower edge, called the shoe. The roof is riveted to cross girders, which are carried between the four main girders, and in some instances attached to the inner shell. These main girders pass from one side of the caisson to the other, and are rigidly fixed to both the inner and outer shells. The inner shell starts at the roof of the working chamber, and is riveted to it and the sloping shoe plates. These plates are at the lower end secured to the outer shell by means of heavy angles, thus completing what is termed the working chamber.

The Inchgarvie caissons are very similar to the Queensferry group, but in them the inner shell only rises to a point 24 feet under the top of the caissons, after which a brick ring takes its place. The lower half of the shoe is formed of brackets, to facilitate the cutting and removing of the rock at the edge of the caisson. The working chambers were all carefully caulked, to prevent the escape of air, and the outer shells were similarly treated, to make the joints water-tight, and thus allow the concrete to be laid in position on a dry and clean bed. The shoes of all the caissons were made of steel, but the roof and upper part were of iron.

BUILDING, LAUNCHING, AND APPLIANCES FOR WORKING THE CAISSONS.

On the shore, near the site of the bridge, were laid two sets of parallel rows of timber, one set being allotted to each caisson in course of erection. These extended nearly the full range of the tide, and rested on concrete blocks, let into the ground. They formed the launching ways, and took the natural slope of the ground, which here was practically one in eleven. Across, and near to their higher end, were erected three timber trestles, upon which the first portions of the caissons were entirely built. These trestles in addition served to carry the work during the first stages of erection, and were so placed as to keep the work clear of high water. A start was invariably made by placing in position, across the trestles, the bottom booms of the main girders; between these were bolted the cross girders; while to both were fixed the floor plates. Round the outer edge of the floor was afterwards run an angle, and to it was riveted two outer shell courses, one above and the other below the floor. To the lower course was riveted yet another, which formed the cutting edge, the bottom being strengthened by an 18″ × 1″ steel belt and heavy angles. Between these angles the lower end of the sloped plates were held, and the diaphragms were afterwards placed and riveted in position. The whole weight (except that borne by the trestles) rested on timber blocks built up from the ground.

During the time the shell plates and shoe were being riveted, the remainder of the main girders was being built, and when completed supplied a platform for the cranes required in building the remaining part of the caisson. This consisted of the bulkheads, and the greater part of the outer and inner shells.

Most of the riveting was done by means of Mr Arrol's patent hydraulic riveting machines of the types shown in Figs. 1, 2, 3,

Plate I. The one is called a fixed, and the other a jointed machine. The fixed machine is thus named because the levers, H,H, are immovable, being connected at the ends further from the cylinder by a bolt passing through the stay, S, and firmly held between by the flat links, L, securely bolted and keyed to the levers. The cylinder, C, is an integral part of one of these levers, while the end of the other is made to carry the fixed die, D, in contradistinction to that in the piston, P, which is inserted into, and actuated by the piston. A recess is formed in the mouth of the cylinder for receiving the cup leather, V, held in position by the cover, B; another similar leather, V', is carried in the piston, being kept in position by a plate bolted thereon. A cock, Q, is screwed into the lower end of the cylinder, while a branch pipe free from control of the cock, is led to the upper end, and thus there is a constant pressure within the small annular space, A, the tendency of which is to keep the piston at the back end of its stroke. Water at a pressure of 1000 lbs. per square inch is led from the mains to the cock on the machine by means of a specially constructed flexible hose built up of canvas, india rubber, and wire. In riveting, the machine is hung at any convenient point, as regards position of cylinder. The work is rapidly done, and if plain, 1500 rivets may easily be closed up in a single day by one machine. The jointed machine is used more for working in contracted spaces, and when the cylinder in the fixed machine would prevent both dies from getting up to their work; for example, a web being riveted to the flange of a girder, when all the parts are in position, which would present difficulties to the fixed machine easily overcome by the jointed one. It is more convenient for this class of work than the other, owing to the dies, D, being both at the points of the levers further from the cylinder. The links, L, instead of being stationary as in the former machine, pivot on the levers, this action being brought into play by means of the connecting rod, R, one end of which is fixed to the end of the upper lever while the other moves in a joint on the piston. This arrangement gives a peculiar motion to the closing die, but when of the proper length is no hindrance to the work, or detrimental to its quality. The action of the cylinder is the same as in the fixed machine. The levers are made of cast steel, and in some cases the metal is under a working stress of 14 tons per square inch, a striking proof of the superiority of steel castings where lightness and strength are desired.

When the outer and inner shells of the caisson had been carried up to near the top of the main girders, the whole caisson was gradually lowered down to within six inches of its final position when resting on the launching cradle. The lowering was effected by means of four 15 inch diameter hydraulic jacks placed under the cutting edge, and resting on timber blocks laid on the top of a large block of concrete bedded in the ground. Between the top of the pistons and the edge of the caisson, logs, extending from one hydraulic jack to the other, were placed. These logs formed a square, and effectually bound all together during the act of lowering. When doing so only two jacks were given the full pressure at one time, the other two being simply used to keep the logs hard up to the caisson edge. Owing to the care taken when lowering any danger was reduced to a minimum, even although the mass weighed well nigh 300 tons. The caisson was now in a position to allow the launching cradle to be placed underneath. This cradle consisted of a series of iron girders and timber beams made up to the level of the cutting edge. On the four fixed timber ways, F, already mentioned, were placed other four, S, directly overhead, with timber runners or guides, R, extending a few inches below the top of the fixed ways, to keep the whole in position when sliding into the water. The caisson was again lowered by easy stages by means of jacks and blocking, acting and applied alternately to opposite sides, till the edge touched the bearing points on the cradle, and it remained in that position until the vertical timbers intended to distribute the weight as evenly as possible over the whole bearing area were placed between the roof of the working chamber and the top of the cradle immediately over the ways. And when finally lowered the pressure induced on the bearing surfaces was about two tons per square foot, but this affected the ways so little that in few places was the tallow found rubbed off after the launch. The whole cradle was loaded with iron to prevent its floating away with the caisson after the launch. When the dog shores had been removed the first thrust to bring the caisson under way was imparted by means of a hydraulic jack acting on a timber frame placed in front, after which, the friction becoming less, the movement was continuous. Owing to the shallowness of the water in front of the caissons, the whole of the arrangements were timed to be ready at the height of the spring tides. In some of the later launches, however, the air within the chamber not being allowed to escape, the draught of the caisson was reduced several feet, and this removed all anxiety though the tide was of only a moderate height. So soon as launched, tugs towed the caisson either direct to the place where it was to be sunk, or alongside the jetty, as occasion required. In some cases this was a work of no small difficulty, owing to the large area presented to the action of the wind and tide, but in every instance it was safely accomplished.

The caissons when towed to their site were securely moored to timber dolphins, which served as supports (while they were yet free to rise and fall), and also as guides to determine their correct position. After this, any of the iron work still remaining was completed and concrete filled in over the floor till sufficient weight had been added to keep the caisson firmly on the ground, at all states of the tide, even after air was forced into the working chamber to allow the men to enter. During part of the time occupied in filling in the latter portion of the concrete just mentioned a temporary caisson was placed in position. It consisted of strong wrought-iron segments, 14 to the circumference, each being 10 feet high, bolted to each other and the permanent caisson. A temporary working platform was now thrown across the top, on which was placed the engines, boilers, crane, &c., requisite for the carrying on of the sinking operations. Three three-feet six-inches diameter shafts were also carried up from the working chamber to this platform. On the upper end of two of these shafts were placed the air locks through which the hard material was raised in buckets, while on the third was fixed the air lock for the ingress and egress of the men.

Opening into the chamber were three ejector pipes led up from the shoe to the outer shell, which were used for discharging the silt, met with, during the excavation of the first 20 feet or thereby. A smaller pipe was also carried from the shaft, entered by the men, to the outside of the caisson designed to clear the bottom of this shaft, and to open up a working space within the chamber. Water pipes were also led into the chamber to dilute the silt, prior to discharge through the ejectors. At the ends within the chamber, of all these pipes, was fixed a cock, or valve, which, along with all the lower parts, was placed in position before the launch, the remainder, or upper parts, being fixed any convenient time thereafter.

The air was led in iron pipes from the compressors to near the caissons, but was afterwards conducted by a flexible hose to a check valve on the air shaft, where it was forced into the caisson.

Water was similarly led, from a raised tank, to the main pipe on the platform, and distributed as required.

Two styles of air locks were employed, one for the men, and the other for the buckets. They were all placed over and firmly bolted to the upper end of the large air shafts at a level convenient to the working platform. The lock used by the men was of the simplest form, consisting of two wrought iron cylinders, the outer one being 7 feet in diameter, while the inner was longer and corresponded in size to the shaft on which it rested. One end plate covered the top of both cylinders, while at the bottom only that between the outer and inner cylinders was enclosed. Two upright plates between these cylinders divided the space between the inner and outer, into two distinct compartments, each of which constituted a separate and independent entrance, by having placed within it two opening iron doors. The lock was wrought thus: — When any one desired to descend he passed into the outer chamber, O, of the air lock, and closed the door, D, behind him. On opening the cock communicating with the air shaft, air entered the chamber, O, until the pressure was equal to that within the caisson. The inner door, D′ D′, now became free to open while the outer one, D, was kept securely shut by the pressure of the air within. Through the inner door a free passage was had to the air shaft, within which a ladder was fixed. (See Plates III. and IV.)

The locks, through which the material was discharged, were similar in principle to the one used by the men, but were necessarily more complicated. The design was that of Mr Baker and Mr Arrol. They were placed on the top of the 3 feet 6 inches diameter shafts, and securely bolted to them. Immediately over the shaft was the barrel, or cylindrical portion of the lock through which the buckets passed, in their up and down movements. At one side of the lock barrel, an air-tight space was left, into which the top and bottom horizontal sliding doors were run when drawn back to allow the bucket to pass through. About midway between the doors, and well back was a drum, round which the chain, used in raising and lowering the buckets, wound after it had passed over a swivelling pulley fixed to the top door. Each door was actuated directly either by a small hydraulic cylinder, or hand wheel with rack and pinion placed outside the lock. The rod was common to both motions, and passed through a stuffing box to prevent any leakage of the high pressure air within. Upon the drum shaft (passed to the outside of the lock in a manner similar to the rod already mentioned) was keyed a large worm wheel, driven by a worm fixed directly to the crank shaft of a small double cylinder reversing engine, firmly secured to the barrel of the lock. Suitable cocks, for admitting and discharging the air, were placed on the lock and so arranged that it was impossible to have more than one open at a time. The cocks employed for working the hydraulics, used in opening and closing the two doors, were so interlocked that one door could be open only when the other was completely closed. Additional protection was also secured by a locking bar, arranged so that if one door was open the other was held fixed, till the first was again closed. A system of lifting rods, passing through stuffing boxes, and wrought by levers, raised, and held the doors against the face of the rubber rings every time the lock was used, till the air pressure behind relieved them of their work. To prevent the over-winding of the bucket, a dial with pointer, served to indicate its true position to the person in charge of the winding engine.

FOUNDING OF THE CAISSONS.

The sinking of the caissons was always commenced by setting a few men to clear away the silt under the air shaft and open up a space in the working chamber, to allow the full complement of men to descend.

As already mentioned, two pipes, with valves on the inner end, were led into the air shaft, for the purpose of removing the silt, which was effected in the following manner.

Through one of the pipes, led to any desired point by an india-rubber hose, water was forced into the chamber for the purpose of diluting the mud. From the place where the dilution was effected, another hose led the silt back to the second pipe, through which it was discharged, by the pressure of air within the caisson. When ejected, the silt had all the appearance of a thick muddy water. To keep the water from spreading over the working surface, in the chamber, a small mud trough was formed into which a constant stream from the pipe already referred to was run. One man manipulated the discharge pipe, by keeping the end alternately in and out of the trough, for such short intervals as might be required to keep the passage clear. As a precautionary measure, a man was placed at the discharge valve to regulate it, and in the event of anything going wrong, to close it to prevent any undue escape of air. Great care had to be exercised during the first stages of sinking, as the gradual removal of the silt, from within the caisson, took away its support and caused it to settle down, sometimes too suddenly, as in one instance when it dropped 7 feet, completely closing up the working chamber, and even causing the silt to rise in the air shaft to about the same height as it had been when excavating was commenced. This occurred about low tide, the most dangerous time, because of the lessened displacement, but fortunately all the men were out as a subsidence had been looked for owing to the silt showing signs of flowing within the chamber. As soon as the silt was solid enough passages were made to the other ejectors, and they in turn, if required, were brought into play till the whole of the silt was discharged.

While the ejectors were at work, the air pressure within the caisson was always kept higher than that due to the head of water outside. The nature of the silt enabled this to be done, the pressure observed at various times being as follows—27 and 26 lbs. per square inch, while the respective heads of water were 40 and 50 feet. Little alteration took place in the pressure within the caisson during the rise and fall of the tide, as it seldom varied more than 2 or 3 lbs. in a tide of 18 feet.

When all the silt had been removed from within the chamber, and the caisson had reached the boulder clay, recourse was had to the common pick and shovel, and as the ejectors were of no further practical value, the material was passed out in buckets, through the two locks already mentioned. These buckets were placed on bogies, and run on a light tram to the mouth of the 3′ 6″ shaft, in the chamber below. They were hoisted in the manner already described. The filled buckets were attached to the chain, unwound from the barrel, B, which was worked by means of the worm, W, on the crank shaft, and worm wheel, W′, on the barrel. (See Plate II.) Water was now admitted to the lower hydraulic cylinder, H, which closed the bottom door, D, below the bucket. This door was held to its bearing by means of tappets, T, wrought by the lever, L, and kept thus till held fast by the air pressure behind, which took place as soon as the pressure of the air within the lock was, by opening the cock, Q, allowed to equalise itself with that of the atmosphere. The upper door, D′, was now drawn back into its recess by means of the hydraulic cylinder, H′, and the bucket was thereafter free to be lifted by the crane. This action is simply reversed when a bucket is being passed into the caisson, but it will be observed the lower door cannot be now drawn open by the hydraulic cylinder till the upper one has been closed, owing to the interlocking form of the hand wheels, D, regulating the cocks through which the water requires to pass on its way to and from the hydraulic cylinders.

The sinking of the first caisson was proceeding very slowly, when fairly into the boulder clay, a result satisfactory in one respect, looking at the foundation to be obtained, but not so when the time it would take to sink it to its final position was reckoned, especially as more clay had to be gone through than was at first anticipated.

Mr Arrol visited the air chamber to see what could be done to hasten the work, and one of the results was his invention of a hydraulic spade. (See Plate II.) It consists of a wrought-iron cylinder, C, having a brass casting, B, screwed on to the one end, through a stuffing box on which the shaft, S, of the spade passes, while to the other end is similarly fixed a cap, capable of having screwed into it short wrought-iron pipes, to vary the length of the spade as required. The spade proper is forged on one end of the shaft or piston rod, S, while to the other the piston, P, with the necessary cup leather, is securely fixed. A cock, Q, is screwed into the lower casting, communicating with both ends of the cylinder, and to it high pressure water is led through a small flexible hose. Another hose is employed to carry off the exhaust water. When working, each spade is served by three men, and the action required is very similar to that in the ordinary hand tool of that name. A working face, about 15″ deep, is necessary, above which two men (one on either side of the spade) stand, while a third man is stationed immediately behind. The two men lift the whole machine by a cross handle, and place the point of the spade on the ground, a few inches behind the working face. The third man now opens the cock, and this allows the water to enter the top, which causes an upward movement of the cylinder, until the pipe screwed into the upper end bears against the roof of the chamber, and that immediately over a rivet head, to prevent it from slipping. The upward movement of the cylinder being thus effectually arrested, the spade is forced into the ground, till the piston reaches the end of its stroke, and then, upon reversing the cock, the cylinder falls and sets free its upper end. The clay in front of the spade being loosened, is finally brought away by the men taking advantage of the leverage of the cylinder, which pushes the material off before the spade. What with difficulty was before taken away in pieces about the size of one's hand, was by this spade dug out in lumps so large, that they had usually to be recut before they could with ease be lifted into the buckets. The whole of the ground within the chamber was accessible to the spade, even to that portion underneath the sloping part of the shoe of the caisson where it was often held to its work at an angle of about 60°.

During the working of the hydraulic spades, an interesting point was always prominent to observers. As already mentioned, the exhaust water was conveyed to a hole in the ground, and as it rapidly accumulated, means had to be employed to expel it from the chamber. Advantage was taken of the air pressure within to effect this. Owing to the boulder clay in which the spades wrought being fairly water-tight, the pressure in the chamber did not, however, as a rule, exceed more than from 12 to 16 lbs. per square inch during the time occupied in digging out the clay. This pressure was much less than that due to the head of water had it been free to enter the working chamber, and so to expel the exhaust water recourse was had to the simple device of allowing a proportion of air to escape with the water, varying less or more according to the difference between the pressure of air within the chamber and the height to which it had to be forced when being discharged. This served the purpose admirably, as in all cases the water was easily and efficiently ejected. It was expelled from the discharge pipe above high water, in spurts, as if coming from a plunger pump, leading one to the conclusion that cores of water and air alternately filled the pipe, thus reducing the effective head of water in proportion to the amount of air in the pipe. Observations taken at irregular intervals showed the pressures within the chamber to be 12 and 22½ lbs. per square inch, while the heights of discharge at the corresponding times were 60 and 80 feet respectively.

Before proceeding to describe the filling up of the working chamber, it will here be in place to revert to some of the difficulties met with, and precautions taken, while sinking the caisson.

During the excavation of the first portions of the material from within the chamber the sinking of the Queensferry caissons was a matter demanding careful attention. If the caisson did not gradually subside as the material was removed, then the whole of the men were ordered out of the chamber at low tide, and if a subsidence was desired the air pressure was slowly reduced (thereby greatly increasing the downward pressure of the caisson) till the desired amount of settling had been effected. The removal of the men was a precautionary measure intended to secure them against the danger of any sudden drop of the caisson (which as already mentioned did occur) while it was still in the silt and liable to sudden subsidence. This precaution became unnecessary, and was therefore discontinued as soon as the caisson was fairly down into the boulder clay.

Another matter of the utmost importance, and one requiring vigilant watchfulness, was the sinking of the caisson vertically. If not in its true position, when operations were commenced, it was so guided as to bring it to the correct place when down. This was generally accomplished by tilting its top in the direction opposite to that in which it was intended to move it, and sinking being continued while so tilted, the lower edge gradually approached its true position. On this being attained the whole caisson was righted, and if still true, was sunk vertically till it reached the proper depth.

Another way of effecting the same purpose was to tilt the caisson as before, but immediately afterwards to right it, when it would be found to have moved through a certain distance sideways, and that in the direction required, owing to the side first tilted having pressed against the lower and harder ground, thus causing the caisson, when righting, to move a little in the one direction. A secondary assistance was to place heavy timbers at an angle against the top of the shoe. These timbers were made to bear against the ground, so that when the caisson was subsiding their tendency was to push it from them, and thus help to bring it back to its true place.

During the greater part of the time occupied in the sinking operations concrete was being added to that already in the caissons to overcome the increased resistance to the downward pressure.

So soon as the caisson reached its proper depth and the work of excavation was finished, and all the material discharged through the two shafts, the working chamber was filled with concrete in the following manner:—

Two iron tubes were fixed into the material shafts, and on the lower end of these shafts, plates, previously prepared, were bolted to receive the tubes through which the concrete passed down to the working chamber.

On the top and bottom of these tubes air locks were fixed to prevent the escape of the air within the chamber, and also for regulating the admission of concrete.

The lower locks consisted of flanged steel castings, to the under side of each of which a balanced door, opening towards the chamber, was securely attached. To the side of these castings cocks were fixed, controlled from within the working chamber, and opening or cutting off communication between the tubes and the chamber. A somewhat similar lock was placed on the top of these concrete tubes, the top of which was raised a little above the level of the floor on which the concrete mixer placed near by rested. The concrete was passed to the chamber as follows:—Any convenient quantity of this material was filled into one of the tubes, and kept there by the closed hinged door at the bottom of the tube. The door on the upper lock was now closed, and on the men below being signalled to they opened the cock communicating with the concrete tube and equalised the pressure therein with that in the chamber. This allowed the lower door to be opened, when the contents of the pipe at once fell to the floor. The concrete was now ready to be laid in position, the portion against the roof being firmly rammed.

The lower lock in the meantime having been closed, this change was signalled to those above, who allowed the compressed air to escape, when the concrete tube was again ready to be filled. With two of these tubes, and with work being carried on night and day, the chamber could be filled within a week. The final closing was effected from the men's shaft after the air pressure had been taken off the caisson.

Grout was run into all the shafts and allowed to find its way into every interstice between the iron roof of the chamber and the concrete till all was completely filled.

The shafts were now removed, and the voids previously left in the concrete to facilitate this were filled up. The concrete was carried up over the full area of the caisson to within 12 inches of the top of the permanent iron work.

The remaining 26 feet of the piers was solid masonry, varying from 55 feet diameter at the base to 49 feet at the top, and consisting of an outer skin of grey granite with hearting of Arbroath stone. Into this masonry were built 48 2½ inches diameter steel bolts, with enlarged ends, protruding sufficiently above the pier to give ample grip to the large cast steel nuts required to hold down the steel super-structure of the piers.

For lighting purposes candles and oil were at first employed, but these were soon displaced by the electric light. The great superiority of this light was especially apparent within the first chamber, for after its adoption a perfectly clear atmosphere was obtained.

INCHGARVIE CAISSONS.

The caissons of the Inchgarvie group were in general outline and parts very similar to those used for the South Queensferry piers, but the lower half of the shoes, in this instance, had supporting brackets instead of sloping shoe plates. These had the effect of stiffening the cutting edge at the bottom and facilitating the excavation, by allowing better access to the rock immediately underneath.

The caissons were launched complete, and at once removed and moored to the end of the temporary jetty, where they received as much of the brick ring and concrete as the shallowness of the water at this point would permit them to carry. (See Plate IV.)

The temporary caisson, trestles and flooring, remaining shafts and air locks, were also added here, being those previously used in sinking the Queensferry caissons, if we except one of the temporary caissons made up of long vertical plates, stiffened by circular girders and vertical tees, instead of complete sections, as in the other. It should be here stated that previous to launching, two solid built up timber blocks, about 9 feet square, intended for support, were securely fastened to the shoe of each caisson, and in the bottom of the river, on the site of the caisson, corresponding to each timber block, a bed of concrete and sand bags was so laid that when the caisson was placed on its site, and sitting level, the blocks would rest fairly on their respective beds. These supports were rendered necessary by the sloping nature of the rock, which at the lowest point, reckoning on the area immediately underneath the caisson, was 16 feet under the highest parts. A third support for the caisson was the steel edge, bearing on these higher parts of the rock.

The caisson being towed to its site was securely held against bearings on the temporary iron staging prepared for it, by having anchoring chains carried back to the rock, and to blocks, sunk in the river.

As the caisson was now floating in deep water, concrete and brickwork was added till it rested at all states of the tide on the three bearings already mentioned.

No difficulty was experienced through instability of the beds of sand bags and concrete, the blocks being firmly and securely supported by them, while the rock bearing was quickly improved by the men going down and removing the protruding parts during the short intervals the caisson rested at low tide. This levelling soon permitted the caisson to be weighted sufficiently to keep it down at all states of the tide, after which work went on, without intermission, the only stoppage being during the day on Sunday.

Rock drills were set to work, to cut a series of holes, under and beyond the cutting edge of the caisson, at the part where it rested on the rock, to permit of blasting. By this means the rock was cut away, and the caisson gradually allowed to settle down.

The sand bags were now released, and part of the concrete underneath the bearing blocks removed, to permit of the still suspended side settling down. It was kept a little lower than the side bearing on the rock; and on this account the air kept the water down to the caisson edge where the men were at work.

The rock drills, disliked by the men at first, after being fairly started, were looked upon (by them) as indispensable.

The drills were driven by air, of a pressure of 70 lbs., led into the caisson by special pipes.

As the first caisson was subsiding, some of the loosened rock got jammed between it and the rock face, which caused it to move gradually from its true position. To bring the caisson back, before the movement had gone too far, timber raking struts were placed at an angle against the top of the shoe and the rock surface, in such a manner that when the caisson subsided the tendency of the struts would be to push the caisson back to its true position. Tapered wedges, with a similar design and tendency, placed on the rock were also made to act on the edge of the caisson. In cutting the rock underneath the shoe, it was blasted beyond and under the edge of the caisson, and this allowed the portion jammed between the caisson and the rock face to fall down and be cleared away. The further lowering soon brought it back to its true position.

The blast holes were usually drilled about 6 inches beyond the edge of the caisson, and as the effects of the blasting cleared away the rock for about 6 inches more, ample room was provided for the removal of the broken stones lying behind and underneath the edge of the caisson.

The cutting of the rock within the caisson itself was simply a matter of quarrying, under the difficulties attendant on the use of air pressure, and confined space. The excavated material was sent above in buckets, being passed through the locks in the manner already described, in connection with the Queensferry caissons.

A little of the sand in this instance, however, was forced out underneath the edge of the caisson into deeper water, where a current quickly washed it away.

Although the original intention was to sink both of the Inchgarvie caissons till a level bed was obtained over the full surface, it was finally deemed expedient to stop sinking at a point, about four feet above the position, where this would have occurred.

A hole in the rock was thus left at one part of the foundation, varying in depth from 4 feet to nothing at the points where the caisson commenced to bear. The deepest part was immediately underneath the edge of the caisson. To step the rock here was a matter of necessity, and was accomplished by blasting. Iron plates were now placed between the free edge of the caisson and the rock. Against the inside of these plates, a wall of concrete, in bags, was formed, and thereafter the hole was filled up with concrete to the level of the caisson edge.

Concrete was now filled into the working chamber and grouted up in the manner already described, and the piers are being completed in a similar manner to the South Queensferry group.

RATE OF PROGRESS.

To excavate a circular hole, 70 feet in diameter and 40 feet in depth, even in open ground, would be a work involving the expenditure of a large amount of energy, but to do so in the bed of a river by means of caissons is a work of much greater importance.

The times occupied in sinking the Queensferry caissons were, for the S. W. caisson, 99 days, the depth sunk being fully 39 feet; for the S. E. caisson, 72 days, with a depth of 37 feet; and for the N. E. caisson, 72 days, with a depth of 43 feet.

The increased rate of progress in the case of the two latter caissons was due principally to the advantage gained by the use of the hydraulic spades already described.

For several days together (each of 24 hours) the average digging rate, while in the stiffest of the boulder clay, with 27 men in all below, was one foot per day. During this time four hydraulic spades were at work, and a filled bucket was being sent above every five minutes.

This represents a digging power for each spade of about 35 cubic yards, and for each man, of about five cubic yards each 24 hours.

Even this rate of progress was exceeded, for at one time, in a single shift of 12 hours with 23 men below, a filled bucket was sent up every 4¼ minutes, giving a discharge of about 86 cubic yards, almost equal to 7½ cubic yards per man in 24 hours.

These results, considering the peculiar circumstances under which this work had to be performed, must be looked on as very satisfactory.

In the case of the Inchgarvie caissons the times occupied in sinking were certainly more extended, but this was chiefly owing to the bed of the river being hard rock.

EFFECTS OF AIR PRESSURE.

To pass from air of any given pressure to that of a higher, or lower pressure, can generally be performed with ease and safety, provided sufficient time be allowed to permit of the change in the pressure being very gradual.

The sensation on entering of those who pass through the air lock for a first time varies with almost every individual. While on the one extreme the effect in a few cases is nil, on the other, and which happily is rare, fainting is even produced.

The most common inconvenience is a feeling of pain in the ears, produced by the inequality of the pressure on the outside and on the inside of the tympanum or drum.

One case which came under my notice was that of a young man, who had severe pain in one ear only. It was afterwards discovered that there was a small perforation in the tympanum of that ear in which no pain had been felt.

After descending to the working chamber the feeling for a time is usually one of exhilaration; this gradually disappears until the only difference commonly felt from usual is work more fatiguing, and more exertion in speaking; for example, whistling is almost impossible.

If, from any cause, the pressure was suddenly reduced, then all within the chamber was instantly enveloped in a dense fog until the pressure remained for a time stationary or was increased, in which latter case it rapidly cleared.

This fog was due to the cooling effect produced by the sudden expansion of an atmosphere loaded with moisture.

So long as the pressure remained from 12 to 18 lbs. no ill effects were experienced by the men, even though they were working every alternate six hours each day, Sunday excepted, and then only throughout the day. When, however, the pressure rose much above this point, and more particularly if the ground being excavated was muddy, sickness appeared among the workers, the true cause of which is still unknown. The symptoms of those affected were severe pains in the legs and arms, in some extreme cases resembling a paralytic shock. The attack, as a rule, came on shortly after coming out of the chamber. Relief was had in some cases by returning to it; in others, by a strong shock of electricity; while in others, again, it came only with time; and in a few even that has not yet brought convalescence.

In all cases when the pressure exceeds 25 lbs., it is well to reduce the working hours, and to continue doing so as the pressure is increased. By this means the men are better able to keep up their strength, and consequently enabled to withstand the ill effects of the high pressure. The value of this precautionary measure has been clearly demonstrated in the case of this undertaking, as well as in other works where compressed air has been much used.

Provided one is in good health, entering and remaining under a heavy air pressure for a time rarely does visible harm, but this may be accepted as a truism, "the more seldom one does so the better."

NO. 4 OR THE TILTED CAISSON.

On the first day of the present year a singular and unfortunate accident happened to the Queensferry north-west caisson.

After launching it was towed to position, there to be partly concreted and to receive the remaining four courses of plates still required to bring it to its proper height.

Concreting was carried on till about 2500 tons had been filled in. This left a freeboard under the riveted work of seven feet, which with two courses of three feet each, bolted in position, raised the top of the caisson 13 feet above the water line. New-Year's day was the day following the height of the spring tides. On the previous night the tide had been exceptionally low. This caused the caisson to take the ground earlier and lie over more than on any previous occasion. The greater list on this date was no doubt largely due to the mud and soft silt being washed away by the current from underneath the caisson as it neared the ground. The lowness of the tide allowed the caisson to rest more heavily, and consequently to sink more deeply, into the ground, where it was firmly held by the mud. The result of this abnormal condition was that though the tide rose the caisson remained fast till water began to pour into it. In a short time all hope of the caisson rising was gone. A body with a displacement so much greater than that necessary to float it, such as this caisson, gives no anxiety with regard to its floating, but the explanation why the caisson in this case did not rise seems to be clearly this, that the water, owing to the firmness with which the caisson was bedded in the mud, was not able to force its way underneath; so long as this lasted there could as a consequence be no tendency to rise. Even with the caisson filled with water nothing very serious was to be anticipated, as valves had been provided for the ingress and egress of water. These, however, were at this time rendered practically useless, on account of those in charge supposing they were fully opened, when in reality they were only partially so. The result was, that as soon as the tide ebbed the water did not escape sufficiently, and soon the caisson was so top heavy that it gradually tilted over until it lay at an angle of about 30 degrees from the vertical, and remained so till the final pumping in connection with the raising operations was well in hand. Lying thus, the lowest part of the caisson was about six feet under low water. Arrangements were accordingly made to have the remaining plates, with a few additional ones, put on, so as to bring all above high-water. Struts were placed across the inside of the caisson, and the water gradually pumped out to allow of more being put in. During the pumping, and before the additional strutting had been fixed, the iron work on the one side suddenly gave way. After this it was determined that it should be raised by other means.

The plan adopted was to completely surround the whole outer iron shell with a casing of 12 inch timbers. This was accomplished by fixing together in twos and threes properly dressed piles, and afterwards bolting them on to the side of the caisson. When doing this, a few tapering spaces were left, into which suitable timbers were afterwards driven, by means of rams, thus tightening up the whole and preventing leakage. Owing to the angle at which the caisson lay, a heavier stress was produced on one side than on the other during pumping. To overcome this, and also to stiffen the timbers, a strong circular girder was placed against the new shell all round the inside of the caisson, and from it struts were made to transfer to the solid work of the caisson the stress produced. The whole of the inside of the caisson was carefully strutted with timber, to assist in keeping it in form, and on the outside a strong steel hoop was run round and tightened up by means of cottars, so as to complete what one is tempted to call a huge barrel.

Pumping, accompanied by dredging, on the higher side was now carried on for several weeks, with the result that the caisson gradually righted till it became nearly perpendicular. Increased pumping was now kept up, and one morning, quite unexpectedly, the caisson became vertical, and was practically floating over its true position. Here it lies, ready to receive a brick ring, close to the outer shell, and thereafter to be sunk in a similar manner to those alongside, composing the other members of its group.

In closing this wide subject, I desire to remind you that even this lengthened paper can only serve to show how briefly it has been treated. A great field of minor though none the less important details must, for the present at least, remain untouched. This we do with full confidence, knowing that what has been said will beget in each the instinctive feeling of the true engineer which tells him, that if he had similar work to perform, he could carry it to a successful issue.

Having watched this great work through its various stages, and seen it almost completed, without a hitch, so far as working under air pressure is concerned, we are forced to endorse that conclusion, predicted by the eminent engineer, Sir John Fowler, C.E., in his address to the British Association, several years ago, that the pneumatic process of sinking large cylinders and caissons would be taken advantage of to a greater extent in the future than in the past; and this I would add, in places which will present fewer difficulties than those encountered in preparing the foundations of the Forth Bridge.

The discussion of the paper was postponed till next meeting,

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

Mr CHARLES P. HOGG thought this a very valuable companion paper to that given by the author in the previous session, on "Sinking the Cylinders of the Tay Bridge by Pontoons," and both papers compelled their admiration for the excellence of the apparatus employed for the special end in view. With regard to the sinking of the caissons, he would like to ask Mr Biggart how they managed to tilt them, seeing that the large ones including the concrete weighed some 3500 tons, and how they righted them so easily. He knew something of the difficulties attendant on sinking small caissons, and therefore he was anxious to know how they tipped over these huge masses of material so easily. As an Institution they had to congratulate themselves that they had among their members those who were capable of successfully carrying out to a practical issue the conceptions of the most eminent engineers of the day.

The PRESIDENT said he had not much to add to what had been said in the discussion. He thought the paper was of great importance to have recorded in their Transactions—a paper which gave an account of such important works, which surpassed greatly any former works of the same kind, in the boldness and originality of their conceptions; and were remarkable for the successful manner in which they had been executed, and for the skill shown in preventing or in overcoming difficulties. It was very desirable to have papers from men who were conversant with the whole procedure in connection with the execution of such works; and he thought the Institution owed Mr Biggart a very hearty vote of thanks for his able paper, which evinced on his part great labour and care in its preparation. They would see from the paper that men were now penetrating into places more and more difficult of access, and he thought they had very nearly reached the limit of human endurance under the conditions described in the paper. It seemed that the men engaged on these works had to carry on their operations under a superatmospheric pressure due to a column of 89 feet of water. He was not an indiscriminate advocate of vivisection, but he thought that in connection with works of this kind experiments on the lower animals might be of use towards finding the phenomena and symptoms and the modes in which the injurious effects arise, and towards finding means of prevention or alleviation. It had been with him a matter of interest to arrive at some knowledge as to how it was that on experiencing relief from such extraordinary pressures, to which they had been subjected for some time, workmen experienced and manifested symptoms of paralysis. Was this due to the fact that their blood had become impregnated with such heavily pressed air? He raised the question, but could not answer it. Was it or was it not an unequal voluminal compressibility of the spinal cord and surrounding bony or other materials; or was it that the air was impressed into the blood, and that in some parts of the body, on the relief of the air pressure, bubbles were formed? Questions of that kind were proper to be raised, and it was very desirable to have them answered. He had spoken on this subject to some of the medical professors in the University, and Professor Gairdner had expressed interest in it, and said that it seemed to be a subject in which very careful physiological or medical investigations should be made.

Mr BIGGART stated that the caisson was, with comparative ease tilted and to a moderate extent moved sideways, even when well down into the silt. These movements were effected by removing the soil from under the shoe at that side of the caisson to which it was desired to give a list. The ground at the opposite side being allowed to remain, served to bear up the shoe at that point. When sufficient soil had been taken away, the air pressure was usually reduced, to allow the caisson to settle down. This it did to a greater extent where the resistance was less, and in this way it became tilted to the cleared side. In this tilted position sinking was continued, with the result that the caisson gradually moved in a lateral direction away from that in which the tilted head lay. So soon as it had gained its true position, the caisson was righted; and sunk vertically. In the case of the caissons of the Forth Bridge, the mass was so great that it was possible by the means described to displace the ground against which the caisson pressed. But with a cylinder only a few feet in diameter it would be quite different, owing to the reversed proportions of the weight and leverage of the cylinder, and the amount of material to be displaced. Replying to the President, he pointed out that the greatest pressure in the air chamber was not necessarily attained when the caisson was at its greatest depth. While this was necessarily the case with the Inchgarvie caissons, it was not so with the Queensferry caissons. In the latter the greatest pressure was during the time they were passing through the silt, when it was intentionally kept high to facilitate the working of the silt-ejectors. So soon, however, as the boulder clay was reached, the higher pressure was no longer required, either on that account or for the head of water, as the ground was practically water-tight. From then until they reached their final depth, the pressure varied from 12 to 18 lbs. per square inch. It was so in the case of the caisson sunk to the greatest depth—namely, 89 feet. It was different with the Inchgarvie caissons, where the pressure was always equal to the head of water outside. This resulted from these caissons being founded on rock, and the water having free access to the air chamber. In the paper a record is given of the pressures and respective heads of water observed at irregular intervals during the progress of the work.

The PRESIDENT then moved a hearty vote of thanks to Mr Biggart for his paper.

Note by the President, 16th December, 1885.

Since the foregoing discussion on Mr Biggart's paper, I have inquired of Dr M'Kendrick, Professor of Physiology in Glasgow University, in reference to the physiological conditions affecting men exposed to great changes of pressure, as referred to in the paper, and he has very kindly made out the subjoined note :—

1. The *quantity* of gas which can be extracted from 100 volumes of blood by the mercurial air pump, measured at 0° C. and 760 mm. pressure, is about 60 volumes. The amount is different in arterial blood from what it is in venous blood, as shown by the following table :—

From 100 volumes	May be obtained in volumes Of oxygen.	Of carbonic acid.	Of nitrogen.
Of arterial blood, .	20	40	1 to 2
Of venous blood, .	8 to 12	46	1 to 2

measured at 760 mm. and 0° C.

2. *Effects of Diminished Atmospheric Pressure.*—Rapid changes of pressure are followed by swelling of the cutaneous vessels and of the superficial veins, hæmorrhages from the nose, mouth, and mucous membrane of the air passages, increase of sweat and of cutaneous perspiration; the respirations become frequent and irregular; the heart beats are increased; the voice is weakened and is altered in quality; the muscles, especially those of the lower extremities, are easily fatigued; there are also pains in the head, giddiness, and at last unconsciousness. The researches of Paul Bert have shown that these symptoms are due to a diminished density of the atmospheric oxygen, and to a consequent diminution of the amount of oxygen introduced into the blood, and Bert combatted these symptoms successfully by causing the individuals in balloon ascents to breathe extra oxygen carried in store under pressure.

3 *Effects of Increased Atmospheric Pressure.*—A pressure of two or

three atmospheres causes the respirations to become irregular, less frequent, deeper; expiration is shortened, and the pause between an expiration and the next inspiration is much longer than the natural pause; the skin is pale; the superficial vessels are emptied; the pulse is slow; and muscular movements are said to be more easily performed. A pressure of five atmospheres intensifies the symptoms and grave accidents may happen, more especially at the instant when the pressure is removed. These accidents have been shown to be due to the sudden formation of small bubbles of gas forming in the capillaries with such violence as to cause anatomical lesions which in such organs as the brain and spinal cord may cause death. The gas is a mixture of the gases of the blood; more especially, according to Paul Bert, of nitrogen and carbonic acid. Bert has further shown that a pressure of 20 atmospheres, corresponding to a pressure of four atmospheres of pure oxygen, causes death with epileptiform and tetanic convulsions, due to an actual poisonous action of oxygen. He states that when the amount of oxygen in the blood reaches 35 volumes per hundred volumes of blood (that is nearly double the nominal amount) these effects are produced.

4. *Suggestions.*—Men working under a pressure of two or three atmospheres should not *suddenly* return to the outside air, and arrangements should be made for allowing them to do this gradually, say by having two or more chambers at intermediate pressures. The men should be warned that the danger lies in the suddenness of the act of coming into the open air more than in working under great pressure. If an accident occur, or if a man feels ill when he comes into the open air, then the right thing to do is at once to return to the place of greater pressure.

On the Present State of the Theory of the Steam Engine, and some of its Bearings on Current Marine Engineering Practice.

By Mr HENRY DYER, C.E., M.A.

Received and Read 24th November, 1885.

THIRTY years ago Professor Rankine, in his inaugural address in Glasgow University, endeavoured to dispel the fallacy of a double system of natural laws, one theoretical and an object of the noble and liberal arts, the other practical and an object of what were once called the vulgar and sordid arts. He also showed that the so-called physical theories of most of those who were under the influence of this fallacy were empty dreams, with but a trace of truth here and there, and at variance with the results of everyday observation, and were thus calculated to perpetuate that fallacy. Since that lecture was delivered, the intercourse between men of science and men of practice, the improved scientific knowledge of practical men, and the improved practical knowledge of scientific men, due in great part to the writings and teachings of Rankine, have caused the notion of the incompatibility between theory and practice to considerably decline; but still one often hears it said that a certain design may be "very well in theory, but it won't do in practice." Such expressions are altogether misleading. When the results of theory do not agree with practice, either the theory is imperfect or the practice is wrong, or possibly both. In such a case it would be more correct to say that the results of practice arrived at by a system of trial and error do not agree with the results of reasoning on certain hypotheses which have been adopted as expressing approximately the conditions of the case, but which make no pretence of taking into account all the physical properties of the materials employed. Before an approach to a complete theory of any structure or machine can be made, these properties, or the most important of them, must be taken into account. It must be distinctly recognised that engineering is an *experimental*, and not an *intuitive* science. We must discover what *is*, and not allow ourselves to be led away by speculations as to what might have been, far less by what ought to be. No doubt such speculations are very useful as showing the directions in which improvements are likely to be made, and also for the purpose of making rapid calculations, and are not to be objected to if kept in their proper place.

In no branch of engineering is it of more importance that theory should be founded on experiment, and practice guided by the deductions made from theory, than in that relating to the manufacture of steam engines. Watt's great improvements were the direct result of the experiments he made on the properties of steam, and the steam engine of to-day is essentially Watt's engine, improved no doubt, but evidently not so much as the result of direct experiments, or of reasoning founded on experiments, as of an expensive system of trial and error, and developed as engineers gained confidence in the use of steam and of the materials of construction.

It would occupy too much time to enter at present into the history of the development of the steam engine, and a comparison of theory with practice. We will limit ourselves to a few historical notes, and to an outline of that theory as it at present exists and some of its bearings on current practice, especially in marine engineering. A complete theory of the steam engine would include not only the study of the action of the steam in the cylinder or cylinders, but also that of combustion and the production of steam, and of the mechanism of the engine. To keep this paper within moderate limits we will confine our attention almost entirely to the first of those subjects.

The theory of the action of steam in the cylinder of an engine to be found in the popular treatises, and largely used by practical men and students, is somewhat as follows. The cylinder is supposed to be absolutely non-conducting, and to have no effect on the condition of the steam, which is supposed to behave like a perfect gas, and to expand according to Boyle's Law. On these two suppositions certain formulæ are developed which have all the appearance of being exact, as they not only employ the ordinary rules of algebra and geometry, but even ascend to the use of the differential and integral calculus. The formulæ are discussed in many ways, and conclusions drawn from them, but the fundamental notions on which they rest are not examined. Now, a little experience shows both those notions to be quite erroneous. The sides of the cylinder are found to exercise a great influence on the state of the steam, and the law of expansion of the steam varies according to its state and to the conditions under which it is used. When the so-called theory is applied to calculate the quantity of steam used by the engine, the results obtained would have been about as near to exactness if we had applied the rule somewhat common in my apprenticeship days, and I suspect not altogether out of use yet—namely, taking a rough guess and multiplying by two—for if we compare the quantity given by the theory with what experience shows to have been actually used, we find that the former exceeds the latter by amounts varying from 20 to nearly 100 per cent.

Similarly, when the formulæ are applied to show the advantage derived from expanding the steam, it is shown to be much greater than that obtained in practice, for it is soon found out that unless expansion is kept within certain limits, expansive working means expensive working. The ideas of engineers on this subject forty years ago are expressed by a writer on the steam engine in the following terms: "Much difference of opinion has been expressed with reference to the degree of expansion most advantageous to be used; many theoretical calculations have been made to determine the point, but they all appear contradictory and unsatisfactory. Practical considerations form the best guide, and these are often left entirely out of view by mere mathematicians. Although theoretically the economical advantage increases with the degree of expansion used, it is evident that a practical limit must be assigned by the inequality of the steam's action on the piston. It is, moreover, necessary to consider the pressure the steam should have on its leaving the cylinder. Experience gives the best rules on these points." Here the notion of the antagonism between theory and practice is clearly stated, but it would be more correct to speak of the antagonism between hypothesis and empiricism. Even when an explanation is attempted of the cause of that antagonism, the chief point is altogether missed. The popular formulæ are illustrations of the evil arising from forgetting that engineering is essentially an experimental science. They are very often convenient, however, for rough calculations, and by applying certain constants derived from experiments with different types of engines, can be made tolerably exact. What should be remembered is that they have no claim to represent a complete theory of the action of steam in the cylinders of engines. They are founded on none of the physical properties of steam, or of the materials used in the construction of engines, but are purely geometrical and mechanical. These remarks apply in great part even to what is called Pambour's theory, which, although an advance on the popular theory already described, in so far as it attempted to apply experimental rules and mechanical principles to the action of an elastic fluid, did not involve the application or discovery of any of the principles of thermodynamics.

The foundation of the thermodynamical theory of the steam engine was laid by Carnot in 1824, when he published his treatise, "Reflexions sur la Puissance Motrice du Feu," in which, although not sufficiently convinced of the correctness of the dynamical theory of heat to adopt it in his reasoning, he enunciated the fundamental principle which governs the efficiency of all heat engines, namely, that the real value of heat as a source of mechanical power depends on the temperature of the working substance relatively to that of surrounding bodies, and consequently that high pressure engines derive their advantage over low pressure engines simply from their power of making useful a greater range of temperature. Although not previously formally enunciated, this principle had been known and acted upon to a certain extent before Carnot's book appeared.

Watt evidently knew its practical effect, at least in so far as to be aware of the necessity for keeping the cylinder "as hot as the steam which entered it, first by enclosing it in a case of wood or other materials that transmit heat slowly; secondly by surrounding it with steam or other heated bodies; and thirdly by suffering neither water nor any other substance colder than the steam to enter or touch it during the time;" and further, that the condenser "ought to be kept cold by the application of water or other cold bodies.' It is doubtful, however, if he fully recognised the extent of the law, as he seldom used steam much above the atmospheric pressure, but on the contrary did all in his power to prevent the introduction of high pressure engines on Trevithick's plan. On the other hand, in some of his letters he appears to anticipate the use of superheated steam, but no evidence exists of him having tried to put his ideas on this subject into practice.

Influenced no doubt largely by Watt's example, the progress of high pressure steam was very slow, and as late as 1839 the greater part of the evidence given before a Parliamentary Committee seemed to show that the only use of high pressure steam was to dispense with condensing water, and as there was always plenty of water in the neighbourhood of a steam-boat, such steam was unnecessary for a marine engine. It was not till after Joule and others had determined by experiment the value of the dynamical equivalent of heat, and Thomson, Rankine, and Clausius, taking advantage of those experiments, had developed the mathematical side of the subject, and put it into a form intelligible to engineers, that the pressure of steam was increased to any great extent, and even then it is not clear whether the increase arose from a distinct recognition of the law of efficiency of heat engines, or simply from greater confidence in the use of steam, gained from experience in the materials used in construction, and from a knowledge of the smaller quantity of fuel consumed. In any case, from the date of these investigations the pressure of steam used in marine engines was gradually increased, until pressures of from 30 to 40 pounds on the square inch became common.

Sir William Thomson first put Carnot's Law into the form now well known to engineers, namely, that the efficiency of a perfect heat engine is expressed by the ratio of the difference of the absolute temperatures of the source and condenser to the absolute temperature of the source. When the results of Joule's experiments first became known, extravagant ideas were formed as to the state of efficiency to which a steam engine might be brought, as these experiments showed that heat and dynamical energy are mutually convertible, and hence it was thought that in a perfect heat engine the whole of the heat used might be converted into work, that is to say, that the actual work, as measured by a brake on the crank shaft, should be equal to the dynamical equivalent of the heat expended in the furnace. Carnot's Law, as expressed by Sir William Thomson, showed that this was entirely wrong, and that only a fraction of that heat can be converted into work, a fraction which depends for its value on the relative temperatures of the boiler and the condenser.

Rankine and Clausius developed the principles of thermodynamics as applied to steam engines. Working independently, they showed that in the expansion of dry saturated steam doing work in the cylinder of an engine, a certain amount of condensation must take place, and that consequently the amount of steam in the cylinder beyond the point of cut-off must be somewhat reduced by the process of expansion. Many of their results were similar, but as Rankine's work is the better known in this country, we will confine our attention shortly to his treatment of the subject. In his papers, and in his work on Prime Movers, he considered the action of dry saturated steam working in an engine with a perfectly non-conducting cylinder, then he proceeded to the case of an engine in which the cylinder is surrounded by a steam jacket and the steam is supposed to be dry at the beginning and also at the end of the expansion, and lastly he studied an engine working with superheated steam in the state of a perfect gas till the end of the stroke. In each of those cases he calculated the efficiency of the steam, and the amount of fuel consumed; but when engineers began to make experiments with steam engines, they found that the efficiency obtained was always considerably less than that given by Rankine's formulæ; and hence the work of a man who did more than any other man to reconcile theory and practice was sometimes pointed out as an example of the saying already mentioned, that a design "might be all very well in theory, but would not do in practice."

When the pressure of steam had risen to about 40 pounds on the square inch, and used with considerable expansion, the variation in the temperature of the cylinder caused a huge amount of initial condensation of the steam, followed by a certain amount of re-evaporation towards the end of the stroke. In engines working under such conditions, if we compare the weight of steam actually used with that obtained by multiplying the volume moved through per stroke by the piston by the density of the steam, it is found that the former is always much greater than the latter. This difference was formerly accounted for by supposing that steam escaped past the piston; but the researches of D. K. Clark in this country, Isherwood and others in America, and Hirn, Leloutre, and Hallauer on the Continent, have fully explained it by the action of the sides of the cylinder on the steam. Between the sides of the cylinder and the steam, or rather the mixture of steam and water, there is a continual exchange of heat. When the steam passes from the steam chest to the cylinder, there is always a considerable fall in the pressure, varying from 5 to 10 pounds on the square inch. A small part of this is due to frictional resistances and the want of uniformity in the velocity of the steam caused by the varying motion of the piston, effects which are generally included under the term *wire-drawing of the steam*, but by far the greater part is caused by the condensation of the steam against the sides of the cylinder, which become covered by a film of water. During expansion, heat is restored to the condensed steam, and the film of water re-evaporates either partially or wholly. With low rates of expansion the temperature of the internal surface of the cylinder is higher than the exhaust temperature, while with high rates the temperature during expansion is so much reduced that when fresh steam enters at the beginning of the next stroke, a large quantity of steam is condensed which is not all re-evaporated during expansion. It is evidently possible (under certain conditions which we will consider further on) to obtain a ratio of expansion in which the re-evaporation balances the condensation.

There is, however, another cause of condensation in cylinders. As already mentioned, Rankine and Clausius showed that when steam expands in a cylinder doing work, a certain portion of it is liquefied, but the effect of this is very different from that produced by the action of the sides of the cylinder, the steam liquefied being distributed throughout the whole mass of steam in the cylinder, and it passes to the condenser without abstracting much heat from the cylinder; whereas in the other case steam is deposited as a film on the walls of the cylinder, and is partly evaporated during expansion and exhaust. Some of it, however, generally remains in the liquid state at the end of the stroke, and being evaporated by the metallic surfaces in contact when these are exposed to the action of the condenser, the steam is carried to the condenser without performing work, thus causing a direct loss, which is generally called the loss through the *cooling action of the condenser*, or the *exhaust waste*, the intensity of the action depending on the amount of moisture on the sides of the cylinder; and in single cylinder unjacketed cylinders, in which high pressure steam is expanded to a considerable degree, it is evident that the loss of efficiency must be great. The amount of this loss is evidently equal to the excess of the heat taken up by the cylinder walls over that necessary to supply the loss of energy from the steam during expansion. In addition to this direct loss, the heat which passes to the condenser also causes an indirect loss by raising the back pressure.

These considerations show us that the statement that high pressure engines derive their advantage over low pressure engines simply from their power of making useful a greater range of temperature must be taken with certain limitations, for when that range is too great in one cylinder, the loss from condensation may be sufficient to neutralise the economy which would otherwise result from high pressure and expansion. They also show us that what are usually called theories of the action of steam in engines are very imperfect, and unless checked by the results of experience, are very misleading. A complete theory of this action should not only take into account the properties of steam, but also the effects on the steam on the special parts of the engine which are designed to transmit the energy exerted. The ordinary popular theory makes certain assumptions as to how steam may be supposed to behave, and proceeds to reason on these, while that of Rankine and Clausius, founded on experimental knowledge of the properties of steam, and deduced from the principles of thermodynamics, is far from being complete, because it fails to take into account the action of the sides of the cylinder. In fact, in the present state of our knowledge, we may agree with Hirn when he says that a theory, properly speaking, of the steam engine is impossible—an experimental theory derived from the results of experiments with engines of different designs can alone lead to vigorous results.

It must not be supposed that in making these remarks I wish to lower the value of such investigations as those of Rankine. As a pupil of Professor Rankine, I have a profound respect and admiration for him as a man of genius who adorned every subject which he touched, and as a teacher whose influence and example stimulated all who had the good fortune to be placed under him; and I am convinced, if he had possessed the results of those experiments which are now available to engineers, he would have been the first to have modified his views in accordance with them. His investigations are excellent so far as they go, and point out the directions in which improvements are likely to take place; what is now wanted is that they may be viewed in the light of experiments with actual engines, and be made more complete by taking into account the causes which prevent his results from agreeing with those given by the experiments. As will be explained further on, he did study the few experiments available in his time, but these were not sufficient to enable him to estimate the extent of the action of the sides of the cylinder, and thus lead him to modify the expressions used in his treatise on the steam engine.*

Although the action of the sides of the cylinders has never been distinctly recognised by British engineers, still their practice has been greatly influenced by that action. The methods which have been adopted, either separately or together, for improving the efficiency of steam engines, are:—

1st. Surrounding the cylinder by a steam jacket.
2nd. Reducing the variation of temperature in the cylinder by the use of compound engines.
3rd. Superheating the steam.

Now the effect of all those is to reduce the action of the sides of the cylinder and thus prevent loss in the manner already explained, but it is doubtful if that effect has always been clearly understood by those who designed the engines. The steam jacket was designed by Watt, but it is not known what ideas he held as to its effect, as he published no reason for using it except that of keeping the steam as hot as possible. His immediate successors certainly did not understand it, the general opinion being that it only served to prevent radiation from the external surface of the cylinder, and as the jacket presented a greater surface than the cylinder, reasoning from the caloric hypothesis they inferred that it was not only unnecessary but wasteful, and its use was abandoned to a great extent in land engines, and entirely in marine engines. In the same way with the compound engine it was shown from theory that it was immaterial whether expansion took place in one cylinder or in two or more successive cylinders, and the early compound engines

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* M. Hirn has said: "*A peine ai-je besoin de dire que si je m'exprime ainsi ce n'est en aucune façon pour critiquer, dans le sens ordinaire du mot, ce qui a été produit jusqu'ici comme théories des moteurs caloriques. Les travaux de Clausius, de Rankine, de Zeuner, de Combes, etc., sur ce sujet, sont et resteront des œuvres mémorables dans l'histoire de la science. Si, comme experimentateur et comme analyste, je viens montrer aujourd'hui que ces œuvres demandent à être complétées, j'espère que, bien loin d'en affaiblir la valeur, je ne la mettrai que plus en relief.*"

were designed chiefly from considerations of strength, and it was not till the principles of thermo-dynamics were developed that some of the real causes of the economy were understood, and even those principles required to be supplemented by reasoning founded on experiments with actual engines. On the other hand, when steam of low pressure was in ordinary use in marine engines it was not unusual to superheat it to a considerable degree above the temperature corresponding to the saturated condition, but now that higher pressures have become common superheating has been almost entirely abandoned, and further economy is now attempted chiefly by raising the pressure of the steam, and increasing the number of cylinders in which it is used.

A most important question at present for marine engineers to consider is, whether this latter practice is a reasonable procedure, which is supported by what we know of the properties of steam and its action in the cylinders of an engine, or whether better results could not be obtained by keeping to moderate pressures, combined with a certain amount of superheating, thus obviating the necessity for any further increase in the number of cylinders. A modern marine engine may be characterised as a collection of compromises, and consequently its design cannot be criticised from ideal standards, but regard must also be paid to the conditions required to be fulfilled in practice. A perfect engine should not only give the highest efficiency when considered as a heat engine, but also when considered as a machine, and in attempting to gain one of those ends we may sacrifice the other. If, for instance, in trying to get the greatest amount of work out of a given quantity of steam at a given pressure, we increased the number of cylinders to such an extent that the extra friction of the pistons, valves, stuffing boxes, &c., more than balanced the saving from the increased efficiency of the steam, and from the lessened friction of the crank shafts, it would evidently have been better to have selected a simpler, although less perfect design for the engine as a heat engine. Moreover economy of fuel is not the only point to be considered in marine engines. Their original cost, their cost for repairs, their liability to break down, and the space they occupy are all matters which require to be carefully weighed, as the loss arising from those causes may be greater than the gain from economy of fuel. It may therefore be set down as an axiom that marine engines should contain as few parts as possible, and that those parts should be simple and conveniently arranged for repair; and as present practice tends in the direction of greater complexity we will proceed to consider shortly some of the bearings of steam jacketing, compounding, and superheating on that practice. Each of those subjects is sufficient in itself for a paper, so that all we can do in the meantime is to confine our attention to their outlines.

In the first place, however, it may be well to note the following results of experiments by M. Hirn showing the effects of the different methods which have been adopted for reducing the loss through the action of the sides of the cylinders.

1st.	Simple engine, without steam jacket,	Loss,	33	per cent.
	Same engine with steam jacket, - -	„	21	„
2nd.	Compound engine, two cylinders, without jackets, - - - - - - -	„	26	„
	Same engine, with jackets, - - -	„	20	„
3rd.	Simple engine, without steam jacket,	„	18	„
	Same engine, without steam jacket but with steam superheated 86° C., -	„	12	„

All those engines cannot of course be compared the one with the other as they worked with different pressures and rates of expansion. The comparison must be confined to the figures relating to the same engine, and these show a considerable reduction of loss by one or more of the methods named.

The following results, also by M. Hirn, show more distinctly than those given above, the economy arising from the use of superheated steam as compared with saturated steam in the same engine.

1st. A compound engine working with superheated steam at a temperature of 210° C., an economy of 20 per cent.

2nd. An engine with one cylinder, working with superheated steam at 225° C., an economy of 31 per cent.

3rd. The same engine, with steam at 245° C., an economy of 47 per cent.

In practice the commercial efficiency of steam engines is generally measured by the consumption of coal per I.H.P. per hour, but this is not a good standard for comparison of different experiments, as the calorific power of coal varies according to its quality. A more scientific standard is formed by taking the weight of dry saturated steam used per Indicated Horse Power, or per Effective Horse Power, per hour. The following results of experiments by M. Hallauer are all reduced to those standards:—

Engines with Two Cylinders.

		Ratio of Expansion.	Consumption in Kilos. per I.H.P.	Consumption in Kilos. per E.H.P.
Beamed Engine, . .	Jacketed	13	7·9	9·1
Do. . .	Do.	7	8·2	9·3
Horizontal Engine, .	Do.	6	8·3	9·2
Vertical Engine, . .	Do.	5	8·5	9·5
Engines with One Cylinder.				
Corliss Horizontal Engine	Jacketed	6	7·9	8·6
Hirn's Beamed Engine, Superheated Steam at temp. 200° C.	Non-jacketed	7	7·2	8·0

These results bring out three facts. In the first place, they show that superheated steam is more economical than saturated; in the second place, that simple engines *can* be designed which are as economical in the steam used per I.H.P. per hour as well-designed compound engines; and, in the third place, that the mechanism of simple engines has a higher efficiency than that of compound engines, that is, that the steam used per Effective Horse Power is less in the former than the latter. M. Hallauer concluded that it is possible to construct a beamed engine with a single jacketed cylinder, having four valves (two for ingress and two for egress of the steam), which shall be as economical as a compound engine, provided the ratio of expansion varies from $\frac{1}{4}$ to $\frac{1}{7}$, and that the clearance spaces do not exceed 1 per cent. of the volume of the cylinder; and also that the expenditure of steam per *effective* horse power, for engines with one, two, or three cylinders, properly designed and constructed, is so nearly equal, that any one of those types may be adopted which is most convenient for the circumstances in which it is to be used. Whether his deductions as to the efficiency of the mechanism hold generally can only be decided by further experiments. Very few experiments on the efficiency of modern compound marine engines have been made, so that it would be rash to generalise on M. Hallauer's results; but this much may be said, that they afford good grounds for reflection to constructors of such engines.

The beneficial effect of the steam jacket arises from the fact that it prevents condensation of the steam in the cylinder, by imparting heat to it through the sides of the cylinder, thus causing condensation to take place in the jacket where the pressure is constant instead of in the cylinder where it is variable. Its action is, however, more complicated than is generally supposed. It not only prevents or lessens the condensation of the steam, which would be caused by the external work done, but it modifies in a great degree the action of the sides of the cylinder by reducing the condensation during admission, and increasing the evaporation during expansion, so that at the end of the stroke the proportion of water in the steam is much less than it would be without the jacket, and thus the cooling effect of the condenser is reduced. The advantage from the use of a steam jacket varies according to the circumstances under which it is employed, and in some cases, as for instance, when low rates of expansion are used, the jacket may not only be useless but wasteful. On the other hand, when high rates of expansion are used, by preventing the temperature of the cylinder from falling below the boiling point, corresponding to the initial pressure of the steam, the economy resulting from the action of the jacket is considerable, the work developed by the engine being increased by an amount varying from 15 to 25 per cent. of what would be developed without the jacket.

But contradictory as it may seem, the use of a steam jacket (as ordinarily used) is a violation of the fundamental law of maximum efficiency of heat engines, which requires that they should receive all their heat at the maximum, and give it out at the minimum temperature, and not as in the case of an engine with a steam jacket at temperatures between these, and at times when the heat imported lessens the efficiency, which it evidently must do at and near the end of the stroke. The steam jacket may thus be looked upon as a necessary evil, justified only by the physical properties of the steam, and of the materials hitherto used in engines, for while it increases the work done by the expanding steam, this increase is by no means so great as it would be if the heat employed in the jacket steam had been applied to generate more steam.

During Session 1861-2, Professor Rankine read a paper before this Institution, giving an account of Mr Isherwood's experiments with the engines of the U.S. steamer "Michigan," in which he stated that the temperature of parts of the cylinder rises and falls with that of the steam, while the great mass of it remains at a nearly uniform temperature. Some of his statements in this and subsequent papers are, however, inconsistent with this view. His latest opinions on the subject may be taken from his Memoir of John Elder, published shortly before his death, in which he said—"One of the earliest consequences deduced from the principles of thermodynamies was, that when steam performs work by expansion, a quantity of heat disappears sufficient not only to lower the temperature of the steam to that corresponding to its lowered pressure, but to cause a certain portion of the steam to pass into the liquid state. The steam thus spontaneously liquefied collects in the form of water in the cylinder; and if the cylinder and piston were made of a non-conducting material, that water would simply be discharged from time to time into the condenser, without causing any waste of heat. But the cylinder and piston, being made of a conducting material, give out heat to the liquid water which adheres to them, so as to re-evaporate it when the communication with the condenser is opened; and that heat is carried off to the condenser with the exhaust-steam, leaving the piston and the inside of the cylinder at a low temperature, even though the outside of the cylinder should be clothed with an absolute non-conductor. When steam from the boiler is admitted at the beginning of the next stroke, part of it is immediately liquefied through the expenditure of its heat in raising the piston and the inside of the cylinder again to a high temperature, the result being that at the end of the second stroke, the quantity of liquid water, which is re-evaporated and carries off heat to the condenser, is greater than it was at the end of the first stroke. At each successive stroke that quantity augments until it reaches a fixed amount, depending mainly on the difference of the temperatures of the steam at the beginning and end of the expansion; and the effect is the same as if a certain quantity of steam at each stroke passed directly from the boiler to the condenser without performing work. In some experiments lately made, the quantity of steam which thus ran to waste was found to be greater than that which performed work; so that the expenditure of steam was more than doubled."

"The remedy for this cause of loss is to prevent that spontaneous liquefaction of the steam during its expansive working, in which the process just described originates; and that is done either by enclosing the cylinder in a *jacket* or casing supplied with hot steam from the boiler, or by superheating the steam before its admission into the cylinder; or by both those means combined. The steam is thus kept in a nearly dry state, so as to be a bad conductor of heat; and the moisture which it contains, though sufficient to lubricate the piston, is not allowed to increase to such an extent as to carry away any appreciable quantity of heat from the metal of the cylinder and piston to the condensers."

It will be observed in those remarks Rankine does not clearly distinguish between the condensation due to expansion and that which takes place during admission, and hence he regarded it as both possible and desirable to prevent condensation altogether by the use of a steam jacket. But as already explained there is a great difference between the two, and at the end of the stroke, even when a steam jacket is used, there is (especially in the cylinders of single engines, and in the low pressure cylinders of compound engines in which it is seldom possible to balance even approximately condensation and re-evaporation) a quantity of condensed steam to be found which causes a considerable loss, whereas Rankine seems to have concluded that the action of the sides of the cylinder was only important where proper precautions had not been taken, and hence we do not find that he modified his theory of the steam engine in consequence.

A considerable number of experiments has been made to ascertain the action of the sides of cylinders of engines, but we have not yet sufficient data to enable us to determine the amount of loss from this cause under any given set of conditions of pressure, ratio of expansion and speed, with that certainty which is essential before we can make a satisfactory design of an engine. The subject is, however, too extensive to be fully treated in the meantime, but an opportunity may be given of it being studied in a future paper.

We will now consider the second method of reducing the loss from the sides of the cylinder that is using the steam in more than one cylinder, so that the range of temperature may be limited and the cooling action of the condenser confined to the low pressure cylinder with which alone the condenser is in communication, for experience shows that the wasteful action of the sides of the cylinders may be avoided to a large extent in the high pressure cylinders of compound engines (as well as in the cylinders of non-condensing engines) by limiting the range of expansion, and using a steam jacket, so that the condensation due to admission may be entirely or nearly balanced by re-evaporation. An exact balance is not absolutely necessary since the water remaining in the small cylinder at the end of the stroke is evaporated and used in the large cylinder, but if the quantity is great the steam is likely to be wet at the end of the stroke in the large cylinder with subsequent loss in the manner already mentioned.

At first two cylinders were employed in compound engines and the pressure of the steam used rose gradually from 40 to 60 pounds on the square inch, at which it remained for some years, but of late a further increase has taken place, so that pressures varying from 70 to 100 pounds on the square inch are now common. It is doubtful whether *saturated* steam at a higher pressure than the first named of those figures can be used with economy in a two cylinder compound engine, for beyond that we have a recurrence of the evils found in simple engines. This has led to triple expansion engines with pressures varying from 125 to 160 pounds on the square inch, and these seem to give the highest efficiency of any engines which have yet been designed for the mercantile service, and there can be little doubt that for *saturated* steam at the pressures named such engines are necessary. Mr Kirk, who has taken a leading part in the design and construction of engines of this class, however, has remarked, "Unfortunately as we get higher in pressure we do not by any means gain in efficiency in anything like a proportionate degree, and the time must come, may indeed not be very far away when further increase of pressure will not pay." This statement simply means, when considered in relation to the principles of thermodynamics, that as the rate of increase of the temperature of saturated steam decreases as the pressure increases, and as the cost of the construction of engines increases with the pressure, a point must soon be reached when the increase of efficiency is balanced by the increase of cost. It ought to be remembered that compound engines are not to be preferred simply because the steam is used in more cylinders than one, since in a perfect engine the amount of work done by a given quantity of steam is independent of the number of cylinders in which it is employed. In fact compound engines, from having more parts, offer a greater amount of resistance to the passage of the steam and probably also are less efficient as machines, so that from this point of view they are inferior to simple engines, and their use is only justified by similar reasons to those which justify the use of steam jackets, that is because they reduce the intensity of the action of the sides of the cylinder, by splitting up the expansion of the steam into parts, and thus *diminishing the waste of energy.*

How this occurs may be easily seen when we compare what happens in a simple condensing engine, and an ordinary compound engine with two cylinders. In the simple engine if the pressure is moderately high, say 80 pounds on the square inch, and the ratio of expansion considerable, the re-evaporation which takes place during the latter part of the stroke of the piston does not nearly balance the initial condensation, and at the end of the stroke a large amount of water is found in the cylinder, and this being evaporated when the exhaust valve is opened, is carried direct to the condenser. If the same steam be used in an ordinary compound engine the re-evaporation may be made to balance or nearly balance the initial condensation in the high pressure cylinder, if proper attention be paid to jacketing and the ratio of expansion, or if it does not, the re-evaporated steam is used in the low pressure cylinder along with the other steam, and is utilised to a considerable degree behind the piston. The low pressure cylinder may thus be looked upon as a simple engine working with a comparatively low pressure of steam, and consequently with a small variation of temperature, so that the quantity of water in the cylinder at the end of the stroke is small, and the action of the condenser on the sides of the cylinder is not intense. The superior efficiency of the compound over the simple engine thus evidently arises from the way in which the re-evaporated steam is used, and this is a sufficient explanation of the fact that the weight of water used, as calculated from the indicator diagram, is more nearly equal to that actually used in the case of the compound engine than in that of the simple engine. The designer should thus aim at attaining such a ratio of expansion that the whole energy of the steam is exerted behind the piston and not allowed to be spent in the condenser.

If steam of a higher pressure is used then the third cylinder of the triple expansion engine becomes necessary, and reasoning in the same way, a greater number of cylinders is required as the pressure is increased. Recently quadruple expansion engines, with saturated steam at pressures of more than 150 pounds on the square inch, have been introduced. The range of temperature in each cylinder is thus kept within moderate limits, but whether the resulting complexity and loss from friction of steam in passages and other causes are balanced by the economy arising from a comparatively small increase in the temperature of the steam is a question which can only be answered by experience, and for this no sufficient data have yet been published.

The compound engine has the further advantage of lessening the excessive variation of stress on the working parts, but as this opens up the wide question of the efficiency of the mechanism we will not enter into it at present. I would only remark that one of the most important pieces of work requiring to be done by marine engineers is the undertaking of a systematic series of experiments to ascertain the efficiency of modern compound engines, not only as regards the steam used in them, but also as regards their mechanism.

Lastly, we will shortly consider the question of the application of superheated steam. The limit to which the temperature of the steam may be raised with the materials at present used in construction seems to be about 450° F., although possibly a higher temperature might be employed, but in order to keep within present possibilities we will suppose the figure named to be the maximum which it is advisable not to exceed. We will also in the first place suppose that the pressure is not more than 80 pounds to the square inch.

If we recognise the fact that the condensed steam in the cylinder at the end of the expansion is the chief cause of the loss of efficiency of the steam used in engines, that steam jackets are not good arrangements for utilising the heat expended on them, and also that compound engines are only allowable because they reduce the range of temperature in the cylinders, it is evident that the most efficient manner of using the steam, if it were possible, would be to superheat it to such an extent that at the end of its expansion in a single cylinder its temperature would be only as much reduced as to bring it into the dry saturated condition. Whether this can be done must be determined chiefly from experience, but from what we already know of the properties of superheated steam we may draw certain inferences.

We know from experiments already made, that the heat required to raise one pound of saturated steam, at a pressure of about 80 pounds on the square inch, to the temperature of 450° F., is only a small part of that required to produce an equal weight of saturated steam; and also that the volume of the superheated steam is considerably greater than that of the saturated, and as the extra quantity of heat required could generally be obtained from the waste heat of the boilers (a temperature of over 600° F. being generally possible), the advantage from this extra volume would be gained, not by increasing the consumption of fuel, but by making the boilers more efficient, by enabling them to evaporate a greater weight of *dry* steam per unit of fuel used. Here it may be remarked, that boilers are often made to appear to have a higher efficiency than they actually have, by the experimenters neglecting to ascertain the state of the steam produced, for if it contains a considerable amount of water, much less heat will be required than if it were dry, and the efficiency of the boiler will appear to be high, although in reality it has been obtained at the expense of the efficiency of the steam in the engines.

When superheated steam is used there is an increase in the initial pressure of the steam, a modification of the law of expansion arising from the sides of the cylinder being at higher temperatures at the beginning and end of the stroke; and there is also a reduction of the back pressure, for Mr D. K. Clark found by experiment, that in the same engine, going at the same speed, the excess of the mean back pressure above the pressure of condensation varies nearly as the density of the steam at the end of the expansion.

Hirn has clearly proved that the action of the sides of the cylinders very soon reduces superheated steam to the saturated, and even to the wet condition. Apart from that action, it may be shown that steam, at a pressure of 80 pounds on the square inch, and at a temperature of 450° F., expanding in an adiabatic cylinder, requires to be expanded to less than twice its volume, in order that it may be reduced to the saturated condition. We may thus infer that although steam jackets are not the best means of utilising the heat spent in them, if we consider how much work might be done by that heat in a perfect engine, still they are necessary in actual engines on account of the physical properties of the materials used, even when superheated steam is employed in order that the loss from the action of the sides of the cylinders may be reduced; and further, that by their aid it is possible to use steam at a pressure of 80 pounds per square inch of pressure superheated to a temperature of 450° F. in *two* cylinders, so that at the end of the expansion it is reduced to the dry saturated condition. If, however, a good arrangement could be designed (as, indeed, has been done to a certain extent in the case of some land engines) whereby the steam which leaves the high-pressure cylinder could be superheated before it enters the low-pressure cylinder, the jackets might be omitted without any great loss of efficiency.

Such a method of using the steam would render the multiplication of cylinders unnecessary, and thus simplify the engines, and make them more efficient, not only as heat engines, but also as machines. Moreover, as we have supposed moderate pressures to be used, the cost of the engines and boilers should be less than when higher pressures are employed, even when allowance is made for the expense of the superheating apparatus. The saving on the *size* of the boilers, arising from the reduction in the amount of feed water, would be sufficient to make up for this expense.

When great power was required, the engines might be arranged in tandem style, two or more sets of practically independent engines being employed, while for moderate powers the two cylinders might be arranged alongside each other, as is usual at present. For smaller powers, Mr Holt's design of single tandem engines might be employed, if arrangements were made for preventing condensation in the cylinder by superheating and steam jacketing. With the temperature we have named it does not seem advisable to attempt the use of steam in a single cylinder, although M. Hirn found that with such an arrangement the efficiency was much higher than when saturated steam was used.

We have supposed that the pressure of the steam is only 80 pounds on the square inch, but as 160 pounds is now reached without much inconvenience, and as the triple expansion engine is not very much more complicated than that with two cylinders, the question to be decided by experience is whether the 50° F. of temperature, which is gained by doubling the pressure, is purchased at the cheapest rate. If that be decided in the affirmative, then triple expansion engines might continue to be designed, and direct superheating to about 90° F. would bring the temperature up to the limit named as at present practicable, and thus further increase in the number of cylinders be rendered unnecessary. It does not seem possible to go much beyond the highest pressure named without requiring a serious modification in the type of boiler at present in use.

There are many practical questions connected with the application of superheated steam which would require careful consideration. Into all of these we do not propose to enter at present, but it may be well to point out that when superheated steam is used, it is advisable to have separate valves for admission and exhaust, as in the Corliss engine. In marine engines it has not hitherto been the custom to attempt such an arrangement, and the efficiency of the steam has been reduced on the supposition that the necessity for simplicity of construction was greater than that for high efficiency. The question for constructors to consider is, Is it not possible to obtain both?

It may be also mentioned that the temperature of the steam used in the cylinder could be regulated by having an arrangement for mixing superheated and saturated steam. Such a method was carried out more than 25 years ago by Mr Wethered, who called the mixture combined steam, and claimed for it special propertie, which it did not possess. Whether steam be superheated by external appliances, or by adding superheated to saturated steam, makes no difference to its properties, and we might as well apply the term combined water to a mixture of two quantities of water at different temperatures. It could be arranged that only a portion of the steam pass through the superheater and be mixed with saturated steam direct from the boiler, if it were necessary to reduce the temperature to what was required in the cylinders or jackets. The temperature resulting from a mixture of superheated and saturated steam could easily be calculated, and no doubt arrangements might be made for keeping that practically constant.

In addition to the questions connected with the *design* of engines using superheated steam, many others connected with the *materials* employed in construction might at first present difficulties. Formerly, when superheating was employed with low pressure steam it was found difficult, and in some cases impossible to keep the packings of the piston rods steam tight, but with the improved materials now in use there should be no difficulty in working up to the temperature we have mentioned, as the limit to which it is advisable to confine present practice. It was also said, at the same time, that in marine engines, superheated steam exercised a peculiar influence on the materials of the cylinders through the action of the hydrochloric acid liberated by the decomposition of the salt in the water. This effect should not now be so difficult to overcome since surface condensation is almost universal, and as arrangements could be made for supplying the loss of steam from the boilers by means of distilled water. When high pressures were first introduced into locomotives the same difficulty was experienced with the cylinders, as they appeared to be affected in the same way as when superheated steam was used, but by modifying the system of casting and the mixture of iron employed in their construction, the difficulty was overcome, and pressures of 160 pounds on the square inch are now freely used in such engines, and also in some marine engines, without inconvenience, and no doubt superheated steam might also be used if the same pains were taken with it. The superheating apparatus itself was also found to give a considerable amount of trouble by getting out of order and being difficult to repair, but these are also matters of design and construction which might be overcome if seriously attempted. M. Hirn informs me that he has known engines working with superheated steam for more than 25 years, and requiring no more than ordinary repairs.

The various arguments in favour of the use of superheated steam, and some of the difficulties connected with its use, have now been stated. The questions for engineers to decide are—Whether further economy in steam engines is to be obtained by directly raising the higher limit of the temperature of the steam by means of superheating, or indirectly by increasing the pressure, and what is the best arrangement for the cylinders in which the steam is to be used. I shall not dogmatise on the subject, but I am inclined to superheating at moderate pressures, that is, at pressures varying from 80 to 160 pounds on the square inch. The pressure to be used, and the consequent design of the engine, will generally be settled by considerations of expediency, determined by a number of practical circumstances connected with the conditions under which the engine is to be worked.

If we consider what an amount of money would have been saved if, say, 35 years ago a series of well-designed experiments had been conducted which would have led to even the present practice as regards pressure and expansion, it naturally strikes us that the matter of economy of fuel becomes a national question in the prospect of the near exhaustion of our coal fields. That such a series of experiments is now wanted to decide in which direction further economy is to be obtained must be evident to all. As, however, there is no special inducement to individuals to undertake this work, there is not much probability of any sudden change in practice as regards the temperature of the steam used, but it is likely rather to be developed in the same way as that regarding pressure, that is, a gradual increase will be made as engineers gain confidence in the materials at their command.

It may be said that in the near future the steam engine will be set aside and replaced by some form of gas engine; but the difficulties in the way of the development of this kind of motor are of the same nature as those which occur in steam engines with superheated steam, namely, that the materials used in construction do not fulfil the conditions assumed in theory. So long, therefore, as we use the steam engine we should follow in the direction indicated by the general principles which are true for all heat engines, and if the materials employed suffer in consequence then steps ought to be taken to improve those materials, or to neutralise the elements which cause the damage, and we should avoid methods which lead to undue complexity in practice. In short, our theories ought to be founded on a combination of general principles and special experimental knowledge of the substances concerned, and they should be checked by the results of carefully conducted experiments, while our practice should be guided by theories thus formed, and not evolved out of an expensive system of trial and error. The science of thermodynamics may be said to be in great part the result of the study of the steam engine, that is to say, it has been developed to explain difficulties which had in great part been overcome by practical experience. It is now, however, sufficiently advanced to lead the way, and not merely to illuminate the path which has been gone over.

There are many points of interest connected with the consideration of the questions mentioned in this paper which admit of mathematical development. I have not entered into these on the present occasion, my object being not to attempt an exhaustive treatment of the subject, but rather to elicit the opinions of those Members actually engaged in the manufacture of steam engines, on the practical side of the questions involved.

In the after discussion,

Mr **Hazelton R. Robson** said the paper was one of great merit, and would be most acceptable to the manufacturing engineers present. It referred to a subject that was worthy of all attention. He approved of the suggestion thrown out, that some experiments should be made; for he was satisfied that if this had been done thirty-five years ago results would have been obtained of vital importance to the engineering manufacturing trade. He remarked further that in the early stages of the compound engine, he had made an ordinary engine, in which he had used steam expansively at 50 lbs. pressure, and had cut off the steam at about a fifth of the stroke: cylinders 38 in. diam. × 3 ft. stroke. His aim was to make, if possible, a simple engine as economical as a compound engine. The ship in which that engine was fitted ran some time, but not at all successfully. He subsequently took out the cylinders and condemned the after one, and substituted for it a new low pressure cylinder 62 in. diameter, 3 ft. stroke; nothing else about engines or boilers was altered, but all remained the same, therefore the comparison was complete. The result was that the ship steamed about one knot an hour faster, and the consumption of coal was reduced from 21 to 18 tons per diem. Therefore a more successful result had been obtained than with the former one.

Mr **S. G. G. Copestake** would like to read over the paper before being called upon to discuss it. Doubtless the compound engine had been a great success in the propulsion of steamers, and it was now attracting much attention from locomotive engineers; but whether it would be a success in locomotives had not yet been settled. He hoped to hear that point discussed at a future meeting.

Mr **Robert Duncan**, Whitefield, appealed to the older Members of the Institution to give their experience with regard to the useful effect of steam jacketing and superheating. Mr Dyer had spoken of these methods as the best for effecting economy of fuel, but explanations of the reasons why they have been so generally discarded after very general adoption, must be instructive.

The discussion was then adjourned till next meeting.

The discussion of this paper was resumed on the 22nd December, 1885.

Mr E. **Kemp** remarked that Mr Dyer looked for considerable improvements and economy being made in the superheating of steam, although in saying that it looked like going back half a century. He had had a good deal of experience in regard to superheating steam twenty or more years ago; and he could not say that it was a very happy experience. They had then a pressure of 30 lbs. on their boilers, and therefore thought it was prudent to superheat the steam. In those days they superheated the steam up to 400 degrees; but he must say that heating it to that extent was not very good for the engine, being altogether too hot and inclined to be very trouble some. He was of opinion that superheating steam with 160 lbs. pressure—which might be taken as the standard now-a-days—was not advisable, as that pressure would cause it to be much more troublesome. He could not further enter into the paper at present.

Mr **Howden** said—Although he had not had time to study Mr Dyer's paper fully, he had read it over with much interest, and considered it in many respects a most satisfactory paper. It treated a very important subject in the simplest possible manner, so that it could be read by any one with ease, and he believed also with profit. Knowing Mr Dyer's high mathematical powers, and remembering the abstruse character of a late paper of his, he confessed to having some qualms of fear when he read the title of Mr Dyer's paper in the notice of last month's meeting. It was therefore with a considerable feeling of relief he cut open the printed copy, three days ago, and found the paper did not contain a single formula. The considerations to which Mr Dyer called attention in his paper came, he thought, very opportunely. Within a very short period steam pressures had suddenly risen in marine practice from 80 and 90 lbs. to 150 and 160 lbs., and a third, and in some cases a fourth, cylinder had been added to the engine. It was therefore a judicious course to call a halt at the present time to see if we are not hurrying too fast, and whether the same ends may not be attained in a simpler way by other means within our reach. How to counteract in the simplest and most efficient way the wasteful cooling action of the metal of the cylinder on the working steam—viewing the engine in its efficiency both as a heat engine and as a machine for transmitting the force applied to it—is what Mr Dyer calls attention to, and he brings some considerations before us, deduced from the experiments and practice of MM. Hirn and Hallauer, which, he thinks, should lead us to examine whether we may not obtain as economical and effective results from superheated steam of moderate pressure in a simple engine, as by using steam of the high pressures now coming into use in marine practice, through the more complicated engine with two or three cylinders. It is not the first time this question has been raised, but he believed it had never before been so fairly and fully brought forward, and the grounds for giving the simple engine worked with superheated steam further consideration so legitimately stated, as had now been done by Mr Dyer. Notwithstanding the facts and arguments presented, however, so far as his experience and reasoning guided him, he had no fear that either the double or triple expansion engine would be superseded by the simple engine, more especially for marine purposes, for the following reasons:—Engines of considerable power work more smoothly with two cylinders on two cranks than with one cylinder and crank. If, therefore, two engines are coupled together, why not make them compound and secure the greater conservation of the heat of the steam obtainable by that arrangement? The combined simple engines referred to by Mr Dyer would not be more simple as machines, while other things being equal, they would not be so economical. Further, for very large engines, it is preferable to divide the strain on three cranks, so that without any greater complication—if not less than when three simple engines are coupled—it would be an advantage to use a triple expansion engine, and take advantage of the greater economy obtainable from the use of steam of a higher pressure. Viewing therefore the engine from the point of simplicity or of its efficiency as a machine, he could not see that any advantage could be gained by using simple engines working on the same shaft in preference to double or triple expansion engines. There would certainly be a greater inequality in the strains on the crank shaft with the simple engines. With two tandem engines working on two crank shafts, mentioned by Mr Dyer for large power, these would, he supposed, be compound engines with four cylinders. This form of engine was quite as complicated as any triple expansion, and did not distribute the strains so well. The single crank type of engine used by Mr Alfred Holt, with fly-wheel and gear for pulling the engine off the dead centre when it happened to stick there, he did not find less costly than compound engines of the same power on two cranks, and he had constructed a considerable number of them to order. Then, considering the amount of superheating required to maintain steam in a single cylinder when expanded, say from 70 or 80 lbs. above, to 9 or 10 lbs. below the atmosphere, so that it would just be about the point of saturation when exhausting; this heat would be far too high to be safely worked in a high speed marine engine. It would be too dry to afford lubrication in itself, therefore the friction between the pistons and cylinder, and the valves and their faces, would become too great and cut up the metals. Extreme heat also injured lubricants and the packings of the piston and valve rods. These considerations were against the probability of obtaining from the simple engine with superheated steam as great an efficiency combined with equal economy as could be obtained by compound or triple engines, with steam of same pressure less superheated. In the cases stated by Mr Dyer, on pages 58 and 59, of the superior results obtained by MM. Hirn and Hallauer, with superheated steam with single cylinders, it would be necessary to have a great many more particulars in regard to pressures, dimensions, mode of working, &c., to enable us to make a proper comparison of these cases. Taking the best result, given on page 59, where it is stated that the engine only used 7·2 kilogrammes of water or steam per I.H.P., he would say that this, which was equal to 15·86 lbs., was considerably more than was used in some compound engines he knew of. He would like if Mr Dyer could give them fuller information regarding these experiments. Again, with double or triple expansion engines, a much smaller degree of superheating would suffice to preserve the steam from liquefaction at the termination of its expansion in the low pressure cylinder, so that the advantage of superheating could be more easily attained in these types of engine than in the simple engine. Mr Dyer classes the cylinders of single engines and the low pressure cylinders of compound engines in the same category as regards liquefaction of steam. He could not agree to these being regarded as alike in this respect, for in working with saturated steam of considerable pressure in a compound or triple expansion engine, it was in the first, or first and second, cylinder the greatest liquefaction was found, and this was a point greatly in favour of these types of engine as regards economy. The reduced liquefaction in the low pressure cylinders of these engines was no doubt partly owing to the smaller range of difference of temperature between the initial and terminal pressure of the steam, but it also was owing to the fact that the heat given up by liquefaction in the cylinders, not in connection with the condenser, and that taken up by re-evaporation, was not lost, but was utilised in the low pressure cylinder, where it assisted in maintaining the temperature, becoming as it were a means of superheating for that cylinder. It was in this that a great advantage of the compound, and still more of the triple engine, lay. Not only did these types reduce the difference of temperatures between the initial and exhaust steam in each cylinder—they also, so long as the heat in the first cylinders was not lost outwardly by radiation and other wastes, so utilised the heat that it did not matter much about the liquefaction in these cylinders. All the heat robbed from the metal of these cylinders, as he had already said, was not lost, as it would be in the cylinder of a simple engine in connection with a condenser, it appeared in the low pressure cylinder, and was well utilised there. These general considerations led him to the conclusion that the simple engine, even with superheating, would not displace the compound and triple expansion types of engine. There was one thing, however, in which he entirely agreed with the scope of Mr Dyer's argument, and that was the mistake of supposing that a higher economy was secured by the mere fact of using a higher pressure. A greater economy could only be got from a higher pressure by using it properly. He remembered that so far back as 1860, when using steam of 100 lbs. pressure in a compound engine, he was led, by studying the results of working that engine, to the conclusion that steam of so high pressure could not be properly utilised by expanding through two cylinders only, and anticipating that pressures even higher than 100 lbs. would shortly be used, he patented in that year, along with some other things, the triple expansion engine, so that he had been somewhat early in the field with this class of engine. Looking at the matter generally, he was of opinion that the limit of advantageous working pressures had been reached, and that a good deal more was yet to be got by judicious means from the more moderate pressures, which were now being superseded.

Mr LAURENCE HILL said he agreed with Mr Kemp's remarks. His experience in using superheated steam with pressures of only one or two atmospheres also was that the valve faces were destroyed; and though he agreed with Mr Howden, that economy of power could be got by using two cylinders and one crank, yet there was little if any saving in cost of this construction, and he was of opinion that they could superheat to a greater extent with steam at 150 lbs. or 160 lbs. than they would do with steam at 30 lbs. pressure, as with the same degree of heat there was more moisture in higher than in lower pressures.

Mr J. J. COLEMAN remarked that he had read Mr Dyer's paper carefully, and while he did not mean to take any part in the discussion of it, he would say that they must all admit that the principles he enunciated were sound, and well worthy of being applied in practice. There could be no doubt but that it was an advantage to jacket the cylinders of engines, but the question was whether the condensation and the radiation of heat from insufficient jacketing, or the loss of steam in the jacket itself (except by making the jacket very clumsy) was not as much as the advantage in jacketing in many cases. Then with regard to the compound engine, it was doubtless an advantage to expand the steam first in a small cylinder and end in a large cylinder; but as to adopting the principle of a simple engine, it was debateable whether the rubbing surfaces, and consequently the friction, would not be less in the simple than in the compound engine. With reference to small engines, such as those of 16 inch cylinders, it was questionable whether there was any advantage in compounding; but when they got to large engines, especially marine engines, there was undoubtedly an advantage in compounding. Then he came to the question of superheating—a subject which had very properly been revived by Mr Dyer. It was of the utmost importance. It was a question of materials; and not a question involving any doubt of the propriety of doing it; but rather the mode of doing it. Mr Dyer had not pointed out any particular way of doing it, or of constructing a superheater, with the high pressures named. No doubt the lubrication of the moving parts with dry steam would be very difficult, as a temperature of 450° to 460° would have to be employed. All vegetable and animal oils were destroyed at this temperature, and most mineral oils evaporated in great part at about 500°, though it was not impossible to get mineral oil boiling as high as 800° Fahr.

Mr H. R. ROBSON agreed with Mr Coleman with regard to superheating. He was well aware of the difficulties encountered with superheating at the date named. The difficulty was to get a superheater that would stand. All the appliances tried, he believed, failed.

Mr W. J. MILLAR (Secretary) said that in reference to the remarks made by Mr Dyer (see page 49) upon the hyperbolic curve in connection with the law of expansion of steam in the cylinder of a steam engine, he would like to point out that the late Prof. Rankine, in an article contributed to *The Engineer* in 1870, and afterwards republished in "The Memorial Volume," discusses the advantages of compound engines, and shows how to draw the curves illustrating the law of expansion of the steam in the cylinders. In this article, Professor Rankine shows that the hyperbolic curve may be used for this purpose. At that time, therefore, Professor Rankine believed that the curve fairly represented the law of expansion in the cylinders of steam engines, under the conditions then existing. He would like, therefore, to ask Mr Dyer if he believed that the experimental data obtained with the use of higher pressures since 1870, are of such a nature that the hyperbolic curve cannot now be accepted as it was by Professor Rankine, as "a good approximation to the true expansion curve."

Mr FRANK W. DICK wished to add a few words to what Mr Millar had said about the curve of expansion in the cylinder. In relation to this curve it did seem strange that Professor Rankine did not take into account the cooling action of the side of the cylinder. It must have been that he thought that the hyperbolic curve showed practically what took place in the cylinder; because Mr John Elder and Mr James Brownlee, who were in constant communication with Dr Rankine, both knew of the two separate condensations and discussed them frequently with him. In one part of his paper Mr Dyer mentions that in some land engines the steam is superheated after it leaves the high pressure cylinder, and points out that this might be beneficially done in marine engines. He (Mr Dick) did not know whether Mr Dyer knew that this had been already done. About the year 1860, Mr John Elder superheated the steam between the high and low pressure cylinders in two steamers named the "Murillo" and "Valasquez." After passing through the high pressure cylinder the steam was carried in cast-iron pipes round the boiler uptake. He was sorry he could not say fully what the economical results were, but he believed they had difficulties with the superheaters. He would like to ask the meeting if any engine was known in which superheated steam was passed round the jackets before entering the cylinders. Of course in such a case the aim would be if possible to have the superheated steam round the jackets at such a temperature that when the steam left the cylinder it would have a temperature just sufficient to prevent condensation. He had been asked the question, "When steam did 'work' in a cylinder, was the loss of heat before the valve had closed of any importance?" That was to say, before the valve was shut, when steam did "work" in a cylinder and an equivalent of heat disappeared, was the effect so far as condensation was concerned confined to the cylinder or did it extend back to the boiler?

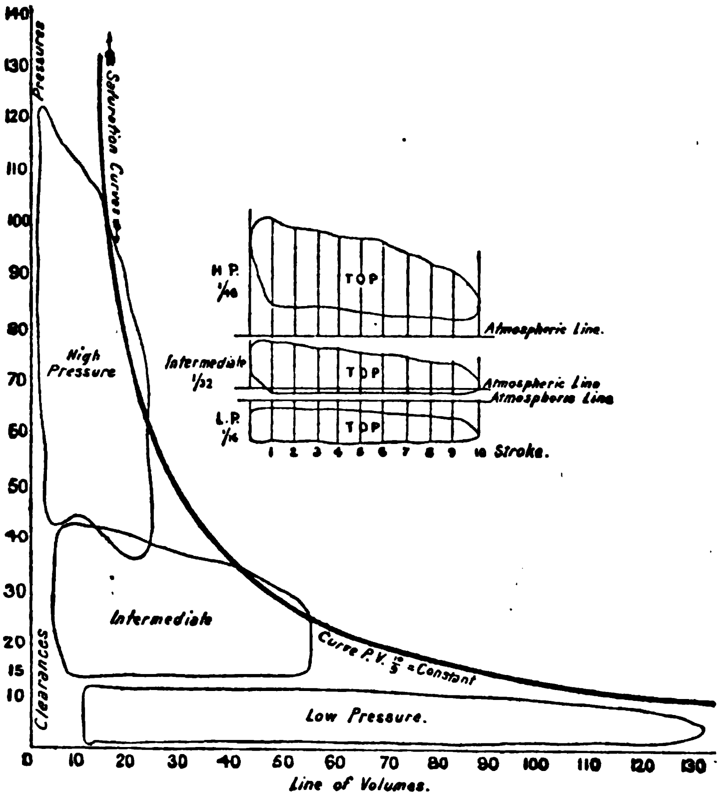
Perhaps Mr Dyer could answer this question. Of course the small amount of condensation, so long as it did not take place on the sides of the cylinder, would not affect the economical working of the engine. He thought they were all indebted to Mr Dyer for his able paper, as well as for the clear manner in which he had expressed his views. It was a clear and simple exposition of the principles on which engines could be made to work economically.

On the suggestion of the CHAIRMAN, the discussion was continued to next General Meeting, so as to give further opportunity to elicit information on this important subject.

The discussion of this paper was resumed on the 26th January, 1886,

Mr R. L. WEIGHTON (through the Secretary) said—Mr Dyer's paper—which I have read with pleasure and profit—is of a nature calculated to arrest the attention of those members of the engineering profession who are engaged in the production of the latest types of the marine engine of the present day. This paper comes very appropriately at a transition period in the history of the marine steam engine—at a time when triple expansion and high pressures are fast displacing the double expansion principle and pressures below 100 lbs. per square inch. This new departure, involving, as it does, a multiplication of cylinders and parts, naturally stimulates inquirers to ask afresh the very legitimate question, Why should we not obtain from a single cylinder as good results as we now realise from double or triple cylinders? This, I take it, constitutes the principal question raised by the writer of the paper. And while he indicates his belief that the single cylinder may under certain conditions be preferable, and gives reasons for this belief, he has been compelled to leave the main question practically without answer. This, in the present condition of the subject, was inevitable, and that simply because, as he himself points out, "Engineering is essentially an experimental science." It is because of the necessity of the experimental treatment of the subject under discussion that there can be no *complete theory* of the action of the steam in the cylinders of steam engines; but I apprehend that Mr Dyer scarcely does justice to what might be called the "theoretico-practical" acquirements of the engine builders of the present day, when he "suspects" that some of their methods may be compared to a guess multiplied by two. Rankine's researches on the steam engine are widely appreciated among marine engine builders. The rules he deduces are based on a theory which is admittedly correct as far as it goes; and it is precisely where it stops that experiment comes in, and enables the engineer, by means of factors derived from his experience, to make calculations which practice corroborates with tolerable uniformity. As a device for lessening, or altogether eliminating, the wasteful action of the walls of a cylinder on the steam expanding within it, there seems no doubt that—considered by itself, and regardless of the mechanism—the practice of superheating is superior to either jacketing or compounding, But the ultimate economy of an engine must include the whole machine, not only as a heat engine, but as a piece of mechanism. In the efficiency of the latter would be included the durability and action of the boilers as well as of the engines. When all these are taken into account, the device of superheating is seen to entail considerable disadvantages, while the plan of compounding has many advantages; and practice has hitherto given its verdict emphatically in favour of the latter. On the vexed question of jacketing, further and more definite experiment is needed. In fine, each and all of the three points raised by the writer of the paper necessarily require, from their very nature, experimental elucidation; and I am glad to see that in the paper this is advocated with some degree of emphasis.

Principal JAMIESON (through the Secretary) said—Mr Dyer would have added considerable interest and importance to his paper if he had brought forward a few sets of "combined" indicator diagrams plotted to one scale, taken from the latest and best practice in compound and triple expansion engines, thereby showing us graphically and exactly where condensation and re-evaporation take



place in the different cylinders. Fig. 1 shows such a diagram of one of the early triple expansion engines, in which the loss of work from condensation and re-evaporation is not very great, and the expansion of the steam follows very closely to the theoretical curve for saturated steam, viz.: $PV^{\frac{10}{9}}$ = a constant. Such combined diagrams of work are most instructive, for they show at a glance where loss of pressure or abnormal increase of pressure take place in each cylinder. It is quite impossible to tell this with any degree of accuracy by a mere inspection of the separate diagrams as taken direct from the cylinders. Perhaps members who have taken part in the manufacture and trials of some of the latest triple expansion engines will come forward with copies of the combined diagrams, and prove a still closer approach to perfection and how the improvements were effected. In order to make the information complete, "crank effort," and, where the power is small, brake horse power diagrams should be added. The condensation of a certain amount of steam on admission to the high and to the intermediate cylinders of triple expansion engines is by no means considered an evil, or a direct source of loss of energy in practice, for the condensed steam forms a simple and automatic means of lubricating the pistons, and being for the most part re-evaporated during exhaust, it enters the next cylinder, and there gives out work on a larger piston area. If no liquefaction took place, more lubricating oil would have to be used in these cylinders, and the use of large quantities of certain kinds of oil has proved most destructive to the boilers. Many oils which lubricate very well when used in cylinders with low pressures do not act well at high temperatures, or in cylinders using dry or high pressure steam. It is found advisable with high pressure marine boilers and surface condensers to return every possible particle of condensed steam to the boilers, so as to prevent as far as possible the necessity for the introduction of any salt water by the "auxiliary feed," consequently, by far the greater part of the cylinder's lubricating oil, after being swept into the condenser, is discharged direct from the hot well into the boilers by the feed pumps. There, this oil settles down on the furnace crowns in the form of a thick chocolate-coloured paste, and ultimately cakes. This paste is of a highly non-conducting nature and prevents a free flow of heat from the furnace crowns to the water in the boiler. In this manner it has been blamed as the cause of bringing down not a few of them. If the high and the intermediate cylinders of triple expansion engines are simply well lagged all over so as to prevent loss of heat by radiation, the mere condensation of a small amount of steam at the beginning of the stroke and re-evaporation at the end, enable them to be run with a minimum quantity of oil, and, in the end, the engines and boilers combined work more economically and sweetly. Steam jacketing in the third or low pressure cylinder is no doubt a decided advantage in most cases, for by the time the steam has reached this cylinder its condition and the condensation may be such, that excessive heat energy would pass direct to the condenser by re-evaporation without doing work. The superheating of high pressure steam of 150 to 170 lbs. on the square inch is impracticable until some better, and, at the same time, cheap lubricant is found to work effectively in connection therewith, and a means of trapping it before it gets to the boilers devised.

Mr JOHN TURNBULL, Jun., said he had read Mr Dyer's paper "On the Theory of the Steam Engine" with considerable interest, and although it seemed to him to be addressed more particularly to marine engineers he would, with their permission, offer a few remarks on the subject, with a view to elicit further comments, as he took it that Mr Dyer was anxious to get the opinions of as large a number of members as possible on the relative merits from an economical point of view of—as he himself puts it—

(1) Surrounding the cylinder by a steam jacket,
(2) Reducing the variation of the temperature in the cylinder by the use of compound or multiple cylinder engines, and
(3) By superheating the steam.

As a contribution to the subject he had placed before them the results of experiments made by MM. Hirn and Hallauer, who might be perfectly correct in their deductions, although the details were altogether insufficient to be held as satisfactory—for they, as engineers, preferred to have the whole facts and conditions of each case stated so that they might make their own deductions and view the question in the light of their own experience. Mr Dyer had told them, however, that he wished to keep his paper within reasonable limits, and he (Mr Turnbull) was sure from personal knowledge of Mr Dyer's high attainments that he had satisfied himself regarding these results; for he also informed them that they had brought out three facts, viz.:—

(1) That superheated steam is more economical than saturated steam,
(2) That simple engines can be designed to produce as much

work with 1 lb. of steam as well-designed compound engines, and

(3) That the mechanism of simple engines has a higher efficiency than that of compound engines.

Now, this opened up a wide field of controversy, for on all these points there was a considerable difference of opinion amongst engineers, both land and marine. The question of the economy of superheated steam had been before the country for very many years, and had been discussed by many men eminent in their profession, and he felt justified in saying that the general concensus of opinion was in favour of superheated steam as a means of economy; but there had been practical difficulties in the way which seemed to have caused this system to be almost wholly abandoned, and not the least of these difficulties was the inability to control the temperature of the superheated steam, as the increase of heat was generally obtained by utilising the waste gases from the boiler furnaces, which, owing to their variation in temperature, overheated, and resulted in the oxidation of the pipes, the cutting up of the valve faces as well as the cylinder itself, and the destruction of the joints of the various parts through which the steam passed. Mr Wethered, of the United States, endeavoured to overcome this by having two pipes leading from the boiler to the engine—one passing through the superheater and the other passing direct to the engine filled with saturated steam, both being mixed up in the valve-chest, as a sort of compromise between over-dry and over-wet steam, but even this arrangement was ultimately abandoned. The system of lubricating the internal parts of the cylinder by fits and starts, as in the old mode, had no doubt something to do with the wear and tear of the cylinder and slides under superheated steam, but he thought this drawback might be said to be now overcome by the drop system, which continuously lubricates the steam, if the lubricant itself does not become decomposed at the higher temperatures. Sir C. W. Siemens had given it as his opinion that an addition of 100° in the temperature of the steam used was sufficient to prevent liquefaction to any appreciable extent, for it was quite well known to practical men 30 years ago the cooling effect of the cylinder sides on the steam entering, or in other words that with saturated steam the cylinder in some degree represented a condenser at the beginning of the stroke and a boiler at the end. Before leaving this section of the subject he would like to say just a word as to the utility of the steam-jacket where ordinary saturated steam was used. He adopted the jacket in his own practice in almost every case, and was satisfied that he got results at least equal to those stated in the paper as got by M. Hirn: but he had been astonished to find, in cases which had been brought before him, the absurd methods which some engineers called steam-jacketing, such as passing the exhaust steam round the cylinder on its way out, or passing the live steam through the jacket on the way to the cylinder, instead of having a separate pipe leading from the boiler for the sole use of the jacket. The Messrs Humphreys, of London, placed on record some years ago the advantages of a steam jacket on a compound engine at their works. Their experience was that with steam in the jacket it took 22 lbs. pressure in the boiler to drive the engine at 130 revolutions per minute, whereas without steam in the jacket 29 lbs. were required to maintain the same speed. He remembered a case in his own experience where through the jacket giving way an increase of 20 per cent. in the steam was incurred until it was replaced. From the study of a large number of diagrams from compound engines he had also found that, taking a gross power of 200 H.P. the high pressure cylinder will develope 110 H.P., and the low pressure 90 H.P. on an average *without* jackets, while *with* steam jackets the case is reversed, the high pressure averaging 90 H.P., and the low pressure 110 H.P. In regard to the second conclusion in Mr Dyer's paper, namely, that the simple engine can be made as efficient economically as a well-designed compound engine,—he was sorry he could not agree with Mr Dyer on that point. Whilst he was willing to admit that the simple engine had been brought to a high degree of efficiency, he took it to be a generally established fact that compound or multiple cylinder engines were still ahead in permanent economical results; and indeed M. Hirn's own results showed the efficiency of an unjacketed compound engine to be greater than that of an unjacketed simple engine, although in a lesser degree than his (Mr Turnbull's) experience had revealed to him. Mr Dyer stated in an early part of his paper that when the steam passed from the steam chest to the cylinder there was a fall in the pressure of from 5 to 10 lbs. on the square inch. Now that was a loss which should not be debited to the cooling influence of the cylinder sides, but rather to the influence of the throttle valve, and was quite overcome in all engines where a proper automatic cut-off valve was adopted and the throttle valve entirely dispensed with. He had not had an engine made with a throttle valve for many years, and the difficulties which were supposed to debar automatic cut-off valves from use in marine engines could quite easily be overcome if marine engineers were desirous that this system should be adopted. He had over 100 engines working with his own form of automatic cut-off, and he hoped at some no distant date to bring the subject before this Institute. Then as to Mr Dyer's third conclusion, that the mechanism of simple engines had a higher efficiency than that of compound engines,—he was at a loss to know what M. Hallauer meant, or how he ascertained "the effective H.P.," and he would like to have Mr Dyer's explanation on this point. It had, however, been shown over and over again that the simple engine exerted a much heavier strain on its working parts than the compound engine, and that with an equal number of expansions the low pressure cylinder of a compound engine needed to be no larger than the cylinder of a simple engine, so that with a reasonable number of expansions, consistent with the economical use of modern pressures, the simple engine performed its work by a series of blows or kicks, receiving the boiler pressure on the large area, whereas by the more reasonable mode of expanding in two or more cylinders these rude shocks were very much modified. He should have liked to have said more on this most interesting subject, for, as he had already said, Mr Dyer's paper opened up a wide debatable field, but as he had been able to glance briefly at the more important points he would not attempt to trespass further on their time or patience.

Mr Robert Duncan, Whitefield, said the interest of the discussion concentrated very much on the questions of jacketing and superheating. He was not sure that Mr Dyer had sufficiently considered the subject of the duration of time in the stroke of a steam engine, as bearing on the former question. Each stroke represented so much work done, irrespective of the time taken, but the condensation and re-evaporation in the cylinder, due to the temperature of its walls, depended on the time taken and the conductivity of the material of the walls. In the ordinary marine engine the duration of the steam stroke might be taken as from half a second to a quarter of a second. Cast-iron no doubt, from one point of view, appeared a good conductor—it was too good where we wished to prevent outflow of heat—but it was by no means a good conductor absolutely or in comparison even with other metals. Its conductivity, he believed, was only one-tenth that of silver, or one-seventh that of copper. In a jacketed cylinder where heat has to be given through the sides, the conduction should be as perfect as possible for efficiency. He was of opinion that the temperature of the walls (including of course the piston and end in this term) did not vary in temperature to any depth of the material sufficient to make the amount of heat concerned very appreciable in its effect on the condition of the steam. For mill engines with long strokes and slow pistons, no doubt it was more considerable. The temperature of the sides of a well-lagged unjacketed cylinder would be about the mean of that of its contents, and in a jacketed cylinder would be that of the steam in the jacket. The latter was of course higher than the former, but the question really was whether any appreciable difference of liquefaction and subsequent re-evaporation in the cylinder was due to this. He considered that the horizontal line, or line of uniform pressure seen in many indicator diagrams to near the point of cut-off, often very far below the boiler pressure, showed that no great amount of condensation was taking place, otherwise there would be a rise of pressure as the cylinder got warmer. The "loss of head" due to friction in the passages (or wire-drawing) would become less as the velocity of the steam decreased, but doubtless this observation was complicated by the question of the varying velocity of the piston and opening of the valve port. Concerning re-evaporation, he was of opinion that the steam which liquefied in the doing of work was not deposited as water on the walls but was mostly distributed through the steam and carried away by the exhaust. With the views he had expressed he could not believe that it was all or mostly re-evaporated, and sea-going engineers would tell us that even in unjacketed high pressure cylinders after they had "warmed to their work" there was no accumulation of water requiring the use of the drain cocks. There was no doubt that the steam jacket had not held its own in the favour of the British shipowner, who was very wide awake to matters of economy. It was also another apparatus to keep in order, requiring attention which it did not always get. It was not uncommon when old cylinders came to be broken up, to find the jackets filled up solid with dirt and grease. He wished that the inspecting engineers of some of the large lines had given the results of their experience on this subject. Concerning superheating, he believed it to be a really scientific attempt at economy of steam. The more nearly the steam was made to approach the condition of a perfect gas the greater would be its efficiency, but the apparatus hitherto had been of an objectionable nature, and the very dry steam was injurious to the mechanism of the engine. Could these difficulties be overcome general adoption of superheating might probably be looked for.

The PRESIDENT said it appeared to him that Mr Dyer had done a good service to the Institution in bringing this subject forward in the clear way that he had brought it. His paper was a valuable one in itself, and also for the discussion it had elicited. It was of great importance that the matters treated of in the paper should be brought under the consideration of the Institution, and that additional information should be brought forward. He would like to ask Mr Dyer in respect to the means of judging of the performances of the different engines in the various experiments referred to—whether the work given out by them had been ascertained, by indicator diagrams or by some more perfect means? The indicator diagram was, he thought, usually quite too much relied upon for determining or estimating the work given out by the engine itself. He was strongly of opinion that great reform was wanted in the way of testing engines, not for the power given by the steam to the piston, but the actual power of the shaft of the engine. He thought one of the best ways of doing this was, when a line of shafting was available, to use the torsional elastic yield of the shaft as the means for indicating the torque transmitted, and so to construct an ergometer. This method of procedure had occurred to himself some years ago, and afterwards he had found in one of Dr Rankine's publications [Rankine, "Machinery and Millwork," p. 387, sec. 344], that the same principle had been previously introduced by Hirn, though under a different mode of application. He thought it likely that the two distinct modes might each have its own advantages for different cases. He was of opinion that that method ought to be brought under notice, and put into practice. He thought it could be carried out without much difficulty. It could be used in steamboat speed trials, and when so much labour and time were spent in investigating the effect of skin friction in resistance to ships, in comparing the power with the speed, there was an element of great uncertainty in the loss in the engine itself. Now, if they were comparing compound engines, and triple and quadruple expansion engines, he did not think indicator diagrams were a satisfactory way of showing the differences of performance of these engines. There was one little matter he would like to ask Mr Dyer's attention to. Mr Turnbull had touched upon the same thing, but he thought it well to bring it out a little further. On page 53 of the printed paper, he thought Mr Dyer had made a mistake. He said:—"When the steam passes from the steam chest to the cylinder, there is always a considerable fall in the pressure, varying from 5 to 10 pounds on the square inch. A small part of this is due to frictional resistances and the want of uniformity in the velocity of the steam caused by the varying motion of the piston, effects which are generally included under the term *wire-drawing of the steam*, but by far the greater part is caused by the condensation of the steam against the sides of the cylinder, which become covered by a film of water." It seemed to him that condensation, without the resistance in the passage, or the so-called "wire-drawing," would not cause this, for practically it would make no abatement of the pressure, and not make a smaller pressure in one place than another. Perhaps Mr Dyer would tell what he thought of the matter.

Mr JAMES HOWDEN said, as he had spoken at the discussion of Mr Dyer's paper at last meeting, he merely wished to refer to what he then had said in connection with the weight of steam used in engines per I.H.P. The best example given in Mr Dyer's paper, in the engines experimented on by M. Hallauer, with superheated steam, is that of Hirn's beamed engine, with which an I.H.P. per hour was obtained from 7·2 kilogrammes, or 15·87 lbs., weight of this steam. He had said that he knew compound engines using less weight of saturated steam than this per I.H.P. It was certainly difficult to prove accurately what the consumption of steam is by weight in any engine, unless it is tested by actual measurement; but, taking the evaporation in the boiler of one of the cases to which he referred—in which the consumption of fuel at sea was 1·6 lbs. per I.H.P. per hour—at the high average of 9 lbs. of water per lb. of coal from 75 lbs. boiler pressure, the feed water being from 115° to 120°, and water in the boiler of at least the density of sea water, these engines must have been using only 14·4 lbs. of steam per I.H.P. In another case which had come under his notice, where the consumption of coal per I.H.P. per hour was 1·34 lbs., but where the evaporation was very high, probably not less than 10½ lbs. per lb. of coal from 80 lbs. boiler pressure, the feed water also about 115° to 120°, and the water in the boiler at least the density of sea water, the weight of steam used by the engines per I.H.P. per hour in this case, at the evaporation mentioned, would be 14·07 lbs. These cases went to show, what he had maintained at last meeting, that the best examples of economy with single engines and superheated steam was surpassed by compound engines with saturated steam. There was just one other point in Mr Dyer's paper which he had intended to notice at last meeting, but which had escaped his memory—it was that of superheating the steam by the waste gases from the boiler. To superheat steam to 450° would certainly require at least the 600° in the waste gases mentioned by Mr Dyer. This was, however, too much heat to lose by the waste gases. He (Mr H.) was endeavouring to reduce the heat of the escaping gases in boilers worked by forced draught to about 300°, which would leave no margin to expend in superheating steam.

Mr DYER, when called on to reply to the discussion, said:—

In making a few remarks on the observations which had been made, he would shortly consider them, not in the order in which they were made, but in what seemed to be their logical order. First, with regard to Mr Millar's question respecting the curve representing the expansion of steam. In the paper mentioned by Mr Millar, Professor Rankine distinctly states the assumptions he makes, and he adds that for most practical purposes the common hyperbola forms a good approximation to the true expansion curve, and that it is convenient because of the simplicity of the processes for finding its figure whether by calculation or construction. That was precisely what he (Mr Dyer) meant at page 50 of his paper, when he said that the formulæ and methods there mentioned were very often convenient for rough calculations, and by applying certain constants derived from experiments they could be made tolerably exact. The expansion of steam under any set of conditions could be represented by an equation of the form of PV^{α} = const., the value of α depending on those conditions, and in practice it very often approximated to unity; that was to say, that the curve very nearly agreed with a rectangular hyperbola. He objected, not so much to the use of the hyperbolic curve as to the manner in which it was used, because the theory on which it was said to be founded did not take into account any of the physical properties of steam, or of the materials used in the construction of engines, but was purely geometrical and mechanical, and *the fundamental notions on which the action of steam in cylinders rests are not examined.* The formulæ have all the appearance of exactness, and this appearance prevented students from understanding the real causes of the chief loss of efficiency in steam engines, and the cause of the improved efficiency of compound engines which, as he had pointed out, arises from the way in which the re-evaporated steam was used—a phenomenon which was concealed by the popular manner of explaining the subject. By comparing the actual indicator diagrams taken from recent compound engines with the hyperbolic curve it was generally found that the latter gives a good approximation to the true expansion curve; and it had the further recommendation of being easily constructed or calculated. What he wished to say, therefore, was not so much that the use of the hyperbolic curve should be given up, but that the reasons why it was used should be explained in a different manner.

Mr Jamieson remarked that the paper would have had more importance and interest if he had brought forward a few sets of "combined" indicator diagrams from recent engines. It would have been more complete if there had been brought forward many other things, but in that case it would have been a treatise on the steam engine, not a paper which could be read in an hour. As explained, his object was not to attempt an exhaustive treatment of the subject, but rather to elicit the opinions of those members actually engaged in the manufacture of steam engines, on the practical side of the questions involved, and, of course, diagrams which had been taken by them would have had an important bearing on those questions. The diagrams shown by Mr Jamieson proved that the ordinary hyperbolic curve was a fair approximation to those diagrams. That gentleman took the equation $PV^{\frac{10}{9}}$ = a constant, and called it the theoretical curve for saturated steam, and applied it to diagrams taken from triple expansion engines with jackets on the two low pressure cylinders. Rankine gave that expression as an approximate formulæ for the *adiabatic* expansion of dry saturated steam, that is, for the expansion of steam in a perfectly non-conducting cylinder; and stated ("Steam Engine," p. 385) that it had been deduced by trial, from the results of numerical calculations of the co-ordinates of the adiabatic curves for steam, for such pressures as usually occurred in the working of steam engines.

But that expression was not even what Rankine thought it was.

Many years ago, Zeuner showed that the value of the exponent of V was not constant, but depended on the amount of water in the steam, and this view has been amply confirmed by such writers as Röntgen and Hirn, in fact by all recent writers on the subject. Professor Cotterill says ("Steam Engine," p. 192)—"When Rankine suggested the equation PV^n = constant he gave the value $\frac{10}{9}$ or 1·11 for the index, on what grounds cannot now be determined; it is certain that numerical calculations from his own equations give a larger result, except when the steam is wet initially." In the paper which he (Mr Dyer) read before the Institution last session, he mentioned that if x represented the weight of water in 100 parts of the steam at the beginning of the adiabatic expansion,

$$n = 1.135 - \cdot001\,x$$

For $x = 0 \qquad n = 1{\cdot}135$

$x = 10 \qquad n = 1{\cdot}125$

$x = 20 \qquad n = 1{\cdot}115$

$x = 30 \qquad n = 1{\cdot}105$

from which it would be seen that the curve approached an equilateral hyperbola as the quantity of water contained in the steam increased, and for Rankine's value of n the steam contained 24 per cent. of water. These considerations showed that the expression quoted by Mr Jamieson had little claim to be considered a theoretical expression, as it was only true for a given set of conditions, and was therefore essentially empirical.

It is a well-known fact that if they attempted to calculate from the indicator diagram, or from the theoretical diagram, the weight of steam used, that the quantity so obtained was much less than that actually used; and he merely mentioned this to enforce what he had said above on the subject of the assumptions usually made, and not for the purpose of depreciating the acquirements of the engine builders of the present day, as Mr Weighton puts it In speaking of a practice which was somewhat common a good many years ago, and in saying that he suspected it was not altogether out of use yet, he merely stated what every engineer knew to be the case, but he did not mean to do any injustice to practical engineers;

for he had previously said that the intercourse between men of science and men of practice, the improved scientific knowledge of practical men and the improved practice of scientific men during recent years had caused the notion of the incompatability between theory and practice to considerably decline.

With regard to Mr Dick's question about the disappearance of heat in the cylinder, they know that in a perfect engine the heat which passed into the cylinder might, with respect to its mechanical effect, be divided into two parts, the first being converted into work done on the engine, and the second passing into the condenser or atmosphere. In actual engines there were various other losses from radiation, conduction, and condensation on the sides of the cylinder, which latter caused steam to pass into the condenser without doing work. The heat which disappeared in the cylinder before the valve was closed caused a certain amount of condensation, but that was complicated by the greater condensation produced by the sides of the cylinder. The temperature, and consequently the pressure, of the steam in the steam pipe would not, however, be lowered to any great extent, except when the condensation was great, as fresh steam was always entering. However, that was a question which did not admit of a definite answer, except by experimenting in the special case under consideration.

With regard to the President's remarks about the fall of pressure between the steam chest and the cylinder, it was stated in the paper that the effects *generally* included under *wire-drawing of the steam* were those due to frictional resistances, and the want of uniformity in the velocity of the steam caused by the varying motion of the piston. Rankine tried to calculate the effect of those on the pressure of the steam, but did not take any account of that of the initial condensation. Similarly, in treatises on the indicator diagram (*e.g.* Porter, p. 149), we find a distinction drawn between wire-drawing and the effect of initial condensation. As the paper was chiefly addressed to men who had used the term *wire-drawing* in this sense, and as one of its chief objects was to emphasise the necessity for considering the action of the sides of the cylinder when the initial condensation was large, the manner of statement used was what first occurred to him as the best for directing attention to that action. As, however, the President has pointed out, the effect of the condensation should be included under wire-drawing, since it is similar in its action to the other two causes already mentioned—that is to say, *for a given size of steam pipe and port opening*, the condensation increases the amount of wire-drawing. Of course, as Mr Turnbull had pointed out, if the supply of steam be increased, by dispensing with the throttle valve, the initial pressure in the cylinder will be little below that in the boiler. For a study of this subject, reference may be made to Cotterill's "Treatise on the Steam Engine," p. 242.

Professor Rankine's opinions on the action of the sides of the cylinder must be judged from his writings. He had quoted what he believed to be his latest views on the subject, and these although recognising the two separate condensations did not attach sufficient importance to the action of the sides of the cylinder, and he seemed to have thought that this action might, in an engine working with saturated steam, be prevented altogether by the use of the steam jacket, whatever the pressure and ratio of expansion; at least he did not appear to have modified any of his expressions for the efficiency of steam in engines, in consequence of that action. That was the more to be wondered at as he was aware that John Elder pointed out that when the ratio of expansion exceeded four, it became necessary to employ a compound rather than a simple cylinder engine, and further, that when the ratio of expansion was greater than nine it would be necessary to expand the steam in three cylinders instead of two. He had already said he had no wish to depreciate Professor Rankine's work, but the same conclusions as he had stated had been arrived at by all who had taken the trouble to investigate the matter; for instance, M. Hirn, Professor Cotterill ("Steam Engine," p. 265), and Professor Thurston. The latter in a paper read before the British Association in 1884, said—"Rankine was not aware of the often enormous difference produced in the performance of the steam engine by the extra thermo-dynamic phenomena involved in its operation, he does not indicate the fact that the results of his calculations must be taken with the qualification just stated above, and his figures are still sometimes supposed to represent those of actual performance. The fact is, however, that the consumption of steam and of fuel in actual practice always considerably exceeds those obtained by the solution of the thermo-dynamic problem, and often by a very large amount." As remarked in the paper, the subject of the action of the sides of the cylinder was too extensive to be fully discussed except in a special paper. His object was simply to give an outline of it sufficient to explain what followed.

Mr Duncan had said he was not sure that he (Mr Dyer) had sufficiently considered the subject of the duration of time in the stroke of a steam engine, as bearing on the question of jacketing. The paper simply gave the results of previous investigations on the subject, and as just remarked above did not pretend to go into detailed examinations of the different parts of the subject. The points mentioned by Mr Duncan have not been overlooked in these investigations, and space will only permit at present of a reference to the accounts which have been published of them.* His opinion that the steam which liquefied in the doing of work was not deposited as water on the walls, but was mostly distributed through the steam, and carried away by the exhaust, agrees with that expressed in the paper.

Messrs Howden and Weighton misunderstood him when they said that he recommended the single in preference to the compound engine. At page 67 he stated the conditions under which a single engine *might* be used, and remarked that it must be ascertained chiefly from experience whether the conditions could be fulfilled or not. He, however, proceeded to consider what was known about superheated steam, and showed that with steam at 80 lbs. pressure superheated to 450° Fahr. at least *two* cylinders were wanted in

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* The following may be consulted:—Cotterill, "Steam Engine," p. 249; Hirn, "Théorie Mécanique de la Chaleur," 3rd ed., Vol. II., p. 35. The paper by Messrs Gately and Kletzsch, mentioned in the appendix, gives some experimental investigations bearing on the subject.

which to expand the steam, and he further remarked that as "triple expansion engines are not very much more complicated than those with two cylinders, the question to be decided by experience is whether the 50° Fahr. of temperature, which was gained by doubling the pressure, is purchased at the cheapest rate. If that be decided in the affirmative, then triple expansion engines might continue to be designed, and direct superheating to about 90° Fahr. would bring the temperature up to the limit named as at present-practicable, and thus further increase in the number of cylinders be rendered unnecessary." It would be observed that he did *not* recommend single cylinder engines, and that he left it to be decided chiefly by experience what should be the minimum number of cylinders used for given temperatures and pressures.

Mr Howden objected to his comparing the low pressure cylinder of a compound engine with the cylinder of a simple engine. He did not mean to make the comparison extend to a simple engine working with the same pressure as that used in the high pressure cylinder of the compound engine, but confined it to an engine working with a low pressure of steam, and small ratio of expansion, in order to show that the quantity of steam which passed direct to the condenser without doing work in these two cases was small. Mr Howden asked for further details with regard to the experiments of MM. Hirn and Hallauer. The object in mentioning those experiments was simply to give some idea of the amount of loss, through the action of the sides of the cylinder, and the effect of jacketing, compounding and superheating on that loss, and not to enter into details concerning them as they would have occupied too much time. On some future occasion he might have a special paper on the subject, and in the meantime would simply refer to the original papers which were to be found in the Library. At present he would only say with reference to one of Mr Turnbnll's remarks that he did not put forward these experiments as deciding the questions under consideration; on the contrary, he had said, "it would be rash to generalise on M. Hallauer's results; but this much may be said that they afford good grounds for reflection to the constructors of engines." His object was to show the necessity for further careful experiments being made, for at present they know practically nothing about the efficiency of modern marine engines, as determined from experiment, and as the President had remarked, we cannot compare the merits of single, ordinary compound, and triple or quadruple expansion engines, unless we know the work given off at the crank shaft per unit weight of steam used in each case.

He ought, however, to say, in reply to the President's question, and to a further remark by Mr Turnbull, as to the manner in which Mons. Hirn's experiments were carried out, that generally the indicated power was ascertained by means of the indicator or a modification of it, and at the same time the feed water was measured, its temperature and the boiler pressure taken, and the weight of fuel burnt ascertained. From those data the quantity of heat used per indicated horse-power, and the efficiency of the boiler were calculated. The effective horse power was ascertained by a brake, or an ergonometer of the kind mentioned by the President.*

It would be interesting to have further details of the experiments mentioned by Mr Howden with respect to the quantity of water used per I.H.P. per hour, as his figures are much lower than those usually given for such experiments. With regard to the temperature of the chimney no doubt that in some recent designs it would be less than 600° Fahr. That, however, is generally given as the average temperature. The superheater was supposed to be put in the uptake simply that this waste heat might be utilised to a certain extent. If the temperature of the uptake was not sufficient for the purposes of superheating, then of course direct heating would require to be resorted to.

In conclusion, he would state briefly that the chief object of his paper was to show the necessity of studying the theory of the steam engine in such a manner that the conditions of efficiency might be

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* Since the paper was written I have read a very excellent paper by Mr J. G. Mair, "On the Independent Testing of Steam Engines," (Proc. I. C. E., Vol. LXX., p. 313) in which M. Hirn's methods are explained, and applied to engines made under the superintendence of Mr Mair.—H. D.

clearly understood, and mainly that the magnitude of the action of the sides of the cylinders might be realised, and that in order to diminish this a very judicious use must be made of expansion, of steam jacketing, and of compounding. These were not necessarily good things in themselves; unless they were well designed and intelligently used they might and very often did become bad things. Too much expansion in one cylinder might cause loss, steam jackets might waste steam, and compound engines be less efficient than simple engines, and hence the necessity for a thorough study of their action in order to understand the conditions under which they were to be used. Another object of the paper was to say a good word in favour of moderate superheating of the steam. Mr Kemp remarked that proposing superheating looked like going back half a century. He would reply that returning to former practice was not always a retrograde movement, especially as many things had happened in the interval. Thirty years ago surface condensation was practically unknown, now it was universal in marine engines, the materials of construction, of packing and lubrication had all been very much improved. He was quite aware of the difficulties which had been previously experienced, but, as Mr Coleman remarked, it was all a question of materials, and in the present advanced state of chemistry and metallurgy, if a little attention were directed to them, he was persuaded that all difficulties might be overcome. Like all changes in practice it might involve some pecuniary sacrifice at first, and this no doubt had a considerable influence on constructors. Thirty years ago a working pressure of 160 lbs. on the square inch, for marine boilers, was considered as far out of the region of practical engineering as superheating is now. He had hoped to have had the results of the experience of more of the members, but want of time may have prevented them from being able to put these in a shape suitable for publication. They were indebted to Messrs Howden and Turnbull for what they had given them, and he hoped they would favour them with further details as soon as they found it convenient. Want of space prevented him from entering into the consideration of practical details, and it would be out of place to enter into them in these remarks, which had been already unduly prolonged. He trusted, however, that he might at least have suggested subjects for some future communications to the Institution. For the purpose of assisting those who might be investigating the subject, he had added, as an appendix, a list of references to books and papers which would be useful in giving them the results of past investigations.

The PRESIDENT then moved a cordial vote of thanks to Mr Dyer for his paper.

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On Arthur's Bevelling Machine.

By Mr Richard Ramage.

(SEE PLATE V.)

Received and Read 22nd December, 1885.

About 14 years ago I endeavoured to produce a machine capable of bevelling angle-iron necessary for the different parts of a ship, and while I could produce a machine to make the bevel uniform I could not produce one to make the various bevels required in the frame of a ship at the forward and after ends.

The inventor of the bevelling machine was, at the time I refer to, one of my workmen, and it was not until 1879, when he was head of the iron department of my firm, that the question was again talked of, and in 1884 the machine which is represented in the drawings (see Plate V.) was produced by Mr Arthur. Beyond giving him encouragement I do not claim to have invented any part of it. My reason for reading this paper is simply to give the members of this Institution all the information I can concerning a machine, the want of which has been long felt by shipbuilders. The pieces of angle-iron before you will give you an idea of what can be done with it; where the bevel is excessive you will see that there is little or no hollow, while if this had been done in the usual way the outside flange of the angle-iron would have been curved to such an extent that the plating would only bear on the two edges, leaving the centre, in way of the rivet, without any bearing, and increasing the tendency of the head or countersink to come off.

My firm has now used it in bevelling the frames of nearly a dozen of vessels, some of them over 2000 tons, and the whole of these frames were bevelled and set with one heat, thus saving time and with an absence of those objectionable marks and irregularities produced by the old-fashioned wheeze, and without in any case injuring a single frame.

For rail and gunwale angle-irons it answers admirably, but I would recommend in using it for the above purposes, that the bevelling should be done before the punching, as the pressure between the rollers when the bar is hot produces elongation.

To bevel Z section of iron or steel the main roller, A, is arranged that it can be reduced to suit the smallest sections. Messrs Palmer, of Jarrow, and Sir Wm. Armstrong, Mitchell, & Co., Limited, have, I believe, bevelled all this section of steel used by them in their Government work with success. In using the machine it is placed opposite the door of the furnace, the power is supplied by a cylinder eight inches diameter, the steam pipe has a joint to allow the machine being shifted to one side without disconnecting any of the joints, heavy angle-iron can be pulled out of the furnace into the machine by means of a chain attached to the warping end.

To enable the workman to perform the bevelling accurately, the hand wheel, K, is the only part requiring attention; by it the three mechanical powers are brought into use, namely, the screw, the lever, and the wedge. The screw is attached to the bevelling wheel, E, at its centre, the latter heels against the horizontal wheel, D, at the bottom edge, thus forming a lever. As the bevelling wheel, E, is brought over the bevel becomes greater, the bottom edge is made to slip downwards, thus wedging itself between the vertical flange of the angle-iron, and the edge of the horizontal wheel, D.

The main shaft is hollow, and the main roller, A, can be set from the outside by the hand wheel, N; this wheel was formerly inside but was found inconvenient. The main roller, A, is adjusted vertically by the hand wheel, B, and horizontally by the hand wheel, N, already mentioned.

The horizontal flange of the angle-iron is caught, kept level by, and fed through the machine between the rollers, A and D, E is the bevelling roller already referred to, and is attached to a beam, F F, the ends of which move in the quadrants, G, in the framing, H. The roller, E, can be brought over to any angle by the hand, wheel, K, and screw, L.

The pointer, M, being attached to the screw, L, at once indicates on the bevel board, N, the bevel that is being given to the angle-iron.

O is a dial worked from the main shaft by a worm wheel, and shows the travel of the bar through the machine and indicating the different bevelling spots.

The bevelling spots are taken at regular intervals of 4 feet, and the dial shows when each spot comes to the bevelling roller.

The inner circle on the dial, O, is for open bevelling, and the outer circle for shut bevelling. The bevels are taken from a board in the usual way, and applied to a small half circle, having the degrees marked on it, and from there transferred to the bevel dial on the machine.

I hope in these few remarks, that I have made the nature and working of this machine quite intelligible to the members of this Institution, and in a word would point out, that for a man to produce such a machine who is not in the true sense a man brought up as a mechanic is, on the whole, a very creditable production, and supplies a long felt want.

I am aware that Messrs Robert Napier & Son, had at one time a machine for bevelling angle-iron, but its use did not become general, therefore, I do not wish you to think that this machine is the first of its kind, although, I believe, it is the best.

With regard to the saving effected by using this machine, I am not prepared to say definitely what percentage it amounts to, but I can confidently say that there is a saving; this coupled with the shorter time taken to do the frame, and the better quality of the work, makes it, in my opinion, one of the most useful additions we have had to shipbuilding plant for many years.

As shipbuilding and engineering are yearly demanding more accuracy in construction, owing to closer supervision and greater pressure of steam now in use, any machine that can be made to save labour and do the work accurately, ought to be welcome to all employers of labour.

In the after discussion,

Mr Laurence Hill heartily approved of the machine for bevelling angle iron invented by Mr Arthur, and he had no doubt it would come into general use before long. He was particularly pleased with the accuracy of the surfaces it produced.

Mr George A. Agnew asked what was the gain in time in executing work by the use of the machine. He had seen it working, and so far as he could observe, the bevelling of the Z parts of the midship frames with two heats was well done. The stern Z frames, where the curvature was excessive, required three heats. Of course, now that the men had got into the way of working the machine, they may also do the stern frames in two heats. The machine seems to do its work well, and to give satisfaction.

Mr William Macmillan said they had to thank Mr Ramage for bringing this machine before them, after having had two years' experience of its working. He understood from Mr Ramage that the frame bars were taken direct from the furnace to the bevelling machine, and that when the bar left the machine, it was sufficiently hot to be turned without re-heating. Under these circumstances the machine would be of considerable advantage; besides, the work would be better done than by the ordinary method, for where there was considerable bevelling upon the bars, it frequently resulted in getting the flanges hollow, whereas the samples before them were bevelled straight out from the heel, thereby enabling the work to be better done, by getting it to fay close on to the bar. Therefore he thought it was well worthy the consideration of shipbuilders. As regarding economy, he had heard that it gave a saving in the labour of about 50 per cent.; that he had grave doubts about. But if it enabled them to make better work, although there might be no saving in the labour, that was sufficient to recommend it for general use.

Mr James Williamson remarked that he had seen the machine at work twelve months ago, and he was extremely pleased with it. It was then bevelling large angles for a 2000 ton sailing ship, of 5½ by 3½ by 8, and they were levelled quite easily with one heat, and were set to the shape at the same time. The work was most excellent. One or two things had struck him in connection with the strength of the machine. It appeared to him rather slight; but he observed by the diagrams that the wheels had been strengthened, and thus met the difficulty that he could see would have occurred. If the machine were made strong enough it would prove to be one of the most perfect machines that he had ever seen in a shipbuilding yard. He had to compliment Mr Ramage on the paper; and while the machine might look rather complicated, he could assure the members that it did its work admirably.

Mr Arrol (Vice-President) said they were all much obliged to Mr Ramage for bringing the description of this machine before them.

The discussion was then adjourned to next general meeting.

On the Proper Use of Animal Power as applied to Tram-cars, Short Inclines, &c., &c.

By Mr LAURENCE HILL, C.E.

Received and Read 22nd December, 1885.

WHILE every possible economy is being practised by the users of the cheapest power we have, viz., "Steam," it is remarkable that so little attention is paid to the daily waste (in many cases a totally unnecessary expenditure) of the most expensive power we have, viz., that of beasts of draught, or horse-power properly so called.

Although the aim of every engineer is to displace the dearer power by the cheaper, yet, nevertheless, as horses must continue to be used to a great extent, the following remarks may lead young engineers or contractors to consider the importance of minor matters. There is a much greater and unnecessary expenditure of horse-effort, and a consequently very large and equally unnecessary expense, than most people are aware of.

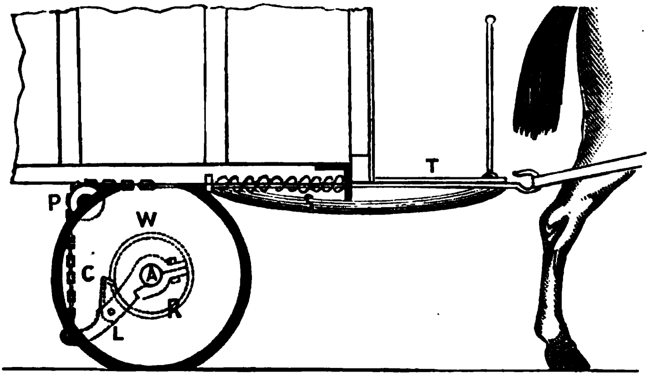
It is not by any means in tram-cars alone that this waste goes on. A large number of horses are unnecessarily strained by railway companies at their goods stations, where horses are kept to arrange the trucks for trains—two or more horses being kept, for one is frequently quite unable to start a truck by its unaided effort; and, besides this, a great loss occurs in horses being strained by their drivers expecting they can start them, and after one or two ineffectual attempts, another horse or some other assistance is got, thus not only losing valuable time, but often to the destruction of a valuable beast. The loss of capital from this cause alone must, on the railways of this and other countries, be very considerable.

By an adaptation of the principle shown in the drawing, but constructed so as to be easily carried, and easily attached to the tire of the waggon wheel, the waggon could often be started at once. I have been often surprised at the want of thought and of mechanical skill, and the consequent misuse of horse-effort, which is exhibited by contractors in the transit of material from deep excavations. I have seen three or more big horses hauling up one truck of soil, where one could have done it, and even worse waste in our own vicinity. Many years ago I estimated that £1000 would more than cover the cost of constructing a short tunnel, but on the work being given out, no contractor would look at it for less than double, their reason being that it would require so many horses to take the stuff up the steep incline, and the job was too small to use an engine. As I could not get them to try it even at a moderate advance on estimate, I undertook the work myself, but instead of using two or three horses working with a long tail rope on the level, I only used one working on the incline. The horse, however, worked down, not up; and by fixing a pulley at top of incline, the horse had thus only to lean, almost without any other exertion, on his collar, and he easily pulled up 12 or 13 cwt. each time; by hooking the rope to his collar and breechings, he was taken up with even less exertion. (I may add that the work was finished for less than £900.)

As to starting heavy waggons, one horse as often as not is unable to start the load, but in many situations, by means of a purchase over a single light block hooked to the waggon, one end of the rope being hooked to a ring fixed a few yards ahead, and the other to the horse, the starting is readily effected. Horses worked in tram-cars are subjected to constantly-repeated straining, entailing great waste of horse flesh and requiring very frequent renewals; and this is so well-known that the horse insurance companies in London charge a higher premium for tram-car horses than for any others.

I had dynamometer trials made of the power required to run, and to start, the cars, and found that after a car was started the pull varied from 10 to 30 lbs. on a level, according to weight and state of road, whereas it required from 2 to 15 cwt. to start them. Now, as one cwt. is as much as a carriage horse should be burdened with, it is easy to see how tram-car horses deteriorate so rapidly.

The apparatus you see illustrated, and the model, show a single arrangement which relieves car horses from their hurtful strains. It is an adaptation of the first mechanical power—the lever—and which can be applied at a cost of about £2 to each car. Its use on entirely level routes would save half of the horses, and on others from 20 to 30 per cent. of the horses employed.



The starting apparatus, which is exceedingly simple, consists of the grip wheel on pulley, W, of a V section, which is secured fast to the axle, A. W is made in two halves for convenience of fixing. The lever, L, and cam or grip, C, are hung loose on the same axle close to the pulley, R, so that, on raising the lever, L, the cam, C, grips the wheel, W, and thus the pull that is exerted at the extremity of the lever, which has a mechanical advantage of two to one, is communicated to the wheels of the car. A chain from the end of this lever is carried over a small carrier pulley, P, and made fast to the draw rod, T, lying beneath the car floor, and extending to the end of the driver's platform, the horses being attached to it in the ordinary way. A spiral spring, S, placed over the rod, brings it and the chain and lever back to their normal position whenever the car is fairly started. This spring is designed so that the ordinary pulling draught does not compress it. When the strain exceeds the usual running tension in starting the car the spring yields, and the horses then pull direct on the lever and obtain all the advantage due to the increased leverage.

The whole apparatus is, moreover, perfectly automatic in its action, requiring no care or attention whatever on the part of the driver.

The economical advantage of a good automatic contrivance to assist the horse in starting the car may be illustrated by considering that if such were introduced, eight horses would easily do the same work as ten horses working without such assistance; and if the routes were level throughout, one horse would certainly be sufficient where two are now required.

In the after discussion,

Mr H. R. ROBSON asked if the ratchet was always in motion when the car was running, and if the paul was constantly in gear? if so, he was afraid it would be very noisy. With regard to the weight or power required to start the tram-car as stated at 15 cwts., he was rather astonished to learn that so much was required; it appeared to his mind to be excessive; possibly some error had crept into the matter. Several years ago he remembered some experiments being made by Mr Dundas to ascertain the power required to draw an ordinary plough through ordinary soil. It was found that it took 100 lbs. to draw it; while, if he remembered correctly, it required about twice that amount to start it. A tram-car being on rails, and on the level, he thought a much less power than 15 cwts. would draw it.

Mr HILL replied that the ratchet wheel was a fixture on the axle of the car; and by reference to a diagram on the wall he explained that there was an arrangement whereby the noises were prevented. Mr Robson was quite correct about the power required to draw the plough. Mr Smith, of Deanston, had also made experiments to the same effect. When he (Mr Hill) said it required 15 cwt. to start a car, that was the maximum or initial starting strain—it was from 1 to 15 cwt., depending upon the state of the road, and the obstructions in the way. He had experimented with the apparatus upon a car, and found that one horse could easily do the work of two; indeed, he had started a car with one mule, on an incline up which the animal could not draw the car after it was started.

Mr E. KEMP asked if the apparatus helped to get over any obstruction in the way.

Mr HILL replied that it did not, it only gave an effect for two or three feet.

Mr DYER asked if the machine had passed beyond the experimental stage, and how long ago had it been designed? In actual experiment it showed a great saving in power.

Mr HILL answered that the apparatus had not yet been adopted by any one, although it had been designed five or six years ago. The saving effected is more than ten per cent. of the power.

The discussion was then adjourned until next meeting.

The discussion of this paper was resumed on the 26th January, 1886.

Mr R. DUNDAS said he had only a few remarks to make, and that only upon the point where Mr Hill remarked on railway shunting by horses. No doubt when railways were first started there was a great deal of shunting by horses; but that had greatly decreased, for it had been found much more economical to shunt with a locomotive engine. At nearly all the large stations that he knew of, the work of shunting was done by locomotives. When Eglinton Street Goods Station was designed it had turntables, which made it imperative to do the shunting by horses; but for years these tables had all been removed, so that a locomotive could do all the shunting necessary. At other stations where shunting could not be done by locomotive power, such as at Broad Street Station, London, where there was one goods station above the other, and where a large quantity of the goods was dealt with in arches on the lower level, running transversely to the rails on the upper level, the shunting was done by hydraulic power, which was far more economical than horse-power. No doubt there were some places, such as Port-Glasgow Harbour, where there might be one or two horses employed to draw the trucks from the vessel's side; but it was almost unavoidable to have horse shunting there. At some other stations where horses were at present used, they would not be required either if permission was given to use locomotives freely. There could be no doubt about it that locomotives were under far better control than horses, and a locomotive attached to one or two waggons was the very best means of shunting at many stations, the men elevated on the foot-plate being better able to see any obstruction. He was glad to say that at the general terminus the horses would soon be dispensed with and give place to hydraulic capstans, the same as those at present in use at the Queen's Dock.

Mr ALEXANDER TURNBULL (through the Secretary) said that he entirely concurred with what Mr Hill had said as to the necessity for some such arrangement as he proposed as a means of assisting to reduce the sudden strains to which tram car horses are subjected. Regarding tram car starting arrangements generally, he might say that he had, at various times given some thought to the subject, but more specially in the direction of utilising the power by which the motion of the car has been arrested. In so far as passenger tramway cars are concerned, and where stoppages are so frequent, he thought that the waste power should be utilised. Mr Turnbull, by means of a diagram, illustrated a plan for accomplishing this, which consists essentially in a drum containing a spiral plate spring, which, by means of gearing under the control of the driver, could be brought in contact with the rail or the wheel of the car. Thus on being signalled to stop the driver turns the brake handle; this brings the face of the drum to bear on the rail, causing it to turn round and coil up the spring; the brake blocks immediately thereafter come into contact with the car wheels. When the car has been stopped, then, by turning the brake handle in the opposite direction the brake blocks are taken off and the drum is brought to bear on the car wheel, and the stored up energy of the spring is given out to the periphery of the wheel just at the starting point, whereby the horses are aided in their efforts to make a start.

Mr HILL said, with regard to Mr Dundas' remarks, that he agreed with them, and trusted that the locomotive would soon become universally used for shunting operations, and Mr Dundas would see that at the beginning of his paper, he (Mr H.) had said it was the duty of every engineer to substitute the cheaper power for the dearer one. He looked on Mr Turnbull's plan as exceedingly ingenious. He had invented a machine on the same principle, which worked very well indeed, but it had the defect that while it worked beautifully so long as the car was stopped when it had impetus upon it sufficient to coil the spring, it could not work when the car was stopped slowly. When he saw that defect, he allowed that patent to lapse, and took up this other system, which he now advocated. Car horses were strained almost entirely by the initial starting. It was also well known that if only the half of that strain could be taken away, they would greatly relieve the horses.

A vote of thanks was then passed to Mr Hill for his paper, on the motion of the President.

On American Railway Freight Cars.

By Mr Alexander Findlay.

The discussion of this paper, adjourned from the 28th of April, 1885, was resumed on January 26th, 1886.

In reply to the President,

Mr Findlay said he had nothing further to add to the paper. There might be some points in the paper which had struck some of the members, and if they would mention them he would be glad to give any further information in his power.

Mr W. J. Millar (Secretary) said that it was interesting to observe that the wheels of the cars in America were made of cast-iron. He had noticed that the Americans seemed to use cast-iron for their locomotive wheels, with steel tires, and that they used cast-iron far more than was customary in this country. If Mr Findlay could state to the meeting what the strength of the cast-iron was, such as the tenacity per square inch, it would be interesting, so that we might compare it with results obtained here.

Mr Findlay knew of nothing special about the mixture of pig-iron which was used for wheels, except that it included two or three of the best brands, known to give it a considerable toughness. There was about half an inch of chill on those wheels, the wheel being only allowed to stand long enough in the chill for that effect He had seen them "bleeding" when taken out and put into the annealing pits. In his experience he had not found any failures in those wheels in the way of running, although he had known them constantly in use for from ten to fifteen years. He remembered once going to a break-down, where a single wheel—in the second rear car of goods train—had given way, but as it did not belong to the

Company he was with, he had no special information regarding it. These wheels were generally adopted for cheapness, but the ordinary wheel was very good wearing and efficient. It might be that this was aided by the much slower speed there than in this country, but still they had much rougher roads, so that he would assume that the wheel that would stand on the American roads should do very well here. He knew, indeed, that a great many of these wheels were shipped to Australia and India for employment on the railways there.

Mr T. ARTHUR ARROL asked if he was correct in his observation, that the centre of the wheel was bored out for the axle? The variations in the diameter had often struck him, and he would like to know what had been the result of these variations in diameter? for if one wheel were one-eighth of an inch part less than another on the axle, the travel of the wheel will be three-eighths of an inch different each revolution; and the friction upon all the working parts would be very severe. He would like to know whether any annoyance in consequence had arisen.

Mr FINDLAY said that was brought out in the paper. The chills were all turned from dead gauges. The company with which he was connected—the Missouri Car and Foundry Company of St. Louis—cast 250 of these wheels per day, and he had never heard of the wheels not being all of the same diameter, and this, he thought was only to be expected, seeing they came out of the same chill. The most common wheel was a double web wheel. The wheels were bored out on a specially constructed boring mill, and held by universal grips of three arms, on a revolving table. As mentioned in the paper, one man could do over seventy wheels in 9½ hours. He had seen eight wheels thus bored in an hour. Again, in this country wheels were keyed on the axles; but in America they did not key them on, but they were put on by hydraulic power. They had a much smarter way of getting through with their work than was the case here, so that a man could turn about 20 axles a day. The wheel seat only got roughing cut, and was made slightly larger in diameter than the bore of the wheel, which was pressed on by hydraulic power. He threw out these suggestions so that members might consider whether or not it would be possible to improve their machinery to cope with their cousins over the water.

Mr T. D. **Weir** reminded the author that at present in this country the regulations of the Board of Trade precluded engineers from trying cast-iron wheels; and then with regard to the keying on of the wheels, he understood that the Caledonian Railway Company pressed on all their wheels by hydraulic power, very much in the manner adopted in America, as described by Mr Findlay. Perhaps Mr Dundas would say whether that was correct.

Mr **Robert Dundas** said that was so.

The **President** then moved a vote of thanks to Mr Findlay for his paper. They all felt thankful to Mr Findlay for bringing this information before them from abroad; for it was very important that they should be informed of all that was going on in other countries; and therefore Mr Findlay well deserved a vote of thanks.

On Some Properties of Cast-Iron and other Metals.

By Mr W. J. Millar, C.E.

Received 26th January; Read 23rd February, 1886.

In some former papers* by the author on the strength of iron and other materials, it was pointed out that the form of fracture in a rectangularly-shaped specimen, such as the ordinary test-bar, indicated the position of fracture in relation to the centre of the bar. Thus it was shown that straight fractures occurred at or close to centre of span, and curved fractures more or less removed from centre of span, and, further, that the curve, when it did occur, invariably pointed toward centre of span or point of application of load.

In the first paper on this subject it was stated that, in no case had a wedge-shaped piece of iron been seen to be forced out at the compressed side, as stated by some writers to be the case, with crystalline substances such as cast-iron.

That such forms of fracture occur in steel under certain conditions seems to be the case, and the author has been shown a steel cutting tool which, on breaking, showed this peculiarity. It appears that in testing steel rails by impact pieces, approximately wedge-shaped, have been observed to fly out from compressed side when struck by the falling weight.

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* See Trans. Inst. Engineers and Shipbuilders in Scotland, Vols. XIX. and XXI.; Minutes of Proceedings Inst. C.E., Vol. LVIII.; Report British Association, Glasgow Meeting, 1876.

This peculiar fracture has also been found to occur in steel test bars when broken by impact.*

Figs. 1, 2, 3, 4, and 5 show the forms of some of these fractures. Fig. 1 is from a cast-iron test bar which broke 5 inches from centre of span A. Figs. 2 and 3 are from steel cutting tools, the bearing surfaces being about B and C, Fig. 3 showing the wedge-shaped fracture. Figs. 4 and 5 are from cast-steel test pieces, the former being broken by impact.

Fig. 1.

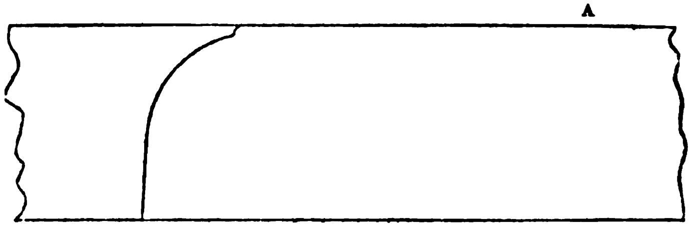
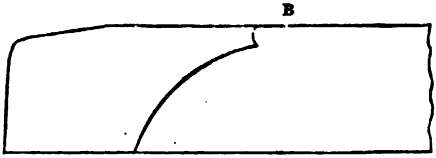


Fig. 2.



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* Webster on Iron and Steel—See Minutes of Proceedings Inst. C.E., Vol. LX.

Fig. 3.

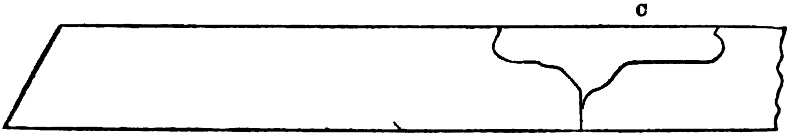


Fig. 4.

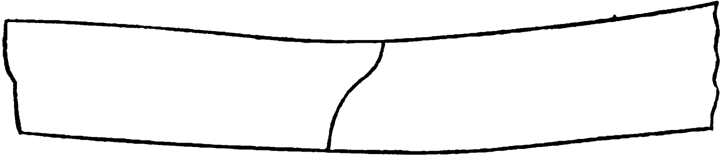
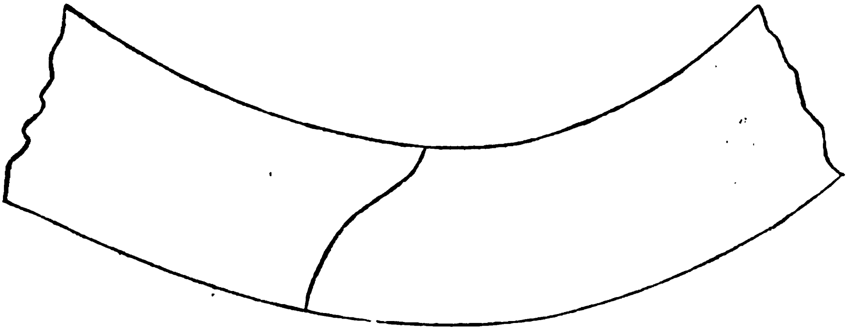


Fig. 5.



It appears not unlikely that where such wedge-shaped fractures occur, the surface of pressure applied is of some breadth, and not the so-called "knife edge."

The following are some additional facts since obtained by personal observation and experiment:—

(1) The strength of cast-iron is materially reduced by vibration. Thus in many cases where test bars did not break at a given load a few taps with a hammer caused rupture, and further, at each stroke an additional increment of deflection was obtained.

This weakening, due to vibration, was very well marked in the case of some test links of cast-iron subjected to a tensile load.

The links under ordinary conditions were found to have a breaking strength of 26,415 lbs. per square inch, or about 11¾ tons. This result was obtained from 20 tests.

In two cases, however, the links were loaded up to seven tons, and while under this load, were lightly struck. At each stroke the effect was noticeable at the end of the compound lever used for putting on additional weights; the end of this lever dropping with each stroke until in one case it had passed through $\frac{9}{16}$ inch, and in the other 1⅝ inch, at these points the link broke, no additional load having been added above the seven tons. The proportion of the lever being as 100 to 1, the average amount of stretching in the links would be about $\frac{1}{100}$ inch.

The strength of the transverse bars corresponding to the first mentioned link was 3700 lbs., and that of the bars for the second link was 3200 lbs.

From an examination of the fractures there was no flaw or indication of weakness, so that in all probability the links, if loaded and treated in the usual way, would have stood 11 or 12 tons.

The strength therefore of these links under vibration may be taken as only seven-twelfths of the average ordinary strength.

The average strength of 40 test bars of 36 inch span, 2 inches deep, and 1 inch broad, cast at same time as the links, was 3586 lbs. with a deflection of ·402 inch, giving a modulus of rupture of 48,411 lbs. per square inch.*

It will be observed that this modulus is very much higher than the tenacity of the material. The cause of such difference is accounted for by some writers as due to the "skin" of the bars. From experiments, however, made with planed and unplaned bars no difference was found in breaking strength, although in the case of the planed bars the skin was quite removed.

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* Where $M_r = \frac{3 \times W \times S}{2 \times b \times d^2}$ = modulus of rupture, W = load at centre in lbs., S = span in inches; *b* and *d* being breadth and depth in inches.

(2) The strength of the casting, if of considerable weight and size, is less than the test bar or link.

Thus a test bar and link were prepared from a piece of a casting, when it was found that the test bar broke with a load at centre of 1830 lbs., deflection ·254 inch, and that the link broke with a load of 8 tons.

Great variations occur in the test bars themselves, several hundred pounds of difference of breaking load being frequently noticeable in bars cast together and of same metal.

In the discussion of a paper* read before this Institution, Mr J. M. Gale stated that he had found pieces of cast-iron, when made into links, to stand much less than the above figures.

That great variations of strength are likely to occur throughout a large casting appears likely from these results.

(3) From a comparison of the tests given it appears that the average tensile strength of the links is just 7¼ times the transverse strength of the bars when these are 2 × 1 in section.

In some former experiments made with bars of one square inch section it was found that the tensile strength was 26 times the transverse strength.†

(4) The question of the action of extreme temperatures on iron and steel has frequently been considered, the evidence being somewhat contradictory. Dr Joule, from experiments, concluded that frost does not make steel or iron (whether wrought or cast) brittle. In a paper read before the Inst. C.E., in February, 1880,‡ it is shown that severe cold does not affect the tensile strength of wrought-iron and steel, but that it lowers the transverse strength of cast-iron test bars by about three per cent., and that wrought-iron, steel, and cast-iron, when subjected to impact at 5° Fah., are found to stand less than when at ordinary temperatures.

(5) The author believes that the elastic limit of cast-iron is very high, and near to the breaking point. Thus, if bars be tested at

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* See Trans. Inst. E. & S., Vol. XXVII.

† See Trans. Inst. E. & S., Vol. XIX.

‡ "Webster on the Influence of Heat on Iron and Steel."

different loads it is found that at first there is a set—the bar when relieved from strain not returning to the zero it started from—after a few applications of the load this set disappears and the bar becomes practically elastic.

Treating some of the bars experimented on by the formula $E = \frac{W \times S^3}{4 \times D \times b \times d^3}$[*] and taking W as the load when set disappears, E becomes about 17,000,000, which accords well with the average value for the modulus of elasticity of cast-iron.

(6) The influence of sudden cooling on iron causing permanent contraction has been noticed by some observers.

The author finds that sudden heating causes a rapid expansion in cast-iron.

Thus, when experimenting on the phenomenon of floating cast-iron,[†] he found that if the pieces were taken out of the molten metal as soon as they rose and appeared floating at the surface, there was an expansion upon them equal to the whole extent of the shrinkage from the molten condition, but yet the temperature of the floating iron when taken out was in some cases so low as not even to show redness, and barely to melt lead. A piece of an ordinary test bar, 12 inches long, 2 inches deep, by 1 inch broad, was found to have become at least 12⅛ inches in length, and a cutting from a pipe 11 inches internal diameter and 12⅞ inches external diameter, was found to have increased to 11⅛ inches and 13 inches respectively.

On measuring some of the pieces experimented on after cooling, it was found that they had returned to their original length.

(7) The cause of this floating phenomenon has been much discussed and different explanations offered to account for it. The author, from his experiments, believes that it is due to expansion suddenly set up in the cold metal by the intense heat of the molten metal into which it is placed, this expansion giving the necessary buoyancy to

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[*] Where W = load in lbs. applied at centre of span, S = span in inches. D = deflection in inches, and *b* and *d* the breadth and depth of the bar in inches. E being modulus of elasticity.

[†] Proceedings Royal Society of Edinburgh, Session 1881-82.

cause the metal to float. Heavy pieces sink, but shortly rise and float with their upper surface at the surface of the molten metal, behaving very much as a piece of wood or cork does when weighted so as just to float in water, and, when immersed, slowly rises to the surface.

These experiments were extended to gun metal, phosphor bronze, type metal, copper, and lead. The gun metal and phosphor bronze floated like cast-iron; the pieces of type metal, copper, and lead showed no flotation, unless when the pieces were very light.

Malleable iron sank and did not rise.

(8) It is sometimes stated that the sharpness of an iron casting is due to an expansion which takes place on setting. The question of the density of cast-iron at, and about the point of fusion, has been investigated with apparently varying results. Some observers holding that it is less dense, and others that it is more dense. So far as the author's experiments have gone, he has been unable to detect any expansive action in cast-iron on setting, and he is inclined to think that one cause of any difference of opinion which may exist is due to the action of air or gases in the casting. As in one of his experiments a distinct rise of the surface of the metal took place just about solidification, but this surface after being allowed to cool was found after drilling across to be quite porous.

The sharpness of an iron casting appears mainly to depend upon the temperature and quantity or head of the metal, and the condition of the mould. Thus in some cases test bars present a smooth or slightly rounded edge, and are said to be dull run in contradistinction to those having a sharp edge when cast at a proper temperature.

The floating phenomena with the accompanying expansion does not seem necessarily to have any bearing on this question of expansion of iron at setting, as, already stated, the temperature of the floating piece is comparatively low, and far below the melting point.

(9) In reference to dull and hot run metal, it may be interesting to state that the average strength of ten bars cast at once from a ladle of hot metal was 3,524 lbs., defn. ·402 inch, whilst that of ten other bars, run from the same metal, left over and kept in ladle till

it was as dull as to allow of pouring, was 3,619 lbs., defn. ·371 inch. The dull run metal being rather the stronger and stiffer of the two.

(10) The surface of some molten cast-irons presents a peculiar appearance which has had more or less consideration bestowed upon it by observers. The markings or "break" on the surface are regular, and present the appearance of stars or of a geometrical pattern made by the interlacing of curves.

The whole surface of the molten mass appears to be agitated in a manner that would suggest repulsion of the parts, as the patterns start out with astonishing rapidity.

This "break" varies with the kind of iron used, and experienced founders can tell the quality of the iron from observing this marking.

Thus, a long broad "break" is said to indicate hard iron, whilst a small star like "break" indicates soft iron.

That the markings are really cracks on the skin, which rapidly grows upon the surface, is most probable. As it is found that with very hot iron, freshly run from the cupola, the markings are not noticeable, as the surface of the metal is in a sense boiling in the ladle. As, however, the metal cools, the markings become observable, showing the well marked significant pattern; with further cooling, however, the regularity of the patterns disappears until irregular and slowly forming cracks take their place—if, however, this crust be broken, the "break" is seen going on below.

The author, without being able to come to any definite conclusion as to the cause of this appearance, suggests that these forms may be due to cracks formed by the shrinkage of the skin during cooling; the shape of the cracks being at first regular, probably due to the bubbling up movements on the surface, these actions being so rapidly repeated that a kind of pattern is formed. That the markings ultimately become true cracks seems obvious by watching the surface.

Brass does not give any particular appearance, but a peculiar wrinkle takes place on the surface of molten copper on cooling; in this case, however, there is no definite pattern, but a series of irregular ripples, as it were, left on the surface.

In the after discussion,

Mr **John Thomson** said they were all greatly indebted to Mr Millar for bringing this paper before them. He knew that Mr Millar had given great attention to the subject, and especially to the changes that took place in the solid iron floating in a ladle of molten metal, and his experiments on test bars and the nature of the breaks in them. He could bear out what Mr Millar had said in the paper, as to the skin of cast-iron not adding anything to its strength. He had himself tested bars of two inches by one inch. One of these bars which had purposely been made larger than those dimensions, had been planed, while the other was cast to the proper size. The one which had been planed stood the largest amount of test—in fact, was not broken at all; while the other bar, which had its skin left on, broke under a considerably less load than the planed bar stood. This result had much surprised him; for it used to be thought that the skin of the casting added to its strength. He would like if Mr Millar would add to his paper the maximum and minimum of the breaking strains of the 10 bars referred to. It would be interesting to note how they varied, although castings from the same ladle, and, he supposed, in the same box; and it would be also satisfactory to know how much they varied. After reading the paper, the members might have some further remarks to make on the subject.

Mr **Millar** said he would be glad to give the maximum and minimum breaking weights of the hot and dull run bars at next meeting.

The **President** said, with reference to the honeycombing of the surface, he would like to know whether that occurred at the vertical face or only on the horizontal surface at the top of a bar.

Mr **Millar** said that when any porous structure showed in the bars it was at the upper horizontal surface and not at the sides, and seemed to be simply due to scum or air in the metal. As a rule the bars showed a sound section.

Mr J. J. **Coleman** said that the subjects embraced in the paper were ones which had engaged his attention for a number of years, and were ones of much interest to scientific men in general and practical engineers in particular. He had had large experience in the use of very low temperatures combined with cast-iron in cylinders, in which the temperature fell sometimes as low as 120 degrees below zero. In all his experiments he had never had the least reason to suspect that the low temperature had an effect upon the quality or strength of cast-iron. Very high pressures had been used with the compressed air, and they had never had the slightest accident. This confirmed the experimental results got by Dr Joule. Theoretically it was quite possible that at exceedingly low temperatures approaching the zero of absolute temperature, cast-iron might become brittle or altered in molecular structure—but it must be remembered that the difference between zero Fah. and 60° Fah. is really the difference between 460° and 520° absolute—so that it is little likely the metal can be much affected between such limits.

The discussion of this paper was resumed on 23rd March, 1886.

Mr MILLAR said that in response to the suggestion of Mr John Thomson he had prepared a table, which was shown upon the wall. It gave details of the actual and reduced strength of the specimens of iron experimented upon. He had noted the maximum and minimum strengths, which were also marked on that table. He might say that the maximum loads bore out what he had stated in the paper—that the dull run metal had rather the advantage over the hot run in strength and stiffness.

DULL METAL.			HOT METAL.		
Breaking Weight.		Defn.	Breaking Weight.		Defn.
Actual.	Reduced to 2 × 1.		Actual.	Reduced to 2 × 1.	
Lbs.	Lbs.	Inch.	Lbs.	Lbs.	Inch.
3340	3602	·381	3300	3548	·390
3300	3548	·374	3350	3350	·389
3740	× 4021	·405	3800	3800	·426
3740	4021	·392	× 3100	× 3100	·342
× 3100	× 3333	·336	3450	3450	·393
3150	3387	·329	3680	3680	·431
3430	3430	·362	3220	3462	·399
× 3950	3950	·424	3350	3350	·384
3300	3548	·358	× 3850	× 3850	·438
3350	3350	·355	3650	3650	·431
34400	36190	·3716	34750	35240	·4023
3440	3619	·371	3475	3524	·402

The SECRETARY stated that he had received communications from Mr T. A. Arrol and Mr Dyer, who were unable to be present, as follows:—

Mr T. A. ARROL wished to know if Mr Millar could state the mixtures of metal used in the experiments, as the tensional results seemed to be very high. Also, whether the bars were cast on end or horizontally, and if any difference in result is obtained if one mould is drier than another. He thought the author's experience of the floating phenomenon quite bore out Mr T. Wrightson's results, and which were communicated some years since to the Iron and Steel Institute.

Mr HENRY DYER, C.E.—Mr Millar's paper might be made the text for a very long discussion, as it treats of the elasticity, the strength, and the molecular constitution of cast-iron, each of which affords many interesting subjects for investigation. In the few minutes at my disposal I propose simply to touch on some of the chief points mentioned in the paper. And first I would remark that the paper requires to be taken along with Mr Millar's previous papers, read before this and other Institutions, to which it forms a supplement, and of which it gives an abstract. The only fault I have to find with it is that it is too condensed, and is deficient in evidence in support of some of the conclusions at which he arrives. The question of the form of fracture has been sufficiently discussed in connection with a previous paper, and there can be no doubt that to Mr Millar we are indebted for an explanation of the relation of the form of the fracture of a bar and the manner of loading. The first additional conclusion which Mr Millar states has long been known and is noticed by several writers on the subject. For instance, Fairbairn in his "Treatise on the Manufacture of Iron" (3rd ed. p. 220), says, "From these facts it is deduced that so long as the molecules of the material are under strain (however severe that strain may be) they will arrange and accommodate themselves to the pressure; but with the slightest disturbance, whether produced from vibration or the increase or diminution of load, it becomes, under these influences, only a question of time when rupture ensues." In connection with this subject of vibration it would be well if the researches of Wöhler and Spangenberg on the action of live loads were better known to British engineers than they seem to be, as the results are of great importance, especially in connection with the construction of machines. They show that it is not sufficient to estimate the dimensions of any part of a structure or machine simply from the maximum stress to which it is subjected, but that these dimensions ought to be made to depend on the range of variation of the stress. With regard to the value of the *modulus of rupture* being greater than the tensile strength of the material, that is easily explained when we consider the manner in which the expression for it is obtained. The *modulus of rupture* is defined as being equal to the stress at the outside layer of the bar when on the point of breaking. Now throughout the ordinary theory of the bending of bars it is assumed that the stress does not exceed the elastic limit, and experiments prove that the formulæ obtained hold very closely so long as that limit is not exceeded, as is shown by Mr Millar from the value he obtains for the *modulus of elasticity* from experiments on bending. When, however, the elastic limit is passed, the assumption usually made—that the stress varies as the distance from the neutral axis—no longer holds, and the lateral connection of the layers into which the bar may be supposed to be divided begins to have effect, and raises the limit of elasticity, and makes the *modulus of rupture* greater than the tensile strength. Tredgold, in his well-known book on the "Strength of Cast-iron," calculated the tensile strength from experiments on transverse breaking, and for the reasons mentioned above his results are far too high, and it was not till Mr Hodgkinson instituted a series of direct experiments that the amount of the error was suspected. The results of Mr Hodgkinson's experiments are published in the sixth volume of the Reports of the British Association. With regard to the action of the skin of the metal, it is usually assumed that it increases the strength, but the experiments mentioned by Messrs Millar and Thomson seem to show that that assumption is not justified by experience. In any case, however, the metal near the skin is likely to be stronger than that near the centre on account of its greater density. The Tables to be found in the ordinary textbooks for the transverse strength of materials require to be very carefully used, as the constants there given have many different meanings. For instance, Rankine says that the *modulus of rupture* is 18 times the load which is required to break a bar of one inch square area, supported at two points one foot apart and loaded at the middle between the points of support. This value follows directly from the general formula for the transverse strength of beams which is

$$m\,w\,l = n\,f\,bh^2$$

where m depends on the manner of loading and n on the form of cross-section, and f is the *modulus of rupture*. Substituting for the case of a rectangular bar 12 inches long and 1 inch square, supported at the two ends and loaded in the middle, we have

$$\tfrac{1}{4}\,\mathrm{W}12 = \tfrac{1}{6}f$$

$$\text{or} \qquad f = 18\,\mathrm{W},$$

which agrees with Rankine's statement. Mr Millar's form of the expression of course also follows simply by substituting the special values of m and n in the general equation. Stoney uses a quantity which he calls the *co-efficient of rupture* which is one-sixth the *modulus of rupture* as used by Rankine, and is equal to the moment of the rupturing couple in a bar of one square inch section, of the same material as the rectangular bar under consideration, that is

$$\mathrm{M} = nf\,b\,h^2$$

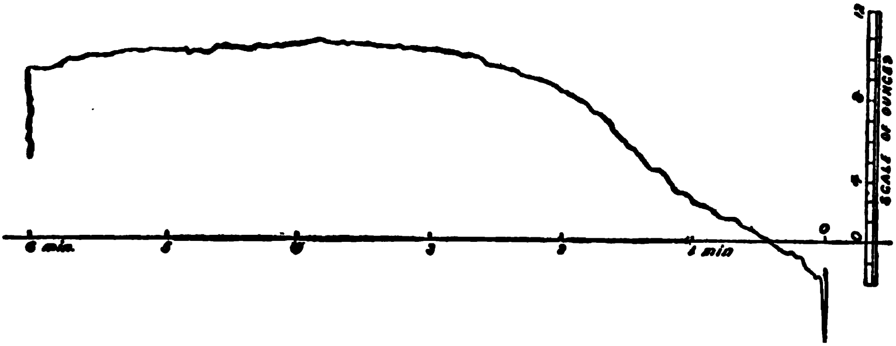
so that for unit rectangular bar

$$\mathrm{M} = \tfrac{1}{6}f.$$

Then again in the Tables you often find different units of weight and dimensions used, and this is apt to lead to mistakes unless care be taken to ascertain exactly how the constants have been obtained. Mr Millar's second conclusion, that the strength of the casting if of considerable size and weight is less than that of the test bar or link, has also been known for a considerable time, and experiments were made by Mr Berkeley (and I think also by Mr Hodgkinson) for the purpose of settling this question. His Reports must be referred to for his results. For girders it has been suggested that instead of test-bars, *unit* girders should be used, that is to say girders made to a small scale with the same form of cross-section as the actual girders, and the strength of the latter calculated from the former. The method adopted by Mr Millar of giving the tensile in terms of the transverse strength is convenient for practical calculations, but care must be taken that the same size of cross-section and length of bar is always used, otherwise the statement is of no value. A very convenient and easily remembered rule for the transverse strength of cast-iron, is that a bar one foot long and one inch square will break with about a load of one ton hung on the middle. This gives a *modulus of rupture* of a little over 40,000 pounds on the square inch, which may be considered an average value. The effects of the variation of temperature, and the elasticity of cast-iron, have been very fully investigated so that we need not dwell on them. There is, however, a considerable difference of opinion as to the cause of the floating of solid on the surface of molten cast-iron. In my opinion Mr Millar gives the most probable reason, that is, that the solid expands sufficiently to cause the density to alter to such a degree as to enable it to float on the molten metal. With regard to the question of expansion on the setting of cast-iron, Mr Millar doubts that any such expansion takes place. That expansion occurs seems to be one of those common beliefs which people take for granted without investigation. Mr Wrightson has, however, made some very careful experiments which seem to confirm this belief, and before setting it aside we must ask Mr Millar to favour us with details of the experiments which have led him to doubt it, as the question must be decided entirely by the weight of experimental evidence. The various surface markings mentioned by Mr Millar in paragraph 10 afford useful indications to founders as to the metal employed, and his explanations that these markings are breaks in the skin is one which is likely to be generally accepted. In conclusion, I would say that papers of the kind given by Mr Millar are very valuable, for although some of the conclusions may have been previously known, they contain the results of the personal experience of the author, and not only confirm the opinions of previous writers, but add to their results and suggest fields of inquiry for further investigation.

The Secretary stated that Mr THOMAS WRIGHTSON, of Stockton, who had given much attention to this subject, had also, by request, kindly forwarded some notes on the subject of the paper:—

The phenomenon of floating cast-iron was investigated by me in 1878 and 1879, and was the subject of two papers read before the "Iron and Steel Institute" in the autumn session of 1879, and the spring session of 1880. No attempt, that I am aware of, had before this been made to determine quantitatively the changes in volume. If a ball of cast-iron without internal cavities (which condition is easily ascertained by taking the specific gravity of the whole ball and comparing it with the known specific gravity of solid iron of the same kind) be lowered gently into a ladle of molten iron of the same kind as the ball, it sinks below the surface. This *first* fact proves that the density of the ball is greater than that of the molten iron. The *second* fact of interest is that very soon after the ball is submerged it rises to the surface, the top of the ball being just visible. This floatation arises from the fact that the volume of the ball has become greater by the expansion due to the heat, and when it just appears at the surface of the molten metal the average density of the ball is equal to the density of the liquid metal. This amount of expansion is quickly arrived at in a 4″ cast-iron ball, the time required being 20 to 30 seconds. Now comes to me the most interest ing part of the experiment, and one which has been much neglected; this is the gradual rising of the ball out of the molten metal. It is, of course, due to a continuation of the expansion of the ball, as the heat is conducted from the surface to the centre. The height to which the ball rises out of the metal is the *third* fact of interest. If the mass of the ball remained intact it would not be a difficult matter to estimate its average density at any time of the experiment, by a comparison of the volume of the submerged and emerged portions of the sphere; but the upper part of the ball being in the air, while the lower part is in the hot metal, causes the under part to begin to melt first, and then the mass is altered, and spoils the calculation. In order to measure the change of volume accurately and get rid of this source of error, I designed an instrument which enabled me to plunge the ball several inches below the surface of the metal, out of the way of scum, &c., and to keep it in that position by means of a stiff and sufficiently heavy rod. This rod was hung at the end of a spiral spring, like a Salter's balance. As the ball expanded below the surface of the metal the floatation caused an upward pressure on the stiff rod, and this upward pressure was registered by the contraction of the spring to which the rod was hung. A pointer on a scale of ounces showed exactly what was occurring beneath the surface of the metal. In order to make the instrument self-recording I had a vertical barrel placed alongside the spring balance, which was worked slowly round by clock work. A sheet of paper was wound round this, and with a pencil carrier attached to the end of the spring I was able to obtain a diagram which showed by its vertical ordinates the expansion of the ball in terms of the ounces of liquid metal displaced; and by its horizontal dimension the time after submersion corresponding to the successive changes of volume. It was found that the high conducting power of the iron favoured the maintenance of the form of the ball, as the heat was conducted rapidly from the surface to the central portions. On several occasions the balls were pulled out before melting, when the whole mass was plastic, the ball still keeping its mass. When once the ball commenced to melt it went very rapidly. The diagrams from the instrument all showed a sudden fall in volume at the melting of the ball.* (See annexed Fig.)



This rapid fall from the plastic to the liquid state is the *fourth* interesting fact to which I wish to draw attention. If the diagram of cast-iron be read backwards, it is a history of the changes in volume as the ball cools from the liquid to the solid, and the sudden rise in the diagram from the liquid to the plastic state records that property of iron which is of such value to the founder, viz., the sudden expansion on setting. Following the diagram we shall find that the volume of the iron remains at near its maximum for a considerable period, and then contracts. Where the base line and the diagram line cross, is when the ball is of the same density as the liquid iron, *i.e.*, when the ball is the same size as the original mould

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* See Plate II., "Iron and Steel Proceedings," 1880.

in which it was cast. The contraction continues a little further, as shown in the diagram, which last justifies the allowance for contraction always made by the pattern-maker. These phenomena were also investigated from the molten to the solid state by casting 15-inch balls in specially constructed loam moulds, and measuring the balls as they cooled. The result was a confirmation of the experiments already described, so far as the general behaviour of the metal in changing volume was concerned. From the average of six carefully conducted experiments with the instrument I have described, on Cleveland No. 4 iron, I obtained the following results:—That the specific gravity of the liquid metal was 6·88. That the iron in passing from the cold solid to the plastic condition expanded from the density of 6·95 to that of 6·5, which is close upon 7 per cent. That the cold solid iron is 1·02 per cent. greater density than the liquid iron.

Mr C. C. **Lindsay** said, that to a great extent what he intended to say regarding the changes in the volume of cast-iron, in passing from the solid to the liquid state, and *vice versa*, had been forestalled by the communications sent to the Secretary from Mr Wrightson, who had, he thought, given greater attention to the subject than any other experimentalist. He (Mr Lindsay) produced a diagram of the volumetric curve (see Fig. in Mr Wrightson's remarks), taken by that gentleman in his experiments, which did not, however, reproduce irregularities known to be due to imperfections in the mode of experimenting, and after describing the instrument invented and used by Mr Wrightson, for producing the diagram, said that it seemed to him that the curve was a confirmation of his belief that cast-iron was at its greatest density when solid, its least at the melting point, and that the density of the molten metal was between the two. He also believed that the sharpness of castings was due to the expansion of the molten metal at the time of setting, that is, when it is assuming its permanent solid condition. A consideration of the curve would explain the cause of the failures produced by extreme cold. In the first place rapid cooling of the exterior of the molten metal would be sure to give rise to enormous internal tensional and compressional stresses, and cause the iron to be more sensitive to extreme emperatures. Where cast-iron is allowed to cool slowly, and develop its normal crystalline structure without internal abnormal stresses, it becomes homogeneous, and its crystals are arranged so as to adapt themselves to changes of temperature with regularity. Practical illustrations of the effect of cold on cast-iron suddenly cooled was shown by the recorded failures of cast-ironwork on the Canadian, Russian, and Scandinavian Railways, where the temperature is often 30° below zero Fahrenheit. When ordinary cast-iron chairs were used, thousands of them give way in the winter time. No doubt this is partly due to the rigidity of road produced by frost, but at the same time he contended that if the chairs were cast of proper strength, and cooled slowly, not, as is usually the case, taken out of the sand red-hot, the breakages would be extremely few. The beneficial effect of slow cooling was shown when the American cast-iron waggon wheel, invariably slowly cooled and of good metal, and the American permanent way were introduced into Canadian Railways in lieu of the English double-loaded rail and chair system. Since then very few failures had taken place. From his own experience he considered that cast-iron is affected to a certain extent by the low temperatures incidental to our climate, and that the metal is certainly affected to a serious extent in countries where very low temperatures are experienced, he was, however, of opinion that a properly designed casting without abrupt changes in thickness, cast from good metal, slowly cooled, would not be much affected. Similarly, good wrought-iron and steel rails would, if made suited to the requirements, stand low temperatures, and ensure but few failures. Proof of this was found in the enormous number of rails which gave way in Canada, Russia, and Scandinavia until steel rails, made to bear a high impact test, were introduced. He wished to emphasise his meaning by stating that although cast-iron is seriously affected by very low temperatures, and wrought-iron and steel were also seriously affected by low temperatures when subjected, as in the case of rails, to impact, yet a knowledge of the behaviour of these metals and the requirements, combined with the proper design and manufacture of the material, would almost, if not wholly, obviate failures due to cold.

Mr THOMAS DAVISON said that this question of the relative weights of cold and hot iron had engaged his attention many years ago. He had made an experiment with a perfectly solid turned ball of four inches diameter, and was disappointed to find that when he put it into the ladle it went to the bottom and never came up again. He concluded that experiments which showed a different result were due to the unsoundness of the casting, or that the surface of the metal was not clean. Certainly an ordinary piece of scrap iron always floated. He had never found any evidence of the expansion of iron in large masses. He found that large quantities of iron, say 20 tons, shrank from the moment it was poured into the mould and required feeding so long as any part remained liquid. He had seen no evidence of any expansion in it. He was afraid that floating was due to the imperfections in the specimens experimented upon.

The PRESIDENT thought it suitable that he should make a small reclamation. So far as he knew, he had been himself the first to put forward the view that bodies which on being loaded moderately at first and relieved of the load showed a permanent set, and then, on having successively increasing loads applied and taken off, showed at each stage an increased permanent set, would ordinarily afterwards be capable of bearing renewed applications of the smaller loads without showing any new permanent sets for the renewed loadings, such as the permanent sets at first given by the original loadings. If there were any changes due to repetitions of loadings these were not at all corresponding to those due to the first application of the loadings on a new piece of material produced in many of the ordinary modes of manufacture. He referred on this subject to a paper by himself, published in the "Cambridge and Dublin Mathematical Journal" for November, 1848;[*] and recently re-

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[*] "On the strength of materials, as influenced by the existence or non-existence of certain mutual strains among the particles composing them.

published in the article on "Elasticity," by Sir William Thomson, in the new edition of the "Encyclopædia Britannica." The views he had put forward there, and in another paper ("On Spiral Springs") which accompanied that paper in the "Cambridge and Dublin Mathematical Journal," he thought afforded explanations of some at least of the physical conditions under which the phenomena occurred which were referred to in Mr Millar's paper, where he said—"Thus, if bars be tested at different loads it is found that at first there is a set—the bar when relieved from strain not returning to the zero it started from—after a few applications of the load this set disappears and the bar becomes practically elastic." Professor Eaton Hodgkinson had previously to those researches and papers which he (the President) had issued 38 years ago, published as results from experiments in which he had been engaged, enunciations to the effect that a strain in cast-iron, however small in comparison to that which would occasion rupture, was sufficient to produce a *set*, or permanent change of form in the beams on which he experimented; and thence Mr Hodgkinson was led to announce as a conclusion, that "the maxim of loading bodies within the elastic limit has no foundation in nature." From this conclusion, he (the President) had at that early period dissented, and in the papers above referred to, had given reasons for his dissent, and had put forward the view which he himself had formed on the subject. In his view the experimental results arrived at by Mr Hodgkinson were probably to be sufficiently accounted for through the supposition that the beams fresh from the foundry would be in various parts self-strained, somewhat like imperfectly annealed glass, so that a very slight loading would be sufficient to strain some parts beyond their limit of elasticity and cause them to take a permanent set through ductile yielding; and that greater loading, short of what would cause rupture, would carry the same process farther. Thus the bar would be left in a condition with its limit of elasticity extended so that it would be able to bear repetitions of the smaller loadings without taking any more such sets as before on each occasion of new loadings; and he thought that practically for engineering purposes the ordinary maxims as to the limit of elasticity were not to be abandoned in virtue of the experimental observations put forward by Mr Hodgkinson. No doubt there was much that was obscure at that time in respect to elasticity, and much remains still to be discovered by further experimental researches jointly with theoretical considerations. He referred on this matter to important experimental researches by Sir William Thomson, published in the Proceedings of the Royal Society of London for May 18th, 1865, in a paper entitled, "On the Elasticity and Viscosity of Metals." These experiments tended to show that materials such as metals can take a *temporary set* after being kept strained for some time, from which set they can recover gradually when freed from the straining forces. The phenomena may be likened to such as might be expected to show themselves in the behaviour of a bar if artificially made up of a perfectly elastic, spongy substance, with its pores filled with a viscid substance such as pitch, if this bar were alternately loaded and unloaded. On this important subject he thought a wide and promising field was open for further refined, experimental researches.

The discussion was then adjourned till next meeting.

The discussion of this paper was resumed on April 27th, 1886.

Mr James M. Gale said that cast-iron was largely employed for water pipes. He had had many failures of cast-iron pipes brought under his notice, and he had worked out the tensile strain on the metal at the time of fracture of some which gave way in 1882 and 1883. The average tensile strength of cast-iron might be taken at 7½ tons, or 16,800 lbs. per square inch. Mr Millar, for small test links, gave it as high as 20,981 lbs. For a rolling load one-sixth was usually taken to ensure absolute security, or 2,800 lbs. on the square inch. It was found, however, that when used for water pipes no such rule could be followed. Weisbach, Bateman, Rankine, D. K. Clark, Fanning, and Unwin all gave formulæ varying from each other, some of them producing thickness not much more than half what was usually employed. To be safe, if 16,000 be taken as the ultimate tensile strength and eight as a factor—making the working strain 2000 lbs.—it would appear to be sufficient. He referred to large pipes only, 36 to 48 inches in diameter, under considerable pressure. In smaller pipes and for light pressures other allowances had to be made. Yet in the average of the six cases given in the subjoined table the pipes failed under a strain of 1170 lbs. on the square inch only, or one-fifteenth part of the ultimate strength of ordinary cast-iron.

Table showing the Size and Thickness of some Cast-Iron Water Pipes which have failed, and the Tensile Strain on the Metal at the time of fracture.

Hour of Fracture.	Diameter of Pipe.	Number of Years in use.	Pressure at time of failure.	Intended thickness of pipe.	Least thickness of metal along line of fracture.	Tensile strain on point of least thickness of metal at time of failure.	Nature of Fracture and Remarks.
	Ins.	Years	Lbs. per sq. in.	Ins.	Ins.	Lbs. per sq. in.	
(1) 8 P.M.	48	23	58·	$1\frac{3}{8}$	$1\frac{3}{8}$	1113	Large piece blown out from end to end of pipe. Casting appeared quite sound, and of equal thickness.
(2) 6-55 P.M.	36	$4\frac{1}{4}$	72·7	$1\frac{1}{8}$	$1\frac{1}{8}$	1163	Piece blown out of socket end. No defect observed in the casting.
(3) 4-25 A.M.	36	$6\frac{1}{2}$	76·23	$1\frac{1}{4}$	1	1372	Piece about 2 ft. in diameter blown out, within 2 ft. of socket end. The metal appeared sound. Socket not injured.
(4) 10-25 P.M.	36	$6\frac{1}{2}$	84·62	$1\frac{1}{4}$	$1\frac{1}{8}$	1354	Large piece blown out of side of pipe, 8 ft. long by 2 ft. 4 in. wide at widest part. Both spigot and socket ends remained entire. The metal appeared sound.
(5) —	36	$4\frac{1}{2}$	61·	$1\frac{1}{8}$	$1\frac{1}{16}$	1033	Piece not blown out. Crack in middle of pipe passing about one-third round it in an irregular curved form. Pipe laid in rock cutting.
(6) 8-50 A.M.	36	7	65·	$1\frac{1}{4}$	$1\frac{3}{16}$	985	A crack at socket end; piece not blown out. The crack did not extend into the sockets. Pipe laid in solid sandstone rock.
Average tensile strain on point of least thickness of metal at time of fracture, - - -						1170	

These pipes had been all proved to upwards of double the pressure they could be subjected to in work; they had all frequently sustained greater pressures than that under which they gave way; and there was no shock at the time of fracture, no valves being closed, and nothing to produce a water ram. There was, of course, a constant vibration of the needle of the pressure gauges attached to these pipes but only 1 or 2 lbs. up or down. The pipes had been in use some 4½, 6½, 7, and one 23 years, and all the castings appeared sound. He was led to the conclusion that the most of these fractures were caused by the pipes having been improperly exposed while cooling after casting. This did not apply to those that failed at the socket end, as they might have been injured in the jointing. He was of opinion that pipes should not be taken out of the boxes till they were comparatively cool, and he thought they ought to remain in the boxes at least one hour for each three inches of diameter, and for each inch of thickness. That is, a 24-inch pipe 1 inch thick should remain in the box eight hours; a 36-inch pipe 1¼ inch thick, 15 hours; and so on. He was not prepared to say that all the weakness found in the case of large cast-iron water pipes was due to cooling improperly; for he had had pieces cut from perfectly sound parts of burst pipes in Lloyd's Proving House which failed at about a half of the usual strain to which cast-iron is generally put, as shown by the following table.

Table showing Ultimate Tensile Strength of portions of Cast-Iron cut from 36-inch Water Pipes.

No. of Test	Description.	Dimensions of Sample.			Breaking Strain.	
		Breadth.	Thickness.	Area.	Total.	Per sq. inch.
	Specimen from Pipe No. 6 (see Tab. I.),	Ins.	Ins.	Sq. in.	Tons.	Tons.
2542	Spigot end, .	3·38	×1·366	=4·617	20·775	4·499
2543	Socket end, .	3·09	1·305	4·032	22·95	5·691
2544	Socket end, .	3·07	1·24	3·806	found to have been fractured	
	Specimen from 36 in. pipe—					
2545	Piece taken from body of pipe, .	1·27	1·39	1·765	6·8	3·852
2546	Piece taken close to fracture, .	1·01	1·35	1·363	7·225	5·300
	Average Breaking Strain, - - - - - or 10,830 lbs. per square inch.					4·835

The large ironfounders had the means of pushing this inquiry further than he had been able to carry it, and he trusted they would yet take it up.

The President asked if Mr Gale could give any opinion as to whether these pipes had been fractured in consequence of any unequal settlements in the ground, or whether they had given way from fair water pressure, or must be attributed to bad annealing or bad cooling?

Mr Gale said it was difficult to say. There were no coal workings beneath these pipes. It appeared to him that while the fractures were undoubtedly due to the water pressure on the pipes, there was something else at work causing damage to them.

Mr John Thomson said that if Mr Gale's ideas were to be carried out and pipes, after casting, were to be allowed to remain so long in the boxes as he had suggested, it would cause ironfounders to double or treble their plant. This would certainly increase their prices. He hoped, however, that Civil Engineers and others would study Mr Gale's remarks, and see that henceforth they specified their pipes to be a good deal stronger than they were in the habit of doing. That he considered was the gist of Mr Gale's remarks—that the pipes should be made heavier—and as an ironfounder he would be very glad to see his ideas carried out. With all respect to Mr Gale's opinion he did not see why the cooling of the pipes should be blamed, or why this should affect them. A pipe was a very equal casting, it was not like a marine cylinder where there was a chance of unequal contraction. A pipe was the same all round, and the contraction should be equal all round. He thought with all due deference to the opinion of the President that perhaps there was something in the over-fatigue of the metal. This subject had been discussed by them before, and he must say that he was of opinion that from five to twenty-three years under constant pressure of so many pounds to the square inch must tell upon the strength of pipes. Now the test bars which Mr Millar had experimented upon had never been at any work at all. They had been cast in the foundry, but without being subjected to any strain whatever. It seemed to him that this was a very natural conclusion—that a strain of so many pounds per square inch for twenty-three years had caused the pipes gradually to deteriorate. He did not say that that was so, but he thought it was a fair enough deduction from the premises. He knew the President opposed that view, but it was well-known that new metal stood a greater pressure than metal that had been under work for years.

The PRESIDENT said he would like to propose that out of some of the next pipes that broke with very small tensile strains some pieces should be cut and put to the test to see whether the strength of the iron was really reduced to one-fifteenth of the strength it ought to be. By that means they would see whether the iron had deteriorated or not.

Mr GALE said he had cut out pieces from some of these fractured pipes, and the average strength was about 10,000 instead of 16,000, or 18,000. He would like Mr Thomson's attention drawn to the manner in which those large pipes were cooled. He did not think they would be injured if they were equally exposed all round, but the way in which he thought they erred in the iron-foundries was, that a portion of the pipe being left with a part of the mould on it, while the other part was exposed. He had seen many pipes exposed for 15 or 20 minutes with half of the mould attached to them, and the other half off. Mr Thomson must have experience of pipes cracking of themselves, in the yard, with a loud noise, which must have arisen from their improper cooling.

The PRESIDENT,—But would not these pipes show the ordinary strength of good iron if cut into test bars, and so relieved from much of the stress caused by the unequal cooling?

Mr GALE answered, that he would expect that they would, but his experience was not great in that matter. Large ironfounders might very easily make experiments, for they had always plenty of burst pipes.

Mr DAVISON said that the thickening of the pipes would cause them to take longer to cool, and therefore increase the difficulty. He would like to know what effect slowness in cooling pipes had upon the strength of the metal, and also irregularity of cooling. It was well known that the cooling of steel in different ways changed its character. He had seen large pistons made of cast-iron crack, after they had been lying in the yard for months. He was not certain whether this subject had been sufficiently investigated with regard to pipes.

Mr GALE did not think that it had.

Mr MILLAR, in replying to the various remarks, stated, in answer to Mr Arrol's question, that there was nothing special in the mixtures of metal used, it being the usual foundry iron used in Glasgow for such castings as water pipes. The bars were all cast horizontally in green sand moulds. In reference to Mr Dyer's remarks as to the *modulus of rupture*, the only object he had in view in referring to this was that its high value in cast-iron bars was sometimes assigned by writers to be due to the skin of the casting, but the experiments recorded in the paper, and Mr Thomson's experience as stated in his remarks in the discussion, showed that even after the removal of the skin the *modulus of rupture* was far above the tenacity of the material. The term *modulus of rupture* was used in the same sense as by Professor Rankine, viz., to represent the value of f in the equation $m\,W\,l = n\,f\,b\,h^2$, when W = ultimate, or breaking load. Mr Dyer indicates that this stress f only rises *above* the tenacity *after* the limit of elasticity is passed. This is not, however, the case with some of the cast-iron bars experimented upon. Thus, some bars which finally broke about 4000 lbs., were previously tested several times with a load of 2800 lbs., a small amount of set showing at first, but at the fourth application of this load there was found to be no further set; the bars might therefore be considered practically elastic, yet the value of f with the load of 2800 lbs. reaches to about 38,000 lbs. per square inch, which is much in excess of the tenacity. The average deflection of four bars at the fourth application of the load of 2800 lbs., and when no set was then recognisable, was ·290 in., which gave 14,000,000 lbs. as the modulus of elasticity. The ultimate value of f (now *modulus of rupture*) when W = 4000 lbs. is reached, becomes about 50,000 lbs. per square inch. Mr Wrightson, who has kindly contributed, by request, the results of his experiments, shows that the cause of the floating of the cold solid iron on the molten iron is *buoyancy* obtained by expansion of the solid iron. This is quite in accordance with the author's experiments. Mr Wrightson states that this phenomenon was investigated by him in 1878. He (Mr Millar) might say that so far back as 1873 he had stated his belief that from the results of his experiments this flotation was due to expansion set up in the cold solid when immersed in the highly-heated molten metal. Direct measurements of this expansion were not, however, made till a later date.* The experimental evidence, therefore, indicates that the floatation is caused by buoyancy due to a rapid expansion set up in the cold solid when it is placed in the molten metal.. The late Mr R. Mallet, F.R.S., in a paper read

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* See Proceedings Royal Society of Edinburgh, Session 1881-82.

before the Royal Society in 1875, gives the results of his experiments on floating cast-iron, but was unable to give any definite explanation of the cause. He had no doubt as to the flotation, as he says that the cold solid, when immersed in the molten iron, behaves like a *buoyant body*. He can only account, however, for this floating tendency by suggesting a "repellant force." Mr Davison, in his remarks on this point, doubts this buoyant action, and instances an experiment in which the ball, four inches in diameter, which he used "went to the bottom and never came up again." This is easily accounted for, as it is usual for heavy pieces to remain sufficiently long below the surface to have their edges melted, and consequently are liable to stick to the bottom of the ladle. Light pieces generally float at once, heavier pieces sinking and then coming to the surface. One very noticeable fact is that, if the pieces be taken out immediately on their rising to the surface, they are found to be comparatively *low in temperature*, at least in some cases lead did not melt when placed upon them, yet it was found by measurement that an expansion of at least ⅛ inch to the foot had taken place. As this expansion is much more than could be expected from the range of temperature—say from 60° to 1000° Fahr.—seeing that about ⅛ inch to the foot is a usual allowance for the shrinkage of a casting from the molten condition to the cold solid state—or say from 2600° to 60° Fahr.—it appears likely that a portion of the surrounding heat disappears as work of expansion, suddenly changing the bulk of the body, but without a corresponding increase of temperature, the main rise in which occurs after the body has changed its bulk. This expansion of the cold iron when placed in the molten iron must not be confounded with any supposed expansion of the molten iron on setting, as the conditions are quite different. In the first case the cold solid is suddenly influenced by a temperature fully 40 times in excess of its own, and rapidly changes its form, yet keeps comparatively cool; whereas in the latter case the molten iron follows its natural law of cooling down gradually. This question of expansion of iron on setting appears to be more difficult to settle than the expansion on floating, Mr Wrightson himself preferring an indirect method—viz., that of measuring from the cold state to the liquid — he therefore considered that if the diagram taken by his instrument of the buoyant action of the floating iron be read backwards it is a history of the changes in volume as the metal cools from the liquid to the solid. This, from the reasons above given, the author thought failed to be quite satisfactory, as the conditions are so different; he therefore preferred direct experiments, such as both Mr Wrightson and Mr Mallet record, as being made with balls cast in moulds. Strangely enough, however, these gentlemen draw quite opposite conclusions from their experiments on this subject, Mr Wrightson finding evidence in favour of an expansion on setting, and Mr Mallet finding only evidence of contraction. The author's own experiments did not point to any expansive action. Mr Dyer very properly asks as to the nature of these experiments. One simple method was to watch the surface of the head of metal in a casting; but, like Mr Davison, he had been unable to detect any uprise on setting. Some experiments were made by filling moulds with molten cast-iron and placing an iron bar across the top just touching the surfaces. In one case a slight uprise took place on each side of the iron bar, but was not of sufficient force to lift it, the force required to do so being only about 1 lb. per square inch. From the fact that this raised upper surface was found to be porous, and that the uprise appeared to take place after setting, he was inclined to attribute it to air or gas held in suspension, combined with contraction of the surfaces of the casting. The most conclusive experiments, however, were some carried out with ordinary test bars cast in a sand bed. The ends of the moulds were closed lightly by pieces of iron, hoop iron was used in one case, and were kept in position so slightly that any expansive action referred to would at once have forced them back, and left a record by a crack in the sand. The mould was left open for a few inches at the ends and covered in the middle, so that any action at setting could be readily observed. The moulds were filled and carefully watched, but not the slightest movement of the ends of the moulds was observable. Now, had there been a large and sudden expansion of the metal on setting, it should surely have been easily observable in such an experiment, as the moulds being from two to three feet long, the large percentage of expansion referred to by Mr Wrightson would have given a readily measurable extension in length. Mr Coleman and Mr Lindsay have made valuable remarks from their own experience in regard to the action of extreme cold on cast-iron; and the President has shown that the peculiar action of the set in loaded materials referred to in the paper had been investigated by him many years ago, thus corroborating what appears at first a curious circumstance, as one usually expects the set of a bar to increase instead of to decrease. In the cases referred to in the paper the set observed took place and disappeared within elastic limits, whereas the change which we speak of as permanent set appears to take place after the limit of elasticity is passed, such as may be observed with a piece of mild steel when broken by tension. The remarks by the gentlemen who had spoken that evening did not appear to call for any reply on his part. The strength of the pipe metal as determined from test pieces cut from a pipe broken in foundry was given in the paper, and showed a considerable reduction when compared with the results obtained from the same metal cast direct into test bars. He had to thank the members for giving the subject so much consideration, and he was very glad that the paper had elicited so much valuable information.

On the motion of the PRESIDENT a vote of thanks was passed to Mr Millar for his paper.

On Hydraulic Plant for Bessemer and Basic Steel Works.

By Mr Finlay Finlayson.

(SEE PLATES VI. AND VII.)

Received 16th, and Read 23rd February, 1886.

It will be my endeavour in this paper to give a short description of the various hydraulic arrangements required for Bessemer and Basic Steel Works, and more especially those which have come directly under my own notice within the past few years.

1.—ENGINES FOR PROVIDING THE HYDRAULIC PRESSURE.

At the Moss Bay Works, Workington, with which I was connected, there were two sets of hydraulic pumping engines. One set had 16 inches steam cylinder by 3 feet stroke, with differential rams, one of which was coupled up behind each engine, with the crank shaft and fly wheel in front, and the other set had 18 inches cylinders by 2 feet 6 inches stroke, with differential rams bolted to the bed-plate in front of cylinder, with crank shaft and guide bars between. There was also a large differential pump, the same as those made by Hawthorn and Davey. The pressure worked with a pressure of 480 lbs. per square inch, but my experience of these engines was that they gave a great amount of trouble, the leathers on the rams requiring to be changed frequently, sometimes even twice a week. Very often pieces of the leathers passed through the pipes into the valves, which caused the valves for the converters, or cranes, as the case might be, to be changed.

Engines and pumps of the kind spoken of are invariably to be found in the older steel works, but at the Glengarnock Steel Works there has been put down one pair of engines, 26 inches cylinders by 3 feet stroke, with variable cut off gear, so that they can condense if required. These engines are fitted with four rams, one in front and one behind each cylinder, and so arranged that they can either work the engines single or coupled. The engines are similar to those designed by Armstrong & Co., and those working at the North Eastern Steel Works, Middlesbro', which have so far given very good results, as there are no leathers about the rams to get into the valves.

At the Vulcan Foundry, Coatbridge, at present a set of engines of similar design to those at Glengarnock, are being made for the Moss Bay Company to do away with all their present differential ram pumps; and I noticed while in America recently, that duplex pumps with four rams are those mostly in use.

2.—THE ACCUMULATOR.

At some of the English Steel Works there are two accumulators with rams 24 inches diameter by 15 feet stroke, working with a pressure of about 480 lbs. per square inch. Only one has been put down at Glengarnock, the ram of which is 24 inches diameter by 15 feet stroke, with wrought-iron tank hanging on the outside for dead weight. This tank is capable of holding 140 tons of pig iron, so that the required accumulator can give a pressure of 700 lbs. per square inch. And by means of an arrangement of levers, it is provided that before the accumulator rises to its full height it shall open a safety valve, and pass the water back into the tank. Instead of passing all the water through the accumulator, an arrangement of the pipes has been made to ensure that the engines may deliver the water into the hydraulic main pipe, the surplus passing into the accumulator. The main pipe is led into the pulpit or distributing box, where the pipes have branches cast on for the various valves. The same is done with the back pressure, the water being delivered back into a tank, so that it is only necessary to make up for leakage. Each of the branches is provided with a hydraulic stop cock, which is bolted on, so that if it is wished to change a valve, that can be done by shutting off the water at that particular point, and thus keep the work going on.

3.—TIPPING GEAR FOR TURNING THE CONVERTERS.

At Messrs Bolckow, Vaughan, & Co.'s Works, Eston, the converters are turned by means of a pair of coupled engines, with steam cylinders, about 14 inches diameter by 18 inches stroke, geared about four to one on to the worm shaft. The worm-wheel, which is keyed to the trunnion of the converter, is about 6 feet diameter by 4½ inches pitch. These engines are worked from the pulpit platform.

At Moss Bay the turning of the converters is done by a moving cylinder 12 inches diameter by 6 feet 6 inches stroke, with cast-steel rack bolted on to top of cylinder. This rack gears into a cast-iron spur wheel 3 feet diameter, and gives the converter a full half turn. The cylinder moves into a cast-iron slide bolted on to foundation. The piston rods, which are hollow tubes, are fixed on to a strong crosshead, and which has to withstand the strain on the piston when turning the converter. The water passes through the piston rod to the back end, and the other rod is plugged at the end, and a hole is drilled through the rod at the front of the piston. This arrangement is not very safe as the water is invariably leaking past the nuts for fastening the piston rod, thus causing the vessel to creep down when the "heat" is in it, unless either the blower or his assistant is at the levers on the pulpit platform to check it.

At Glengarnock the tipping gear is the same as that shown in Figs. 1 and 2 (see Plate VI). The converters are virtually 12 tons Bessemer or 8 tons basic, and when they are lined with brick, and filled with the charge to be blown they will weigh about 60 tons. The tipping cylinder is 16 inches diameter with a clear stroke 10 feet 2 inches. The pinion is of cast-steel 4 feet diameter by 12 inches broad, between caps by 4½ inches pitch. The caps are turned to pitch line, and keyed on to a cast-steel trunnion fixed on to converters. The rack is of cast-steel with caps planed to pitch line so that the teeth cannot get too deep into gear. This rack is bored with a taper for the piston rod, and the rod is made a good fit into it and held by means of a steel cotter. The guide bar for the rack is of cast-iron, and is bolted and keyed between the snugs on the columns as shown on the drawing. The girders for the cylinders are bolted to the columns, and riveted to the main or hot metal railway girders, so that the whole stageing is in one piece. This gearing enables the operators to turn the converters fully ¾ of a turn, so that they can pour off the slag behind prior to adding the spiegel or ferromanganese.

4.—JACK RAM.

At a number of the English, and also at some of the American works, jack rams are used to put in the blast plugs. At Moss Bay two top supported cranes are used for this purpose.

For the Glengarnock Steel Works I designed a jack ram the same as is shown in Figs. 3 and 4 (see Plate VII.) for putting in the plugs, and taking off the bottom or dished piece, as also the nose piece of the vessel; so that it is only necessary to reline the centre piece of vessel in its place. The ram rests upon, and is bolted to, a very strong carriage, the cheeks of frame being made of wrought-iron. It is wrought by means of a valve bolted on to the carriage as shown in Figs. 3 and 4, and a pipe is brought up from the main culvert with two hydraulic swivel joints, so that the pipe can be coupled or uncoupled, and the pipes turned out of the way when not in use. The ram is made of cast-iron, it is 11 inches diameter and is capable of lifting 20 tons. When the piece is on the ram the carriage is pulled back into the repair shed by means of another ram geared four to one inverse, and the piece is lifted off by means of a 20-ton overhead travelling crane.

5.—THE VARIOUS HOISTS REQUIRED IN CONNECTION WITH STEEL WORKS.

At the North Eastern Steel Works, Messrs Bolckow, Vaughan, & Co., and some of the works in Wales, the direct carriage and ladle are lifted by means of a hydraulic hoist with the cylinder sunk underneath the floor. At the Moss Bay Works we lifted the ladle out of the carriage and tilted it into a runner, but we abandoned this method about four years ago, and made an incline behind the furnaces, so that the locomotive could bring up the ladle right behind the converters, and we tapped the ladle into the runners for that purpose. At Glengarnock the hoist was also done away with for the direct ladle. The liquid metal at that establishment is brought up an incline from the furnaces, right in front of the converters, and the ladle (which is hung on trunnions) is tilted by means of a worm and wheel. As they always require to keep one cupola for melting the pig-iron, which is made at the end of week, I designed a hoist for lifting that material to the charging stage, the lime to the calcining cupolas, and one for lifting lime for the converters—three hoists in all. The rams for the two lime hoists are 10 inches diameter, and the one for pig-iron hoist is 12 inches diameter, all geared four to one inverse. (See Figs. 5 and 6, Plate VII.)

6.—THE CASTING CRANE.

At Moss Bay they have two cranes, one of which is a transfer crane for three converters; this is on the American system. The converters stand within a radius of 22 feet so as to suit this crane, The ram is 18 inches diameter and is made of cast-iron, and the jib, which is 18 feet in radius, is turned by means of two hydraulic rams with pitched chain and pulleys, the ends of which are fixed on to each ram. This crane, if kept in good order, works very well. The ladle is transferred from this crane to the casting crane, the ram of which is 2 feet diameter and the jib 20 feet radius. The jib is hung and revolves on a brass step with four rollers for taking up the thrust of the jib. This crane is turned by hand gear, so that it can be easily steadied when casting. It has given no trouble, and is very easily kept up, but it is very extravagant on water. I may state that this class of crane is to be found in most of the older steel works.

At Glengarnock I designed a special crane for this purpose the same as that shown in Fig. 7, Plate VI. The ram and top guide are all in one solid steel forging with a cast-steel collar shrunk on for a roller bearing to revolve against for taking up the thrust of the jib. There is a very strong casting fixed into the concrete foundation. This casting is bored out to receive the bottom of the cylinder, which has a close end and is carefully turned to make a good tight fit into the casting. There is also a heavy and strong casting fixed on to the top of foundation, which is bored and faced to receive the cylinder which is bolted to this casting. The whole is bound together by means of six 2½ bolts passing through the casting on the top, and the one in the bottom together with about four feet of the concrete. The ram is 16 inches diameter in cylinder becoming reduced to 15 inches diameter for top guide. Above the 15 inches diameter is shrunk a forged steel ring which is grooved for a ball bearing to carry the weight of the jib. The top guide is of wrought-iron of box section, and is riveted to hot-metal railway girders. Inside of this is a cast-iron bush, with flat cast in to prevent the ram from turning. The jib is built of wrought-iron, with an iron casting riveted on to receive the ladle, and the tie bolts are fastened to a 3½-inch bolt which passes through the cheeks and strong snugs on the casting just referred to. The jib is fixed to a cast-steel centre by means of turned and fitted bolts. The effective weight of the jib is taken up by a back balance, and the whole is hung on a very strong cast-steel gland, and revolves on a ball bearing, all as shown on Figs. 7 and 8, Plate VI. The whole crane and ladle (when the ladle is full) will weigh about 34 tons, and the pressure on the ram, when working at 500 lbs. pressure per square inch, is fully 45 tons.

Next we come to the ingot stripping cranes.

At Moss Bay there are two balanced cranes, but as they would not work quick enough with one ram on the accumulator, both rams are coupled up, and the water passes through the valve into both rams. At Moss Bay there is a turn out of 2500 tons per week from one pit, so that the stuff has to be handled very quickly, hence the reason of both rams being coupled up.

There are also a number of top-supported cranes which cost very little for up-keep in comparison to the others. For the Glengarnock works I designed a similar crane of which there are five for stripping the ingots in the two casting pits, and they are so arranged that if only one pit is being worked it can have three of the cranes for itself. (See Fig. 9, Plate VI.)

The rams for these cranes are of forged steel, 10 inches diameter, and the two plates or cheeks are also made of steel.

These checks are fastened to the ram and top guide by means of turned and fitted bolts. The jib is made of two steel plates ½ inch thick by 14 inches deep riveted to channel steel 11 inches broad by ½ inch thick and planed on top for the monkey roller path. The bottom of the jib is stiffened by means of distance ferrules and rivets passing through both plates. The effective weight of jib is taken up by means of a back balance. The top guide revolves in a roller bearing, thus reducing the friction to a minimum. There are other two cranes the same as these, one for lifting the ingots from the soaking pits, and landing them on to the live rollers, and one for loading blooms and billets.

At Glengarnock instead of trying to knock out the ingots, as is generally done in Scotland, there are three sticker presses, each of which consists of a very strong casting with a hydraulic cylinder bolted on and a differential ram, so that a pressure of 50 tons can be applied to push out the ingot, and leaving as much space under the piston as will bring it back. These presses, wherever they have been in use, save from 20 to 25 per cent. of ingot moulds, and the the ingots can be stripped hotter, which is an advantage for the soaking pits. (See Figs. 10 and 11, Plate VII.)

At most of the American and English works, the ingot is pulled from the heating furnaces by means of hydraulic power. Either by a battery of rams and wire rope, or, as is the case in America, with one ram for two furnaces, and a bench is fixed to the floor with a number of pulleys, one opposite each furnace door. At Glengarnock there is a small ram geared four to one inverse, and placed so that one ram serves two furnaces.

A hydraulic arrangement, designed and patented by myself, for turning up the ingot at the large cogging mill at Glengarnock, similar to that shown in Fig. 12 (see Plate VII.), has proved very successful.

The cogging mill referred to can be worked by two men and a lad, and in America, on a similar mill, I counted ten men doing the same work. Messrs Miller & Co., Coatbridge, who are the makers of nearly the whole plant at Glengarnock, put a very neat hydraulic arrangement on this mill for lifting the loose roller in the front of the rolls, so that the mill can go on to any section without any trouble or altering the rollers as in the case in most of the mills of this class.

In two or three places hydraulic shears have been put down for cutting blooms. From the experience with these, I prefer a set of hot bloom shears, driven by means of a pair of. 26 inches engines, and having steel gearing worked to cut 30 inches by 10 inches, as they are cheaper in the first place, and, I think, are a saving on steam. Steam shears on this principle are doing splendid work both in Jarrow and Glengarnock. The engines of these shears are reversed by means of two hydraulic rams, and the hydraulic stopper arrangement adopted in them is so worked that the blooms can be cut to the exact length required. This is a new departure, but I understand it is doing very well at Jarrow, where it is at work. Of course there are a great many other purposes for which hydraulic power can be taken advantage of, such as ramming up the bottoms for converters, and making bricks, &c., in the repair shed, all of which are taken advantage of when sufficient hydraulic power can be applied, but my intention in this short descriptive paper has been more to show a comparison of the new style of hydraulic working as against the old, alike in respect of the first cost, effective working, and application.

The discussion on this paper took place on 23rd March, 1886.

Mr James Riley said that with regard to the paper of Mr Finlayson, which he had been privileged to read, he would like to be permitted to say, that he was of opinion that it did very great credit to Mr Finlayson, and that it was quite evident, by the manner in which he had brought this matter before the

Institution, that he had worked out a very capital plant at Glengarnock to deal with the large masses of metal there made. At the same time although he had introduced to some extent hydraulic power, it might be permissible for him (Mr Riley), having connection more particularly with the other class of steel works, namely, the open hearth manufacture of steel, not mentioned by Mr Finlayson, to make a few remarks. It was quite evident that Mr Finlayson in this short paper, had a vast amount of material. He had not wasted his words in description, and his paper was worthy of their study. In the matter of the engines, in which he referred to the hydraulic pressure, he had evidently arrived at a just conclusion, that it was well fitted to the work. He had embodied all the more recent improvements in these engines, introduced by Sir William Armstrong and others, who had devoted years to the perfection of this class of engine. He would pass on to notice the cranes to which Mr Finlayson referred. Doubtless Mr Finlayson had improved upon the old fashioned cranes, long in use, which Sir Henry Bessemer had invented, and which yet remained in practical use. One rather wondered, however, why Mr Finlayson, while approving of hydraulic power, had yet stopped short in using it to the full extent in these cranes. He (Mr Riley) confessed he did not understand why Mr Finlayson had, in his casting cranes, for instance, left it to manual labour to pull round the jib, more especially as hydraulic power would do it much more quickly—a matter of the utmost importance in dealing with hot metal in steel works, in order to save the heat stored up when casting. In the cranes put down at Glengarnock he considered that was a remarkable deficiency. In the cranes that were put down at Newton and Blochairn they were each controlled by one man. There might be a reason why the same system was not adopted by Mr Finlayson. He believed that in no work had it been done, except at Newton and Blochairn. At Glengarnock all his centre casting cranes were likewise worked, he believed, by manual labour. A very simple engine was applied to the casting cranes at Newton and Blochairn. Mr Finlayson in his paper referred to another appliance (see page 161):—"At Glengarnock instead of trying to knock out the ingots, as is generally done in Scotland, there are three sticker presses." Usually a mould will cast 50, or 60, or even 90 charges, and therefore he would like to ask why it should be necessary to apply a sticker press when very generally it would be found that rather than push out the ingots these could be knocked out, or released by their own weight as required, whereas Mr Finlayson seemed to have in view the desirability of using the ingot directly after it was cast, with a view to the use of the last inventions or appliances, and utilize that heat which otherwise would be lost. Now, if Mr Finlayson had tried he would know that if any one attempted to take the ingot out of the mould before it had shrunk it would in all probability burst, or "bleed" as the workmen said; that is, that the molten metal would flow from it. Hence, he saw no reason why there should be one sticker press; more especially was he puzzled to know why there should be three at Glengarnock. Perhaps Mr Finlayson was not aware that the system described in the paper was the one patented by Mr George Wilson, of Charles Cammell & Co. some ten or twelve years ago, and had never been adopted elsewhere. Coming to the hydraulic shears, Mr Finlayson gave the result of his experience with them. He (Mr Riley) believed that the only set of large shears at present in existence were those in the Blochairn Works. There those shears were set to work some 18 months ago, and since they were finally and fairly set to work they had never ceased working until now, and they had given no trouble. They cut slabs 24 inches by 8 inches, and even 30 inches by 9 inches easily, and had done so all the time since the accident that they had at the commencement. That accident was due to a little oversight, but it led to the utilisation of a little machine that was there. Of set purpose they had no accumulator connected to those shears. When those shears were designed it was intended that the pressure should be direct from the pump into the cylinders of the engines on the shears. They, therefore, kept away all complications of accumulators; but they had depended too much on manual labour, as they found out; for the man in charge was rather too late in working the valve, and, it being shut, necessarily the valve box burst. That had taught them a lesson; and now anyone could see the engines at work controlled automatically under the pressure of a ton on the square inch, without the intervention of an accumulator at all. Mr Finlayson had spoken of the dispensing with packing leathers in the engines and pumps. He might say that in the whole of the works of the Steel Company of Scotland all the engines and the whole of the hydraulic pumps throughout, even in the cylinders of the large shears, with a pressure of a ton on the square inch, giving 1000 tons to the ram, were all packed merely with hemp packing, and no leather at all was used. It might be of interest for the members to be informed that the shears at first were only intended to cut slabs of 24 inches by 8 in., but now they were found able to cut slabs of 30 inches by 9 inches, with the pressure indicated. They had no data to go upon until they tried the shears themselves. Mr Dick had made sundry experiments, and these enabled them to come to the conclusion that a pressure of five tons per square inch was required for the purpose intended; and they had since found that a mass of red-hot steel of the above dimensions required a force of 2 tons 15 cwt. to shear it. Hence they had an abundance of power. He wanted it to go forth that, in the matter of comparison between the hydraulic and the steam shears, the best mechanical effect was produced by the former; there was also a much steadier motion with the hydraulic than the steam arrangement, and it was more perfectly under control and might be trusted to the full in every way.

Mr FRANK W. DICK remarked that he did not know he had very much to say on this paper. In the case of their ingots it happened that no more than one or two stickers occurred in the course of a week. He spoke of the Newton Works, where the "stickers" were never such as to require a press. He wanted to point out what he thought Mr Riley had omitted—and that was the reason why they worked directly from the ram. In working shears from the accumulator there would be a great loss of power. They would have to make the pressure sufficient to cut the larger pieces, while it would be the same for the smaller slabs. When the pumps were connected directly to the shears the pressure on the pump was the measure of the power excited by the engine of the shears and was strictly in proportion to the force required.

Mr SINCLAIR COUPER said he had been through the Glengarnock Works, and also through the principal steel works in England, and found that Mr Finlayson's appliances and arrangements were exceedingly well adapted for the purposes for which they were designed. In regard to Mr Riley's and Mr Dick's remarks he would like to observe that there seemed to be none of the Siemens-Martin works on this side of the Border which used sticker presses. In these works, by simply dropping the casting, it was found that if there was anything holding it quickly gave way; and if it did not it was considered high time to reject the mould. There was little loss in thus rejecting it, as the broken pieces of the ingot mould went back into the furnace.

Mr FINLAYSON, in reply, said that with regard to the cranes referred to by Mr Riley he had been in nearly all the leading Bessemer steel works in England and America, and had remarked as a singular fact that in none of them were hydraulic cylinders applied to turn their casting cranes. Mr Snellus of West Cumberland had tried to turn his with hydraulic power, but it was found not to be steady enough over the mould—that was at the time he (Mr F.) was designing the Glengarnock Works. At Moss Bay the casting crane was turned by means of two men, and in Mr Riley's works at Blochairn, he had noticed two men on the top of the crane, apparently working it. In Glengarnock they employed only two men at the crane—one for steadying the ladle, and the other for turning the crane—so in reality they have just the same number of men as were employed at the Blochairn Works at this crane. In regard to the turning of the ingot stripping cranes, in Moss Bay they had two cranes for doing this work similar to those at Blochairn. At first it took three men to pull the cranes round, but subsequently they were able to do it with two. There were also four or five top-supported cranes for charging the ladle, &c., in Moss Bay, which he had shown to Mr Riley, and in America where they used the same class of cranes, in no case had they hydraulic turning gear, because they would consider it in the way, in the turning out of 3500 tons of steel per week; and also that it would cause expensive upkeep. That was the reason why he had adopted this system at Glengarnock, viz., top supported cranes with roller bearings similar to what is shown in the drawings, and which only requires one man to do the pulling round, this same man unhooking the chain from the ingot. In regard to the sticker press, he did not know what it may be in the Siemens-Martin process at present, but five years ago he had seen the workmen pick up the ingot, and drop it on the ground; and in Bessemer shops where they had a run of stickers very often caused by the metal pouring out so quickly that it could not be stopped from running over the side, and the consequence is a sticker ingot. At Messrs Bolckow, Vaughan, & Co.'s Works they had a sticker press; and since he had left Moss Bay they had got two. These were for Bessemer works, and in Basic works it was required even more, for sometimes the metal boiled over, and then the sticker press was particularly required. He thought that at the Siemens-Martin Works they would find it boiling over sometimes. Now with regard to the hydraulic shears. About four years ago as a test they had put up hydraulic shears at Moss Bay out of an old brick press to cut eight-inch square blooms. They were not designed like Mr Riley's shears for the work to which they were put. He had learned that other two sets of hydraulic shears for cutting steel had been put up at Moss Bay. With regard to the packing of the rams in Bessemer shops, he might say that leather would not stand, so that all the packing had to be done with hemp. In their engines they had to use leather, the construction of the rams causing this; but in every case where they could they packed the glands with hemp.

Mr Riley said he thought he was entitled to make a remark as it appeared to him that the impression which Mr Finlayson had intended to leave with them was that two men were required to work the casting crane at Blochairn. Mr Finlayson might have seen a man and a boy on the crane. The boy's function was to work the crane, and the man was there to attend to the ladle.

On the motion of the President, a vote of thanks was passed to Mr Finlayson for his paper.

On Bridge Construction (with Special Reference to a Cantilever Span for Bridge over the Hooghly River).

By Mr ALEXANDER FINDLAY.

(SEE PLATES IX. AND X.)

Received and read 23rd March, 1886.

STRUCTURES for carrying roads, railways, &c., under or over other roads, railways, or over rivers, are increasingly becoming a necessity in this age of enterprise, and in attempting a paper on this important subject, it must be of the most general description, as time would entirely fail us to enter on definitions or theoretical considerations, involving as this would do a large amount of research and data; for, like other studies, we find that the further we investigate the wider does it open out in its intricacies, in the various and altering relations of stresses, strength of materials, elasticity, ductility, and so forth, which is quite beyond our present purpose.

Some consider that there is not much room for further invention in connection with the design for the superstructure of a bridge, as what can and what cannot be done with existing materials is very well understood. The elementary rules, such as that the strength of a girder is as the square of its depth, and inversely as its length, are clear enough, but improvements in the distribution of material are not yet at an end. Wood and stone as constructive materials have been giving place to iron and steel, enabling structures to be designed and executed which 50 years ago were impracticable. The dimension and design of a bridge must depend on the nature of the obstacles to be crossed, and on the traffic to be carried; and the designer will be guided by those requirements, so as to have the best form of construction, and in determining the materials to give best results at a minimum of cost.

It may, however, be observed, that in these days of increasing weights and speeds, it may be found poor economy to cut the calculations for a bridge very fine, when only a few tons of material are being saved thereby.

Where foundations cannot be easily obtained, large spans are preferable to small spans, and indeed, unavoidable, as in such cases as the gigantic suspension bridge over the East River from New York to Brooklyn, and the still greater engineering effort now in progress at the Forth Bridge Works.

The steel cantilever span over the Hooghly, which we shall consider later on, is also a case in point, where the available position of foundations must necessarily determine the design best suited to meet this.

The data fixed by law to regulate the minimum dimensions of over and under-bridges may be briefly stated.

Over-bridges—these are bridges carrying a road over a railway—should be 35 feet wide for a turnpike road, and 12 feet wide for a private road; and the span required for our ordinary double line railways is 26 feet in the clear, with 14 feet 6 inches of head room above the rail.

Under-bridges are those in which the road goes under the railway, must have 35 feet width for turnpike road, and 16 feet height at centre. For bridges crossing a river or stream, the maximum floods must be provided for, and so on.

The usual load allowed in calculating for a public road bridge, is one hundredweight per square foot, and for a railway bridge, "one ton per foot run," that is one ton for each foot in length of each line of way, with the dead weight of structure added.

The Board of Trade stipulates that a bridge constructed of wrought-iron must not—with the greatest load which can be brought upon it—be subject to a greater stress or strain than 5 tons per square inch of section, and with steel not more than 6½ tons per square inch. In large bridges and viaducts, it is also considered best that the permanent way should be between the main girders.

Where cast-iron is used for the construction of intermediate piers, they should not be of small diameter, such as 12 inches to 18 inches; and in all large structures a wind pressure of at least 56 lbs. per square foot must be provided for.

There are important structures of wood and stone, showing the ingenuity and constructive skill of past ages; but in this, as in many other matters, it has been during this century that immense strides have been made in the forward march all over the world. In a country like the United States of America, with its ever increasing railway ramifications, some important structures are to be found. Take the old suspension bridge over the Niagara River, with its span of 821 feet, at a height of 245 feet above the river, completed in 1855—over 30 years ago—at a cost of about £80,000, and which must have been considered a triumph of engineering skill then. The superstructure is like a hollow rectangular box, 18 feet deep and 24 feet wide, the railway running on the top, and a roadway for public traffic inside along the bottom. It was the privilege of the writer to be resident in St. Louis, Missouri, from 1871 to 1874, when a very fine bridge across the Missisippi River was being constructed, and which was for that time an outstanding example of engineering enterprise, and inventive application of means to ends, and even now this bridge is as fine an example of an arched bridge as any yet erected. The difficulties encountered with the substructural work were of the most formidable kind, for the river here, confined to a channel 1600 feet wide, through which the whole volume of the Mississippi flows, with an extreme range between high and low water of about 40 feet, was constantly changing the river bottom, and in 1870 during a freshet the scour cleared out to a depth of 51 feet below low water mark, so that it was considered advisable to go down to the rock with the masonry piers. This was done by means of large caissons—provided with air chambers and locks—at depths reaching 136 feet below high-water mark, at which depth the foundations were satisfactorily laid, some ingenious compressed air and other appliances being used in the work. There are 3 spans in this bridge, the centre span being 520 feet, and the side spans 500 feet wide in the clear. The piers and abutments are of rubble masonry up to low-water mark, and above this faced with gray granite. The superstructure is of cast-steel arches, having a rise at centre of about 47 feet, formed of cast-steel tubes 18 inches diameter in 12 feet lengths, in thicknesses varying from 2⅛ inches to 1½ inches. Those tubes form an upper and lower rib, and each span has four of those double ribs all strongly braced together at their relative distances to each other, and the tubes forming them being jointed butt to butt with wrought-iron couplings, havin steelg pins 4½ inches to 7 inches diameter passing through for supporting and connecting the spandril and arch bracings and ties. The vertical bracing between the upper and lower tubular ribs, which are 12 feet apart from centre to centre, convert the two members into a single arch. The spans were very satisfactorily erected without using any centering or staging, each span being commenced by the steel ribs being well secured by cast-iron sockets and tie bolts to piers and abutments, and each intermediate length being coupled on to its fellow, and braced in position till the connection at centre of arch was completed.

Double lines of railway are carried on top of arches, and above this a roadway 54 feet wide, for carriages and foot passengers, and the bridge was opened for traffic on 4th July, 1874. We shall not be able to enter further into general descriptions, and indeed the very able papers given here during the past year on the Forth Bridge appliances, gives us the ingenuity of all the ages up to date; but in parting with this portion of our paper, it may not be out of place to say, that the prices paid for the superstructural work of the St. Louis bridge just described, does in these times fairly make one's mouth water. Steel work, £60 per ton; wrought-iron £40 per ton; rolled iron £28 per ton, and all calculated at 2000 lbs. to the ton, makes contractors, at any rate, sigh for the "good old times."

Now, we shall turn our attention as briefly as possible to another country—*India*, one of our best colonial possessions or dependencies—and what a world of railway extension is evidently continually going on there, with the varied and increasing railway requirements, keeping our bridge works employed, on East Indian Railway, Indian State, Bombay and Baroda, Oude and Rohilkund, and many others, and without which, in those depressed times, the pinch would have been severely felt in many quarters, which have been kept well employed. On the East Indian and Indian State Railways, there are at present in progress many large structures varying from 800 feet span downwards, designed and superintended under the instructions of A. M. Rendel, C.E., of 8 Great George Street, Westminster, Engineer to those Railways; and among others a bridge over the Hooghly River, above Calcutta, the centre cantilever span of which has been constructed and recently shipped to India by Messrs James Goodwin & Co., of Motherwell. As this span is the largest of its kind yet constructed in Scotland, and is something unique in its way, a short description may not prove uninteresting.

The complete bridge now under consideration crosses the Hooghly River about 20 miles above Calcutta, and is to carry a double line of the East Indian Railway, 5 feet 6 inches gauge, and the length of bridge is about 1200 feet. Owing to the peculiar nature of the river it was exceedingly difficult to get piers put in, sudden and heavy floods existing during a good portion of the year, and from this and other causes it was found advisable to have a centre cantilever span, which we have now to describe.

The piers are 120 feet 6 inches apart centres, and central to each abutment. From high-water mark to foundation of piers is 90 feet, and the piers were formed with a steel shell with semi-circular ends, measuring 66 feet × 25 feet over all, with upper portion 55 feet × 25 feet on which girders and wind struts rest. Those shells are strongly constructed and braced internally, and filled in with concrete. From foundation to seat of girders the piers measure 112 feet high over all. The shore girders, which rest one on each end of cantilever span, are each 420 feet long, resting *solid* on seat formed on ends of cantilever, and having pendulum bearing to allow for expansion on abutments, and form one clear span of 540 feet on each side between pier and abutments—less width of bearings. These two shore spans of 420 feet each were made in Gateshead, and required a total of 1800 tons of steel, whilst the double central cantilever made at Motherwell required over 1400 tons of steel. (See Fig. 1, Plate X.)

The main difficulties experienced were in the very large and heavy steel plates required in the structure, and the enormous amount of drilling and smithwork. Some of the large gusset plates were over 10 feet high and 9 feet wide by 1¼ inch thick, weighing from 30 to 40 cwts., and some of the main tie plates made in half lengths were 26 feet long × 45 inches wide × 1¼ inch thick, weighing, when finished, over 42 cwts. each. The work throughout was drilled, and principally 1 inch holes, requiring about 10 miles of drilling and 5000 pieces of smithwork—many of rather an elaborate kind—in the work.

The following are some extracts from the specification as to the testing of the material.

The cantilever is to be of mild steel throughout, except such diaphragms and packings marked iron on drawings. All rivets throughout the work are to be of steel.

All materials are to be well and cleanly rolled to the full sections shown on the drawings or in the specification, and free from scales, blisters, laminations, cracked edges, and defects of every sort, and the name of the maker and the distinguishing mark or number of the plate or bar to be rolled or stamped on every piece. The steel and wrought-iron must be of such strength and quality as to be equal to the following tensional strains, and to indicate the following percentages of contraction of the tested area at the point of fracture, viz.:—

	Tensional strains per square inch.	Percentages of Contraction.
Steel in plates either with or across the grain, angle T channel or flat bars —not less than	27	30
or more than	31	
Steel rods for rivets, not less than	25	40
or more than	28	
Wrought-iron, round and square bars, and flat bars under 6 inches wide,	24	20
Wrought-iron, angle and T bars and flat bars 6 inches wide and upwards,	22	15
Wrought-iron plates,	21	8
Wrought-iron plates across the grain,	18	3

The percentages are to be taken from pieces so cut as to show the extension on 8 inches of length.

Strips of steel, whether cut lengthwise or crosswise of the plate, bar, angle bar, or T bar, heated to a low cherry red and cooled in water at a temperature of 82° Fahrenheit, must stand bending double round a curve of which the diameter is not more than three times the thickness of the piece tested. In addition to this, angle and flat bars must stand the tests known at Lloyd's as the ram's horn tests.

Every steel plate used in the work is to be tested for tensile strength, samples being taken from both end and side shearings, and at least one angle or flat bar from every charge of steel to be similarly tested.

To guard against the occasional acceptance of brittle or dangerous steel, the manufacturer is to preserve a side and an end shearing from every plate, and an end shearing from every flat bar, angle bar, and T bar, in order that it may be tested by bending cold in the presence of the company's engineer or his deputy. Every such shearing to bear a stamped number corresponding to the plate or bar from which it was taken, and this number is to be stamped by the contractor to the satisfaction of the company's engineer. It is to be understood that the company's engineer will insist on this inspection with regard to every item, and no piece of steel will be permitted to be used in the work until its corresponding marked shearings are produced and pronounced to be satisfactory by the company's engineer or his deputy.

All these tests are to be conducted in the works where the steel is manufactured, by some person appointed by the company's engineer. The steel used throughout the work is to comply on analysis with the following conditions—its carbon must not exceed ·3 per cent.; silicon, phosphorus, and sulphur must not be present in greater proportions than ·06 per cent. each; and the manganese must not exceed ·6 per cent.

It will be unnecessary to enter further into the general specification relating to drilling, riveting, planing all edges of plates, taking all risks of temporary erections, and so forth, although such a specification shows that bridge building of this class requires much skill and care, and is not the rough work that many imagine.

Considerable difficulty was experienced, and delay occasioned in getting delivery of the large 1¼ inch steel plates, and also in flattening and handling same, with usual girder work appliances.

In the compass of this paper it will be impracticable to enter into detail as to the various scantlings and separate pieces in this structure, there being in the shipping list 100 pieces over 3 tons weight each, and in all 1800 different items, incluging 243 boxes of steel rivets in the whole shipment, and any description must therefore be very general; but the drawings and diagrams submitted herewith will illustrate partly what is needful. The top and bottom booms are of box section, 4 feet 8 inches wide on flanges, with webs 3 feet 6 inches deep, having ¾ in. inner and 1 in. outer web (that is 1¾ in. thickness of web), with 6 inch × 6 inch × 1 inch angles top and bottom, all rivetted up with 1 inch diameter steel rivets 4 inch pitch, reeled in double line on angles. Those are connected by the large gusset plates, previously mentioned, with ties and struts as shown, the greatest stress or strain on booms being 1563 tons on bottom and 1575 tons on top boom, while strut over pier is subject to 574 tons compression, and tie connecting to it 624 tons tensile strain. Is will also be noticed that the strains are reverse from usual girder, as in this case the bottom boom is in compression, and the top boom in tension.

The main girders (see Fig. 1, Plate X.) are 362 feet 9 inches long over all, and 52 feet deep, and are built with 6 inches rise from pier to seat at end, on which the spans to bank of river rest. The width from centre to centre of main girder is 30 feet 6 inches, and the roadway is carried by heavy cross girders, 30 feet apart, with 4 lines of rail-bearing girders running between the cross girders and riveted thereto.

The whole floor area (see Fig. 3, Plate IX.) is covered by $\frac{5}{16}$ steel plates, which are 42 inches wide, and stiffened along centre of each by 5 inch × 2½ inch Z bars, full width of roadway.

From rail level to under side of lower horizontal cross bracing girders is 18 feet clear, and the additional 30 feet height of main girders is completely braced by upper cross bracing girders, and diagonal bracing girders between each, both vertically and horizontally. (See Figs. 2, 4, 5 and 6, Plates IX. and X.)

For the temporary erection of the cantilever, brick piers were built at 30 feet centres, with cross logs under each vertical member, and the whole was erected complete by 3 steam derrick cranes—one being up on staging so as to reach over the 52 feet high centre, and one of the other cranes at each end, mounted on bogies, travelling on usual railway gauge, so as to be movable and sweep over every part. Every item of the work had to be erected complete in position, each quarter of the bridge painted a distinctive colour, and each individual piece stencil and type marked for indentification, and to facilitate re-erection abroad.

It can well be understood that the handling and manipulating of so much heavy and special work, and the risk while temporarily erecting, was likely to give some concern to those engaged in it; and it is gratifying to note that this was satisfactorily completed, and the whole taken down, bundled, and despatched for shipment—during the month of January—without damage to limb or property, and has now reached its destination in India. It may be of interest to observe that it is intended to rivet up the spans on land, at site, and then float them out on pontoons into position, during the favourable season for this purpose—which is of limited duration there.

There are several practical points connected with bridge construction which might be of interest, such as iron *versus* steel; punching *versus* drilling; painting, and so on; but those are doubtless being so fully discussed elsewhere, as to render any reference here undesirable. The writer regrets that his time has been so fully occupied otherwise, as to preclude him from giving the subject justice, but it is hoped that the general description given may have been of sufficient interest, as to justify the time taken up with this paper.

In the after discussion,

Mr C. P. HOGG asked what was the weight of steel in the girders and cross-girders?

Mr R. Douglas, Kirkcaldy, asked if Mr Finlay could give any information as to how they were getting on now with the bridge over the Ganges at Benares, as he saw when there two years ago, that they had great difficulty in getting in the foundations, and it would be very interesting to have some information as to the progress of the heavy work.

Mr FINDLAY replied that he had no special information at present, but believed he would be able to give some particulars at next meeting.

Mr RILEY said he believed that extreme difficulty had been found in laying the foundations of the piers.

The discussion was then adjourned till next meeting.

The discussion of this paper was resumed on the 27th April, 1886.

Mr T. D. WEIR would have liked if the paper had contained some information as to the nature of the bearing on the piers. This point appeared to have been omitted from the paper. Mr Findlay described the shore end span as resting on the end of the cantilever span with a pendulum bearing, but there was nothing to show the nature of the connection between that span and the centre piers. He would like to know whether there was any arrangement to take up the expansion that might occur between the two piers, or whether the cantilever span was bolted down. There were several points regarding which it would have been interesting to have had particulars—notably that about the treatment of the large steel plates in the bridge. The author referred to difficulties having been met, but left them in the dark as to what they were, and how they were overcome.

Mr FINDLAY said he had treated the subject in a very general manner. He found that he had less time at his disposal than it required, and only brought forward the paper at the request of the Secretary. He was sorry that the members did not come forward with more papers, giving their views on many important matters that were not brought before the Institution. In regard to the weights of the structure it could be easily seen that a very large part of the weight must be in the main girders. The live and dead load of 420 feet span resting on ends of cantilever gave about 1000 tons' weight on each. Hence it came that the material in main girders weighed 540 tons each; that is, 1080 tons in the two. The floor had only the usual weights for double line of railway to carry, and the cross girders, 30 feet apart, with rail bearing girders between, would weigh 120 tons in all. The floor plates and two bars about 75 tons and the remaining 130 tons was in the horizontal an vertical cross bracings between the main girders. There was no special allowance for expansion or contraction on cantilever except three inches of space at each end, between the end of the 420 feet girders and the pendulum bearings of same on abutments. Mr Findlay described the structure by reference to the drawings, showing how the weight was distributed.

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

On a Peculiar Form of Corrosion in Steel and Iron Propeller Shafts of Steam Ships.

By Mr THOMAS DAVISON.

(SEE PLATE VIII.)

Received and Read 23rd March, 1886.

I THINK it may be useful to direct attention to the corrosion that sometimes takes place on propeller shafts of steam ships, with the object of ascertaining the best means of preventing it. It has long been observed that corrosion to a considerable extent occurs at the ends of the covering brasses of propeller shafts, forming a groove round the shaft, frequently so deep as to render the shaft unfit for use, but what I wish to direct particular attention to, is a different form of corrosion that takes place at the same part of the shaft, but instead of taking the form of a groove, it resembles a crack round the shaft extending towards the centre, to distinguish it I may call it "radial corrosion;" in some cases it may extend two inches inwards although not more than $\frac{1}{16}$ inch wide at the surface, it frequently extends completely round the shaft, but in some cases only partially so at irregular intervals; the corroded part has generally a radiated crystalline appearance quite different from any fracture produced by mechanical force, but the appearance before breaking the shaft is so like a crack produced by strain, that it may easily be mistaken for such, particularly when it extends all round, but when the shaft is broken the difference between this corrosion and the fracture produced by force is very marked. In some cases both forms of corrosion appear at the same part of the shaft. I have seen a groove $\frac{1}{10}$ inch deep and $\frac{1}{2}$ inch wide, produced by ordinary corrosion, and radial corrosion $\frac{1}{8}$ inch deep occurring at the same place.

This corrosive action takes place more rapidly in steel than in iron shafts, and it was this fact that called my attention particularly to it.

In consequence of the frequent failures of iron shafts, I have for some years advised the use of steel shafts, made from one ingot, so as to avoid the flaws and imperfections inseparable from iron made of many pieces welded together, and not having a single case of failure in the use of steel, in steel ships, steel boilers, steel shafting, piston rods, and other parts of engines. I was much disappointed to find this form of corrosion interfere with the use of steel in propeller shafts, but I hope soon to ascertain that even for that purpose, when properly protected, it is greatly superior to iron. I felt so certain of its suitability, that some years ago, I advised the removal of many iron propeller shafts to avoid the risk of breaking, and they were replaced by steel shafts, but the steel has corroded so rapidly in some cases that some of them have been replaced by iron shafts until we have time to test the means used to prevent this corrosion. The means that I have adopted so far is to keep the shafts absolutely dry at the parts where this corrosion takes place, and prevent the possibility of water reaching it. My reason for doing so is that I find shafts in every way the same—the one corroded and the other still remaining perfectly sound. I conclude that those that did not corrode were watertight, and the others not so. When propellers are being put on their shafts, the spaces between the propeller boss and the covering brass on the shaft, is usually filled with thick red lead paint, and as the boss is made to extend slightly over the brass covering of the shaft no water ought to reach the shaft, but it was not supposed to be so very important as to require special care, so that in some cases it may be perfectly watertight and in others not. One other fact bearing upon the subject is that no steel shaft inside the ship where no water reaches it has shown any appearance of this corrosion.

As some of my friends do not agree with me that these flaws are caused by corrosion, I will give my reasons for thinking so. First the shafts that have failed are all of the usual size of iron shafts (with one exception); the shafts were tested by Kirkaldy, and found to have an elastic limit and ultimate strength of over 40 per cent. more than iron, and in some cases greatly increased extension, and in every case were found to be sound, and when tested by Kirkaldy, after the corrosion, found not to have deteriorated. It seems impossible, therefore, that material so much stronger could be broken by strains that would not break iron. Secondly, if broken by strain, it seems impossible to cause a fracture that would not be continuous, or, in the least, resemble the specimen represented in Fig. 8 (Plate VIII.) In it the corrosion has taken place in four different parts of the circumference of the shaft, the intervening parts being perfectly sound, and one of these places the corrosion is ⅜ inch deep, and only ¼ inch wide; this could not be produced by any conceivable strain. This same shaft corroded at the forward end of the brass covering, and in one place two corrosions took place alongside each other, on lengthwise of the shaft. This could not be produced by strain. I have seen this double corrosion in other cases even more distinctly. Thirdly, the appearance and shape of the corroded surface is quite different from any known form of fracture produced by strain, the radiating corrugations of the corroded surface differs so much from the uniform surface produced, when fractured by strain. Fourthly, the shaft represented in Fig. 8 (Plate VIII.), corroded at both ends of the brass cover, any transverse strain produced by the propeller could not act with half of the intensity at the forward end of the brass cover, and as the tortional strain is the same at both ends, if broken by strain, the after end must give way first, whereas both ends have corroded nearly equally.

I will now describe the drawings and specimens exhibited. Fig. 1 (Plate VIII.) is a general plan of a steel shaft with propeller and stern frame of ship, showing where radial corrosion took place at A. Fig. 2* (Plate VIII.) is a similar plan of an iron shaft with propeller,

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* This specimen was exhibited by the President and described in his introductory address on 27th October last.

&c., showing where radial corrosion took place at B. Fig. 3 (Plate VIII.) is a full-sized drawing of specimen No. 3, which was cut from the end of a steel shaft that was found corroded radially between the forward end of the propeller boss, and the brass covering of shaft, after being in use for about two years; the shaft was 11 inches diameter, cylinders 38 and 60 × 42, with 60 lbs. steam. When new the shaft was tested, by Kirkaldy, with the following result—Elastic limit, 34·667 lbs. per square inch; ultimate, 72·680; elongation in 10 inches length, 1⅛ inch diameter, 19·7 per cent. A portion of the corroded shaft was also tested, by Kirkaldy, with the following result—Elastic limit, 33·450; ultimate, 72·285; extension in 10 inches, 1⅛ inch diameter, 19 per cent. I placed this corroded shaft in a hydraulic press as represented by Fig. 4 (Plate VIII.), supported its ends 5 feet 7 inches apart, and brought the strain on the corroded part at A until it broke. I found the elastic limit to be 40 tons, and it required 101 tons to break it. I then placed the broken part in the hydraulic press as represented by Fig. 5 (Plate VIII.), and bent it as represented by Fig. 6 (Plate VIII.) It required 453 tons to bend it 9⅛ inches, and as this was the limit of the power of the press, I could not bend it further. No flaw of any kind was visible when bent. I may add that a sister ship, built at the same time, with shaft made of the same quality of steel, the shaft is still perfectly sound. This I attribute to the one being watertight, and the other not so.

Specimen No. 7 represented by Fig. 7 (Plate VIII.) The steel shaft from which this piece was cut had been running for about four-and-a-half years when it was found to be radially corroded in the same place, between the propeller boss and the brass covering of the shaft. It was 9 inches diameter; cylinders, 30 and 52 × 36; steam, 60 lbs. It was broken by dropping an iron ball upon it, and specimen No. 7 was cut from the broken end. A piece of this broken shaft was tested, by Kirkaldy, with the following results—Elastic limit, 32·100 lbs. per square inch area; ultimate, 72·995; extension in 10 inches, 1⅛ inch diameter, 23·8 per cent.

Specimen No. 8 represented by Fig. 8 (Plate VIII.) This steel shaft (Fig. 8 A, Plate VIII.) after running about five years was found to be radially corroded in the same place as the others. At after end of brass cover it was 6⅝ inches diameter; cylinders, 23 and 42 x 27, with steam of 60 lbs. Corrosion of the same radial description had also taken place at the forward end of the brass cover, as seen in specimen 9 and Fig. 9 (Plate VIII.) This shaft was broken by dropping an iron ball upon it. This specimen is a good illustration of the irregularity of the radial corrosion, as it extends at one place ⅝ inch inwards, and is only ¼ inch wide at the surface, as seen at B. The corrosion also takes place intermittently round the shaft. The forward end also exhibits the double radial corrosion that I referred to. A short piece of this shaft was tested, by Kirkaldy, before it was used with the following results—Elastic limit, 33·250 lbs. per square inch; ultimate, 69·650 lbs; extension in pieces, 2½ inches long, ·87 diameter, 31 per cent.

Specimen No. 10, represented by Fig. 10 (Plate VIII.), is from a steel shaft 9¼ inches diameter; cylinders, 30 and 52 x 36; steam, 60 lbs. The fracture is somewhat different from any of the others, being less corrugated. This shaft broke at sea, having been only nine months in use. The steel is the softest of any that I have used; tested after it broke the ultimate strain was 57·700 lbs. per square inch; extension in 10 inches, 1⅛ diameter, 29 per cent. I have known an iron shaft break in as short a time as this, but did not note its fracture.

Specimen No. 11, represented full size by Fig. 11, Plate VIII., is part of an *iron* shaft, 11½ inches diameter, that was found to be radially corroded in the same place as the steel shafts, after being in use about four years; cylinders, 40 and 70 x 42; steam, 60 lbs. I broke it by dropping an iron ball upon it, and the broken part was tested, by Kirkaldy, with the following results — Elastic limit, 24·600 lbs. per square inch; ultimate, 46·405; extension in 10-inch length, 1⅛ inch diameter, 18 per cent. This shaft was broken 10 years ago. I then attributed the fracture to want of strength, but I think, comparing it now with the broken steel shafts, there can be no doubt but that it failed from exactly the same cause, and

I have no doubt many iron shafts have suffered in the same way without the cause being observed; the appearance so exactly resembles a crack that unless the shaft be broken any one may be excused for not detecting the cause.

Specimens Nos. 3 and 10 are from shafts made of Siemen's steel by the Parkhead Iron Works. Specimens No. 7, 8, and 9 were made of fluid compressed steel by Messrs Whitworth; and specimen No. 11 was made of iron by the Lancefield Forge Co.

I think it would be well for engineers to pay special attention to this subject, so as to find a perfect remedy for what seems to be the only objection to the use of steel for propeller shafts. I do not think the quality of steel will make much difference, as I have used steel of various qualities, both hard and soft, and in some cases both have corroded, and in others both have stood. If there be any indication in this direction it is that the softest steel is more liable to corrosion than the harder, but I think we must find protection in some other direction.

In the after discussion,

Mr G. W. **Manuel** thanked Mr Davison for bringing this subject before them; for the *breaking* of a shaft might be the *loss* of a ship, and if that could be averted in any way, by finding out the real cause and using means to prevent it, he thought a great benefit would be conferred. He had a great deal of experience in regard to the particular corrosion shown by Mr Davison; and he thought that Mr Davison was correct in distinguishing it from what was termed a *fracture.* He quite agreed with Mr Davison that a fracture was caused by *weakness* in a shaft, but the defects shown in diagrams and samples of shafts were due to a cause he had observed and would try to explain. He did not know whether the shafts shown on the diagram were constructed like those he had observed when the brass casing was carried down with a square corner, and the result of which was a particular kind of cutting corrosion, such as Mr Davison explained, and which he illustrated (see Fig. 12, Plate VIII.) On looking at the diagram he found the brasses to have a square corner with the shaft, and the results of this he would endeavour to explain. As the shaft revolves, the *force* of the sea or salt water pressure varying according to depth, say 5 lbs. per square inch, is, by the angular corner of brass sheathing when cut square as shown, concentrated in a *line circumferentially* close to the brass sheathing, and when the usual paint put on for protection is worn off, and the *iron* or *steel* shaft laid bare, an action of what may be termed as water-sawing is set up, combined and fed by the corrosive action due to the presence of iron or steel, brass, and salt water, the result being an incision circumferentially at right angles to shaft, varying very slightly from the straight line, caused by inequalities of material or the intervention of a key way, &c., and in depth according to similar circumstances—though he thinks in a more severe extent in steel than in iron shafts, the former requiring more protection—and eventually the shaft is weakened, causing a *complete* break or *fracture* of the shaft by reduced section, and shown by the granular appearance. In order to remedy this without any special protection, he had the corner of brass altered, doing away with the sharp corner, rounding it over, as shown in Fig. 13, Plate VIII.; and when this was done in all cases found the result beneficial. He did not know of any case that his company had lately of this nature or a broken shaft as experienced by Mr Davison; and he always instructed the engineer to curve the brass in the manner described to prevent this cutting and corrosion. At the outer end forward of screw boss there is a further means of preventing this action, and that is by fitting an angle-iron clamp round the shaft close to screw boss This angle-iron clamp was bored out to fit the shaft correctly, and faced next the boss, and when screwed up was kept tight against the boss. (Fig. 14, Plate VIII.) The shaft and it were painted before it was put on, and the spaces filled up with cement, which covered the shaft and protected it thoroughly from the corrosive action referred to. The action on the shaft in the stern tube was similar at ends of sheathing, and was similarly treated. He was indebted to Mr M'Laine, superintendent engineer, Belfast, for another expedient to protect the portion of shaft inside the stern tube in addition to rounding the corner of brass as shown. The shaft between brass sheathings was first tarred and over it was wound a serving of tarred canvas, which was held in place by fine tarred marline, and this formed so effectual a protection that no water could reach the shaft while tight. Particular care was taken to have this parcelling well done, so that the surface of the shaft and the end of the brass sheathings were entirely covered. From time to time the shaft was examined and wherever the covering was found disturbed or worn it was renewed; and by this means the corrosion complained of at this part was entirely averted. This latter plan more especially refers to screw shafts that are removed for examination every two or three years. He considered the former plan, however, practically efficient where the shaft may be in the tube five to six years.

The discussion was then adjourned till next meeting.

The discussion of this paper was resumed on the 27th April, 1886.

Mr F. W. Dick remarked that as a steel maker he had almost felt it a necessity to find out something that would enable him to speak upon the fractures mentioned in the paper. The fractures themselves did not appear so very extraordinary to him as the reason given for their recurrence. He was of opinion that if they were caused by corrosion it would be a very extraordinary thing. It seemed to him that they were caused simply by fatigue. They saw similar effects occur to metal after long-continued strains, and with that idea in his mind he had asked Mr Swaine, the engineer to the Steel Company of Scotland, to make some experiments, which seemed to corroborate his opinion. He had taken bars of steel, 2 feet long, and ¾ inch, 1 inch, and 2 inches in diameter, centred them, and put them into a lathe. They were notched in the middle, and then deflected by pressure. Under ordinary circumstances the strain put upon the bar would, he thought, have been borne by it for ever, but when the bar was rotated rapidly in the lathe, it broke in from two to four minutes and upwards, and the fracture produced was such a good reproduction of those of the propeller shafts exhibited that he thought it went far to explain the cause of those fractures. There was an outer and an inner line showing radial marks which were fully one-sixteenth of an inch wide, such as those depicted on the diagrams. The interior of the fracture was prettily crystalline, as was the case in all sudden fractures of steel. He thought this explained the cause of the fractures brought under notice by Mr Davison.

Mr HENRY DYER said he had to confess, along with Mr Dick, that he had a difficulty in accepting the explanation which Mr Davison gave of the phenomenon described in his paper. That explanation is sufficient to account for the ordinary corrosion with which every marine engineer was well acquainted, but so far as he understood, the chief object of the paper was to explain a new phenomenon, which Mr Davison called radial corrosion, and which only occurred in steel, which is a new material for propeller shafts. He doubted if this deserved the name of corrosion, for although a certain amount of corrosion will take place after fracture has been produced by other causes, that is merely a secondary action. Mr Manuel said that fracture was caused by weakness in a shaft. That statement may seem self-evident, but as a matter of fact it is not always true. A shaft may be strong enough when first used, but if it is deficient in stiffness, the continual bending and unbending would ultimately cause fracture. The shaft mentioned by Mr Davison would be subject to this bending action on account both of the weight and re-action of the propeller, and Mr Kirkaldy's experiments would prove nothing, as the effect would be confined to a very small part of the shaft. He had heard of some experiments recently which showed that some kinds of steel are not so stiff as malleable-iron, and he thought it would be found that this want of stiffnesss is the cause of what Mr Davison calls radial corrosion. He mentions that both forms of corrosion appear at the same part of the shaft. This can be easily explained, for in a shaft which was originally stiff enough, after its diameter had been reduced it would of course be deficient in stiffness, and fracture would ensue in course of time. This of course also explains why the so-called radial corrosion did not appear when the salt water was excluded, for the ordinary corrosion being prevented, the shaft had sufficient stiffness to resist the bending action. The irregularities in the action can only be explained by want of uniformity in the material of the shaft, or by special conditions connected with its design.

Mr **William Laing** would like to know whether Mr Davison could inform them as to the extent of the wear down in stern bush, as if this was excessive with a shaft weakened by corrosion the natural result would be a fracture ultimately at the ring, as shown on the diagram. He might mention that he had two vessels fitted with steel shafting and, he was glad to say, that after three years' running they did not show any sign of the disease referred to.

Mr **Hector MacColl** regretted that more of the gentlemen present, well qualified to speak on the subject, did not seem inclined to express their opinions. They were much obliged to Mr Davison for reading such a paper, for although the subject was one which many of them knew something of, yet they appeared to get so accustomed to these defects as to consider them unavoidable, and so the reading, or even discussion, of such papers did not occur to them. Views had been expressed that night which he felt bound to object to. The corrosion of propeller shafts at the ends of the brass liners was very well known to those who had to deal with screw steamers, and he did not think it could be satisfactorily accounted for on the theory of the two gentlemen who had last spoken, for it was found that if they carefully excluded salt water from contact with the brass and shaft, there would be no corrosion. If the propeller boss were fitted watertight against the end of the brass there would be no corrosion there, and if the inner ends of the brass liners were also made watertight, there would be no corrosion at these points; but as sure as water was allowed to come in contact with the shaft and brass, so surely would corrosion follow, and in due course fracture of the shaft. He had had little experience with steel shafts, and if the diagrams accurately described the corrosion, then probably some of the peculiarities arose from differences in the nature of steel as compared with iron. At all events, the corrosive marks were very different from the many cases he had observed in iron shafts. Those cases showed that where the end of the brass was left square, and in contact with salt water, the shaft was corroded under the brass as well as outside of it, forming a V, then from the root of the V, corrosion ran towards the centre of the shaft in what he understood Mr Davison to call "radial" corrosion, the complete corrosion running round the shaft and extending from the surface inwards somewhat in the form of the letter Y. In some such cases, where the shaft was much in excess of its requirements, they had tried to turn out the corroded groove, and had always found great difficulty in getting to the bottom of it. It was also always very irregular, and almost always more or less eccentric—deeper on one side of the shaft and shallower on the opposite side. He had no doubt in his own mind that this corrosion was caused by the brass and salt water acting on the iron or steel shaft. The action appeared to be galvanic, although he was not chemist enough to say positively that it was so. He certainly did not think it was caused by what Mr Manuel termed the "water saw" action. He thought the following example would go to show that. In many cases, unfortunately hailing principally from this district, the after liner had been made in two lengths butted together in the middle. Now it would be difficult to imagine that the "water saw" could act in such a case, and yet corrosion was more rapid the finer and closer the butt joint. He had seen a shaft destroyed in 15 months, the butt joint in the brass liner being so fine as to escape detection by an unpractised eye. But if any further proof of the incorrectness of the "water saw" theory were required, he thought it would be found in cases where the liner, instead of being butted, was half checked at the joint; such a mode of construction would not permit of "water sawing," and yet as sure as such a joint was leaky, so surely would corrosion be found underneath. There were several methods of preventing this corrosive action, or of reducing its amount. As regards corrosion at the end of liner next the propeller, one plan is to make the propeller a good fit, as it always should be, on the shaft, recess the boss at the forward end so that it embraces the after end of liner, fill the recess with moderately stiff red lead putty and force the propeller into its place, squeezing the superfluous putty out before it. Another plan is to proceed as in the last case, but putting a soft rubber ring on the shaft to be squeezed watertight into the recess in propeller boss; this is probably the most effective method of making the joint watertight, although some object to the corrosive powers which some qualities of rubber possess. Another and more mechanical plan is to face up the fore end of propeller boss and bolt watertight to it an ⌊ shaped collar made in two halves, and well fitted on the brass liner. Then the other ends of the liners require protection, viz., the after end of forward, or stuffing box liner, and the forward end of after liner. If these are bevelled off so that the thickness at the shaft is about one-eighth of an inch, with a backward angle of about one and a half to one, corrosion will be much reduced. Another plan is to cover the shaft all over with brass, but this he thought was a remedy infinitely worse than the disease, for it was difficult to cover a shaft so that there should be no joints in the covering, and however well the joints might be made, there was a risk of their being faulty. When a shaft was drawn in for examination and sounded hollow at the liner joints, doubts were raised in the minds of those concerned, and these doubts could only be allayed or dispelled by stripping off the intermediate covering. In a case which had come under his notice some time since, a large shaft was drawn for examination. It was covered from end to end, with several joints, most of which sounded hollow under the hammer. The spare shaft was fitted, and when, later on, the original shaft was stripped, it was found to be perfectly sound and free from corrosion, the joints being all half checked, and the hollow sound probably arising from some warping of the lighter parts of the liners. Thus a shaft might at great expense be covered all over, and yet give rise to such doubts as to render the covering of no use. Mr Manuel credited Mr MacLaine with another method of protecting the central part of the shaft. Of it he might say that it had been tried long ago in Liverpool, and had been given up, as it was found difficult, if not impossible, to keep the "parcelling" on the shaft.

Indeed it was said, but he could not say with what truth, that in one case, the parcelling had become unwound at sea and had been swelled out by the revolution of the shaft so as finally to burst the stern tube. This, therefore, did not appear a very reliable method of treating the shaft. There was another plan, which he spoke of with great diffidence, because it had been patented by himself, and because he was at present unable to speak of it from actual experience. This plan consisted in following up the first liner, after it was shrunk on the shaft, by a wrought-iron ring, also shrunk on the shaft. Next followed another iron ring at the forward end of after liner, then the liner itself, followed up by another ring at its after end. When the shaft was put into the lathe to finish turning the liners, dovetail grooves were cut between each ring and the liner; and these grooves were filled up with tin, melted and run in and made perfectly watertight. This plan was intended to cut off the contact with brass and wrought-iron in salt water which had been shown to cause this corrosion. It had been applied to many shafts, but as engineers, he was sorry to say, did not draw their shafts so frequently as they ought, he was only able to say that in the one or two cases he had examined, the plan was a success. But even if it had not been so far successful, he was confident that it could be made a complete preventative of this corrosion.

Mr Dyer wished to be allowed to say that, however valuable the remarks which had just been made might be, as a record of experience with the ordinary corrosion of malleable iron shafts, they had no bearing on the subject under discussion, which was supposed to be a new phenomenon, observed in a new material.

Mr H. R. Robson said he had had little experience in steel propeller shafts, but he had had considerable experience with iron shafts, and had seen much corrosion even before the days of covering them with brass. More than thirty years ago he had drawn a propeller shaft which had no brass casing, and found it was much deteriorated by corrosion. In several large ships he had tried the effect of putting the brass to the boss of the propeller to keep the water out. He found the boss got slack, and corrosion went on, it very soon became leaky, and then they had the same action as they had before when the brass simply went up to the boss. He then commenced keeping the brass from a quarter to half an inch from the boss, and had very hard tarred rope put in, and thus the water being excluded no corrosion took place. Now this scarcely agreed with Mr Dyer's theory of bending. He thought that it was more a water action than bending, for as soon as the shaft was made watertight the corrosion stopped. The corrosion generally commenced at the key place and then went on round the shaft. That was the way with iron shafts, and, therefore, he did not think it was from the bending of the shaft, as the exclusion of the water prevented the corrosion. They had tried indiarubber to exclude the water, but he preferred the hard rope, well-tarred, pulled tight, and well-hammered in, as it gave no further trouble. He agreed with Mr MacColl, with regard to to the lining of the shaft throughout, that there was great danger in it. He had seen a shaft so covered, which looked perfectly good, but when drawn was found to be more than half corroded through. He advocated the use of brass only where the bearing of the shaft was.

Mr SANDERSON said he had read Mr Davison's paper very attentively, and also listened to the speakers that evening, and he thought that Mr MacColl had expressed the opinion generally held by engineers regarding corrosion of propeller shafts. If the discussion were continued to another evening he had no doubt some fresh light would be shed upon this interesting subject.

Mr SYME said with reference to the lining of the shafts, it was his practice to, what brassfounders called, "burn" the joints, and thus make the lining perfectly solid from end to end. He believed this was the only way to keep the water out and to effectually prevent corrosion taking place under the brass lining. He was not aware of a single instance of failure since this plan was adopted at Fairfield Works some years ago.

Mr DAVISON, in reply to the remarks of the speakers, said he had added another drawing (see Plate VIII.) since the previous meeting. It represented a piece of the shaft between the two brass casings, which he pointed out on the diagram. After the shaft had run about three years, there were several cracks found upon it, at A and at B, one of which was one-sixteenth inch wide, and one inch deep, and extending five or six inches round the shaft. It seemed to him that such a crack as that could not be caused by strain, as it was only subjected to torsional strain. This shaft was not corroded close to the propeller—no doubt due to the propeller being well fitted and watertight. A sister shaft had worked as long without any crack. He did not think that the shaft in question had been strained beyond its elastic limit. All experience pointed to the fact that cracks of this nature were due, not to strain but to corrosion; and if the water could be kept out by the shaft being properly covered there would be an end to this corrosion.

On the motion of the PRESIDENT a vote of thanks was passed to Mr Davison for his paper.

Dunnachie's Continuous Regenerative Gas Kiln: A Supplementary Note.

By Mr JOHN MAYER, F.C.S.

Received 16th April, 1886.

TOWARDS the end of last session (24th March, 1885), I had the pleasure of communicating to the Institution a paper upon the regenerative gas kiln, devised by Mr James Dunnachie, for working upon the continuous principle, and intended for the burning of fire-bricks, pottery, &c.; and in the course of the discussion to which it gave rise, I promised to return to the subject, so as to supply additional information on one or two points which seemed to require a little clearing up. The points on which I now propose to give the further information desired were chiefly suggestions by Mr Dyer.

Mr Dunnachie informs me that he has made some tests as to the temperature of the waste gases at the chimney connected with the gas kiln at the "Star" Works, Glenboig—a Siemens pyrometer being used for the purpose. With the kiln on "full-fire," and one chamber filled with "green" bricks, between the burning chamber or kiln and the chimney, the following results were observed:—

Time of observation. End of—	Temperature at chimney.
4 hours,	150 degs. Fahr.
12 ,,	200 ,,
24 ,,	250 ,,
36 ,,	300 ,,

Then, with two "green" kilns or chambers between the "full-fire" chamber and the chimney, the temperature of the effluent or waste gases materially decreased, as indicated by the following figures:—

At end of—	Temperature at chimney.
4 hours,	90 degs. Fahr.
12 „	120 „
24 „	150 „
36 „	180 „

The small copper ball used as part of the Siemens apparatus was kept in the current of the effluent gases one hour each time. In addition to these data, I may mention that the copper ball was, in like manner, exposed to the heat of the gases escaping from one of the common coal-fired kilns when on "full-fire," and that the temperature observed was at least 2000° Fahr. The ball was only exposed for a period of 3½ minutes, there being a fear that the wire by which it was suspended might have melted if the exposure had been continued any longer. In each of the other determinations, the copper ball was exposed to the heat of the waste gases for one hour before being put into the vessel containing the water, whose rise of temperature was to be observed, in accordance with the principles of the Siemens system of pyrometry.

The heat determinations given in the foregoing tables afford abundant evidence in favour of the statements made in my paper as to the fuel-economy attending the use of the regenerative gas kiln when compared with the results got in firing bricks in the common kiln, even of the excellent type used at Glenboig.

By way of showing that the Dunnachie gas kiln is doing good and successful work at other places besides the Glenboig Company's own establishments, I may mention one or two facts. The proprietors of the Tamworth Carbonising Works state that the kiln is doing exceedingly well, and that it has quite answered their expectations. In a recent communication which I got from Mr Lowood, of Sheffield, that gentleman states that he considers the kiln in question to be admirably adapted for burning bricks, composed chiefly of fire-clay, such as the Glenboig, Stourbridge, Newcastle, &c.; and he contemplates using it himself for burning bricks of a similar class to the Glenboig or Stourbridge.

Mr **James M. Gale** proposed a vote of thanks to Dr Thomson on the termination of his period of office as President. He had devoted a great deal of time and attention to the business of the Institution, and had conducted the meetings in a most careful and able manner, and therefore he had great pleasure in proposing that they now give him a very hearty vote of thanks for his services during the past two years.

The motion was cordially and unanimously adopted.

Institution of Engineers and Shipbuilders
IN SCOTLAND

(INCORPORATED).

TWENTY-NINTH SESSION, 1885-86.

MINUTES OF PROCEEDINGS.

THE FIRST GENERAL MEETING of the TWENTY-NINTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 27th October, 1885, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of Annual General Meeting of 28th April, 1885, was read and approved, and signed by the President.

The presentation of Awards for papers read during Session 1883-4 took place, viz :—

The Institution Medal—to Mr RALPH MOORE, C.E., for his paper on "Cable Tramways."

The Marine Engineering Medal—to Mr JOHN HARVARD BILES, for his paper on "The Stability of Ships at Launching."

A Premium of Books—to Mr ROBERT L. WEIGHTON, M.A., for his paper on "The Compound Engine Viewed in its Economical Aspect."

A Premium of Books—to Messrs PURVIS and KINDERMANN, for their paper on "Approximations to Curves of Stability from Data for Known Ships."

The **President** delivered his Introductory Address.

On the motion of Mr J. M. **Gale**, a hearty vote of thanks was accorded the President for his address.

The Discussion of Mr **Staveley Taylor**'s Paper on "The Butt Fastenings of Iron Vessels" (read 24th March, 1885), was continued and terminated.

The discussion of Mr **Alexander Findlay**'s Paper on "American Railway Freight Cars" (held as read at Annual General Meeting of 28th April, 1885), was deferred on account of the inability of the Author, through illness, to be present.

The discussion of Mr **Andrew S. Biggart**'s Paper on "Sinking the Cylinders by Pontoons for the Tay Bridge" (held as read at Annual General Meeting of 28th April, 1885), was terminated, and, on the motion of the President, a vote of thanks was awarded Mr **Biggart** for his Paper.

A Paper on "The Great Caissons of the Forth Bridge," by Mr **Andrew S. Biggart**, C.E., was read, the discussion of which was postponed till next General Meeting. On the motion of the President a vote of thanks was awarded Mr **Biggart** for his Paper.

The President announced that the Candidates balloted for had been unanimously elected, the names of these gentlemen being as follows:—

AS A MEMBER:—

Mr **John Lyall**, Mechanical Engineer, 6 Ibrox Place, Glasgow.

AS GRADUATES:—

Mr **Wm. C. Borrowman**, Draughtsman, 15 Breadalbane Street, Glasgow.

Mr **Francis Coutts**, Draughtsman, 96 Shamrock Street, Glasgow.

Mr **Henry George Gannaway**, Engineering Draughtsman, 17 Caroline Street, Jarrow-on-Tyne.

Mr **John M. Malloch**, Engineering Draughtsman, Carron Iron Works, Falkirk.

Mr **Alexander Scobie**, Apprentice Engineer, Culdees, Partickhill, Glasgow.

Mr **Peter Tod**, Apprentice Draughtsman, 509 Sauchiehall Street, Glasgow.

Mr **Archibald Blair**, Ship Draughtsman, 12 Arthur St, Glasgow.

THE SECOND GENERAL MEETING of the TWENTY-NINTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 24th November, 1885, at 8 P.M.

Professor **James Thomson**, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 27th October, 1885, was read and approved, and signed by the President.

The discussion of Mr **Andrew S. Biggart's** Paper on "The Great Caissons of the Forth Bridge" was proceeded with and terminated, a vote of thanks being awarded to Mr Biggart.

A Paper on "The Present State of the Theory of the Steam Engine and Some of its Bearings on Current Marine Engineering Practice," by Mr **Henry Dyer**, C.E., M.A., was read. A discussion followed and was continued to next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

AS A LIFE MEMBER:—

Mr **Frank E. Kirby**, Engineer, Detroit, United States.

AS MEMBERS:—

Mr **Hartvig Burmeister**, Copenhagen.

Mr **William S. Beck**, 246 Bath Street, Glasgow.

Mr **Andrew M'N. Brown**, Castlehill, Renfrew.

Mr **James Conner**, Isle of Wight Railway, Sandown.

Mr **Archibald Hamilton**, New Dock Works, Govan.
Mr **Guybon Hutson**, Kelvinhaugh Engine Works, Glasgow.
Mr **Andrew M'Lean**, Jun., Viewfield House, Partick.
Mr **George M'Farlane**, 65 Great Clyde Street, Glasgow.

AS A GRADUATE :—

Mr **James Welsh**, Apprentice Engineer, 51 St. Vincent Crescent, Glasgow.

The Third General Meeting of the Twenty-Ninth Session of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 22nd December, 1885, at 8 p.m.

Mr T. **Arthur Arrol**, Vice-President, in the Chair.

The Minute of General Meeting of 24th November, 1885, was read and approved, and signed by the Chairman.

The discussion of Mr **Henry Dyer's** Paper on "The Present State and Theory of the Steam Engine and some of its Bearings on Current Marine Engineering Practice" was resumed, and with the view of eliciting a full discussion of the subject was continued to next General Meeting.

A Paper on "Arthur's Patent Bevelling Machine," by Mr **Richard Ramage**, was read. A discussion followed and was continued to next General Meeting.

A Paper on "The Proper Use of Animal Power as Applied to Tram-Cars, Short Inclines, &c.," by Mr **Laurence Hill**, C.E., was also read. A discussion followed and was continued to next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows :—

AS A MEMBER :—

Mr **ROBERT MACLAREN**, Jun., Eglinton Foundry, Glasgow.

AS GRADUATES :—

Mr **JOHN HENRY ALEXANDER**, Draughtsman, 42 Sardinia Terrace, Glasgow.

Mr **PETER MACLEOD BAXTER**, Apprentice Engineer, 8 Mansfield Place, Glasgow.

Mr **HUGH BROWN**, Draughtsman, Holmfauldhead, Govan.

Mr **BENJAMIN CONNER**, Apprentice Engineer, 9 Scott St., Glasgow.

Mr **WM. B. MORRISON**, Apprentice Draughtsman, 340 Dumbarton Road, Glasgow.

Mr **CHARLES ANDREW FELIX RUBIE**, Student of Naval Architecture, 10 Glasgow Street, Hillhead.

THE FOURTH GENERAL MEETING of the TWENTY-NINTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 26th January, 1886, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 22nd December, 1885, was read and approved, and signed by the President.

The discussion of Mr HENRY DYER'S Paper on "The Present State of the Theory of the Steam Engine and Some of its Bearings on Current Marine Engineering Practice" was resumed and terminated.

The discussion of Mr RICHARD RAMAGE'S Paper on "Arthur's Patent Bevelling Machine" was resumed and terminated.

The discussion of Mr LAURENCE HILL'S Paper on "The Proper

Use of Animal Power as applied to Tramcars, Short Inclines, &c.," was also resumed and terminated.

Votes of thanks were passed to the Authors of these Papers.

Mr **Robertson** explained his Anti-friction Stuffing Box, and it was agreed that Mr **Robertson** should have an opportunity at next General Meeting of exhibiting his model of the apparatus.

On account of the lateness of the hour, Mr **Millar's** Paper on "Some Properties of Cast-Iron and Other Metals," was deferred till next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows :—

AS MEMBERS :—

Mr **Chas. Armstrong**, Surveyor and Architect, Albert St., Carlisle.
Mr **Andrew Campbell**, Master Shipwright, 53 Crookston Street.
Mr **Thomas M'Gregor**, Mechanical Engineer, 10 Mosesfield Terrace, Springburn.
Mr **Alexander Mitchell**, Mechanical Engineer, 4 Bellevue Terrace, Springburn.
Mr **James Murray**, Mechanical Engineer, 8 Brown Street.
Mr **George Oldfield**, Mechanical Engineer, Greenlea, Johnstone.
Mr **John Ward**, Shipbuilder, Leven Shipyard, Dumbarton.

AS AN ASSOCIATE :—

Capt. **Duncan M'Pherson**, Marine Superintendent, 142 Pollok St.

AS GRADUATES :—

Mr **James G. Garraway**, Apprentice Engineer, 13 Govan Road.
Mr **John King**, Apprentice Ship Draughtsman, 8 Hamilton Street, Partick.
Mr **John Lee**, Engineering Student, 8 The College, Glasgow.
Mr **Thomas Nicholson**, Engineering Draughtsman, 6 Annfield Place.
Mr **Samuel Paxton**, Apprentice Engineer, 4 Lorne Terrace, Pollokshields.

THE FIFTH GENERAL MEETING of the TWENTY-NINTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 23rd February, 1886, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 26th January, 1886, as read and approved, and signed by the President.

Mr ROBERTSON illustrated the action of his Anti-friction Stuffing Box by means of a model.

The following Papers were read :—

On "Some Properties of Cast-Iron and Other Metals," by Mr W. J. MILLAR, C.E. A discussion followed and was continued to next General Meeting.

On "Hydraulic Plant for Bessemer and Basic Steel Works," by Mr FINLAY FINLAYSON. The discussion of this Paper was deferred till next General Meeting.

The President announced that the Candidate balloted for had been elected, viz. :—

AS A MEMBER :—

Mr GEORGE STANBURY, Lloyds' Surveyor, 16 Hamilton Terrace (West), Partick.

THE SIXTH GENERAL MEETING of the TWENTY-NINTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 23rd March, 1886, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 23rd February, 1886, was read and approved, and signed by the President.

The discussion of Mr W. J. MILLAR'S Paper on "Some Properties of Cast-Iron and Other Metals" was resumed and continued to next General Meeting.

The discussion of Mr FINLAY FINLAYSON'S Paper on "Hydraulic Plant for Bessemer and Basic Steel Works" was proceeded with and terminated, a vote of thanks being awarded Mr FINLAYSON for his Paper.

The following Papers were read :—

On "Bridge Construction, with Special Reference to Cantilever Span for a Bridge over Hooghly River," by Mr ALEX. FINDLAY.

On "A Peculiar Form of Corrosion in Steel and Iron Propeller Shafts of Steam Ships," by Mr THOMAS DAVISON.

Discussions followed and were continued to next General Meeting.

As time did not permit of the reading of Mr MAYER'S supplementary note to his paper of last session on "A Continuous Regenerative Gas Kiln for Burning Fire Bricks, &c.," it was agreed that it should be held as read and printed with the Transactions of the Meeting.

Mr ANDREW MACLEAN and Mr DAVID KINGHORN were unanimously chosen to audit the Treasurer's Annual Financial Accounts.

The President announced that the Candidates balloted for had been elected, viz.:—

AS MEMBERS :—

Mr GEORGE BROWN, Naval Architect, Dumbarton.
Mr GEORGE LISLE HINDMARSH, Lloyd's Surveyor, Glasgow.
Mr JOHN F. RANKIN, Mechanical Engineer, Eagle Foundry, Greenock.
Mr JAMES THOMSON, Jun., Naval Architect, Leven Shipyard, Dumbarton.

AS GRADUATES :—

Mr THOS. DANKS, Assistant Civil Engineer, Burgh Chambers, Govan.
Mr THOMAS R. SEATH, Junior Draughtsman, Langbank.
Mr WILLIAM Y. SEATH, Apprentice Engineer, Langbank.

THE TWENTY-NINTH ANNUAL GENERAL MEETING of the INSTITUTION was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 27th April, 1886, at 8 P.M.

Professor JAMES THOMSON, C.E., LL.D., &c., President, in the Chair.

The Minute of General Meeting of 23rd March, 1886, was read and approved and signed by the President.

The Treasurer's Annual Financial Statement, duly audited, was submitted and unanimously adopted.

The Railway Engineering Medal was unanimously awarded to Mr ANDREW S. BIGGART, C.E., for his Papers on "Sinking the Cylinders by Pontoons for the Tay Bridge," and on "The Great Caissons of the Forth Bridge."

The proposal by the Council that the Past Presidents of the Institution should be *ex officio* Honorary Members of Council, was, on the motion of the President, unanimously adopted.

The Election of Office-Bearers then took place :—

Mr WILLIAM DENNY being unanimously elected President.

Mr ROBERT DUNDAS was unanimously elected a Vice-President. And, by a majority of votes, the following gentlemen were elected Councillors: Messrs A. C. KIRK, G. L. WATSON, ALEXANDER SIMPSON, W. RENNY WATSON, and WILLIAM FOULIS.

Mr JOHN THOMSON was unanimously re-elected Representative from the Institution to the College of Science and Arts.

The discussion of the following Papers was resumed and terminated, viz. :—

On "Some Properties of Cast-Iron and Other Metals," by Mr W. J. MILLAR, C.E.

On "Bridge Construction, with Special Reference to Cantilever Span for a Bridge over Hooghly River," by Mr ALEX. FINDLAY.

On "A Peculiar Form of Corrosion in Steel and Iron Propeller Shafts of Steam Ships," by Mr THOMAS DAVISON.

Votes of thanks were awarded the authors.

As time did not permit of Mr MACFARLANE's Paper on "The Safety Governor" being read, it was reserved for reading at opening meeting of following Session.

On the motion of Mr J. M. GALE, a cordial vote of thanks was passed to Professor Thomson on his retiring from the Presidential Chair.

The PRESIDENT announced that the Candidates balloted for had been elected, viz. :—

AS A MEMBER :—

Mr JAMES RILEY, General Manager, Steel Co. of Scotland, Glasgow.

AS GRADUATES :—

Mr WILLIAM WEIR, Mechanical Draughtsman, Glasgow.
Mr JOHN IMBRIE FRASER, Apprentice Engineer, Glasgow.
Mr PERCY F. C. WILLCOX, Pupil Engineer, Glasgow.
Mr ROBERT ROBERTSON, B.Sc., Assistant Civil Engineer, Glasgow.

TREASURER'S STATEMENT—1885-86.

GENERAL FUND.

Dr.

To Balance in Union Bank at close of Session 1884-85,				£351	17	9
Subscriptions received:						
Session 1885-86, ..	£649	10	0			
Arrears of Previous Sessions,	31	0	0			
	£680	10	0			
Deduct Entry Money transferred to Building Fund, ..	4	10	0			
				676	0	0
Sales of Transactions, ...				9	5	6
Bank Interest, ...				2	13	7
				£1039	16	10

Cr.

By Amount paid Treasurer of House Committee as Institution's proportion of Expenditure, for Session 1885-86,...				£145	0	0
Printing, ...				195	10	3
Lithography, ...				174	12	6
Institution Medal, ...				10	0	0
Premiums for Papers read, ...				10	0	0
Graduate Section Medal, Session 1884-85, ...				1	7	6
Reprint of Vol. VII., ...				49	4	0
Salary to Secretary, ...				125	0	0
Commission Collection of Arrears of Subscriptions, viz.:—						
For Session 1885-86, ...	£408	0	0			
For Previous Sessions, ...	31	0	0			
	£439	0	0 at 5%	21	19	0
Postages, and Delivery of Annual Volumes,...				43	16	7
Stationery, &c., ...				9	11	3
New Books for Library, ...				20	19	6
New Catalogue for Library, ...				39	11	6
Cash to New Buildings Account to meet Interest on Loan, from Medal Funds, ...				16	0	0
Petty Cash, ...				2	5	8
Balance in Union Bank, ...				174	19	1
				£1039	16	10

MARINE ENGINEERING MEDAL FUND.

Dr.		Cr.	
To Balance in Union Bank at close of Session 1884-85, ...	£44 1 3	*By* Medal, Session 1884-85, ...	£10 0 0
Interest on Capital lent to New Buildings Account, ...	10 0 0	Balance in Union Bank, ...	44 11 1
Bank Interest, ...	0 9 10		
	£54 11 1		£54 11 1

RAILWAY ENGINEERING MEDAL FUND.

Dr.		Cr.	
To Balance in Union Bank at close of Session 1884-85, ...	£34 2 2	*By* Balance in Union Bank, ...	£40 9 9
Interest on Capital lent to New Buildings Account, ...	6 0 0		
Bank Interest, ...	0 7 7		
	£40 9 9		£40 9 9

GRADUATE MEDAL FUND.

Dr.		Cr.	
To Balance in Union Bank at close of Session 1884-85,	£21 12 3	*By* Balance in Union Bank, ...	£21 19 8
Bank Interest, ...	0 7 5		
	£21 19 8		£21 19 8

BUILDING FUND.

Dr.				Cr.	
To Balance in Union Bank at close of Session 1884-85, ...	£246 1 5			*By* Balance in Union Bank, ...	£293 8 10
Entry Money, ...	4 10 0				
Two Life Members at £20, ...	40 0 0				
Bank Interest, ...	2 17 5				
	£293 8 10				£293 8 10

NEW BUILDINGS ACCOUNT.

Dr.			Cr.		
To Capital to meet Cost of New Buildings, viz. :—			*By* Paid on New Buildings, ...		£2,047 8 1
From General Fund, ...	£542 15 7		Interest on Loans, viz. :—		
,, Marine Engineering Medal Fund, ...	351 11 2		To Marine Engineering Medal Fund,	£10 0 0	
,, Railway Engineering Medal Fund, ...	213 13 3		,, Railway Engineering Medal Fund,	6 0 0	
,, Building Fund, ...	939 8 1	£2,047 8 1			16 0 0
Cash received from General Fund to meet Interest on Loans, ...		16 0 0			
		£2,063 8 1			£2,063 8 1

Glasgow, *15th April, 1886.*—We have examined the foregoing Annual Financial Statement of Treasurer, the Accounts of the Marine and Railway Engineering Medal Funds, the Graduate Medal Fund, the Building Fund, and the New Buildings Account, and find the same duly vouched and correct, the Amounts in Bank being as stated.

(Signed) ANDw. MACLEAN, } Auditors.
D. KINGHORN, }

SUBSCRIPTION ACCOUNT.

Dr.

To Subscriptions due as per Roll :—			
Arrears due at close of last Session, ...		£93 0 0	
Deduct Irrecoverable, ...		23 10 0	
		£69 10 0	
Add elected at Annual General Meeting, April, 1885, ...		4 10 0	
Add received formerly struck off as irrecoverable, ...		4 10 0	
			£78 10 0
Session 1885-86 :—			
383 Members	at £1 10 0 —	£574 10 0	
1 New Member	,, 2 10 0 —	2 10 0	
7 ,,	,, 2 0 0 —	14 0 0	
14 ,,	,, 1 10 0 —	21 0 0	
34 Associates	,, 1 0 0 —	34 0 0	
1 New Associate	,, 1 10 0 —	1 10 0	
179 Graduates	,, 0 10 0 —	89 10 0	
			737 0 0
			£815 10 0

Cr.

By Subscriptions received, as per Cash Book, viz. :—			
Arrears of Sessions previous to Session 1885-86, ...			£31 0 0
Session 1885-86 :—			
342 Members	at £1 10 0 —	£513 0 0	
1 New Member	,, 2 10 0 —	2 10 0	
6 ,,	,, 2 0 0 —	12 0 0	
11 ,,	,, 1 10 0 —	16 10 0	
30 Associates	,, 1 0 0 —	30 0 0	
1 New Associate	,, 1 10 0 —	1 10 0	
148 Graduates	,, 0 10 0 —	74 0 0	
			649 10 0
			£680 10 0
Arrears due for Session 1885-86, ...		£87 10 0	
Arrears due for previous Sessions,		47 10 0	
			135 0 0
			£815 10 0

BANK ACCOUNT.

Dr.				Cr.			
To Balances at close of Session 1884-85 :—				*By* Amounts Drawn, Session 1885-86, ...	£565	0	0
General Fund, ...	£351	17	9	Balances in Union Bank, ...	575	8	5
Marine Engineering Medal Fund, ...	44	1	3				
Railway Engineering Medal Fund, ...	34	2	2				
Graduate Medal Fund, ...	21	12	3				
Building Fund, ...	246	1	5				
Amounts lodged, Session 1885-86, ...	435	17	9				
Interest, Session 1885-86, ...	6	15	10				
	£1,140	8	5		£1,140	8	5

CAPITAL ACCOUNT.

GENERAL FUND.

Loan to New Buildings Account, ...	£542 15 7	
Cash in Union Bank, ...	174 19 1	
		£717 14 8

MARINE ENGINEERING MEDAL FUND.

Loan to New Buildings Account, ...	£351 11 2	
Cash in Union Bank, ...	44 11 1	
		396 2 3

RAILWAY ENGINEERING MEDAL FUND.

Loan to New Buildings Account, ...	£213 13 3	
Cash in Union Bank, ...	40 9 9	
		254 3 0

GRADUATE MEDAL FUND.

Cash in Union Bank, ...	...	21 19 8

BUILDING FUND.

Amount to New Buildings Account, ...	£939 8 1	
Cash in Union Bank, ...	293 8 10	
		1,232 16 11

ARREARS OF SUBSCRIPTIONS.

Arrears due for Session 1885-86, ...	£87 10 0	
Do. previous Sessions, ...	47 10 0	
		135 0 0
		£2,757 16 6

DR. HOUSE EXPENDITURE ACCOUNT. (ABSTRACT 1885-86.) CR.

DR.						
To Rents for Letting Rooms, ...				£53	12	6
Amounts Received by Treasurer to meet Expenses, viz.:—						
From Institution of Engineers and Shipbuilders, ...	£145	0	0			
From Philosophical Society, ...	147	16	6½			
				292	16	6½
Balance due Treasurer, ...				21	19	1½
				£368	8	2

CR.			
By Balance due Treasurer, ...	£19	2	7
Interest on Bond, ...	130	15	7
Salary to Curator, ...	90	0	0
Salary of Attendant at Library, Cleaning, &c., ...	42	3	7
Taxes, ...	30	18	4
Feu-duty, ...	0	18	1
Gas, ...	12	18	9
Water, ...	7	13	4
Coals, ...	9	18	0
Insurance, ...	7	15	0
Repairs, ...	6	6	2
Furnishings, ...	9	18	9
	£368	8	2

The Account of the House Committee is kept by Mr John Mann, C.A., Treasurer to the Committee, and is periodically audited by the Auditors appointed by the Institution and the Philosophical Society.

W. J. MILLAR, *Secretary to House Committee.*

DECEASED MEMBERS.

DURING the Session 1885-86, the Institution has lost from the roll of membership several well-known and prominent gentlemen, who have been long associated, more or less actively, with the usual sessional work carried on, and the many special matters which from time to time have been discussed and dealt with.

These are—Mr WILLIAM ALEXANDER, Mr ANDREW BURNS, Mr JOHN HUNTER, Mr WALTER MACFARLANE, Mr ANDREW M'ONIE, Mr JOHN PAGE, Mr DANIEL RANKIN, and Mr THOMAS RUSSELL, *Members;* and Mr THOMAS WESTHORP, *Associate.*

Mr WILLIAM ALEXANDER was one of the Original Members of the Institution, and during the earlier sessions took an active part in the management as a Member of Council, holding office as such during the First Session. During the Fifth Session, he contributed an important paper on "Mining by Longwall." Mr Alexander by assiduous study early qualified himself for the engineering profession, the particular branch to which he ultimately devoted himself being that of Mining; and since 1855 he held the important appointment of one of the Government Inspectors of Mines in Scotland.

Mr ANDREW BURNS joined the Institution in 1865, in which year the Scottish Shipbuilders' Association, of which he was a Member, became incorporated with the Institution. Mr Burns was originally with Messrs R. Napier & Sons, afterwards becoming manager with Messrs J. & G. Thomson of Clydebank, where his ability as a shipbuilder and engineer, during over thirty years, showed itself in the quality of the workmanship turned out. He retired some years ago from active interest in engineering, and resided in the country, occasionally, however, acting as consulting engineer.

Mr JOHN HUNTER joined the Institution in December, 1857, and has thus been connected with the Institution almost since its commencement. Mr Hunter was connected with the Dalmellington Iron Works for about forty years, being latterly managing partner; he directed all the extensions of these works which were carried out from time to time. Mr Hunter was Chairman of the Dalmellington School Board, and of the Parochial Board, and took a leading and active part in all affairs affecting the welfare of the district.

Mr WALTER MACFARLANE was one of the Original Members of the Institution, identifying himself with its operations from time to time, and by his active co-operation and hospitality on special occasions, such as during the Summer Meetings of the Institution of Naval Architects, and of the Institution of Mechanical Engineers, held in Glasgow on the invitation of the Institution, he did much to make these meetings a success. Mr Macfarlane and his partners reared up the large ironfounding business known as the Saracen Foundry, at one time in Washington Street, now at Possil Park, the castings from which are widely known for elegance of design and careful workmanship. Mr Macfarlane was a patron of art, and he took an active interest in politics.

Mr ANDREW M'ONIE was one of the Original Members of the Institution, and held office in the Council of the Institution at different times, notably in the first Council elected. Mr M'Onie was a member of the engineering firm of Messrs W. & A. M'Onie, the important position of which is largely due to his great practical skill as an engineer. Mr M'Onie took much interest in the work of the charitable institutions of the city, and held important offices in connection with the Trades House, Clyde Trust, and other bodies.

Mr JOHN PAGE joined the Institution in 1864. He acted on the Council, and took much interest in the sessional work of the Institution, by contributing papers and taking part in discussions. During Session 1873-74, Mr Page assisted in developing the Graduate department of the Institution, in the formation of the Graduate Section, now forming an important part of the Institution, and was a member of the New Buildings Committee, appointed in

1879 for the erection of the buildings now jointly occupied by the Institution and Philosophical Society. Mr Page contributed the following papers—on "Railway Carriages," read December, 1866, on "An Improved Bar Testing Machine, and Callipers for Testing the Thickness of Pipes," read February, 1868; and on "Street Tramways," read December, 1872. Mr Page had an extensive experience of home and foreign engineering work, latterly acting as a consulting and inspecting engineer in Glasgow.

Mr DANIEL RANKIN joined the Institution in 1866. He was senior partner of the firm of Messrs Rankin & Blackmore, Eagle Foundry, Greenock. Mr Rankin had considerable and varied experience as an engineer, and at his works in Greenock developed several important improvements in marine engines. He acted at one time as one of the Greenock Harbour Trustees.

Mr THOMAS RUSSELL joined the Institution in 1859. He was also a member of the Scottish Shipbuilders' Association, and as a member of the firm of Messrs Hall, Russell, & Co., of Aberdeen, he was well known as a practical and experienced engineer. Mr Russell started as an apprentice with Messrs R. Napier & Sons; he afterwards spent some time at sea, returning from which he became manager to Messrs J. & G. Thomson, now of Clydebank; after which he entered into partnership with the firm of Messrs Hall, Shipbuilders, Aberdeen, the business being thereafter carried on under the name of Hall, Russell, & Co. Some years ago Mr Russell retired from business, and resided at Bridge of Allan.

Mr THOMAS WESTHORP joined the Institution as an Associate in 1865, in which year the Scottish Shipbuilders' Association was incorporated with this Institution. Mr Westhorp was connected with this Association as an Associate from its commencement in 1860. He resided in London, but always kept up his interest in the Institution.

DONATIONS TO LIBRARY.

"Lectures on Heat—Institution of Civil Engineers, London." From the Institution.

"Metallurgy of Iron"—Baurman; "Conservation of Energy"—Stewart; "Steam and the Steam Engine"—Evans; "Stewart's Descriptive History of the Steam Engine." From Henry Dyer, Esq, C.E., M.A.

"British Association Report, Montreal Meeting, 1884." From the Association.

"An Introduction to the Differential and Integral Calculus, with Examples of Application to Mechanical Problems," by W. J. Millar, C.E. From the Author.

"Modern Shipbuilding and the Men Engaged in it," by David Pollock, Esq. From the Author.

"Vital, Social, and Economical Statistics of Glasgow," by James Nicol, Esq., City Chamberlain. From the Author.

"Organic Philosophy," by Hugh Doherty, Esq., M.D. From the Author.

"Hydro-Mechanics," a Series of Lectures Inst. C.E., London. From the Institution.

"Description of the Ceremonial of Laying the Foundation Stone of the New Municipal Buildings, Glasgow." Presented by the Corporation Authorities.

"Recent Improvements in Wood Cutting Machinery," by George Richmond, Esq. From the Author.

"Micrometrical Measures of Gaseous Spectra" and "The Visual Solar Spectrum in 1874," by Prof. Piazzi Smyth, F.R.S.E., &c.

"The Universal Electrical Directory. From the Publishers.

Paper on "Gas Producers," by F. J. Rowan, Esq. From the Author.

"Transactions of the Philosophical Society of South Africa," Vols. I.-III. From Thomas Stewart, Esq.

NEW BOOKS ADDED TO LIBRARY.

Patent Laws of the World—by Alfred & Edward Carpmael.
Elementary Applied Mechanics—by Thomas Alexander and A. W. Thomson.
Manual of Naval Architecture—by W. H. White.
Woodworking Machinery—by M. Powis Bale.
Harbours and Docks—by L. F. Vernon Harcourt.
Recent Practice in Marine Engineering—by Wm. H. Maw.
Gas Engines—by Wm. Macgregor.
Modern Shipbuilding—by David Pollok.
Manufacture and Distribution of Coal Gas—by Wm. Richards.
Rock Blasting—by George G. Andre.
Electricity as a Motive Power—by Count du Moncel and Frank Geraldy.
Reflexions Sur la Puissance Motrice du Feu—by S. Carnot.
Second Report of the Royal Commission on Technical Education.
Strains in Ironwork—by Henry Adams.
Ure's Dictionary of Arts, Manufactures, and Mines.
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Société des Ingénieurs Civils de France.
Société Industrielle de Mulhouse.
Société d'Encouragement pour l'Industrie Nationale.
Société des Anciens Elèves des Ecôles Nationales d'Arts et Metiers.
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* * *

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Members have also the privilege of consulting the Books in the Library of the Philosophical Society.

The use of Library and Reading Room is open to Members, Associates, and Graduates.

The Portrait Album lies in the Library for the reception of Members' Portraits.

Members are requested when forwarding Portraits to attach Signature to bottom of Carte.

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Early notice of such Papers should be sent to the Secretary, so that the dates of reading may be arranged.

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Scottish Society of Arts, at his Office, 117 George Street, be furnished with Billets for attending the Meetings of that Society.

The Meetings of the Royal Scottish Society of Arts are held on the 2nd and 4th Mondays of each Month, from November till April, with the exception of the 4th Monday of December.

LIST

OF

HONORARY MEMBERS, MEMBERS, ASSOCIATES, AND GRADUATES

OF THE

Institution of Engineers and Shipbuilders in Scotland

(INCORPORATED),

SESSION 1885-86.

HONORARY MEMBERS.

JAMES PRESCOTT JOULE, LL.D., F.R.S., 12 Wardle Road, Sale, near Manchester.

Professor CHARLES PIAZZI SMYTH, F.R.S.E., Astronomer-Royal for Scotland, 15 Royal Terrace, Edinburgh.

Professor Sir WILLIAM THOMSON, A.M., LL.D., D.C.L., F.R.SS.L. and E., Professor of Natural Philosophy in the University of Glasgow.

Professor R. CLAUSIUS, the University, Bonn, Prussia.

Sir JOSEPH WHITWORTH, Bart., C.E., LL.D., F.R.S., Manchester.

Professor JOHN TYNDALL, D.C.L., LL.D., F.R.S., &c., Royal Institution, London.

HIS GRACE THE DUKE OF SUTHERLAND, Trentham, Stoke-upon-Trent.

Sir WM. G. ARMSTRONG, C.B., LL.D., D.C.L., F.R.S., Newcastle-on-Tyne.

Professor H. VON HELMHOLTZ, Berlin.

MEMBERS.

DATE OF ELECTION.			
1883, Mar. 20:	Geo. A.	Agnew,	2 Osborne Terrace, Govan.
1859, Jan. 19:	James	*Aitken, jun.,	Shipbuilder, Whiteinch, Glasgow.
1860, Dec 26:	William	Aiton,	Sandford Lodge, Peterhead.
Original:	Alexander	Allan,	Glen House, The Valley, Scarborough.
1872, Feb. 27:	A. B.	Allan, C.E.,	Burgh Surveyor, Burgh Chambers, Govan.
1869, Jan. 20:	William	Allan,	Scotland House, Sunderland.
1864, Dec. 21:	James B.	Alliott,	The Park, Nottingham.
G. 1865, Feb. 15: M. 1877, Dec. 18:	Wm. M.	Alston,	24 Burnbank Gardens, Glasgow.
1880, Nov. 23:	Thomas	Anderson,	20 Armadale Street, Dennistoun.
G. 1874, Feb. 24: M. 1880, Nov. 23:	James	Anderson,	100 Clyde St., Glasgow.
1860, Nov. 28:	Robert	Angus,	Lugar Ironworks, Cumnock.
1886, Jan. 26:	Charles	Armstrong,	Albert Street, Carlisle.
1883, Dec. 18:	J. Cameron	Arrol,	18 Blythswood Square, Glasgow.
1875, Dec. 21:	Thomas A. (*Vice-President.*)	Arrol,	18 Blythswood Square, Glasgow.
1885, Jan. 27:	William	†Arrol,	10 Oakley Ter., Glasgow.
Original:	David	Auld,	65 Rochester St., Glasgow.
1885, Apr. 28:	John	Auld,	Whitevale Foundry, Glasgow.
1881, Oct. 25:	Allan W.	Baird,	Eastwood Villa, St. Andrew's Drive, Pollokshields.

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

Names marked thus † are Life Members.

1880, Feb. 24:	William N.	Bain,	Collingwood, Pollokshields, Glasgow.
1873, Apr. 22:	H. W.	Ball,	Averley, Gt. Western Road, Hillhead, Glasgow.
1858, Dec. 22:	Andrew	Barclay, F.R.A.S.,	Caledonian Foundry, Kilmarnock.
1876, Jan. 25:	James	Barr,	c/o Galside Villa, Paisley.
1882, Mar. 21:	Prof. Archd.	Barr, B.Sc., C.E.,	The Yorkshire College, Leeds.
1868, Apr. 22:	Edward	Barrow,	Rue de la Province, Sud, Antwerp, Belgium.
1881, Mar. 22:	George H.	Baxter,	Ramage & Ferguson, Leith.
G. 1871, Feb. 21: M. 1865, Nov. 24:	William S.	Beck,	246 Bath Street. Glasgow.
1875, Jan. 26:	Charles	Bell,	4 Clifton Place, Glasgow.
	David	*Bell,	Shipbuilder, Yoker, near Glasgow.
1880, Mar. 23:	Imrie	Bell, C.E.,	1 Victoria Street, Westminster, London, S.W.
1880, Nov. 2:	Alfred G.	Berry,	33 Carnarvon St., Glasgow.
G. 1883, Mar. 20: M. 1884, Nov. 25:	Andrew S.	Biggart, C.E.,	Forth Bridge Works, South Queensferry.
1884, Mar. 25:	John Harvard	Biles,	Clydebank Shipyard, near Glasgow.
1866, Dec. 26:	Edward	Blackmore,	Eagle Foundry, Greenock.
1864, Oct. 26:	Thomas	Blackwood,	Shipbuilder, Port-Glasgow.
1869, Feb. 17:	Geo. M‘L.	Blair,	127 Trongate, Glasgow.
1867, Mar. 27:	James M.	Blair,	2 Bute Gardens, Hillhead, Glasgow.
1883, Jan. 23:	Chas. C.	Bone, C.E.,	23 Miller Street, Glasgow.
1883, Oct. 23:	William L.	Bone,	Ant and Bee Works, West Gorton, Manchester.
1874, Jan. 27:	Howard	Bowser,	13 Royal Crescent, W., Glasgow.
1880, Mar. 23:	James	Brand, C.E.,	109 Bath Street, Glasgow.

G. 1873, Dec. 23: M. 1884, Jan. 22:	James	Broadfoot,	55 Finnieston St., Glasgow.
1865, Apr. 26:	Walter	*Brock,	Engine Works, Dumbarton.
1859, Feb. 16:	Andrew	*Brown,	London Works, Renfrew.
G. 1876, Jan. 25: M. 1885, Nov. 24:	Andrew M'N.	Brown,	Castlehill House, Renfrew.
1886, Mar. 23:	George	Brown,	Comely Park, Dumbarton.
1885, Apr. 28:	Walter	Brown,	Castlehill, Renfrew.
1880, Dec. 21:	William	Brown,	Albion Works, Woodville Street, Govan, Glasgow.
G. 1874, Jan. 27: M. 1884, Jan. 22:	William	Brown,	61 Queen Street, Renfrew.
1858, Mar. 17:	James	Brownlee,	23 Burnbank Gardens, Glasgow.
1860, Dec. 26:	James C.	Bunten,	100 Cheapside St., Glasgow.
1866, Apr. 26:	Amedee	Buquet, C.E.,	15 Chemiss, St. Martin, Pontoise, S. O. France.
G. 1872, Oct. 22: M. 1885, Nov. 24:	Hartvig	Burmeister,	Burmeister & Wain, Copenhagen, Denmark.
1880, Dec. 21:	James W.	Burns,	5 Cecil Street, Paisley Rd., W., Glasgow.
1881, Mar. 22:	Thomas	Burt,	371 New City Rd., Glasgow.
1884, Jan. 22:	Edward H.	Bushell,	G 19 Exchange Buildings, Liverpool.

1878, Oct. 29:	Edward B.	Caird,	20 Lyndoch St., Glasgow.
1878, Dec. 17:	James	Caldwell,	130 Elliot Street, Glasgow.
1885, Mar. 24:	John B.	Cameron,	160 Hope Street, Glasgow.
1875, Dec. 21:	J. C.	Cameron,	24 Pollok Street, Glasgow.
1886, Jan. 26:	Andrew	Campbell,	53 Crookston Street, S.S., Glasgow.
1868, Dec. 23:	David	Carmichael,	Ward Foundry, Dundee.
1859, Nov. 23:	Peter	Carmichael,	Dens Works, Dundee.
1862, Jan. 8:	John	Carrick,	6 Park Quadrant, Glasgow.
1881, Nov. 22:	John H.	Carruthers,	Craigmore, Queen Mary Avenue, C'shill, Glasgow.

1859, Oct. 26:	Robert	Cassels,	168 St. Vincent Street, Glasgow.
1867, Jan. 30:	Albert	Castel,	3 Lombard Court, London, E.C.
G. 1873, Dec. 23: M. 1883, Oct. 23:	Walter	Chambers,	24 Ulster Chambers, Belfast.
1883, Jan. 23:	John	Clark,	British India Steam Navigation Co., 16 Strand, Calcutta.
1875, Oct. 26:	W. J.	Clark,	Southwick, near Sunderland.
1880, Nov. 2:	James	Clarkson,	Maryhill Engine Works, Maryhill, Glasgow.
1860, Apr. 11:	James	Clinkskill,	1 Holland Place, Glasgow.
1884, Feb. 26:	James T.	Cochran,	Duke Street, Birkenhead.
1881, Oct. 25:	George	Cockburn,	Rhodora Villa, St. Andrew's Drive, Pollokshields, Glasgow.
G. 1876, Dec. 19: M. 1884, Mar. 25:	Charles	Connell,	Whiteinch, Glasgow.
G. 1877, Dec. 18: M. 1885, Nov. 24:	James	Conner,	Isle of Wight Railway, Sandown, England.
Original:	Robert	Cook,	Woodbine Cottage, Pollokshields, Glasgow.
G. 1876, Jan. 25: M. 1884, Jan. 22:	William M.	Cooke,	———
1864. Feb. 17:	James	Copeland,	16 Pulteney St., Glasgow.
1864, Jan. 20:	William R.	Copland, C.E.,	146 West Regent Street, Glasgow.
1868, Mar. 11:	S. G. G.	Copestake,	Glasgow Locomotive Works, Little Govan, Glasgow.
1866, Nov. 28:	M'Taggart	Cowan, C.E.,	109 Bath Street, Glasgow.
1868, Apr. 22:	David	Cowan, C.E.,	Mount Gerald House, Falkirk.
1861. Dec. 11:	William	Cowan,	46 Skene Terrace, Aberdeen.
1883, Dec. 18:	Samuel	Crawford,	Clydebank, near Glasgow.

1881, Mar. 22:	William	Crockatt,	2 Marjory Place, Pollokshields, Glasgow.
1866, Dec. 26:	James L.	Cunliff,	Plewlands House, Merchiston, Edinburgh.
1872, Nov. 26:	David	Cunningham, C.E.,	Harbour Chambers, Dundee.
1884, Dec. 23:	Peter N.	Cunningham,	5 North East Park Street, Glasgow.
1869, Jan. 20:	James	Currie,	16 Bernard Street, Leith.
G. 1874, Feb. 24: M. 1882, Dec. 19:	James	Davie,	234 Cathcart Road, Crosshill, Glasgow.
1861, Dec. 11:	Thomas	Davison,	248 Bath Street, Glasgow.
1864, Feb. 17:	St. J. V.	Day, C.E.,	115 St. Vincent St., Glasgow.
1869, Feb. 17:	James	Deas, C.E..	Engineer, Clyde Trust, 7 Crown Gardens, Glasgow.
1882, Dec. 19:	J. H. L. Van	Deinse,	85 de Ruyterkade, Amsterdam.
1883. Nov. 21:	James	Denholm,	360 Dumbarton Road, Glasgow.
1866, Feb. 14:	A. C. H.	Dekke,	Shipbuilder, Bergen, Norway.
	Peter	*Denny,	Helenslee, Dumbarton.
1873, Feb. 18:	William	Denny,	Leven Shipy'd, Dumbarton.
G. 1873, Dec. 23: M. 1884, Jan. 22:	Peter	Dewar,	25 North Street, Glasgow.
1878, Mar. 19:	Frank W. (*Member of Council.*)	Dick,	Hallside Steel Works, Newton.
G. 1873, Dec. 24: M. 1878, Jan. 22:	James S.	Dixon,	170 Hope Street, Glasgow.
1882, Nov. 28:	John G.	Dobbie,	British India Steam Navigation Co., Mazagon Dockyard, Bombay.
1871, Jan. 17:	William	Dobson,	The Chesters, Jesmond, Newcastle-on-Tyne.
1864, Jan. 20:	James	Donald,	Abbey Works, Paisley.

1876, Jan. 25: James Donaldson, Almond Villa, Renfrew.
1863, Nov. 25: Robert Douglas, Dunnikier Foundry, Kirkcaldy.
1884, Dec. 23: John W. W. Drysdale, 5 Whitehill Gardens, G'gow.
1882, Oct. 24: Chas. R. Dubs, Glasgow Locomotive Works, Glasgow.
1864, Oct. 26: Robert *Duncan, (*Past President.*) Shipbuilder, Port-Glasgow.
1881, Jan. 25: Robert Duncan, Whitefield Engine Works, Govan, Glasgow.
1873, Apr. 22: Robert Dundas, C.E., (*Member of Council.*) 3 Germiston Street, Glasgow.
1869, Nov. 23: David Jno. Dunlop, Inch Works, Port-Glasgow.
1877, Jan. 23: John G. Dunlop, 17 Goulton Road, Lower Clapton, London.
1880, Mar. 23: Hugh S. Dunn, Earlston Villa, Caprington, Kilmarnock.
1879, Dec. 23: Wm. T. Courtier Dutton 36 Oswald Street, Glasgow.
1883, Oct. 23: Henry Dyer, C.E., MA., (*Member of Council.*) 8 Highburgh Terrace, Dowanhill, Glasgow.

1876, Oct. 24: Jn. Marshall Easton, Redholm, Helensburgh.
1885, Feb. 24: Francis Elgar, LL.D., F.R.S.E., (*Member of Council.*) 17 University Gardens, Glasgow.

1875, Oct. 26: James G. Fairweather, C.E., B.Sc., 8 Findhorn Place, Grange, Edinburgh.
John *Ferguson, Shipbuilder, Whiteinch, Glasgow.
G. 1869, Nov. 23: M. 1878, Mar. 19: John Ferguson, jun., Shipbuilder, Leith.
1874, Feb. 24: Immer Fielden, 6 Lorne Terrace, Holderness Road, Hull.
1880, Jan. 27: Alexander Findlay, Hamilton Road, Motherwell.
G. 1873, Dec. 23: M. 1884, Nov. 25: E. Walton Findlay, Ardeer, Stevenston.

1884, Dec. 23:	Finlay	Finlayson,	Alexandria Place. Colt Terrace, Coatbridge.
Original:	William	Forrest,	66 Bath Street, Glasgow.
1872, Nov. 26:	Thomas	Forrest, M.E.,	Dumfries Ironworks, Dumfries.
1883, Dec. 18:	Lawson	Forsyth,	10 Grafton Sq., Glasgow.
1870, Jan. 18:	William	Foulis,	Engineer, Corporation Gas Works, 42 Virginia St., Glasgow.
1880, Nov. 2:	Samson	Fox,	Leeds Forge, Leeds.
1862, Nov. 26:	Alexander	Fullarton,	Vulcan Works, Paisley.
1879, Nov. 25:	John	Frazer,	P. Henderson & Co., 15 St. Vincent Place, Glasgow.
1885, Jan. 27:	Peter	Fyfe,	234 Parliamentary Road, Glasgow.
1858, Nov. 24:	James M.	Gale, C.E., *(Past President; Member of Council, and Treasurer.)*	Engineer, Corporation Water Works, 23 Miller Street, Glasgow.
1862, Jan. 8:	Andrew	Galloway, C.E.,	St. Enoch Station, Glasgow.
1883, Oct. 23:	Gilbert H.	Garrett,	Robert Stephenson & Co., South Street, Newcastle-on-Tyne.
1873, Dec. 23:	Bernard	Gatow,	Veritas Office, 29 Waterloo Street, Glasgow.
G. 1873, Dec. 23: M. 1882, Mar. 21:	Andrew	Gibb,	Rait & Gardiner, Millwall Docks, London.
1859, Nov. 23:	Archibald	*Gilchrist,	11 Sandyford Pl., Glasgow.
G. 1866, Dec. 26: M. 1878, Oct. 29:	James	Gilchrist, *(Member of Council.)*	Stobcross Engine Works, Finnieston Quay, Glasgow.
1859, Dec. 21:	David C.	Glen,	14 Annfield Place, Dennistoun, Glasgow.
1868, Nov. 25:	Thomas	Goldie,	Waverley Mills, Ceres Road, Cape of Good Hope.
1864, Feb. 17:	James	Goodwin,	Ironfounder, Ardrossan.

1866, Mar. 28:	Gilbert S.	Goodwin,	Alexandra Buildings, James Street, Liverpool.
1868, Mar. 11:	Joseph	Goodfellow,	136 Sackville Place, Stirling Road, Glasgow.
1858, Dec. 22:	Henry	*Gourlay,	Dundee Foundry, Dundee.
1882, Apr. 25:	H. Garrett	Gourlay,	Dundee Foundry, Dundee.
	Edwin	*Graham,	Osbourne, Graham, & Co., Hylton, Sunderland.
1858, Mar. 12:	George	Graham, C.E.,	Engineer, Caledonian Railway, Glasgow.
1876, Jan. 25:	Thomas M.	Grant,	4 Clayton Terrace, Dennistoun, Glasgow.
1871, Mar. 28:	Thomas	Gray,	Chapel Colliery, Newmains.
1862, Jan. 8:	James	Gray,	Pathhead Colliery, Cumnock, Ayrshire.
1870, Feb. 22:	P. B. W.	Gross, M.E.,	4 Albion Place, Cumberland Road, Bristol.
1881, Dec. 20:	L. John	Groves,	Engineer, Crinan Canal, Ardrishaig.
1879, Nov. 25:	Robert	†Hadfield,	Hadfield Steel Foundry Co., Attercliffe, Sheffield.
1872, Feb. 27:	A. A.	Haddin, C.E.,	131 West Regent Street, Glasgow.
1881, Jan. 25:	William	Hall, jun.,	Shipbuilder, Aberdeen.
1876, Oct. 24:	David	Halley,	Burmeister & Wain, Copenhagen, Denmark.
G. 1874, Feb. 24: M. 1885, Nov. 24:	Archibald	Hamilton,	New Dock Works, Govan.
G. 1873, Dec. 23: M. 1881, Nov. 22:	David C.	Hamilton,	Clyde Shipping Co., 21 Carlton Place, Glasgow.
G. 1866, Dec. 26: M. 1873, Mar. 18:	James	Hamilton, jr.,	Ardedynn, Kelvinside, Glasgow.
	John	*Hamilton,	22 Athole Gardens, Gl'gow
G. 1869, Nov. 23: M. 1875, Feb. 23:	J. B.	Hamond,	Didsbury, Manchester.

1876, Feb. 22:	Walter	Hannah,	Board of Trade Surveyor. 7 York Street, Glasgow.
G. 1880, Nov. 2: M 1884, Jan. 22:	Bruce	Harman,	R. Napier & Sons, Govan. Glasgow.
1878, Mar. 19:	Timothy	Harrington,	61 Gracechurch Street, London, E.C.
1875, Jan. 26:	Peter T.	Harris,	19 West St. (S.S.), Glasgow.
G. 1874, Feb. 24: M. 1880, Nov. 23:	C. R.	Harvey,	166 Renfrew St., Glasgow.
1864, Nov. 23:	John	Hastie,	Kilblain Engine Works. Greenock.
1871, Jan. 17:	William	Hastie,	Kilblain Engine Works, Greenock.
1879, Nov. 25:	A. P.	†Henderson,	30 Lancefield Quay, Glasgow.
1877, Feb. 20:	David	*Henderson,	Meadowside, Partick, Glasgow.
1873, Jan. 21:	John	†Henderson, jr.,	Meadowside, Partick, Glasgow.
1879, Nov. 25:	John L.	†Henderson,	Westbank House, Partick Glasgow.
1878, Dec. 17:	William	Henderson,	Meadowside, Partick, Glasgow.
1880, Nov. 2:	William	Henderson, C.E.,	121 W. Regent St., Gl'gow.
1870, May 31:	Richard	Henigan, C.E.,	Alma Road, Cotford Place, Southampton.
1877, Feb. 20:	George	Herriot,	7 York Street, Glasgow
	Laurence	*Hill, C.E.,	5 Doon Gardens, Hillhead, Glasgow.
1886, Mar. 23:	Geo. Lisle	Hindmarsh,	Lloyds Surveyor, 36 Oswald Street, Glasgow.
1880, Nov. 2:	C. P. (*Vice President.*)	Hogg, C.E.,	175 Hope Street, Glasgow.
1883, Mar. 20:	John	Hogg,	Victoria Engine Works, Airdrie.

1880, Mar. 23:	F. G.	Holmes, C.E.,	109 Bath Street, Glasgow.
1883, Mar. 20:	Matthew (*Member of Council.*)	Holmes,	551 Sauchiehall St., Glasgow.
Original:	James	Howden,	8 Scotland Street, Glasgow.
1884, Apr. 22:	John G.	Hudson,	18 Aytoun Road., Pollokshields, Glasgow.
Original:	Edmund	*Hunt,	87 St. Vincent St., Glasgow.
1860, Nov. 28:	James	Hunter,	Coltness Iron Works, by Newmains.
1881, Jan. 25:	James	Hunter,	Aberdeen Iron-Works, Aberdeen.
G. 1878, Dec. 23: M. 1885, Nov. 24:	Guybon	Hutson,	Kelvinhaugh Engine Works, Glasgow.
G 1873, Dec. 23: M. 1877, Feb. 20:	P. S.	Hyslop,	Ferro Carril del Sud, Plaza Constitucion, Buenos Ayres.
Original:	John	*Inglis,	64 Warroch St., Glasgow.
1861, May 1:	John (*Vice President.*)	Inglis, jun.,	Point House Shipyard, Glasgow.
1879, Jan. 21:	Thos. F.	Irwin,	2A Tower Chambers, Old Churchyard, Liverpool.
1880, Nov. 2:	Lawrence N.	Jackson,	c/o The Manager Galle Face Hotel, Colombo, Ceylon.
1875, Dec. 21:	William	Jackson,	Govan Engine Works, Govan, Glasgow.
1884, Jan. 22:	J. Yate	Johnson, C.E.,	115 St. Vincent Street, Glasgow.
1879, Feb. 25:	David	Johnston,	6 Osborne Place, Copeland Road, Govan, Glasgow.
1870, Dec. 20:	David	Jones,	Highland Rlwy., Inverness.
1883, Jan. 23:	F. C.	Kelson.	Angra Bank, Waterloo Park, Waterloo, Liverpool.

1872, Mar. 26:	Ebenezer	Kemp,	Linthouse Engine Works, Govan, Glasgow.
1875, Nov. 23:	William	Kemp,	Ellen St. Engineering Works, Govan, Glasgow.
1878, Mar. 19:	Hugh	Kennedy,	Redclyffe, Partickhill, Glasgow.
1877, Jan. 23:	John	Kennedy,	R. M‘Andrew & Co., Suffolk House, Laurence Pountney Hill, London, E.C.
1876, Feb. 22:	Thomas	Kennedy,	Water Meter Works, Kilmarnock.
1876, Oct. 24:	Andrew	Kerr, C.E.,	Town Surveyor's Office, Warrnambool, Victoria, Australia.
	David	*Kinghorn,	172 Lancefield St., Glasgow.
1879, Dec. 23:	John G.	Kinghorn,	Tower Buildings, Water Street, Liverpool.
1885, Nov. 24:	Frank E.	†Kirby,	Detroit, U.S., America.
1864, Oct. 26:	Alex. C.	Kirk,	19 Athole Gardens, Hillhead, Glasgow.
Original:	David	*Kirkaldy,	Testing and Experimenting Works, 99 Southwark Street, London, S.E.
1885, Jan. 27:	Charles A.	Knight,	107 Hope Street, Glasgow.
1880, Mar. 23:	Frederick	Krebs,	M. B. M. S. S. Co., Tokio, Japan.
1875, Oct. 26:	William	Laing,	17 M‘Alpine St., Glasgow.
1858, Apr. 14:	David	Laidlaw,	Chaseley, Skelmorlie, by Glasgow.
1884, Mar. 25:	John	Laidlaw,	98 Dundas St., S.S., Gl'gow.
1862, Nov. 26:	Robert	Laidlaw,	147 E. Milton St., Glasgow.
1880, Feb. 24:	James	Lang,	°/o John Lang, 552 St. Vincent St., Glasgow.
1884, Feb. 26:	John	Lang, Jun.,	Church Street, Johnstone.

Original:	James G.	*Lawrie, (*Past President.*)	2 Westbourne Terrace, Glasgow.
1882, Mar 21:	Henry A.	Lawson,	Craigenly Cottage, Lenzie, near Glasgow.
1880, Mar. 23:	Allison	Lennox,	131 W.Regent St.,Glasgow.
1878, Mar. 19:	John	Lennox,	131 W.Regent St.,Glasgow.
G. 1873, Dec. 23: M. 1876, Oct. 24:	Charles C.	Lindsay,C.E., (*Member of Council.*)	167 St.VincentSt.,Glasgow.
1884, Feb. 26:	John	List,	Messrs D. Currie & Co., Blackwall, London, E.
1862, Apr. 2:	H. C.	Lobnitz,	Renfrew.
1865, Dec. 20:	John L.	Lumsden,	Alex. Jack & Coy., Seacombe, Cheshire.
1885, Oct. 27:	John	Lyall,	———
1873, Jan. 21:	James M.	Lyon, M.E.,	Engineer and Contractor, Singapore.
1884, Dec. 23:	John	M‘Beth,	5 Park Street, S.S., Glasgow.
1858, Feb. 17:	David	M‘Call, C.E.,	160 Hope Street, Glasgow.
1874, Mar. 24:	Hector	MacColl,	Jas. Jack & Co., Engineers, Liverpool.
	Hugh	*MacColl,	Manager, Wear Dock Yard, Sunderland.
1883, Oct. 23:	James	M‘Creath,C.E.,	95 Bath Street, Glasgow.
1871, Jan. 17:	David	M‘Culloch,	Vulcan Works, Kilmarnock.
1884, Feb. 26:	James	M‘Ewan,	Cyclops Foundry, 50 Peel Street, London Road, Glasgow.
1880, Nov. 2:	James W.	Macfarlane,	Valeview House, Overlee, Busby.
G. 1874, Feb. 24: M. 1885, Nov. 24:	George	M‘Farlane,	65 Great Clyde Street, Glasgow.
	Andrew	*M‘Geachan,	20 Union Street, Glasgow.

1886, Jan. 26:	Thomas	M'Gregor,	10 Mossesfield Terrace, Springburn.
1880, Apr. 27:	Wm. Rae	M'Kaig,	17 Water St., Liverpool.
1881, Mar. 22:	William A.	Mackie,	3 Broomhill Terrace, Partick, Glasgow.
1873, Jan. 21:	J. B. Affleck	M'Kinnel,	Dumfries Iron Works, Dumfries.
1859, Dec. 21:	Robert	M'Laren,	22 Canal St., S.S., Glasgow.
G. 1880, Nov. 2: M. 1885, Dec. 22:	Robert	M'Laren, jun.,	Eglinton Foundry, Glasgow.
	Andrew	*Maclean,	Viewfield House, Partick, Glasgow.
G. 1874, Feb. 24: M. 1885, Nov. 24:	Andrew	M'Lean, jun,	Viewfield House, Partick.
1884, Dec. 23:	James	M'Lellan,	10 W. Garden Street, Glasgow.
1858, Nov. 24:	Walter	M'Lellan.	127 Trongate, Glasgow.
	John	*M'Millan,	Shipbuilder, Dumbarton.
	William *(Member of Council.)*	*MacMillan,	19 Elgin Terrace, Partick. Glasgow.
1884, Dec. 23:	John	M'Neil,	Helen St., Govan, Glasgow.
Original:	Andrew	M'Onie,	1 Scotland Street, Glasgow.
1883, Jan. 23:	William	M'Onie, Jr.,	128 West Street, Glasgow.
1883, Jan. 23:	James	M'Ritchie, C.E.,	Singapore.
1864, Oct. 26:	Robert *(Past President.)*	*Mansel,	Shipbuilder, Whiteinch, Glasgow.
1875, Dec. 21:	George	Mathewson,	Bothwell W'ks, Dunfermline.
1884, Apr. 22:	Henry A.	Mavor,	140 Douglas St., Glasgow.
1876, Jan. 25:	William W.	May,	142 Fountain Road, Walton, Liverpool.
1883, Feb. 20:	James	Meek,	———
G. 1876, Oct. 24: M. 1882, Nov. 28:	James	Meldrum, C.E.,	3 Elmbank Street, Glasgow.
1883, Jan. 23:	William	Melville, C.E.,	Caledonian Ry., Buchanan Street, Glasgow.

1881, Mar. 22:	William	Menzies,	7 Dean Street, Newcastle-on-Tyne.
1861, Dec. 11:	Daniel	Miller, C.E.,	204 St. Vincent St., Glasgow.
	James	*Miller,	Sallachan, Ardgour.
G. 1873, Dec. 23: M. 1881, Nov. 22:	John F.	Miller,	Greenoakhill, Broomhouse.
Original:	James B.	Mirrlees,	45 Scotland St., Glasgow.
1886, Jan. 26:	Alexander	Mitchell,	4 Bellevue Terrace, Springburn.
1876, Mar. 21:	James	Mollison,	Lloyd's Register, 36 Oswald Street, Glasgow.
1869, Dec. 21:	John	Montgomerie,	210 Great Northern Ter., Possil Park.
1883, Nov. 21:	Joseph	Moore,	East Finchley, London.
1862, Nov. 26:	Ralph	Moore, C.E.,	13 Clairmont Gardens, Glasgow.
1878, Apr. 23:	Robert H.	Moore,	Mount Blue Works, Camlachie, Glasgow.
1868, Feb. 12:	Alexander	Morton,	241 W. George St., Glasgow.
G. 1878, Dec. 17: M. 1883, Jan. 23.	Robert	Morton,	53 Waterloo St., Glasgow.
1885, Mar. 24:	Edmund	Mott,	Board of Trade Surveyor, 7 York Street, Glasgow.
1864, Feb. 17:	Hugh	Muir,	7 Kelvingrove Ter., Glasgow.
1882, Jan. 24:	John G.	†Muir,	———
1870, Mar. 22:	Wm. T.	Mumford,	36 Oswald Street, Glasgow.
1882, Feb. 21:	George	Munro,	254 Bath Street, Glasgow.
1882, Dec. 19:	Robert D.	Munro,	141 Buchanan St., Glasgow.
Original:	James	Murdoch,	Shipbuilder, Port-Glasgow.
1880, Jan. 27:	William	Murdoch,	20 Carlton Place, Glasgow.
1886, Jan. 26:	James	Murray,	8 Brown Street, Glasgow.
1877, Jan. 23:	Robert	Murray,	25A Coltman Street, Hull.

1881, Jan. 25:	Henry M.	Napier,	Shipbuilder, Yoker, near Glasgow.

1857, Dec. 23: John †*Napier, 23 Portman Sq., London.

1881, Dec. 20: Robert T. †Napier, Shipbuilder, Yoker, near Glasgow.

Original: Walter M. Neilson, (*Past President.*) Queen's Hill Kirkcudbrightshire.

1869, Nov. 23: Theod. L. Neish, 78 Finnart St., Greenock.

A. 1865, Apr. 26:
M. 1879, Oct. 28: R. S. *Newall, F.R.S., F.R.A.S., &c., Ferndene, Gateshead-on-Tyne.

1883, Dec. 18: Thomas Nicol, Clydebank, near Glasgow.

1884, Dec. 23: Wm. H. Nisbet, Mavisbank, Partickhill, Glasgow.

1876, Dec. 19: Richard Niven, C.E., Dalmottar House, Old Kilpatrick.

1861, Dec. 11: John Norman, 475 New Keppochhill Road, Glasgow.

1886, Jan. 26: George Oldfield, Greenlea, Johnstone.

1882, Jan. 24: Robert S. Oliver, C.E., Highland Railway Co., Inverness.

1860, Nov. 28: John W. Ormiston, Shotts Iron Works, by Wishaw.

1885, Mar. 24: Alex. T. Orr, Hall, Russell, & Co., Aberdeen.

1867, Apr. 24: T. R. Oswald, The Southampton Shipbuilding & Engineering Works, Southampton.

1882, Mar. 21: Geo. S. Packer, F.I.C., Hallside Steel Works, Newton, near Glasgow

1864, Oct. 26: John Page, C.E., 1 Kersland Ter., Glasgow.

1876, Apr. 25: William Parker, 2 White Lion Court, Cornhill, London.

1883, Nov. 21: W. L. C. Paterson, 19 St. Vincent Crescent, Glasgow.

1877, Apr. 24: Andrew Paul, Levenford Works, Dumbarton.

1880, Nov. 2:	James M.	Pearson, C.E.,	Strand Street, Kilmarnock.
1866, Dec. 26:	William	Pearce, M.P.,	Fairfield Shipyard, Govan, Glasgow.
1868, Dec. 23:	Eugène	Perignon, C.E.,	105 Rue Faubourg, St. Honoré, Paris.
	John	*Price,	6 Osborne Villas, Jesmond, Newcastle-on-Tyne.
1877, Nov. 20:	F. P.	Purvis.	Craig Villa, Dumbarton.
1868, Dec. 23:	Henry M.	Rait,	155 Fenchurch St., London.
1873, Apr. 22:	Richard	Ramage.	Shipbuilder, Leith.
1872, Oct. 22:	David	Rankine.	75 West Nile St., Glasgow.
1886, Mar. 23:	John F.	Rankin,	Eagle Foundry, Greenock.
1876, Dec. 19:	Robert	Rankin,	2 Morrison St., Glasgow.
1881, Jan. 25:	Charles	Reid,	Lilymount, Kilmarnock.
1883, Nov. 21:	George W.	Reid,	Highland Railway, Inverness.
1868, Mar. 11:	James	Reid. (*Past President.*)	Locomotive Works, Springburn, Glasgow.
1869, Mar. 17:	James	Reid.	Shipbuilder, Port Glasgow.
	John	*Reid,	Shipbuilder, Port-Glasgow.
1880, Apr. 27:	John	Rennie,	Ardrossan Shipbuilding Co., Ardrossan.
G. 1873, Dec. 23: M. 1881, Nov. 22:	Charles H.	Reynolds,	Cuprum House, Hamilton Ter., Partick, Glasgow.
1876, Oct. 24:	Duncan	Robertson,	8 Brighton Place, Govan, Glasgow
1886, Apr. 27:	James	Riley,	Steel Co. of Scotland, 150 Hope Street, Glasgow.
Original:	James	Robertson.	21 Gower Street, Paisley Road, Glasgow.
1873, Jan. 21:	John	Robertson,	Grange Knowe, Pollokshields, Glasgow.
1863, Nov. 25:	William	Robertson, C.E.,	123 St. Vincent Street, Glasgow.

1884, Apr. 22:	R. A.	Robertson,	42 Aytoun Road, Pollokshields, Glasgow.
Original:	Hazltn. R.	*Robson, (*Past President.*)	14 Royal Cresct., Glasgow.
1877, Feb. 20:	Jno. MacDonald	Ross,	11 Queen's Cres., Glasgow.
1861, Dec. 11:	Richard G.	Ross,	21 Greenhead St., Glasgow.
G. 1864, Nov. 23: M. 1870, Jan. 18:	Alex.	Ross, C.E.,	Lynnwood, Alva.
Original:	David	*Rowan, (*Past President.*)	231 Elliot Street, Glasgow.
G. 1875, Dec. 21: M. 1885, Jan. 27:	James	Rowan,	231 Elliot Street, Glasgow.
1877, Oct. 30:	Alexander	Russell,	186 North Street, Glasgow.
G. 1858, Dec. 22: M. 1863, Mar. 4:	George	Russell, (*Member of Council.*)	Engineer, Motherwell.
1881, Feb. 22:	Joseph	Russell,	Shipbuilder, Port-Glasgow.
1859, Dec. 21:	Thomas	*Russell,	Albyn Lodge, Bridge of Allan.
1876, Oct. 24:	Peter	Samson,	Board of Trade Offices, Downing Street, London, S.W.
1885, Feb. 24:	James	Samuel, jun.,	238 Berkeley St., Glasgow.
1883, Feb. 20:	John	Sanderson,	Lloyd's Registry, 36 Oswald Street, Glasgow.
1882, Dec. 19:	Prof. Jas.	Scorgie, F.C.S.,	Civil Engineering College Poona, India.
1884, Apr. 22:	Andrew	Scott,	56 Græme Street, Glasgow.
1872, Jan. 30:	James E.	Scott,	13 Rood Lane, London.
1881, Jan. 25:	John	Scott,	Whitebank Engine Works, Kirkcaldy.
1860, Nov 28:	Thos. B.	*Seath,	42 Broomielaw, Glasgow.
1875, Jan. 26:	Alexander	Shanks,	Belgrade, Aytoun Road, Pollokshields, Glasgow.
1858, Nov. 24:	William	Simons,	Renfrew.
1862, Jan. 22:	Alexander	Simpson, C.E.,	175 Hope Street, Glasgow.

1871, Mar. 28:	Hugh	Smellie,	Belmont Grange Terrace, Kilmarnock.
Original:	Alexander	Smith,	57 Cook Street, Glasgow.
1880, Nov. 2:	Alexander	Smith,	1 Braeside Terrace.Maxwell Rd., Pollokshields, Glasgow.
1869, Mar. 17:	David S.	Smith,	Hellenic Steam Navigation Co., Syra, Greece.
1859, Jan. 19:	George	Smith,	Kennedy Street, Parliamentary Road, Glasgow.
1871, Dec. 11:	Hugh	Smith,	9 Kelvinside Terrace, North Kelvinside, Glasgow.
G. 1868, Dec. 23: M. 1874, Oct. 27:	Hugh	Smith,	Argyle Engine Works, Mansion St., Glasgow.
1870, Feb. 22:	Edward	Snowball,	Engineer, Hyde Park Locomotive Works, Springburn, Glasgow.
1883, Oct. 23:	Andrew	Sproul,	Palmerston Blds., Greenock.
1886, Feb. 23:	George	Stanbury,	16 Hamilton Terrace, West, Partick.
1883, Dec. 18:	Alex. E.	†Stephen,	12 Park Terrace, Glasgow.
	John	†*Stephen,	Linthouse, Govan, Glasgow.
1881, Nov. 22:	Alex.	Steven,	Provanside, Glasgow.
1867, Jan. 30:	Duncan	Stewart,	47 Summer Street,Glasgow.
1874, Oct. 27:	Peter	Stewart,	53 Renfield Street,Glasgow.
G. 1873, Dec. 23: M. 1882, Oct. 24:	W. B.	Stewart,	18 Newton Place, Glasgow.
1866, Nov. 28:	James	Stirling,	Loco. Engineer, S. Eastern Ry., Ashford, Kent.
Original:	Patrick	Stirling,	The Great Northern Railway, Doncaster.
1881, Jan. 25:	Walter	Stoddart,	Caledonian Railway, Carstairs.
1864, Nov. 23:	Edward	Strong,	$^{c}/_{o}$ Kent Cottage, Queen's Cres., Southsea, Hants.

1877, Jan. 23: James Syme, 8 Glenavon Ter., Partick, Glasgow.

1879, Oct. 28: James Tait, C.E., Wishaw.
1882, Apr. 25: Alex. M. Taylor, Java Cottage, Lenzie.
1885, Apr. 28: Peter Taylor, 59 Queen Street, Renfrew.
1879, Mar. 25: Staveley Taylor, Russell & Co., Shipbuilders, Greenock.
1873, Dec. 23: E. L. Tessier, Veritas Office, 29 Waterloo Street, Glasgow.
1885, Jan. 27: George W. Thode, 107 Hope Street, Glasgow.
1882, Apr. 25: Geo. P. Thomson, Clydebank Shipbuilding Yard, Glasgow.
1883, Dec. 18: George Thomson, 9 Buckingham Ter., Partick, Glasgow.
1886, Mar. 23: James Thomson, jun., Leven Shipyard, Dumbarton.
1874, Nov. 24: Prof. James Thomson, C.E., LL.D., F.R.SS.L. & E., *(President.)* 2 Florentine Gardens, Hillhead Street, Glasgow.
1868, Feb. 12: James M. Thomson, 36 Finnieston St., Glasgow.
1882, Mar. 21: James R. Thomson, Clydebank Foundry, Glasgow.
1868, May 20: John Thomson, 36 Finnieston St., Glasgow.
1876, Feb. 22: John Thomson, *(Member of Council.)* 147 East Milton Street, Glasgow.
1875, Jan. 26: Robert S. Thomson, 3 Melrose Street, Queen's Crescent, Glasgow.
1864, Feb. 17: W. R. M. Thomson, 96 Buchanan St., Glasgow. gow.
1878, May 14: W. B. Thompson, Ellengowan, Dundee.
Original: Thomas C. Thorburn, 35 Hamilton Square, Birkenhead.
1874, Oct. 27: Prof. R. H. Thurston, M.E., C.E., Sibley College, Cornell University, Ithaca, N.Y., U.S.A.

1875, Nov. 23: John Turnbull, jun., Consulting Engineer, 255 Bath Street, Glasgow.

1876, Nov. 21: Alexander Turnbull, 15 Whitehill Terrace, Dennistoun, Glasgow.

1880, Apr. 27: John Tweedy, Neptune Works, Newcastle-on-Tyne.

1865, Apr. 26: W. W. Urquhart, Blackness Foundry, Dundee.

1883, Jan. 23: Peter Wallace, 25 Argyle Place, Partick, Glasgow.

1885, Mar. 24: W. Carlile Wallace, Maryland, Dumbarton.

1886, Jan. 26: John Ward, Leven Shipyard, Dumbarton.

1875, Mar. 23: G. L. Watson, 108 W. Regent St., Glasgow.

1864, Mar. 16: W. R. Watson, 16 Woodlands Ter., Glasgow.

1883, Jan. 23: D. W. Watt, 58 Union Street, Glasgow.

John *Weild, Underwriter, Exchange, Glasgow.

1874, Dec. 22: George Weir, M.E., 18 Millbrae Cres., Langside, Glasgow

1874, Dec. 22: James Weir, M.E., Silver Bank, Cambuslang, near Glasgow.

G. 1876, Dec. 19:
M. 1884, Feb. 26: Thomas D. Weir, C.E., 97 W. Regent St., Glasgow.

1869, Feb. 17: Thomas M. Welsh, 63 St. Vincent Cres., Glasgow.

1868, Dec. 23: Henry H. West, 14 Castle Street, Liverpool.

1883, Feb. 20: Richard S. White, Shipbuilder, Sir Wm. Armstrong, Mitchell, & Co., Newcastle on-Tyne.

1884, Nov. 25: John Wildridge, Consulting Engineer, Sydney, N.S.W., Australia.

1876, Oct. 24: Francis W. Willcox, 45 West Sunniside, Sunderland.

	1884, Dec. 23:	James	Williamson,	Barclay, Curle, & Co., Whiteinch.
	1883, Feb. 20:	Robert	Williamson,	Lang & Williamson, Engineers, &c., Newport, Mon.
	1878, Oct. 29:	Thomas	Williamson,	Bairdsike, Mossend.
		Alex. H.	*Wilson,	Aberdeen Iron Works, Aberdeen.
	1868, Dec. 23:	James	Wilson, C.E.,	Water Works, Greenock.
	1870, Feb. 22:	John	Wilson,	165 Onslow Drive, Dennistoun, Glasgow.
	1858, Jan. 20:	Thomas	†*Wingate,	Viewfield, Partick.
G. M.	1873, Dec. 23: 1884, Jan. 22:	Robert	Wyllie,	Hartlepool Engine Works, Hartlepool.
	1879, Oct. 28:	John	Young,	Phœnix Iron Works, Glasgow.
	1867, Nov. 27:	John	Young,	Galbraith Street, Stobcross, Glasgow.

ASSOCIATES.

	Thomas	*Aitken,	8 Commercial Street, Leith.
1883, Oct. 28:	John	Barr,	Secretary to Glenfield Co., Kilmarnock.
1882, Dec. 19:	Wm.	Begg,	47 West Cumberland Street, Glasgow.
1882, Jan. 24:	John	Black,	4 Alexandra Terrace, Govan, Glasgow.
1884, Dec. 23:	W. S. C.	Blackley,	10 Hamilton Crescent, Partick.

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

1876, Jan. 25: John Brown, B.Sc., 11 Somerset Place, Glasgow.
1865, Jan. 18: John Bryce, Sweethope Cottage, N. Milton Road, Dunoon.

1880, Dec. 21: John Cassells, 56 Cook Street, Glasgow.
1870, Dec. 20: Joseph J. Coleman, F.C.S., Ardarroch, Bearsden, by Glasgow.

1885, Feb. 24: Robert Darling, 5 Summerside Place, Leith.

1859, Nov. 23: Sir A. Orr Ewing, Bart., M.P., 2 W. Regent Street, Glasgow.

1885, Mar. 24: James S. Gardner, 52 North Frederick Street, Glasgow.
1863, Mar. 18: Robert Gardner, 52 North Frederick Street. Glasgow.

1860, Jan. 18: George T. Hendry, 79 Gt. Clyde St., Glasgow.

1882, Oct. 24: Wm. A. Kinghorn, 6 Colebrooke St., Hillhead, Glasgow.
1864, Dec. 21: Anderson Kirkwood, LL.D., 7 Melville Ter., Stirling.

1878, Oct. 29: John Langlands, 88 Gt. Clyde St., Glasgow.
1884, Feb. 26: C. R. Lemkes, 198 Hope Street, Glasgow.

1873, Feb. 18: John Mayer, F.C.S. 2 Clarinda Terrace, Pollokshields, Glasgow.
1874, Mar. 24: James B. Mercer, Broughton Copper Works, Manchester.

George *Miller, 1 Wellesley Place, Glasgow.
1865, Dec. 20: John Morgan, Springfield House, Bishopbriggs, Glasgow.
1883. Dec. 18: W. M'Ivor Morison, Mayfield, Marine Place, Rothesay.

1886, Jan. 26:	Capt. Dun.	M‘Pherson,	142 Pollok Street, Glasgow.
	James S.	*Napier,	33 Oswald Street, Glasgow.
	John	Phillips,	17 Anderston Quay, Glasgow.
1869, Nov. 23:	Capt. John	Rankine,	31 Airlie Terrace, Pollokshields, Glasgow.
1867, Dec. 11:	William H.	Richardson,	19 Kyle Street, Glasgow.
1882, Dec. 19:	Colin Wm.	Scott,	30 Buchanan St., Glasgow.
1876, Jan. 25:	George	Smith,	45 West Nile St., Glasgow.
	John	*Smith,	Aberdeen Steam Navigation Co., Aberdeen.
	Malcolm	M‘N. *Walker,	45 Clyde Place, Glasgow.
	H. J.	*Watson,	5 Oswald Street, Glasgow.
1882, Dec. 19:	John D.	Young,	141 Buchanan St., Glasgow.
	William	*Young,	Galbraith Street, Stobcross, Glasgow.

GRADUATES.

1884, Dec. 23:	Arthur C.	Auden,	379 Dumbarton Road, Glasgow.
1882, Nov. 28:	William H.	Agnew,	Laird & Coy , Birkenhead.
1880, Nov. 2:	James	Aitken,	2 Lawn Villas, Harringay Road, W. Green, Foltenham, London, N.
1885, Dec. 22:	John Henry	Alexander,	42 Sardinia Terrace, Hillhead, Glasgow.
1880, Feb. 24:	George	Almond,	Belmont, Bolton-le-Moors, Lancashire.

1877, Nov. 20:	James T.	Baxter,	9 Brighton Terrace, Copeland Road, Govan.
1885, Dec. 22:	Peter M'L.	Baxter,	8 Mansfield Place, Blythswood Square, Glasgow.
1883, Dec. 18:	Seymour H.	Beale,	Banbury, Oxon.
1883, Dec. 18:	Ludwig	Benjamin,	20 Allerton Road, Higher Tranmere, Birkenhead.
1882, Feb. 21:	Alfred G.	Berry, jun.,	33 Carnarvon St., Glasgow.
1885, Mar. 24:	Alexander	Bishop,	3 Germiston St., Glasgow.
1885, Oct. 27:	Archibald	Blair,	12 Arthur Street, Glasgow.
1883, Dec. 18:	David	Blair,	Allan Line Works, Mavisbank Quay, Glasgow.
1884, Jan. 22:	George	Blair, jun.,	6 Alfred Terrace, Hillhead, Glasgow.
1884, Jan. 22:	Henry	Blair,	Clutha Ironworks, Glasgow
1885, Oct. 27:	William C.	Borrowman,	15 Breadalbane Street, Glasgow.
1880, Mar. 23:	Alexander	Bowie,	M. Langlands & Sons, Porter Street, Liverpool.
1878, Dec. 17:	Rowland	Brittain,	11 Mount Pleasant Road, Stroud Green, London, N.
1883, Apr. 24:	Arthur R.	Brown,	5 Prince of Wales Terrace, Hillhead.
1879. Feb. 25:	Alex. T.	Brown,	6 Olrig Terrace, Glencairn Drive, Pollokshields, Glasgow.
1883, Dec. 18:	Eben. H.	Brown,	2 Carmichael Street, Govan.
1885, Mar. 24:	Matthew R.	Brown,	Little Park Cottage, Yoker.
1881, Jan. 25:	Matthew T.	Brown, B.Sc.,	7 Bentinck St., Glasgow.
1885, Dec. 22:	Hugh	Brown,	Holmfauldhead, Renfrew Road, Govan.
1876, Dec. 19:	Lindsay	Burnet,	Moore Park Boiler Works, Govan, Glasgow.
1882, Dec. 19:	Hugh	Campbell,	———

1884, Feb. 26:	John	Cleland, B.Sc.,	Woodhead Cottage, Old Monkland.
1881, Nov. 22:	Alfred A. R.	Clinkskill,	1 Holland Place, Glasgow.
1884, Feb. 26:	Alexander	Conner,	9 Scott Street, Glasgow.
1885, Dec. 22:	Benjamin	Conner,	9 Scott Street, Glasgow.
1884, Jan. 22:	Alex. M.	Copeland,	Bellahouston Farm, Paisley Road, Glasgow.
1874, Feb. 24:	Andrew	Corbett,	———
1880, Dec. 21:	Sinclair	Couper,	6 Clayton Terrace, Dennistoun, Glasgow.
1885, Oct. 27:	Francis	Coutts,	96 Shamrock St., Glasgow.
1880, Nov. 23:	James M.	Croom,	Earle's Shipbuilding and Engineering Co., Hull.
1882, Feb. 21:	Wm. S.	Cumming,	Blackhill, by Parkhead, Glasgow.
1882, Mar 21:	Alex.	Cunningham,	———
1884, Jan. 22:	James	Dalziel,	20 Kelvinhaugh Street, Glasgow.
1886, Mar. 23:	Thomas	Danks,	Burgh Chambers, Govan.
1883, Apr. 24:	Alexander	Darling,	Upper Assam Tea Coy., Maijen Dilbrugdah, Upper Assam, India.
1881, Mar. 22:	David	Davidson,	24 Dixon Avenue, Crosshill, Glasgow.
1885, Feb. 24:	William S.	Dawson,	Broomhill Iron Works, Glasgow.
1883, Dec. 18:	William	Denholm,	Hamilton Place, 370 Dumbarton Road, Glasgow.
1883, Feb. 10:	Lewis M. T.	Deveria,	Tharsis Huelva, Spain.
1882, Oct. 24:	Daniel	Douglas,	Earle's Shipbuilding Co., Hull.
1880, Nov. 2:	Geo. C.	Douglas,	Douglas Foundry, Dundee.
1882, Oct. 24:	John F.	Douglas,	18 Meadowpark Street, Dennistoun.

1883, Oct. 23:	Harry W.	Downes,	The Lawn, Twigworth, Gloucester.
1884, Jan. 22:	William	Dunlop,	———
1882, Dec. 19:	A. Von	Eckermann,	———
1885, Mar. 24:	Robert	Elliot, B.Sc.,	The Engineers' Club, 10 Hare Street, Calcutta.
1878, Jan. 22:	James R.	Faill,	Craig-en-Callie, Ayr.
1882, Feb. 21:	Albert E.	Fairman,	Woodlands, Garelochhead,
1880, Dec. 21:	Henry M.	Fellows,	Westbourne Lodge, Great Yarmouth.
1883, Dec. 18:	John James	Ferguson,	———
1884, Jan. 22:	Thomas G.	Ferguson,	14 Queen's Cres., Glasgow.
1881, Feb. 22:	William	Ferguson,	37 Bentinck St., Glasgow.
1885, Jan. 27:	Wm. D.	Ferguson,	———
1881, Nov. 22:	Charles J.	Findlay,	10 Belmont Cres., Hillhead, Glasgow.
1883, Oct. 23:	Duncan	Finlayson,	1 Osborne Place, Govan, Glasgow.
1869, Oct. 26:	F. P.	Fletcher,	South Russell St., Falkirk.
1886, Apr. 27:	John I.	Fraser,	13 Sandyford Pl., Glasgow.
1885, Oct. 27:	Henry G.	Gannaway,	17 Caroline Street, Jarrow-on-Tyne.
1886, Jan. 26:	James G.	Garraway,	13 Govan Road, Glasgow.
1874, Feb. 24:	James	Gillespie,	21 Minerva St., Glasgow.
1884, Dec. 23:	D. C.	Glen, Jun.,	14 Annfield Pl., Glasgow.
1885, Jan. 27:	Alex. M.	Gordon,	3 Wallace Grove Place, Paisley Road, Glasgow.
1882, Jan. 24:	Arthur B.	Gowan,	3 Octavia St., Port-Glasgow.
1884, Feb. 26:	Alexander	Gracie,	9 Great George Street, Hillhead, Glasgow.
1881, Dec. 20:	Andrew	Hamilton,	2 Belmar Terrace, Pollokshields, Glasgow.

1881, Feb. 22:	James	Harvey,	Park Grove Iron Works. Paisley Road, Glasgow.
1883, Feb. 20:	David	Henderson,	11 Hayburn Crescent. Partickhill, Glasgow.
1882, Nov. 28:	F. N.	Henderson,	11 Princes Terrace, Dowanhill, Glasgow.
1881, Oct. 25:	Charles G.	Hepburn,	Ben Boyd Road, Neutral Bay, North Shore, Sydney, N.S.W., Australia.
1882, Feb. 21:	Wm. S.	Herriot,	Leonora, Demerara.
1881, Jan. 25:	A. C.	Holms, jun.,	Hope Park, Partick, Glasgow.
1884, Dec. 23:	John	Howarth,	37 Bentinck St., Glasgow.
1883, Jan. 23:	John A.	Inglis,	23 Park Circus, Glasgow.
1885, Feb. 24:	John	Inglis,	Bonnington Brae, Edinburgh.
1873, Dec. 23:	David	Johnston,	12 York Street, Glasgow.
1883, Feb. 20:	Eben. D.	Kemp,	Overbridge, Govan, Glasgow.
1886, Jan. 26:	John	King,	8 Hamilton Street, Partick.
1885, Feb. 24:	John	Lang,	6 Elderslie St., Glasgow.
1882, Jan. 24:	Andrew	Laing,	Glenavon Ter., Crow Road. Partick, Glasgow.
1886, Jan. 26:	John	Lee,	8 The College, Glasgow.
1883, Nov. 21:	William R.	Lester,	2 Donne Terrace, North Woodside, Glasgow.
1885, Mar. 24:	William	Linton,	3 Radcliffe Rd., Northam. Southampton.
1885, Mar. 24:	Fred.	Lobnitz,	2 Park Terrace, Govan.
1884, Dec. 23:	Robert	Logan,	3 Hayburn Cres., Partick, Glasgow.

1885, Oct. 27: John M.	Malloch,	Carron Iron Works, Falkirk.
1884, Nov. 25: Archd.	M‘Beth,	111 Govan Road, Glasgow.
1880, Nov. 2: Patrick F.	M‘Callum,	Fairbank Cottage, Helensburgh.
1881, Dec. 20: H.	M‘Coll, jun.,	c/o Harland & Wolff, Engine Works, Belfast.
1883, Dec. 18: Peter	M‘Coll,	Stewartville Place, Partick, Glasgow.
1883, Dec. 18: John	MacDonald,	293 New City Road, Glasgow.
1882, Oct. 24: James L.	Macfarlane,	Meadowbank, Torrance.
1883, Dec. 18: John Bow	M‘Gregor,	22 Church Street, Partick, Glasgow.
1882, Dec. 19: Allan	M‘Keand,	———
1880, Feb. 24: Neil	M‘Kechnie,	8 Glenavon Ter., Partick, Glasgow.
1881, Oct. 25: James	Mackenzie,	c/o Messrs D. Rollo & Sons, 10 Fulton St., Liverpool.
1883, Jan. 23: Thos. B.	Mackenzie,	342 Duke Street, Glasgow.
1884, Dec. 23: Jas. M‘E.	M‘Intyre,	The Crescent, Dalmuir.
1883, Feb. 26: Robert	M‘Kinnell,	56 Dundas Street, S.S., Glasgow.
1876, Dec. 19: John	M‘Kirdy,	21 St. James Square, Edinburgh.
1883, Dec. 19: Colin D.,	M‘Lachlan,	3 Rosehill Terrace, South-Queensferry.
1882, Dec. 19: Peter	M‘Lean,	Waverley Ironworks, Galashiels.
1874, Feb. 24: William	Maclean,	Viewfield House, Partick, Glasgow.
1885, Jan. 27: John	M‘Millan,	26 Ashton Ter., Glasgow.
1875, Dec. 21: Allister	M‘Niven,	———
1879, Oct. 28: Donald	M‘Taggart,	48 Overnewton St., Glasgow.
1884, Dec. 23: Robert	Mansel, jun.,	4 Clyde View, Partick.

1884, Dec. 23:	W. J.	Marshall,	3 Minerva Street, Glasgow.
1880, Nov. 2:	Ivan	Mavor,	Wincomlee, Low Walker-on-Tyne.
1882, Jan. 24:	Robt. Alex.	Middleton,	20 Merryland St., Govan, Glasgow.
1884, Nov. 25:	Thomas	Millar,	8 Wilberforce Street, Wallsend-on-Tyne.
1880, Feb. 24:	Robert	Miller,	13 Park Grove Terrace, W., Glasgow.
1883, Dec. 18:	Charles W.	Milne,	7 Carmichael Street, Govan.
1880, Feb. 24:	James F.	Mitchell,	———
1881, Jan. 25:	Ernest W.	Moir.	Forth Bridge Works, South Queensferry.
1882, Feb. 21:	C. J.	Morch,	Horten, Norway.
1882, Nov. 28:	M. J.	Morrison,	8 Annfield Terrace, Partick, Glasgow.
1885, Dec. 22:	William B.	Morrison,	340 Dumbarton Road, Glasgow.
1884, Feb. 26:	Andrew	Munro,	629 Govan Road, Govan, Glasgow.
1878, May 14:	Angus	Murray,	47 Kelvinhaugh Street, Glasgow.
1883, Dec. 18:	James L.	Napier,	22 Salisbury Pl., Hillhead. Glasgow.
1884, Feb. 26:	D. J.	Nevill,	352 St. Vincent Street. Glasgow.
1886, Jan. 26:	Thomas	Nicholson,	6 Annfield Place, Glasgow.
1879, Nov. 25:	Alex. R.	Paton,	Redthorn, Partick, Glasgow.
1884, Feb. 26:	Matthew	Paul, Jun.,	Levenford Works, Dumbarton.
1886, Jan. 26:	Samuel	Paxton,	4 Lorne Terrace, Pollokshields.

1873, Dec. 23:	Edward C.	Peck,	Yarrow & Co., Poplar, London, E.
1881, Oct. 25:	William T.	Philp,	284 Bath Street, Glasgow.
1885, Jan. 27:	James L.	Proudfoot,	154 West George Street, Glasgow.
1885, Feb. 24:	John T.	Ramage,	The Hawthorn's, Bonnington, Edinburgh.
1883, Nov. 21:	Hugh	Reid,	10 Woodside Terrace, Glasgow.
1884, Dec. 23:	James G.	Reid, jun.,	8 W. Princes St., Glasgow.
1884, Feb. 26:	Walter	Reid,	104 Armadale Street East, Glasgow.
1886, Apr. 27:	Robert	Robertson, B.Sc.,	154 West George St., Glasgow.
1882, Nov. 28:	J. M'E.	Ross,	Ravensleigh, Dowanhill Gardens, Glasgow.
1885, Dec. 22:	Chas. A. F.	Rubie,	10 Glasgow St., Hillhead.
1884, Mar. 25:	J. B.	Sanderson,	15 India Street, Glasgow.
1885, Oct. 27:	Alexander	Scobie,	Culdees, Partickhill, Glasgow.
1879, Mar. 25:	John	Scobie,	Samana Railway, Samana, St. Domingo.
1886, Mar. 23:	Thomas R.	Seath,	Sunny Oaks, Langbank.
1886, Mar. 23:	William Y.	Seath,	Sunny Oaks, Langbank.
1880, Apr. 27:	Archibald	Sharp,	City and Guilds of London Institute, Exhibition Rd., London, S.W.
1882, Oct. 24:	John	Sharp,	461 St. Vincent St., Glasgow.
1883, Jan. 23:	Adolph U.	Sheldon,	———
1883, Dec. 18:	George	Simpson,	13 Maxwell Street, Partick, Glasgow.

1877, Mar 20: Nisbet	Sinclair, jun.,	54 La Crosse Place, Hillhead, Glasgow.
1884, Mar. 25: Russell	Sinclair,	c/o J. R. C. Sinclair, 2 West Quay, Greenock.
1882, Nov. 28: Geo. H.,	Slight, jun.,	413 East India Road, London, E.
1881, Nov. 22: John A.	Steven,	12 Royal Crescent, Glasgow.
1881, Jan. 25: William	Stevenson,	R. & J. Hawthorn, St. Peter's Works, Newcastle-on-Tyne.
1873, Dec. 23: John	Stewart,	270 New City Road, Glasgow.
1875, Dec. 21: Andrew	Stirling,	175 North Street, Glasgow.
1884, Dec. 23: David W.	Sturrock,	11 Florence Pl., Glasgow.
1880, Dec. 21: Stanley	Tatham,	Northern Counties Club, Newcastle-on-Tyne.
1883, Dec. 18: Lewis	Taylor,	12 Hillsborough Terrace, Hillhead, Glasgow.
1882, Nov. 28: William	Taylor,	57 St. Vincent Cres., Glasgow.
1880, Nov. 23: George	Thomson,	120 Soho Hill, Handsworth, Birmingham.
1874, Feb. 24: George C.	Thomson,	39 Kersland Terrace, Hillhead, Glasgow.
1884, Dec. 23: John	Thomson, jun.,	15 Burnbank Gardens, Glasgow.
1883, Dec. 18: Nicol	Thomson,	39 Kelvinhaugh Street, Glasgow.
1884, Dec 23: William	Thomson,	15 Burnbank Gardens, Glasgow.
1885, Oct. 27: Peter	Tod,	509 Sauchiehall Street, Glasgow.
1885, Feb. 24: Charles H.	Wannop,	12 Derby Street, Glasgow.

1884, Feb. 26:	William	Warrington,	23 Miller Street, Glasgow.
1881, Mar. 22:	Robert	Watson,	1 Glencairn Drive, Pollokshields, Glasgow.
1880, Apr. 27:	Robert D.	Watt,	Butterfield, Swire, & Co., Shanghai.
1875, Dec. 21:	Richard G.	Webb,	John Fleming & Co., Bombay.
1878, Dec. 17:	Robert L.	Weighton, M.A.,	R. & J. Hawthorn, St. Peter's, Newcastle-on-Tyne.
1884, Apr. 22:	John	Weir,	Ramage & Ferguson, Shipbuilders, Leith.
1886, Apr. 27:	William	Weir,	30 Nithsdale Drive, Pollokshields, Glasgow.
1885, Nov. 24:	James	Welsh,	51 St. Vincent Crescent, Glasgow.
1882, Nov. 28:	Geo. B.	Wemyss,	511 Springburn Rd., Glasgow.
1883, Dec. 18:	John	Whitehead,	10 Witch Rd., Kilmarnock.
1877, Jan. 23:	Robt. John	Wight,	7 Berlin Place, Pollokshields, Glasgow.
1886, Apr. 27:	Percy F. C.	Willcox,	139 Allison St., Glasgow.
1879, Oct. 28:	William	Willox, M.A.,	———
1883, Jan. 23:	John	Wilson,	175 North Street, Glasgow.
1883, Dec. 18:	David	Wood,	124 West Nile Street, Glasgow.
1885, Mar. 24:	Fred. W.	Zucker,	———

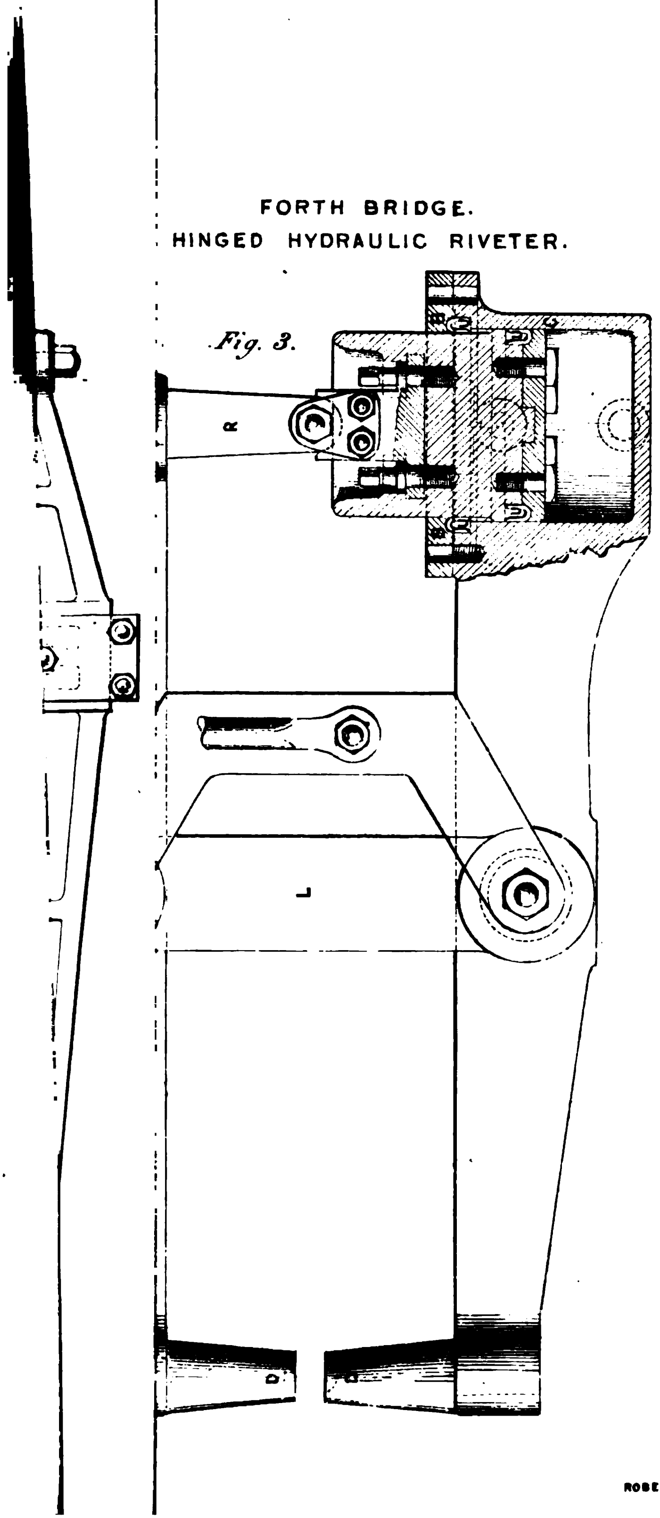
INDEX.

William Munro, General Printer, 80 Gordon Street, Glasgow.

PLATE I

FORTH BRIDGE.
HINGED HYDRAULIC RIVETER.

Fig. 3.



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THE BRIDGE.
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PLATE II

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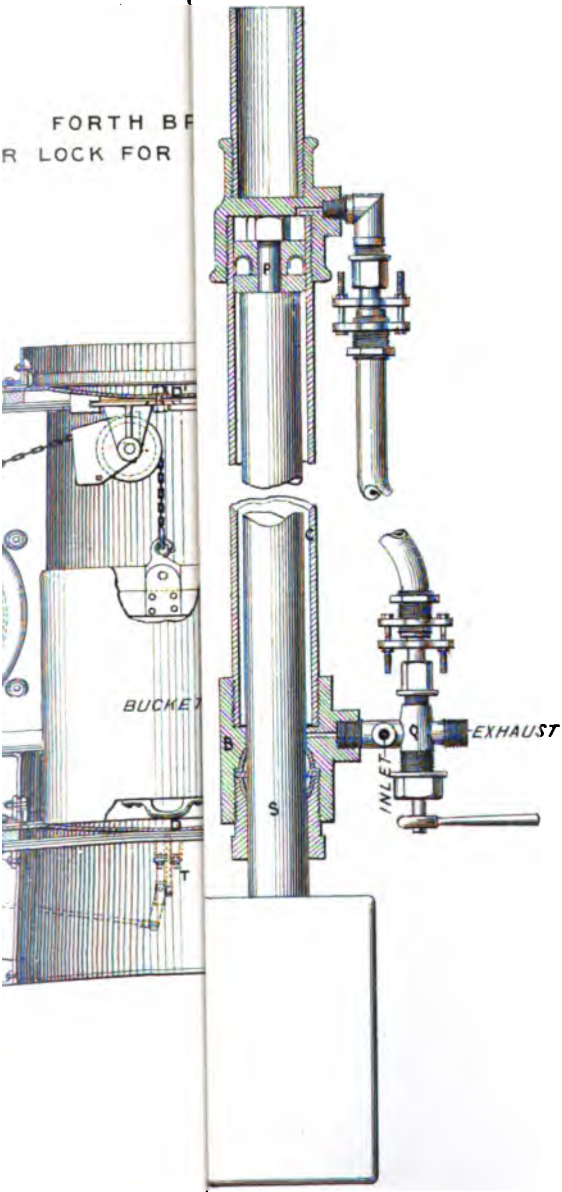

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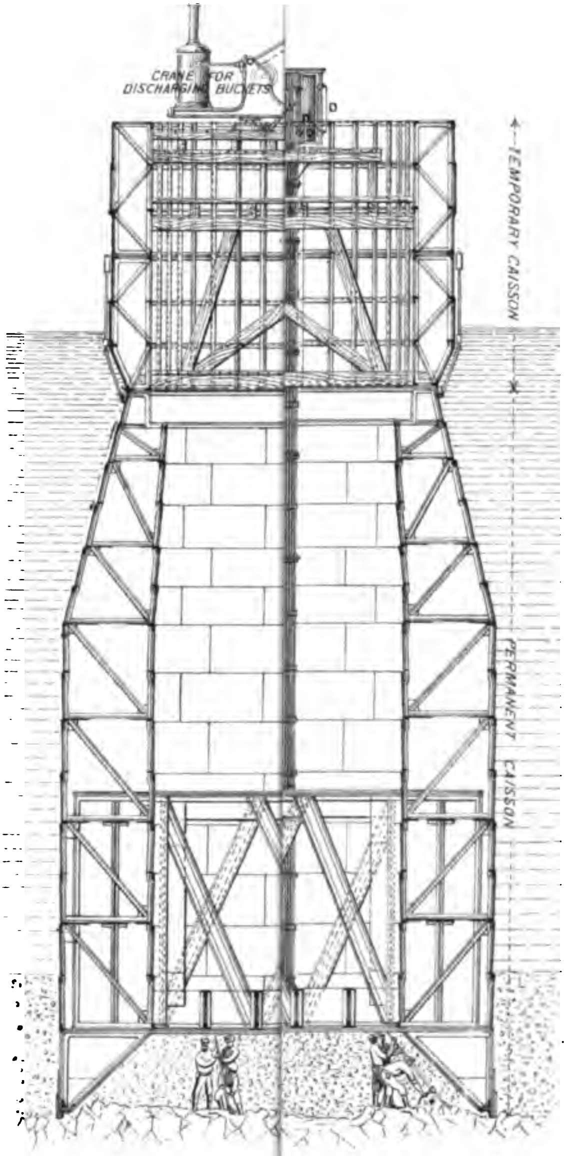

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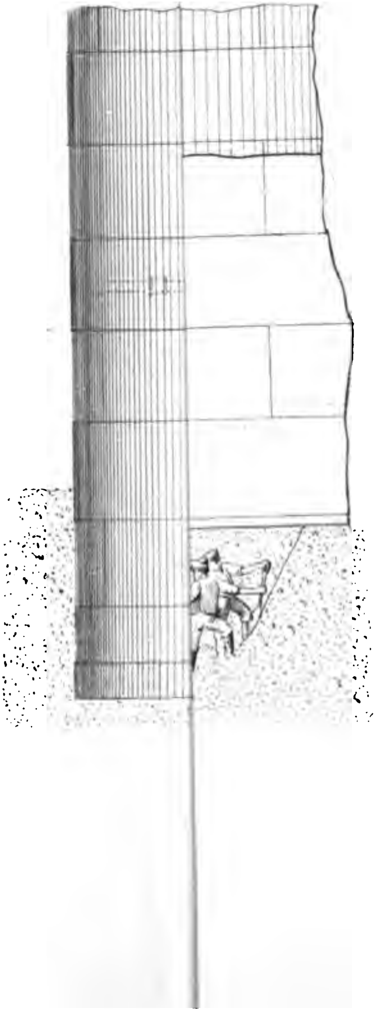
PLATE IV



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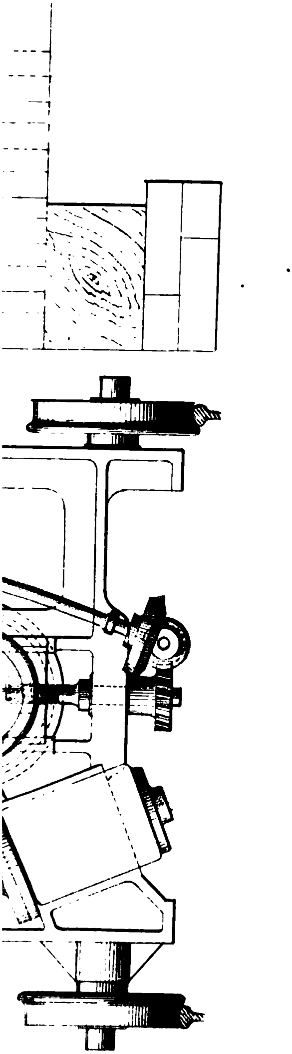
HUR'S PATENT BEVELLING MACHINE

PLATE V



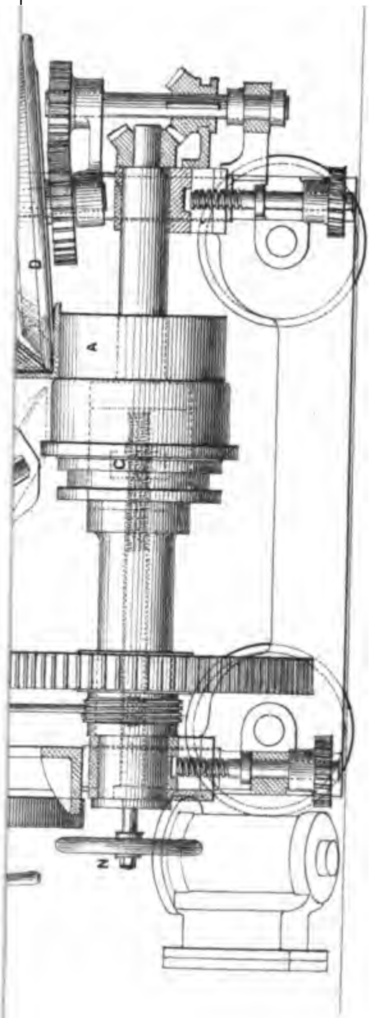


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PLATE VI

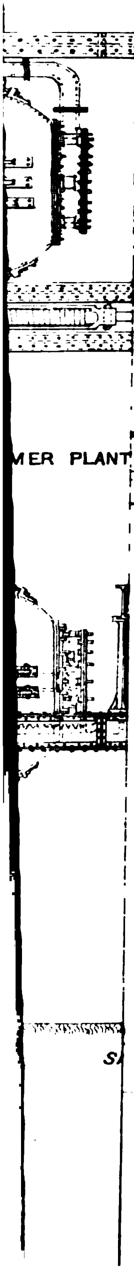
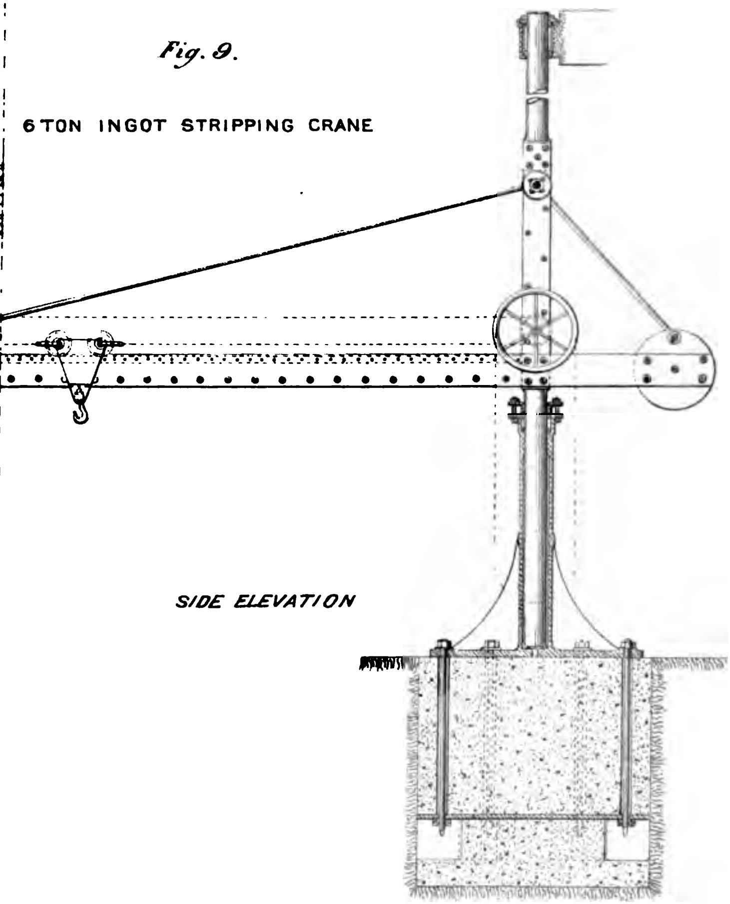


Fig. 9.

6 TON INGOT STRIPPING CRANE



SIDE ELEVATION

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PLATE VII

Fig.

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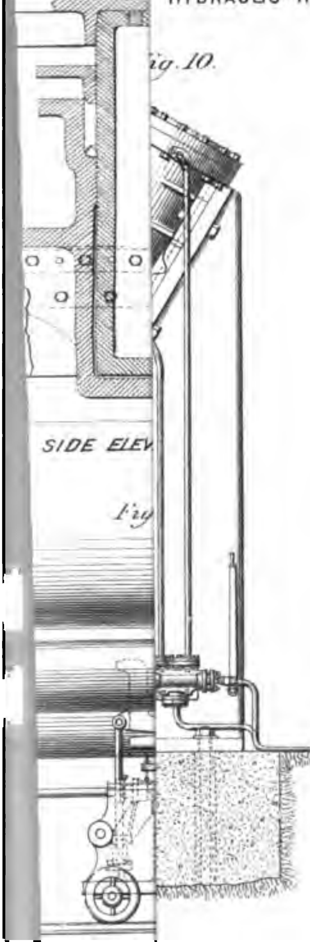

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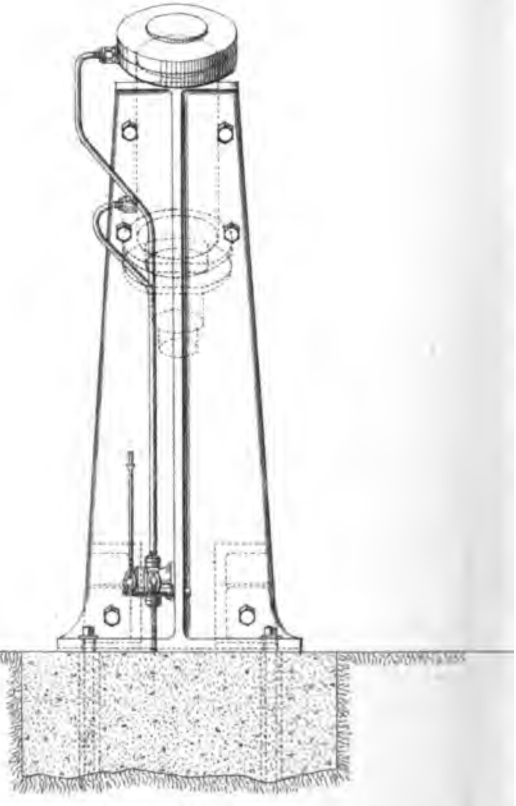

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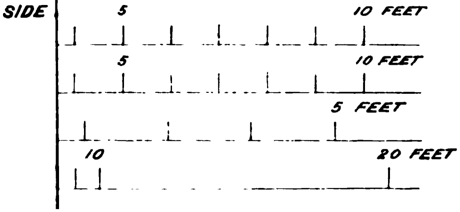

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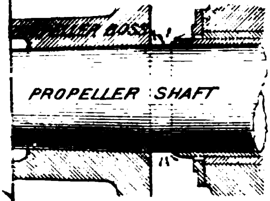
PLATE VIII

Fig. 13.



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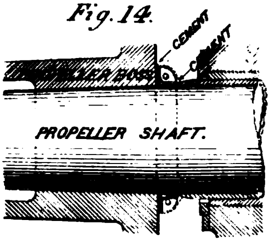

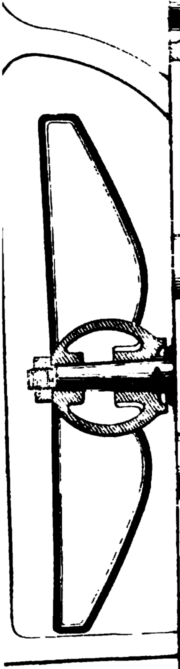

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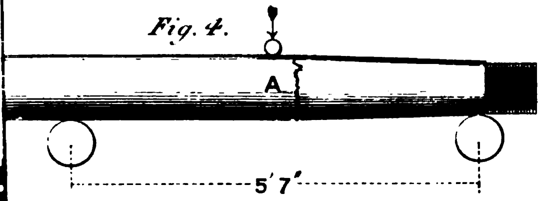

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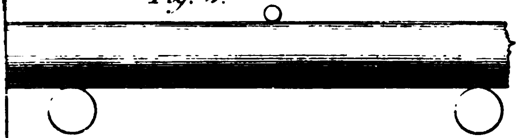

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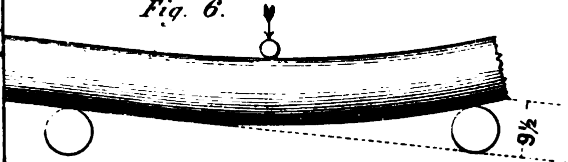


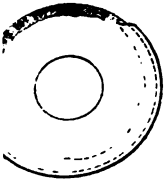
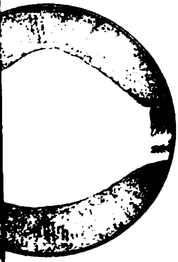


Fig. 10.



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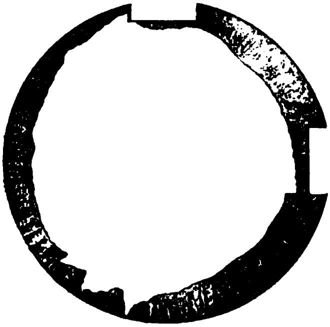

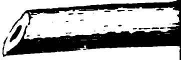

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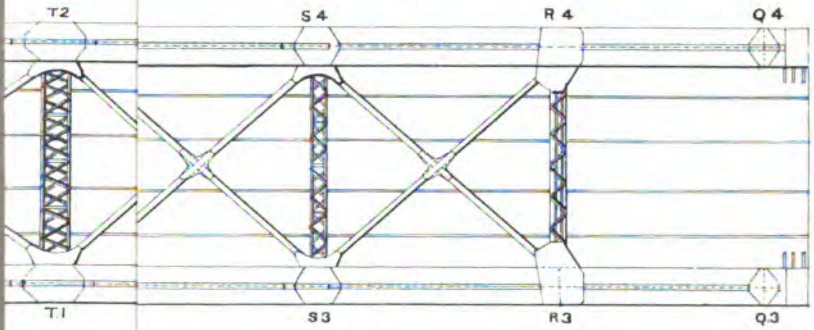
PLATE IX



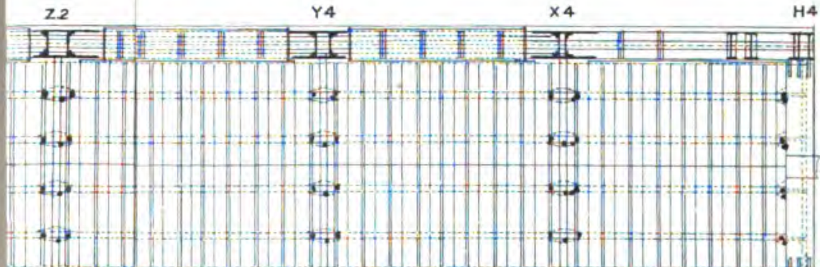


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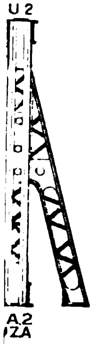
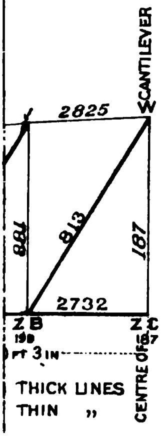
PLATE X





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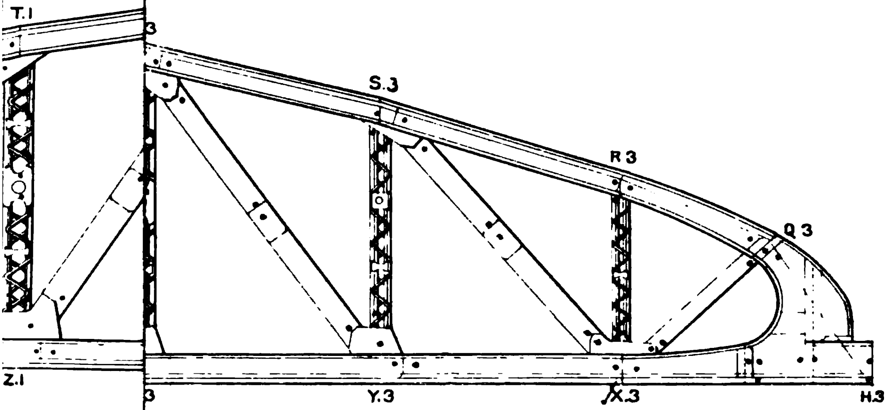
SIDE ELEVATION OF STRUT W Z.B



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