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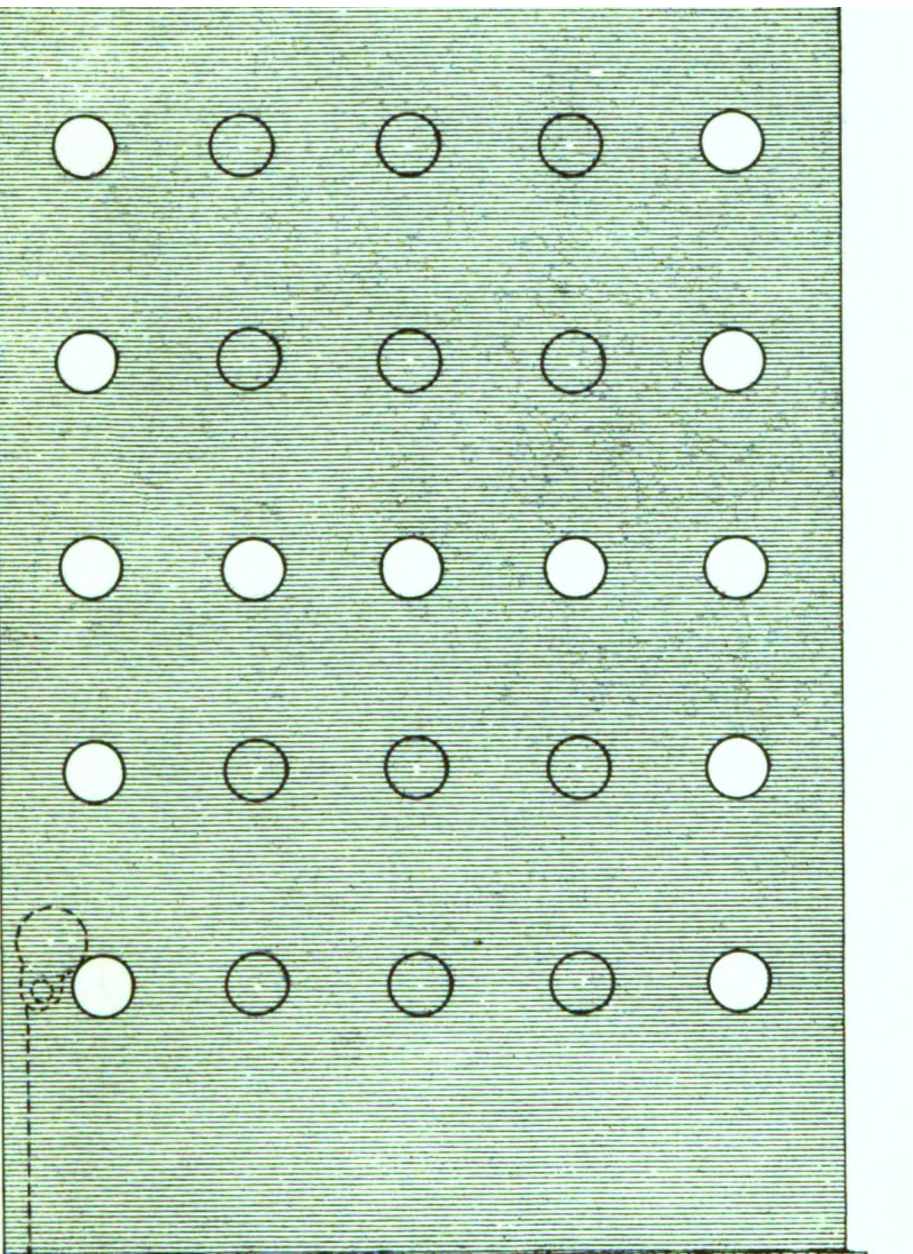
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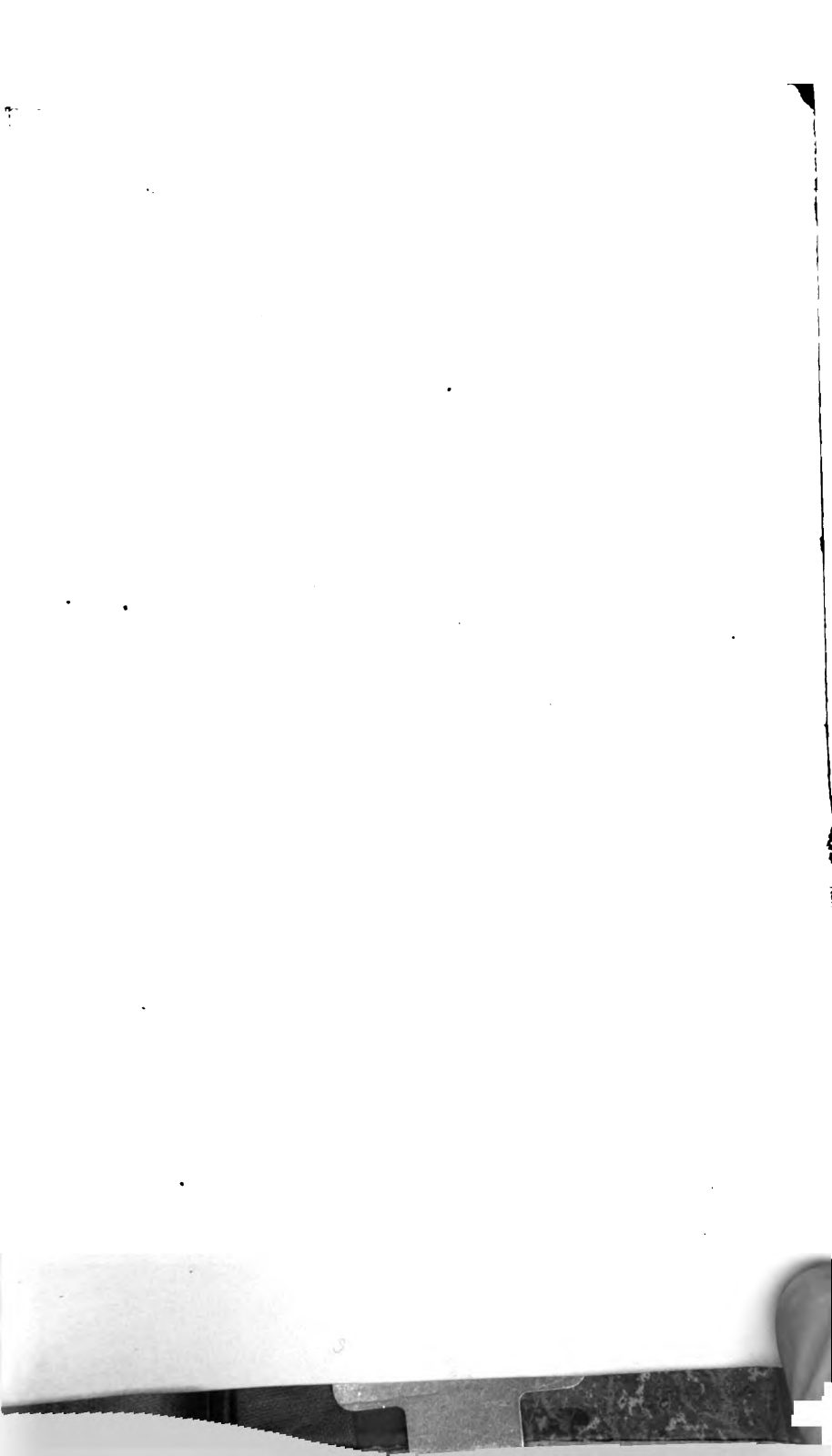
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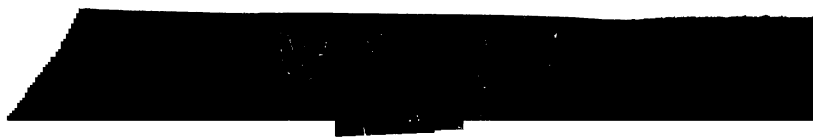
TRANSACTIONS FOR 1863.

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LONDON:

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PREMIUMS FOR 1863.

At the Meeting of the Society, on January 18th, 1864, Premiums of Books were presented to :—

ZERAH COLBURN, for his Paper “On the Ultimate Strength of Iron.”

And to

GEORGE GORDON PAGE, for his Paper “On the Construction of Chelsea Bridge.”

January 12th, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON "STEAM-BOILER EXPLOSIONS."

BY PERRY F. NURSEY.

FROM whatever point of view the subject of "Steam Boiler Explosions" is considered—whether in the office, the works, or the study—a seriousness and earnestness of purpose must ever accompany either practical directions or theoretical deductions professing to ameliorate the evil. An amount of responsibility attaches to the expression of an opinion, either actively or passively, which may influence ultimate results, even to life or death. It often turns upon a very little point whether one of the greatest instruments of peace and prosperity shall not produce results far more fatally disastrous than those issuing from engines of war. The history of steam engineering is accompanied, step by step, by a corresponding history of disaster; and in the present day, notwithstanding the many preventive measures adopted, it is lamentable to find that, on the whole, no greater immunity in respect of boiler accidents is insured than was in years long gone by—indeed, it would almost appear to be far less. Although much has been done, and is still doing, to diminish the probabilities of accident, the best exertions can command but partial success while the primal causes remain shrouded in mystery, for it is notorious that cases are constantly occurring for which the most searching investigation fails to establish an adequate cause. Many theories as to the cause of the violence exhibited have from time to time been called into existence by the characteristics attending various steam-boiler explosions; some of these find support in circumstances which are fatal to others, and *vice versa*, whilst most of them are open to entire objection upon the score of being inconsistent with certain fixed laws of cause and effect.

The records of every passing year tell, with sad persistency, the tale of death upon death, and the destruction of valuable property.

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During the past twelve months these accidents have been especially numerous and fatal. At a cursory glance there are to be found on record more than thirty boiler explosions, each attended with loss of life—in one case six, and in another fatally disastrous one no less than twenty-eight lives were sacrificed. In the previous year, 1861, twenty explosions were known to have happened in various parts of the kingdom, whereby twenty-seven persons were killed and nearly fifty wounded.

It will be as well to notice the various causes from which accidents generally arise. Unequal expansion in different parts of the boiler gives rise to the fracture of plates and angle irons. The bottoms of double-flued boilers internally fired are specially subject to these fractures, which takes place in the transverse or ring seams in the middle of their length, and are more severe in long than in short boilers, especially when the feed is admitted cold and at the bottom. In most cases fractures in the angle irons occur at the upper part of the ring at the furnace crown, and result from the constant buckling action induced by the alternate expansion and contraction in the upper part of the furnace.

There are many instances of internal corrosion, caused by the action of water impregnated with acid. In some cases the whole plate is honeycombed, in others this appearance is confined to small spots and some of the rivet heads. Badly made boilers suffer severely at the seams from the use of corrosive water. External corrosion is attributable to external damp, but it may be caused by water penetrating into the flues and saturating the seating, or from blowing seams and rivets. Those injurious effects are much accelerated when the boiler is in contact with brickwork, which retains the water and holds it against the plates; and oxidation rapidly reduces the thickness of the plates.

Channelling has been attributed to various causes. Cases frequently occur near to the ring seams at the bottom of double-flued boilers, internally fired, under similar circumstances to fractures already noticed. The similarity of their position has led to the supposition that they are the result of oxidation caused by a slight leak at the joint. This may frequently be the case, but instances have been met with where no external damp could be discovered, neither could any

trace of leak be detected. This channelling may be due to disintegration of the metal caused by the constant buckling action in plates in immediate proximity to seams of rivets when submitted to heavy strains. Channelling has also been attributed to the chemical action of the gases consequent on the nature of the fuel used; but it is admitted that the question is one which requires further minute observation, as well as careful consideration of facts recorded.

Safety valves of defective construction are not unfrequent; in some the dangerous practice of passing the spindle through a stuffing-box is still retained, in others the point at which the lever presses on them is placed too high above the face, so that they do not readily seat after blowing; a very bad system is that of *irregularly* loading the safety valve with any pieces of scrap iron or chain that may serve as weights.

The safe working of a boiler depends further upon the fittings for blowing out, the taps and valves for which demand careful attention as to their proper action. Slides or sluices work freely, but are sometimes uncertain in their closing; the lodgment of sediment in the bottom of the boxes will render it impossible to close them. The feed apparatus and glass water gauges (of which, for safety, there should be two to each boiler), also require special care. With regard to the former, Mr. L. E. Fletcher (the chief engineer to the Manchester Boiler Association), observed in his annual report for 1861, that there was a description of feed-stop valve which he found in very general use of an inconvenient and even dangerous character; and he calls attention to the fact, with a view to discontinuing its use in new boilers and correcting it in existing ones. The valve he refers to is constructed on the suspension principle, and is opened against the force of the feed by a spindle attached to it, so that, should the valve at any time break away, it drops to its seat and cuts off the supply. He found several cases where the connexion with the spindle had given way unawares, in some instances causing the stoppage of the works, and, in one case explosion, and loss of life. The remedy he suggests is simple and inexpensive—to dispense with the suspension feed-stop valve altogether, and regulate the supply by controlling the lift of the back pressure valve by a screwed spindle placed above it, but entirely disconnected from it. Mr. Fletcher

notices also a gauge of a novel description, which admits of the interior of the boiler being seen when in actual work; this is accomplished by a glass eye-piece, the boiler being illuminated by means of a light shed through a lens which lights up the whole interior of the boiler and shows the surface of the water in ebullition.

Incrustation may prove a frequent cause of accident, if due precautions to prevent it are not taken. Much inconvenience arises in some districts in this respect, owing to the nature of the water employed; but the evil may be much lessened by proper attention to blowing out, which is far preferable to the use of boiler compositions where the operation is not confined to one point only at the bottom of the boiler.

With regard to the accidents before noticed, which occurred during the year 1861, Mr. Fletcher observes in his able report, already quoted, that from personal examination of several of the boilers after the accidents, and careful consideration of the facts in the other cases, he found that due care and periodical inspection, with the application, where necessary, of the hydraulic test, would have prevented every one of the explosions, and thus that the word "accident" could not be applied to any one of them. No explosion had happened to any boiler under the inspection of the Association during the whole year (1861), although at its close there were under inspection, at 535 factories and other works, 1454 boilers of every variety in mechanical construction. The value and importance of this supervision cannot be overrated when it is considered how many accidents arise from the want of an occasional careful examination, which it is not always possible to expect from the attendants in charge, who, in many instances, have shown a carelessness highly criminal. Witness the recent explosion at Rising Bridge, by which three lives were lost; here the cause of the explosion was clearly corrosion of the plates, which in one part were found to be as thin as a sixpence. No one could have swept the flues without discovering the corrosion, which had been going on for two years. Had the engine-tenter done his duty, he must have detected it.

The locomotive boiler explosion that occurred on the 4th of July, 1861, near Rugby, on the London and North-Western Railway, is another of many instances illustrating the necessity of an occasional

careful examination. In this case the barrel of the boiler was blown to pieces while the fire-box remained almost uninjured. The engine was constructed by Sharp Brothers and Co., and had been working about ten years. It had 142 square feet of heating surface in the fire-box, and 1152 square feet in the tubes, of which there were 195. It was a six-wheeled single engine, with 16 in. cylinders, 22 in. stroke, and 7 ft. driving wheels, was provided with two safety valves $3\frac{1}{2}$ in. diameter, set to a pressure of 100 lb. on the square inch, and a Bourdon's pressure gauge. It ran 196,885 miles between October, 1851, and October, 1857, when it was supplied with a new set of tubes, and ran 139,485 miles between that date and the period of explosion. The barrel of the boiler was composed of $\frac{3}{8}$ in. plate, and the outer shell of the fire-box $\frac{7}{8}$ in. plate. On examination, after the accident, Captain Tyler found that corrosion had been actively going on in several parts, particularly along two seams of rivets, one on the middle ring and the other on the fire-box ring: in some places the metal had been reduced to about 1-16 in. in thickness, at which rupture might be expected at the ordinary working pressure of the engine = 120 lb. The corrosive action which clearly led to the explosion must have set in before the re-tubing in October, 1857, but was probably considered undeserving of notice at that time. Captain Tyler observes that it is more probable that the reduction to about 1-6th of its original thickness, which was apparent in the plate that failed, had been spread over the nine years and eight months that the engine had been at work, and that the plate must have been in a condition to require renewal when the new tubes were put in. If not, then the plate had been reduced in three years and eight months from its original thickness, or nearly, to its then condition; clearly proving the necessity of more frequent inspection. The factor of safety originally allowed in that boiler was only $4\frac{1}{2}$ instead of 6, which, in Captain Tyler's opinion, it ought to have been, inasmuch as the working pressure was 120 lb. and the bursting pressure 520 lb. per square inch; and the full working pressure was maintained while the plates were getting thinner and thinner, until at length the boiler exploded in the ordinary performance of its duty.

It will be a matter of somewhat anxious interest to ascertain the number of boilers of from 9 ft. to 10 ft. 6 in. diameter now at work

in the mining and manufacturing districts. Made originally of $\frac{3}{8}$ in. or $\frac{7}{8}$ in. iron, they are worked, after fifteen or twenty years' use, at pressures of 35 lb. and 40 lb. per square inch, seldom receiving thorough inspection by competent men, and generally attended by ignorant and poorly-paid enginemen. About the middle of April last year one of these huge vessels exploded at Priestfield without warning, and twenty-eight men were killed and ten more severely injured. The boiler was 10 ft. 6 in. diameter and about 30 ft. high, made eight or nine years ago of $\frac{1}{2}$ in. iron, and was considered safe at 50 lb. pressure, being often worked at 40 lb. A few weeks previously a boiler 9 ft. diameter, 21 ft. 3 in. high, made of $\frac{7}{8}$ in. plates, and worked eight years under a pressure of 50 lb., blew up, killing six men. In that case there was little doubt as to the cause of the explosion, for there was a vertical internal flue no less than 4 ft. 8 in. diameter, the nearly flat head of which was not stayed at all. A few years ago a boiler 36 ft. 6 in. long, 9 ft. 1 in. diameter, made of $\frac{3}{8}$ in. plates, and which had been worked for eleven years at a pressure of 40 lb. per square inch, blew up with fearful violence; and a little later a boiler 24 ft. long and 10 ft. diameter, made eighteen years before of $\frac{3}{8}$ in. plates, exploded at Tipton.

In these explosions, however, there is little, if anything mysterious. Out of all proportion in size, made of ordinary and sometimes inferior iron, old, badly tended, and, in the mining districts, often exposed to the insidious action of corrosive water, what is to be expected in these huge bombshells but ultimate explosion?

Returning to more recent catastrophes, note the fearful explosion at High Moor Farm, near Alnwick, which resulted in seven deaths and as many severe injuries. The boiler appears to have been a very old one, and was carried completely through the roof of the building, which was knocked into ruins. The boiler was hurled to a height of forty feet over a straw barn, falling in the yard on the other side. Then the explosion at the Midland Ironworks, Masborough, by which a number of men were killed. Several boilers of various dimensions were fixed in the rolling mill, one of these—nearly the largest—was embedded between two smaller ones about eighty yards from the entrance gates. They were under a shed covered with sheet-iron and slate, and supported by wooden and iron beams crossed

upon iron pillars. Work was proceeding as usual, when, with a tremendous report, the boiler launched itself forward into the mill, and in an instant the whole place was in ruins. The two large boiler tubes were forced to the rear of the premises. The supports of the roof were broken, solid iron columns 12 in. or 14 in. thick, snapping short off. The collapsed tubes and numerous rents in the boiler indicated a want of the proper supply of water.

A Great Western passenger engine blew up while steam was being raised on it in the Paddington shed, on the morning of 8th November last. A North-Western goods-engine exploded on the 5th of May previous, and a North-Eastern engine met a like fate at Stella Gill in October, 1861.

Each of these three last cases could be readily traced to similar causes to those mentioned in the case of the Rugby explosion, namely, corrosion of the plates—and it is by no means unusual for locomotive boilers to fail in this way. It is a noticeable fact that these explosions are common only to certain lines of railways, while others are exempt from them. The question, how this is to be accounted for, very naturally suggests itself, and finds a solution in the fact that, on the Eastern Counties, South-Eastern, Lancashire and Yorkshire, Caledonian, Edinburgh and Glasgow, and some other lines on which boiler explosions are unknown, the hydraulic test has, for some time, been employed for ascertaining the soundness of boilers; hence it happens that boilers do not blow up on these lines; whereas, on the Great Western, London and North-Western, Great Northern, Midland, North-Eastern, and some other great lines, where this test is not adopted, such disasters are becoming somewhat common. The objection raised by experienced locomotive engineers to the hydraulic test is that it is likely to injure the boiler. Injury would, doubtless, result if a boiler were once strained beyond the limit of elasticity of the iron. But a boiler, the bursting pressure of which was, as it ought to be, 600 lb. per square inch, could not be injured by a temporary water pressure of 250 lb. At the same time, to ascertain the actual strength of a boiler, it must be burst open experimentally; and, therefore, the boiler, supposed to be capable of bearing 600 lb. pressure before giving way might really have a strength only equal to 300 lb., in which case a test pressure of

250 lb., although it might not actually rupture the boiler, would undoubtedly injure it, and thus hasten the explosion, which, even without the test, would have been likely, sooner or later, to have taken place. Notwithstanding, therefore, that the hydraulic test is extensively and successfully used, there may be reason, in some cases, for hesitation in adopting it. And yet, again, there need be no grounds for apprehension on this point, for—to follow the sound arguments and practical suggestions advanced by the able writer of a recent article in *The Engineer*—it is well known that iron is only injured by injuring its elasticity, and its elasticity is injured only when it shows, after being strained, permanent elongation of the fibres, and this may be produced by a strain somewhere between one-third and one-half its breaking strength. Thus, a boiler, the bursting strength of which was 800 lb., would be found to have had its diameter slightly but permanently increased under a strain of 400 lb.; under a strain of 250 lb. the boiler would remain safe under a steam pressure of 150 lb. per square inch. But why not adopt the practice of measuring the diameter of boilers under successive increments of strain? Take the exact diameter of the largest ring of a boiler charged with cold water to a pressure of 100 lb.; measure it again, with the pressure increased successively to 150 lb., 200 lb., 250 lb., and so on. Then ease off the pressure, and note to what extent the iron has been permanently stretched. If at 100 lb. pressure, or when free from strain, the diameter is exactly what it was before the test, the boiler is safe with any ordinary load on the safety valves. If permanently enlarged to any considerable extent, the boiler needs immediate strengthening. Iron bridges, before being opened for traffic, are loaded with the greatest weight that can ever come upon them, the deflection of the arches or girders is carefully noted, the amount of permanent set—if any—ascertained, and their safety is inferred not so much from the fact that they have survived the ordeal, but from the amount and character of their deflections. Every chain cable received into the Royal Navy is tested, and the action and effects of the test strain are exactly analogous to those of the hydraulic test for boilers. In France all wrought-iron boilers are required by law to be tested up to three times their working pressure by the hydraulic pump, and cast-iron boilers up to five times. So in America, all Government

boilers must bear a hydraulic pressure two-thirds greater than that at which they are intended to work.

Wrought-iron—following the same writer—it is true, elongates but from the $\frac{1}{800}$ th to the $\frac{1}{1000}$ th of its own length before the limit of elasticity is reached, and at this rate a boiler 12 ft. 6 in. in circumference would be enlarged only from $\frac{1}{12}$ th to $\frac{1}{10}$ th of an inch in diameter; but with even much less than this small alteration, the difference should be readily observable and measurable, with a thin steel wire placed around the boiler, and with its ends lapping over each other, the extension of the circumference of the boiler by from $\frac{1}{10}$ in. to $\frac{1}{8}$ in. would be very readily seen. That the test by the force-pump must be a useful one, it requires but little consideration to perceive; it may be safely, cheaply, and expeditiously applied; and there is no fear of the boiler bursting from weakness, original or produced, so long as careful measurement—both before and after the test—discloses no permanent distension.

In most instances of steam-boiler explosion but little difficulty exists in determining the immediate cause, although cases do occur in which no sufficient reason can be assigned. But there is a question underlying this which has, as yet, received no determinate solution at the hands of those whose disquisitions on the subject entitle them to be considered authorities. To what cause shall be attributed the violence attending a boiler explosion? To what agency shall be ascribed the partial demolition of a bridge by huge fragments of an exploded locomotive, as in the Rugby accident? How account for the flight of a boiler 40 ft. over a straw barn, as at High Moor Farm? or the snapping asunder of 14 in. solid cast-iron columns by a huge boiler launching itself forwards into a rolling mill many yards from its bed, as at the Midland Ironworks? Granted that the cause of explosion is correctly traced to the groove which, in the Rugby case, had been gradually eaten into one side of the middle ring of plates in the barrel of the boiler. The explosion of the boiler, thus weakened, would appear inevitable; and yet, an accident of frequent occurrence to steam-boilers is the mere rupture of corroded or otherwise weakened plates, or whole seams of rivets, but which accidents are generally harmless. But, then, they take place generally in the lower plates or seams of the boiler, well under water. If, in some cases, the local

weakness of a plate produces an explosion, why not in all, or nearly all, the general circumstances being the same? On the other hand, why should the rupture of any seam or plate produce a violent explosion at all? It is certain that the pressure of a jack screw within the boiler, pushing outwardly with the same force as the steam, would produce no explosion. It is equally certain that hydrostatic pressure, to the same extent, would merely open the plate and allow the water quietly to escape. Likewise, a boiler full of steam only, whether of 100 lb. or 500 lb. pressure per square inch, could never produce the wreck and ruin attendant on all violent explosions; there is not sufficient power in 150 cubic feet of high-pressure steam to account for these dire effects. Such a quantity of dry steam would, probably, have escaped harmlessly into the air before it could have imparted momentum to the heavy masses actually projected to considerable distances—there could be no exhibition of that terrible energy which characterises a true boiler explosion. The cause, then, of explosions is to be sought in some circumstance beyond the mere pressure of steam, the effect of which would rather be comparatively harmless rupture.

Mr. D. K. Clarke, in the *Mechanics' Magazine* in February, 1860 (which the author read), suggested the momentum of combined steam and water as being the cause of violent explosions. But perhaps no man of the present day has done more towards determining this question than Zerah Colburn. In a treatise, in which the subject is carefully investigated, he has elaborated Mr. Clark's idea, and has clearly and consistently disposed of all the arguments upon which were based the favourite hypotheses of superheated steam, electricity, explosive gases, spheroidal water, overpressure, and water freed from air, and has successfully established, upon perfectly tenable evidence, the indisputable fact that the violent explosion of steam boilers is mainly due to the combined percussion of the steam and water leaping up on the sudden removal of the steam previously resting on the surface of the water.

Taking, then, the views expressed by Mr. Colburn, it is to be observed, with regard to overheating, that although it is possible a boiler may be exploded by the formation of a great quantity of steam from water thrown upon red hot plates, overheating cannot be

assumed to be the general cause of explosions. Cases have occurred where a moment previously the gauges indicated an ample supply of water. In these, as well as in cases where there has been no positive evidence as to the amount of water in the boiler, the furnace plates have been found without any appearance of having been burnt. But assuming that water was suddenly thrown upon overheated plates, it is doubtful if the quantity of steam disengaged would be sufficient to increase greatly the pressure already within the boiler. In *The Engineer* of April 3rd, 1857, it is stated in a letter that an empty boiler, 25 ft. long, 6 ft. diameter, and with the safety valve loaded to 60 lb. per square inch, was made red hot. While in this state the feed was suddenly let on, and the boiler filled up. The experimenters were surprised at the result, which was simply a sudden contraction of the overheated iron, which caused the water to pour out in streams at every seam and rivet as far up as the fire mark extended. A consideration of some of the phenomena of heat leads to the conclusion that a red hot boiler may be as safely filled with cold water as a hot railway tyre may be quenched in a cistern. The metallic plates of a steam boiler are not capable of containing sufficient heat to change a very large quantity of water into steam. The total quantity of heat which would raise the temperature of 1 cwt. of iron through 1 deg., would, according to the best authorities, impart the same additional temperature to 12½ lb. only of water, and the quantity of heat which would raise the temperature of 1 cwt. of copper through 1 deg., would raise that of 10½ lb. only of water to the same extent. That overheating is not the sole cause of an explosion is clear, although it may lead to a rupture by weakening the plates.

With respect to the spheroidal state of water when thrown upon heated plates, it is observed that if ebullition were delayed until after a considerable quantity of water had been admitted, the heat of the plates would be so far absorbed in an equal or greater weight of water, that no explosion of the latter into steam could occur, thus rendering the spheroidal condition of water, under these circumstances, rather an argument against, than in favour of, the probability of explosion.

Many engineers hold that the presence of highly superheated steam within a boiler is sufficient to account for the most violent explosions

but neither by experiment nor by calculation has it ever been proved that such a result could arise from such a cause. It was the theory of Jacob Perkins (who heated steam out of contact with water to very high temperatures) that steam, being similarly superheated when the water in a boiler was low, the subsequent agitation of the water from any cause instantly produced a large additional quantity of steam, sufficient to cause an explosion. Another writer on the subject has assumed that ordinary steam, superheated to about 435 deg., would instantly convert water thrown among it into steam of a pressure of 360 lb. per square inch. But the fact must have been overlooked that 75 cubic feet of 140 lb. steam weigh but 26 lb., and that at ordinary temperatures the specific heat of steam is less than one-third that of water. Therefore, supposing the steam of a locomotive boiler superheated even to 350 deg. above the temperature due to its pressure, all the heat contained in 26 lb. of such steam could not generate much more than 3 lb. of additional steam, which would not raise the pressure at 140 lb. to more than 160 lb. to the square inch. Hence it is apparent that the conversion of water into steam, by being thrown up in a divided state, into highly superheated steam, is not sufficient of itself to account for any boiler explosion.

The electrical hypothesis was probably built upon the fact that electrical properties are sometimes developed by the discharge of steam into the air. But it would be impossible for electricity—if it were generated by ebullition or in confined steam—to collect within a boiler which is in direct communication with the earth. Professor Faraday's examination of Armstrong's hydro-electric engine, which produced electricity by the discharge of steam through a series of nozzles bushed with box-wood, afforded no evidence of electricity within the boiler, which was perfectly insulated. But he found that by using spring water instead of distilled water, as used by Armstrong, and by using other than box-wood nozzles at the points of discharge, no electricity whatever was obtained; and, further, that on discharging compressed air through box-wood nozzles the same results were obtained as from steam discharged under similar circumstances. If, as it has been argued, the boiler should become insulated by an internal coating of boiler scale, it would be necessary that the scale should completely cover every atom of the internal surfaces

of not only the boiler but its fittings also. But, perhaps, the best answer to the whole question is to be found in the fact that water may be boiled in a perfect Leyden arrangement, without the slightest development of electricity.

The decomposition of water by heat on a large scale, for the purpose of applying its elementary gases separately, has generally been attended with very unsatisfactory results. But this has not prevented the propagation of the fallacy that, under certain conditions, all the steam in a boiler is decomposed, and the liberated hydrogen violently exploded. This question was fully discussed by the author in a paper on superheated steam, in April, 1861, in which are included the elaborate reports of Dr. Taylor and Professor Brande, whom the patentees of a superheating apparatus had consulted; likewise that of Professor Faraday, who had been instructed by the Board of Trade to report upon the safety of the same apparatus. The question arose from the steam being superheated in pipes passing through the boiler furnace, in which case it was contended by some that the steam would become decomposed, and a violent explosion result. That steam is decomposed by being passed over red-hot iron is correct; but the iron absorbs and retains all the disengaged oxygen, and with the iron once oxidised decomposition ceases. And, further, hydrogen is not explosive, but simply combustible; and if even liberated, as suggested, its combustibility would not be manifested amid the enormous quantity of aqueous vapour liberated with and condensed around it. There is, therefore, no support whatever to the explanation of boiler explosions by the decomposition of steam.

An over-pressure is a pressure which exceeds the safe working pressure, and it may be generated either gradually or momentarily in a boiler; and, strictly speaking, there must always be over-pressure whenever a boiler is burst. When, however, an explosion is said to have occurred from over-pressure, it is generally understood that the pressure has been allowed to increase gradually up to the limit of the strength of the boiler, and if this has been calculated to correspond to a pressure of, say, 700 lb. per square inch, the actual pressure at the moment of explosion is accordingly assumed at that amount. Boilers may perhaps be generally capable of withstanding nearly their full calculated bursting pressures. But there are numerous instances of

the quiet rupture of steam boilers under ordinary working pressures, so that even a violent explosion does not absolutely prove that the pressure under which it occurred was anything like the calculated bursting pressure of the boiler. Suppose the bursting pressure to be 758 lb. per square inch, steam might be raised to that point and burst the boiler. But it is very improbable that anything like a pressure of 758 lb. ever accumulates in a boiler intended to work at 100 lb. or 125 lb. per square inch. If boilers burst only from over-pressure, they would of course give out first in the weakest part—say, along a seam of rivets. But after the seam had opened, the relief of pressure would be so instantaneous that, without subsequent percussive action, the rupture could hardly extend itself through solid plates of twice, or even ten times, the strength of the part which first gave way.

Repeating the original question: To what cause shall be attributed the violence attending a boiler explosion? Mr. Colburn, who has demolished the superficial theories of those who prefer mystery—or, at the least, obscure explanations, to circumstantial investigation, has not left the question there, but has raised upon no insecure foundation a theory well supported by science, and which effectually meets the proposition. His reasoning is conclusive. In all boiler explosions the pressure of steam is instantaneously liberated from the surface of the hot water present. If the boiler be at work at a pressure of 45 lb., the water must be at a temperature of about 290 deg. It is presumed to be sufficiently well known that fresh water cannot for an instant be maintained at a temperature much greater than 212 deg. under the ordinary atmospheric pressure. It may be true that, with great care and delicate apparatus, water may be heated to a little above this point without boiling, but, upon the least agitation, a portion will instantly flash into steam, and the temperature of the water will fall to 212 deg. For all practical purposes a body of fresh water cannot be kept a moment in the open air at a temperature above 212 deg., while, if the pressure upon it be liberated when it is heated to, say, 290 deg., a most violent disengagement of steam and projection of water along with it must inevitably take place. The force with which disengaging steam must carry water before it and along with it, must exceed any known force of the waves of the sea, not-

withstanding that the waves occasionally beat down some of the strongest constructions of man.

High-pressure steam rushes into the air with a velocity of from 1500 ft. to 2000 ft. per second—a fact not only demonstrable from natural principles, but proved by the exhaustion within the fiftieth part of a single second of a cylinder full of steam in locomotives running at high speeds. The force with which it must hurl masses of water on all sides, like the fragments of an exploding shell, may be conjectured from this fact; and it is well known that, when moving with great velocity, a mass of water will strike with nearly the force of an equal weight of iron. For in firing a bullet from a gun down upon a surface of water—the concussion being necessarily identical with that which would result if the water were made to strike with the same velocity upon a stationary bullet—the bullet is flattened.

Inasmuch, then, as all boilers are constantly liable to rupture from original unsoundness of the iron, bad riveting, corrosion by bad water, or by the somewhat mysterious action of “furrowing,” what is to be expected when, the steam pressure being thus suddenly liberated through a rent of moderate dimensions, 30, 40, or even 60 tons of heated water are waiting below to burst partly into steam?

It is clear, then, that there is always more or less danger in working boilers which are made to contain a great quantity of water. Safety lies in the use of numerous small water spaces, as in the duplicate retort boiler, the water-tube boiler, &c. In the latter class of boilers one or more tubes, where they happen to have been originally unsound, will now and then burst open, but so limited is the quantity of explosive water (as it may justly be termed when highly heated), that nothing like a violent explosion, disturbing the boiler as a whole, ever takes place in such cases.

According to Mr. Colburn, the distinct and consecutive operations into which a boiler explosion, although practically instantaneous, may probably be resolved, are, therefore:

1. The rupture, under hardly, if any more than, the ordinary working pressure of a defective portion of the shell of the boiler—a portion not much, if at all, below the water line.
2. The escape of the free steam from the steam chamber, and the

consequent removal of a considerable part of the pressure upon the water, before its contained heat can overcome its inertia and permit the disengagement of additional steam.

3. The projection of steam, combined as it necessarily must be with the water, with great velocity, and through a greater or less space upon the upper sides of the shell of the boiler, which is thus forced completely open, and perhaps broken in pieces.

4. The subsequent disengagement of a large quantity of steam from the heated water now no longer confined within the boiler, and the consequent projection of the already separated parts of the boiler to a greater or less distance.

Master minds have dealt so comprehensively with the subject of steam-boiler explosions, and it has been so well ventilated of late, that it is well nigh exhausted, and little, if any room, left for further comment. The present paper has no pretensions to originality. Taking advantage of opportunity, it simply proposes to separate fallacies from correct principles, and seeks to disseminate such opinions as are reconcileable with reason and supported by known facts. It was undertaken at a short notice, and draws largely upon Mr. Colburn's able exposition. An important conclusion, however, is clearly deducible—viz. that the general adoption of frequent and careful examinations, such as are made by the Manchester Boiler Association, together with the occasional application of the hydraulic test, would combine to effect a very considerable reduction in the number of fatal explosions, if it did not, indeed, wholly prevent them. Unless this course is adopted, the category of misfortune will go on swelling its dark pages until authority shall step in and, overriding obliquity, compel that which moral obligation ought to insure..

DISCUSSION.

Mr. E. REYNOLDS said the subject had been so fully treated in Mr. Nursey's excellent paper, that he did not think he could throw any additional light on the subject, but there were some few matters that might, perhaps, be usefully discussed. It appeared to him that it was more a matter of interest than of practical value, to trace the precise way in which the effects of boiler explosions were developed, though in general he had been more surprised at the smallness of the

results than the reverse. Of course, the prevention of explosion was the result to be aimed at, and he believed that there was no well-authenticated instance of explosion of a really good boiler, in good order, without some easily ascertainable cause. Such cases as have arisen from actual neglect, such as deficiency of water, or tampering with safety valves, or injudicious construction, and deficiency of stays, were sufficiently touched upon in the paper; he would, therefore, confine his remarks to those which care in attendance could not provide against, such as unequal expansion, and the most treacherous of all diseases that boilers were liable to—*i. e.* furrowing. He believed that danger seldom arose from the effects of unequal expansion, except where it induced furrowing without attracting notice. In its direct effect it was usually confined to producing injury and leakage from opening of seams, &c., rather than danger. The opening of seams in plates over the fires of Cornish boilers, and those of the under side of the shells of long double-fueled boilers, if the cold water was made to impinge upon them, were common instances of its operation; but if the consequent leakage were allowed to exist long enough to cause extensive corrosion of the plates, then the danger came under the head of neglect. The same may be said of the effect of acids, &c., in the water used. It was scarcely likely that any water possessing sufficient corrosive powers to cause danger within the period that should elapse between examinations, could be used without its being suspected, if any care was taken in the matter at all. As to furrowing, which might generally be traced to the action of local alternating strains, operating in a manner which had been frequently explained, whether the local strain was produced by the steam in its endeavour to draw an overlapped seam into the line of pressure, or by machinery, or brackets fixed to a boiler, it was seldom so rapid as to render its detection difficult; but whenever the first trace of it is observed, the part should immediately be repaired, as the bending action of the strains which induced the furrowing would obviously go on in an increasing ratio as the plates got locally thinner.

He thought too much importance was attached to the hydraulic test for working boilers. It might be useful for ascertaining the absolute strength, and in new or repaired work was very useful; but

as a means of detecting local weakness, unless the weak parts were actually ruptured by the test, he thought it might be deceptive; for, as in cases of furrowing, the furrowed part might not exceed one inch in the 12 ft. circumference of a locomotive boiler—the excessive strain on this small part could scarcely be detected by the increase in the whole circumference of the boiler, though possibly the furrowed part might have been strained within one per cent. of bursting.

Another point to be remembered was, that to be of any utility the test must be much above the working pressure, and in the special case of locomotive boilers, they needed the test most after they had been some time at work, when the tubes were getting thin, and would not bear excessive pressure.

His own practice in examining boilers, was to test by hydraulic pressure all new or repaired boilers, and in other cases to drill holes wherever there were any indications of weakness, so as to ascertain positively what amount of strength was left for work.

He thought that there was too great a tendency at the present day, to use excessive pressures for large boilers for condensing land engines, where it was seldom productive of either economy or convenience. Where high-pressure was really required, as in locomotives, and other non-condensing engines, he thought a system of more careful supervision, and more frequent examination than had hitherto been prevalent, was highly desirable.

Mr. J. LACEY had heard that old boilers had been known to burst simply from the effects of electricity, which he thought might be accounted for by the insulation of the corrosion on the inside of the boiler. By the chemical action of steam, positive electricity was induced, which would produce an explosion in attempting to gain an equilibrium.

Mr. OLRICK said that, with reference to this point Mr. Colburn, in his work on boiler explosions, had given Mr. Faraday's report, and the deduction drawn from that was, that no production of electricity was due to any change in the state of the liquid contained within the boiler, neither does the current of the steam produce any current of electricity. The electricity that could be produced, was only outside the boiler, and even in that case it could only be produced where there was a boxwood nozzle, and where that

nozzle was wet, the larger the pressure was the more friction there would be, and consequently the more electricity would be produced. As one of the professors had made careful researches, and had proved that it was impossible to produce electricity in boilers, they must bend to his opinion. The best engineers had not been able to prove anything against Mr. Faraday's researches. Mr. Colburn agreed so thoroughly with Mr. Faraday, that there was not a question about it. It was impossible to conceive that a violent boiler explosion could occur from any other causes than those given in Mr. Colburn's pamphlet, which was the most engineer-like explanation he had ever seen. It was explained by an engineer, and not a learned professor; and he should certainly, in a question of so much importance, sooner listen to the engineer than to a scientific professor, because there were many points which a scientific professor did not know, and consequently could not take notice of. He (Mr. Olrick) would offer a few remarks in respect to a recent boiler explosion noticed in Mr. Fletcher's report, recently published. The jury came to the conclusion that it was a boiler explosion owing to shortness of water. Mr. Fletcher, who was a boiler maker and engineer, came to quite a different conclusion. He said there had been no overheating of the boiler plates, and no injury from that cause. The boiler was 25 ft. long, the outer shell 6 ft. diameter, and the inner shell 2 ft. 3 in. The working pressure of that boiler was 70 lb. Having made a calculation, he (Mr. Olrick) found that the pressure at which it would collapse was 168 lb., that was if the boiler was new and there was no fault with the iron or the workmanship, and as they ought never to work with less than 6, 8, or even 10 for marine boilers as a factor of safety, every one would see that it was entirely wrong to work it at 70 lb., especially as the boiler was not new, so that it was easily explained why that boiler burst. The shell had a bursting pressure of 333 lb., and consequently the factor of safety was less than 5 instead of 6 or 8. There were many small points to be considered, which were often the cause of explosions. For instance, they would find many boilers where there were no proper back pressure valves between the feed-pipes and the boilers. In many cases of cheap boilers, delivered at prices of from 15% to 20% per ton, there was no such thing as a back pressure valve. In steam-ships, where there were donkey

pumps with fixed valves, moved by eccentrics; it often happened, instead of pumping water into the boiler, it was pumped out; it was therefore necessary whenever there was fixed, instead of loose valves, to be careful in starting these donkey engines. The safety valve was often made too small. The correct way was to make it in proportion to the coals burnt, or the heating surface. The safety valve ought to be so large that it could discharge easily the utmost amount of steam which the boiler could produce, and instead of one, have two safety valves; because if, as some maintain, boilers are burst by over-pressure, certainly they would be doubly safe; for if one safety valve should stick, it was not likely that both would stick at the same time. He did not mean to say that he maintained the theory that boilers were burst by over-pressure; but for the safety of those who took that view, it would be well for them to put two or three safety valves on. There was one thing which engineers ought to pay attention to, and that was the kind of water they pumped into the boiler; they should blow out three or four times a day, as they did in marine boilers. A boiler lately came under his notice which used to be blown out once in three months, and that was considered ample. The consequence was, he found half an inch of scale on the bottom of the boiler. The water used could be filtered, and the impurities would then be removed. Mr. Riley looked at him as much as to say that there were many things which a filter could not take away; but he (Mr. Olrick) had seen them within the last six months taken away by a process which was now in use, and which, perhaps, Mr. Riley was not aware of. Many hundred gallons of water could be purified in a day. He was sure if more attention was paid to the purity of the water, there would be a great deal less chance of boiler explosions from incrustation, because the consequences of impure water was incrustation. A great deal of sediment in boilers often choked up the different passages. Mr. Olrick then spoke of the construction of boilers, and showed by a diagram an example of a wrongly-constructed boiler that came under his own notice. The proper plan for introducing the feed-water was at the coolest place of the boiler, and not, as he saw it once in London, at the top of the fire-box. There was one kind of safety valve which had a great advantage over the usual kind. The valve was kept up by a column of water until the boiler

was entirely cleared of the over-pressure. It had been said that the usual hydraulic test was three times more than the usual pressure. That was wrong, it should never be more than twice, and in some cases he thought that would be too much. He could not agree with Mr. Reynolds in his objection to the hydraulic test. He (Mr. Olrick) considered it a great deal better to use a test if it was a careful one that would not injure the boiler. If there should be a faulty place, it would then be found out, and perhaps be the means of preventing a great destruction of life and property. The hydraulic test would not injure a boiler a bit more than a test under steam—some persons had objected because it was cold water—but they might warm it, make it boiling hot, and then try it by the hydraulic test, which would be just the same. Mr. Olrick concluded by illustrating by a diagram, a particular sort of plug used in boilers.

February 1st, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON "BOILER EXPLOSIONS."

By PERRY F. NURSEY.

ADJOURNED DISCUSSION.

Mr. NURSEY requested permission to make a few observations previous to the discussion. He stated that since the last meeting at which he read his paper he had received a communication calling his attention to the fact that he had not given sufficient prominence to the part Mr. D. K. Clark had taken in the question of steam-boiler explosions, and directing his (Mr. Nursey's) notice to the *Mechanics' Magazines* of May 3rd and 10th, 1861, in which would be found a summary of the question up to that time. He had read the articles and correspondence referred to by the writer of the letter, and found that they embodied to some extent a difference of opinion between two gentlemen, which was a matter he could not with propriety

obtrude upon the meeting. Beyond this, the letters contained copious reference to various papers and articles, some of which were inaccessible, and which it was not possible for him to examine, which he felt bound to do before he could alter his views. With reference to Mr. Colburn's position in the matter, he (Mr. Nursey) had gone through the subject before writing his paper, and, taking Mr. Colburn's views from the earliest period, he would notice having observed the germ of the theory of steam-boiler explosions in articles written by Mr. Colburn in the *American Engineer*, in 1857, and before coming to England, and he had noticed its gradual development up to the time of the publication of the treatise by Mr. Colburn in March, 1860. It would be observed that the letters above referred to did not appear until May, 1861, more than a year after Mr. Colburn's treatise was published. In that work Mr. Colburn gave Mr. Clark credit for several suggestions, and inserted a letter which Mr. Clark had written to the *Mechanics' Magazine* upon the subject. He (Mr. Nursey) would not trouble the meeting further upon a point which was somewhat irrelevant to the present question. He had only introduced it in fulfilment of his promise, and could not upon the insufficient evidence before him, alter the views expressed in his paper.

It had occurred to him, since reading his paper, that the method therein suggested of measuring the successive increments of strain upon boilers while under the hydraulic test, by means of a length of steel wire, was open to objection, and he hoped to hear from the meeting an improvement upon this suggestion.

Mr. GLYNN said, at the last meeting Mr. Reynolds stated that he considered high-pressure boilers entirely wrong in principle, or that high-pressure boilers were not so good as low-pressure. Now, for his (Mr. Glynn's) part, he could not see any difference between them, provided they were equally well constructed according to the pressure they were intended to bear. He had been looking over some papers referring to several kinds of explosions, and it seemed to him that if they were taken in rotation, some conclusion might be arrived at as to the causes that led to explosions. In the first place, two or three of the explosions referred to in his papers, were entirely due to the want of safety-valve room. An explosion at Ashton-under-Lyne,

was traceable to the insane act of the stoker or engineer having prevented steam from escaping, by tying down the safety valve. There was a case in 1854, at Rochdale, where the boiler was 18 ft. long, 5 ft. diameter, $\frac{1}{8}$ in. plates, hemispherical ends, and part of boiler $\frac{3}{8}$ plates. This boiler should have been equal to a bursting pressure of 300 lbs. on the square inch. This, like many other boiler explosions, burst just after starting the engine, it having been standing for some time. The safety valves were weighted too heavily, and were also out of order. It seemed strange that these explosions occurred immediately after the starting of the engines; and the only way he could account for it was that as soon as the engine was started the steam valve was opened, and the rush of steam increased from all parts of the boiler. As the engine was at only $\frac{1}{2}$ or $\frac{3}{4}$ of its stroke, the valve was shut down, and the consequence was that a violent blow was struck on the boiler by the return wave of steam. His opinion with reference to the boiler at Rochdale, was that the ordinary pressure of steam (76 lb.) was too much for a boiler, the supposed bursting pressure being 200 lb. Another cause of boiler explosions was the incrustation which was formed along the top flue, and often in the bottom flues: he was speaking of land boilers. That boilers did get incrustated, more or less, was certain; and in some cases, to a large extent, and was a fruitful cause of boiler explosions. He cited the case of the explosion on board the *Malaga*, and read an extract from a description of it by Mr. Hick. It was to the effect that a very thick incrustation of salt had been formed on the lower part of the boiler, immediately over the fire. He (Mr. Glynn) thought that this incrustation being almost a non-conductor of heat, or absorbent, would absorb as much as 50 to 75 per cent. According to Mr. Bridges Adams, the plates under the incrustation were very liable to get red hot, thus weakening the boiler, if only by continually overheating them. But there was more than this; the expansion of the boiler plates and the incrustation being unequal, the incrustation breaks up, and suddenly brings water on the red-hot plates, and at least produces that extra amount of pressure in the boiler which the plates cannot bear, having lost $\frac{1}{4}$ of their strength by this heat. This was borne out by Dr. Ritterbrandt, who says "that a sudden evolution of steam under circumstances of incrusta-

tion, is no uncommon occurrence." Another cause of boiler explosions was deficiency of water, which caused the plates to come down over the fire and to leak, and, consequently, rendered the boiler very liable to explosion. Another case where the water could not be properly ascertained was in boilers that primed a great deal, which was more especially the case in locomotives and marine boilers. The steam gets into the water, and then there was registered in the gauge a larger amount of water than there really was. He had not heard anything said about the spheroidal condition of water, or explosions arising from that condition. With reference to this, he would read an extract from a report on the bursting of the Union Steam-Packet boilers, written by Mr. Pearsall, a pupil of Professor Faraday, wherein it is said, "I consider the immediate cause of the bursting of a boiler from the expansive power of steam, and decidedly not from gas," &c. &c.

He (Mr. Glynn) called attention to a case which might occur to an engine that had been standing for some time, and where it was usual to blow through before starting. In blowing through, the water was thrown out of the hot well through the blow valves, if it had not leaked previously, through the valves themselves being out of order. When the engine was started, until the passages to, and the air pipe as well as the hot well was full, and the whole of the passages covered completely up to the feed pump, air would be directly pumped into the boiler; and if the valves leak, the whole connexion from the boiler and the force pump would be charged with air: consequently, there would be two reservoirs of air. He could not positively say there would be sufficient air to cause an explosion. This makes a boiler very liable to explode by its generating an amount of dry steam; and although Mr. Colburn thought there was no elasticity, he (Mr. Glynn) believed there would be a great deal of elasticity at that temperature. At the last meeting mention was made of a plug being employed. He had made a sketch of a fusible plug made by Mr. Forsyth, of Wolverhampton, in 1854. This plug was made of a large brass bolt (in which the fusible metal was soldered), capped on the fire side, so as to bring the lower side of the tin or lead plug within it, some distance above the water-side of the roof plate, thus leaving a margin, and allowing the plug to be fused while there was

sufficient water upon the plates to prevent their being burned, and to drown out the fire after the plug had been fused. In France, the use of a fusible plate (made of lead, tin, and bismuth) let into the boiler, was found advantageous, as it was out of the power of the engineer or stoker to interfere with it. Another plan proposed was to have a small tube projecting out of the top of the boiler, and screwed into a frame, so that if there was any extra pressure in the boiler, the tube would blow out with a report, thus giving notice that attention was required. There was another point with regard to water gauges; these did not always tell exactly the quantity of water that was in the boiler. Some were so badly constructed that, instead of the water working on the bottom, it was really the reverse. Another probable cause for the production of boiler explosions was this: In large towns space was very valuable, and when a manufactory was started, a boiler and engine were erected just large enough to do the work required at the moment. By-and-by, when the work increased, there would be no spare room to put up another boiler, and the consequence would be that an additional duty must be put upon the one boiler, when it was least fitted to bear it. He believed that to be one of the strong causes of boiler explosions. Another cause, especially in some works, arose from the boilers not being covered; the result was, that oxidation took place on the outside of the boiler, which caused it to scale, and thereby to become weaker. The grand secret in preventing boiler explosions could be thus summed up: First, the boiler should be properly constructed in its shell, and good, not cheap, materials used; there should be *two* safety valves, so constructed that the engineer or stoker could not tamper with them; there should be proper feed apparatus, self-acting, and to be worked by hand; and also large water gauges and gauge-cocks. With these precautions, and a proper engineer and stoker, explosions need not be feared. When he said "a proper engineer," he meant one sober and well conducted, who considered it a necessity to have his safety valves working easily, and kept clean, attention to the gauges, use of fire-plug and feed apparatus. When such men as these were employed (and they could be obtained, but not at 18s. or a £. per week), it might surely be said that everything had been done to prevent boiler explosions. He concluded by saying that he considered the chief

engineer as acting criminally when he neglected to look to these points; and that he hoped yet to see the day when it would be made legally a criminal matter when those conditions were not attended to.

Mr. Z. COLBURN said, the most conclusive evidence of the soundness of the theory which had been referred to would be suddenly to condense steam in the steam chamber of a boiler at work, and see what results would take place. It might be attended with some danger, but he thought it might be managed. Supposing the boiler to be half full of water, and the steam up to 30 lb. or 40 lb., it might be reasonably expected that the strength of the boiler would be equal to 200 lb. If a quantity of cold water were suddenly thrown into the steam space, the steam would be suddenly condensed, which, probably, would immediately produce an explosion of the boiler. He (Mr. Colburn) proposed trying the experiment at some future time, and he would lay the results before the Society. The question did not appear to have been sufficiently considered, why did boilers explode so violently? A vessel, like a boiler, but filled only with steam, or compressed air, produced no such effects as were produced in boiler explosions; but the bursting of water into steam would tear open the boiler with a loud report, and the pieces would be hurled eighty or a hundred yards, or more. He believed that boilers quietly ruptured, without violently exploding, very much oftener than they exploded; but such cases were seldom heard of, because nobody was injured. He had been twice upon locomotives when the whole side of the fire-box, perhaps a foot below the water line, opened instantaneously. Of course, these might be called explosions, but still the engines were not thrown off the rails. During the last year a fatal case of rupture was reported in the newspapers. It was a consideration of the above facts that led him to investigate the question why water should explode violently, and he found no other reason than the sudden removal of steam pressure. With regard to the remarks made by Mr. Glynn, relative to the shock communicated through steam, it seemed to him (Mr. Colburn) that the theory was not correct.

Mr. DANCHELL considered that Dr. Clark's process of precipitating lime in solution, which had been alluded to at a previous meeting,

and the invention of which had wrongly been attributed to him, did not go far enough in removing salts out of water which had to be used for boiler purposes. Carbonate of lime was thrown down by Clark's process, but not the sulphate of lime; and it was the latter which principally caused the hardness of those incrustations in boilers. It appeared to him desirable that all salts, and, in fact, all impurities, in water should be removed from the water before entering the boiler. There were many who believed that boiler explosions often arose from the incrustations. Whether or not this was the case he was not prepared to say; but one thing was quite certain, and that was, that they did not lessen the chances and could do nothing to *prevent* explosions; and, therefore, as they were injurious in many other respects, they ought to be prevented altogether. The only way of effecting this was by preventing the salts, which occasion the incrustations, from entering into the boiler. Carbonate of lime, in precipitating, formed a loose powder, but sulphate of lime a hard crust. Both together would likewise form a solid incrustation, more or less hard in proportion as the one or the other of these two were predominant. He exhibited some specimens of water containing the different salts in solution, and also some experiments of their mode of precipitating. He pointed out the fallacy of estimating any water from appearance. He showed a specimen of the water from Red Lion-square pump, containing 140 grains in the gallon of different kinds of salts; that from the Thames—considered to be bad enough—contained at most 20 grains, and yet the first appeared much purer than the latter. The water in many places used for steam purposes was even worse, and the great inconvenience, loss of time, and waste of fuel, ought to be inducements enough, independent of what possible danger may accrue, to adopt proper means to purify the water before letting it into the boiler.

Mr. CARRINGTON almost entirely coincided with Mr. Colburn's views on boiler explosions. He thought Mr. Glynn had partially misunderstood Mr. Colburn, for the latter had said, that in many cases boilers were burst, or torn open, without any violent or loud and dangerous explosion. He (Mr. Carrington) admired Mr. Colburn's theory very much. It was self-evident that a large amount of water would be carried by the steam as it expanded in the water on the

pressure being relieved, and, of course, the force of the blow would depend upon the velocity with which it struck. Assuming a quantity of water under 15 lb. pressure of steam, about $\frac{1}{10}$ th of such water would change into steam on the pressure being relieved; and if the pressure was relieved instantaneously, the velocity of the mass of water and steam would be great and very destructive. A blow from this mixture would be far more severe than from the same weight of steam alone; for the mixture having only about $\frac{1}{10}$ th of its weight of steam in it, would have far less elasticity than if it were all steam.

Mr. F. YOUNG thought that where the steam had become surcharged with heat, and the water mixed with it, the water became steam, and added to the pressure in the boiler. Mr. Colburn had referred to the effect produced by throwing cold water into the steam in a boiler. This was done a long time back by Mr. Brunel; but, as far as he (Mr. Young) had been able to learn, no result whatever was produced. Allusion had been made to the blow of the steam against the boiler. He recollected the case of the *Parana* steam-ship; and here the whole force of the explosion seemed to be exactly opposite the steam-pipe. He certainly considered it very important that all boilers should be subjected to hydraulic tests; filling the boiler with water, closing the valve, and lighting the fire, would be a very much fairer way of testing, and certainly cheaper than by the pump. With regard to incrustation, he recollected in the steamer *Dart* the engineer allowed the boiler to become salted, and about an eighth of an inch thick of salt was found in it. The result was the roof of the furnace came down; but, owing to the goodness of the plate, there was no explosion. The furnace had to be taken out and restored to its original form, and the steamer was now running. It was most necessary that there should be a reliable inspector of boilers. A man must not make a gun, or sell it, without having it proved, under a penalty; and why should it not be the same with boilers? He certainly could not see the wisdom of interfering with the gun and not interfering with the boiler. In France all boilers were proved by Government. He agreed with Mr. Glynn that boilers should not be placed in inefficient hands, but that they should be managed by efficient engineers only. In France a man must pass an examination before he is considered competent to take charge of a boiler. With the

manufacturers of boilers, at the present time, it was not the question of quality, but that of cheapness, for boilers were now supplied at a price little more than the cost of the materials. As regarded the statement that revolving boilers gave more steam, he thought it seemed reasonable enough; and the motion must be of very great assistance to the circulation of the water, which should certainly be attended to, but which seemed to be very much neglected.

Mr. RILEY stated that it was a well-known fact that boiler explosions frequently took place at the starting of the engine. This might be explained by the fact that in suddenly withdrawing the pressure in the boiler a violent ebullition took place, and caused a great evolution of steam from the water being thrown upon the heated plates, if the water in the boiler had been allowed to get at all low. He also referred to the violent explosions that occur occasionally in iron-works, by the use of water for removing the cinder from refined metal. After the molten metal is run in the moulds cold water is thrown upon it with buckets. This operation is safely performed. If, however, there is the smallest particle of iron or cinder in the water thrown on the metal, then a violent explosion ensues, scattering the molten iron in all directions. The same effect is produced when cold water is poured into a puddling furnace, to cool the bottom. If the water contain a particle of iron or cinder the furnace is not unfrequently blown down altogether. It certainly was very difficult to explain the reason of this. It might be due to the cold piece of iron causing contact, as it were, between the hot metal and the water, by which steam is suddenly generated; whereas, all the other water enters into the spheroidal state. In boiler explosions it was probable that cold water being introduced in a boiler with red-hot plates reduced their temperature, and when the water came in contact with the iron a sudden evolution of steam would be the result, which might account for the boiler giving way.

Mr. LOUCH stated that several instances had come under his observation, in which corrosion had occurred from the boilers having been set with a line of rivets resting immediately upon the brick-work setting. Any leakage from this line of rivets would settle between the boiler and brickwork, and the moisture thus—assisted no doubt by the sulphurous vapours generated in the flues—caused

very rapid corrosion, and probably led to many explosions. He therefore recommended that great care should be exercised in setting boilers to avoid as much as possible a line of rivets coming in contact with the brickwork. He also strongly condemned the too prevalent practice of entrusting boilers to inexperienced and under-paid attendants, to which in his opinion many boiler explosions might be attributed.

Mr. OLBICK referred to a plug of quite a different character to that to which allusion had been made. He considered it a superior one, and for this reason, that this plug would act while there were still 2 in. of water over the boiler plates, consequently the plates could not be injured by overheating. When the water was too low the soldering with which it was soldered would melt away, and the top part would by the force of steam be blown into the fire, when the fireman would be woken up, if he should happen to be asleep, by the velocity with which the steam rushed out, besides that, if he did not attend to it at once, he would have the fire out. Mr. Glynn had mentioned that it was possible, under certain circumstances, that there would be a great deal of electricity in a boiler. He (Mr. Olrick) thought they ought to rely upon the opinion of Professor Faraday, who says, "The production of electricity is not due to any change in the state of liquid contained in the boiler, a current of dry steam produces no development of electricity." If that was the opinion of Professor Faraday, he considered they should conclude that it was impossible for any boiler to explode simply on account of electricity, and consequently all those who brought that forward as one cause of explosion, should at once consider they were labouring under some mistake. Mr. Glynn had stated that one of the professor's pupils thought differently from the professor upon the question of decomposition of steam. As Professor Faraday considered that decomposition could not occur to any extent under any circumstances in the working of ordinary steam boilers, and that if it did occur the hydrogen thus liberated would have no access to oxygen, without which it could not inflame or explode, even if oxygen were present, steam would prevent ignition; and if oxygen were present and no steam existed, the hydrogen would only inflame and burn silently, as fast as it was produced. Consequently, explosion was impossible from decomposition.

If, he said, the master held these views, and had come to this conclusion, he thought they were bound to accept the views of the master in preference to those of the pupil. He quite agreed with the remarks that the circulation of the water in boilers was much neglected. He believed the want of proper attention in this respect had caused many explosions. Mr. Young had referred to an incrustation of salt having been formed upon a boiler; that ought not to have happened, for it could have been prevented by using a proper salinometer. As regarded high-pressure boilers, he thought they were the safest, not only with respect to pressure, but also with respect to the prevention of accumulation of deposits, because the circulation of the water was increased. There was another point to which no reference had been made. Professor Miller had made experiments which had thrown considerable light upon some of the causes by which ebullition was facilitated. He found that the presence of air singularly assisted the evolution of vapour. That was an easy explanation of the many explosions shortly after starting the engine, because the pumps would first pump air into the boiler. There was another point which he thought ought to be mentioned, as it had been published. He referred to water that contained organic matter producing inflammable gases. As to the management of boilers, it was of as much importance to have good men, as it was to have good boilers; and let them be properly attended to, and let the water be cleansed as it came into the boiler by having proper scum-pans. It was an easy matter to prevent the impurities entering the boiler, by filters, or some other means, such as Dr. Clarke's process, which throws down the carbonate of lime, but retains the sulphate. He had seen two tubes taken out of a boiler where sulphate and carbonate of lime and salt had formed into a solid mass, and he had seen an eighth of an inch of salt accumulate in a voyage from Sunderland to London. Mr. Olrick considered that zig-zag tubes would be difficult to clean, and that they would materially interfere with the circulation of the water.

Mr. E. REYNOLDS rose to explain some remarks made by him at the last meeting, which appeared to have been misunderstood, and adden, that with reference to the part performed by the water in the act of explosion, it was only fair to Mr. Craddock to point out that he

appeared to have fully understood, and published what was perhaps put in a clearer light by Mr. Colburn, but could not now be claimed as new. In his lecture, published in 1847, fol. 30, this passage occurs :

“As we, then, see the part the 9 deg of sensible heat contained in in the 160 cubic feet of water play, on the latter varying only 9 deg. in temperature ; if we, then, suppose steam at 40 lb. on the inch, which on the bursting of the boiler is instantly reduced to 20 lb. per inch, including the pressure of the air, we shall find here that we have 40 deg. difference of temperature, and that as much water would instantly flash into steam as would produce upwards of 10,240 cubic feet : this explains the diffusive character the water on such occasions assumes.”

And at fol. 23 : “But supposing it were to burst, it contains nothing but steam, which is in itself not a very dangerous element, the chief danger in boiler explosions arising from the great quantity of water, together with the solid matter which is blown about in all directions.”

Mr. SHRUBB said he had been inspector of boilers at the Royal Gun Factories at Woolwich for three years, and his experience told him that compositions to prevent corrosion did not answer when there was so much salt and hard silica substance contained in the water, as was the case at Woolwich with the water taken from the Thames, although it would answer where boilers were supplied with purer water. He found that by regularly blowing out, boilers were kept much cleaner than with the scum-cock. The composition, he found, only took off the lighter products of the sediment, and left the heavier and hard silica portion, which, though much thinner, was impenetrable to water, but if the two were kept together they formed a porous body which allowed the water to reach the plates.

As regarded the effect of cold water thrown upon a heated plate, he had seen a plate ripped right through the centre of the rivets from cold water being thrown upon it.

With respect to safety valves, he had adopted Hopkinson's compound safety valves throughout the Gun Factories, as they acted inside the boiler, and could not be tampered with without taking off the man-hole door. If the attendant neglected to keep a sufficient

supply of water in his boiler the valve would open, and in three or four minutes the boiler would be empty of its steam and his engine would stop, and there would be no steam to work his pump to put cold water on the tubes of his boiler; consequently, it could be immediately proved that it was the entire neglect of the attendant. In fact, explosions would never occur if the boilers were properly examined and attended to.

To facilitate the process of cleaning the boilers, Mr. Shrubbs said that he had the refuse oil from the drip cans of the shafting collected, and after the boilers were cleaned this oil was put on the inside of the boiler with a brush; although it did not stop the incrustation it made it come off the plates much easier, and effected a great saving in time and expense in cleaning the boilers.

He considered the mushroom valve, so much in use now as a safety valve, should be done away with entirely, and flat surface valves substituted in their place, as the mushroom valves were continually getting jammed from dirt and expansion from heat, which was not the case with flat surface valves.

Mr. NURSEY, in reply, observed that a great many points had been raised during the discussion by some members, but which had been so completely answered by others, that it would be unnecessary for him further to notice them. He thought, however, that distinction had not been sufficiently drawn between the cause of steam-boiler explosions and the cause of the *violence* attending them. The object of the paper was to enquire into, and as far as possible to determine, the *cause* to which this violent action was attributable. It had been shown that the percussive force of steam alone, was incapable of producing the destruction attending most steam-boiler explosions. Overheating the metal of the boiler could not be assumed to be the general cause, for explosions had often taken place where, but a moment before, the water gauges indicated an ample supply of water. A boiler had even been filled while red-hot with water, but no explosion took place. The electrical hypothesis had been proved untenable. That the decomposition of steam was a cause of the violence—or even of explosions at all—had been negatived upon the highest authority. The legitimate solution to the question, therefore, appeared to be found in Mr. Colburn's views, which were, in effect,

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the rupture of a defective portion of the boiler shell—the escape of steam, and consequent instantaneous reduction of pressure upon the water—the rapid disengagement of steam and its projection in combination with water through a greater or less space, and the conversion into steam of part of the heated water thus projected. Practically, these operations were instantaneous, and to them was clearly due the resistless energy of steam-boiler explosions. There could be no question that periodical examinations and the occasional application of the hydraulic test would go far to prevent steam-boiler explosions, but, above all, a careful supervision was necessary over those in charge, who should be properly qualified.

The CHAIRMAN thought they were greatly indebted to Mr. Nursey for his judicious and well-considered paper, and for the manner in which he had laid the subject before the meeting. The discussion had resolved itself into this:—the general position and nature of the fracture of exploded boilers, and the causes of these explosions. He considered it to be their especial duty to attend to the different causes of explosions. Further scientific research was very desirable, so as to enable them to come to some satisfactory conclusion upon the extraordinary phenomena observed in terrific explosions. The whole of the discussion (he thought) led to the opinion that Mr. Colburn's theory bore greater evidence of truth than any other they had discussed. Mr. Colburn had told them that he intended to practically test the truth of his theory upon a small scale; and he (the Chairman) hoped he would conduct the experiments with safety, and not blow himself up as well as the boiler he was experimenting on. (Laughter.) The Society would be very glad to know the result of these experiments when finished. A strong opinion had been expressed that there should be a Government intervention with regard to a proper supervision of boilers. One gentleman mentioned that Government exercised such a supervision over fire-arms. He was not aware himself whether this was so or not, but if Government did interfere with fire-arms, there could be no doubt whatever that there was a strong reason why they should exercise the same control over steam boilers. And who, he would ask, were the proper persons to bring such a subject before Government for consideration? Was it not the duty of such a society as the Society of Engineers? (Hear,

hear.) If they did he thought there was every reason to believe every careful attention would be given to the subject; and did not this proposed control interfere with any general principles, he thought a strong probability existed that any such representations made from the Society might result in a permanent benefit to the public. He should certainly be glad to see the Society memorialise Government on this subject.

March 2nd, 1863.

W. T. CARRINGTON IN THE CHAIR.

ON THE RELATION BETWEEN THE SAFE LOAD AND
THE ULTIMATE STRENGTH OF IRON.

BY ZERAH COLBURN.

A GREAT number of experiments have been made by many experimenters to ascertain the ultimate resistance of iron to tension and compression, and its strength has thus been determined with perhaps as much precision as is possible in the case of a material presenting almost constant variations of quality. Every engineer is now aware that, as an average result, the tensile strength of good cast-iron may be taken as about 8 tons per square inch and its crushing strength as 48 tons. Wrought-iron of fair quality will bear not far from 22 tons per square inch in tension, while its crushing strength is variously stated at from 12 or 15 tons per square inch up to $28\frac{1}{2}$ tons, the last named being given by Mr. Mallet as the result of experiments upon large hammered bars which bore but from 23 tons to 24 tons in tension.

When, however, we come to the question of safe working strength much difference of opinion exists among engineers, the permanent supporting power of iron being variously estimated at from four-tenths down to one-tenth of its breaking strength. Thus, when some fifteen years ago, a royal commission sat to inquire into the application of iron to railway structures, the late Mr. Glynn, in his evidence,

recommended that a cast-iron bridge should never be loaded beyond one-tenth of its ultimate strength. The late Mr. Stephenson, with several other engineers, thought a ratio of one-sixth sufficient, while the late Mr. Brunel was satisfied with a ratio of from two-fifths to one-third; or, in other words, if a girder would just bear 100 tons of distributed load, he would put from 33 tons to 40 tons upon it, where Mr. Stephenson would allow not more than 17 tons and Mr. Glynn only 10 tons.

Were we now to have another commission entrusted with the same inquiry, it is not unlikely that as great a difference of opinion would be found still existing. For there is no acknowledged natural principle upon which the safe load of iron has yet been determined, and in the absence or oversight of such a principle each engineer must be governed by his own judgment of what is safe and prudent. It is true that the authority of the Board of Trade has been so far exercised in this matter as to have limited engineers, in the design of wrought-iron railway bridges, to maximum tensile strains of 5 tons per square inch; and although it is commonly believed that with wrought-iron a compressive strain of from 4 tons to $4\frac{1}{2}$ tons corresponds to a tensile strain of 5 tons, the Board of Trade impose the same limit of strain for both the top and bottom chords of a wrought-iron girder. The limit of 5 tons per square inch, it is hardly necessary to say, is an entirely arbitrary one, nor is it modified according to the quality of the iron and workmanship in a structure. Thus, in girder bridges, plate-iron is used of which the breaking strength is occasionally not more than 18 tons per square inch. In punching the rivet holes, however, and irrespective of the loss of the metal actually punched out, the solid iron remaining between the holes is injured, so much so, that in a series of experiments made many years ago by Mr. Fairbairn, the mean tensile strength of seven specimens was reduced from 52,486 lb. per square inch before punching to 41,590 lb. per square inch of solid iron left between the holes after punching—more than 20 per cent. of the strength of the iron being destroyed by punching, a loss distinct from that of the metal actually punched out. Drilled rivet holes, it is satisfactory to know, are now being adopted in the best class of bridge work, but in bridges already erected and containing plates occasionally no stronger than 18 tons

per square inch before punching, the loss of strength ascertained by Mr. Fairbairn would diminish this to about $14\frac{1}{2}$ tons for the net section of metal between the rivet holes. On the other hand the best suspension bridge links have a strength of from 26 tons to 28 tons per square inch of section, and yet the Board of Trade inspecting officers would not probably depart from the arbitrary limit of a maximum strain of 5 tons in either case. As far, therefore, as is necessary to meet the requirements of the authorities, good iron and sound workmanship go for little or nothing; and not only does this remark apply to the interference of the Board of Trade, but, in the case of Chelsea Suspension Bridge, the chains of which are believed to have a tensile strength of upwards of 25 tons per square inch, two of the leading members of our profession have declared that structure to be unsafe until it shall have been so strengthened that the greatest load which the heaviest traffic is likely to bring upon it shall not exceed 5 tons per square inch of the sectional area of the chains. The highest authorities, we are justified in supposing, would, at the same time, be satisfied with the same maximum strain in the chords of a plate girder bridge even if the actual breaking strength of the solid iron between the rivet holes did not, as we have reason to believe it often does not, exceed 15 tons, or three-fifths that of the links of Chelsea Bridge.

While some of the conditions of elastic action are now under consideration, it appears desirable to define, as nearly as may be, what elasticity really is. We are all familiar enough with its manifestations, as in a watch-spring, an air cushion, or a bit of india-rubber, but we seldom inquire to what condition of matter these manifestations are to be attributed. When, however, we regard matter of any kind as a collection of distinct molecules or atoms, between which the force of attraction is always predominant in solids, and the force of repulsion in gases, we have no difficulty in perceiving that any alteration in the amount of either of these forces, acting within a body, must produce a corresponding change in the distances between its atoms, and in this very change of distance we have the whole phenomenon of elasticity. Elasticity is not a force, but the result of the antagonism of two opposite forces: an antagonism in which either force continues to act through a certain range or distance

—a range differing greatly in different materials—before being completely overcome by the opposite force. Under the influence only of attraction the particles of all matter would cohere, and without repulsion there could be only solid bodies in nature, for the existence of liquids and gases would then be impossible. The repulsive force appears to be the force of heat, to which physical repulsion may be generally traced. Under the influence of heat alone, and without attraction, there could be neither cohesion nor liquidity, and all substances could exist only in the gaseous state. We shall soon be able to trace the bearing which these distinctions have upon the elasticity of iron. At present we have only to bear in mind that there is reasonable physical evidence to show that the atoms of matter, so far as we can comprehend their existence, are never in actual contact with each other, even in solid bodies. Iron, as we know, is a comparatively porous material, since air and water may be readily forced through it under moderate pressures. Mercury, it is well known, will rapidly insinuate itself between the pores of an apparently solid body, for gold immersed in mercury will acquire the condition of an amalgam, and will crumble to powder. We know, furthermore, that the particles of most, if not all, solid bodies may be so far separated by heat as to convert them into liquids, and it is quite reasonable to suppose that, with an additional degree of heat, or repulsive force, all these liquified solids might, like mercury, and even melted gold, be converted into vapours. Now, any force applied after the manner of tension in a bar of iron, thus tending to separate the particles, in opposition to their cohesion or force of mutual attraction, may be considered to act in the same manner as the repulsive force of heat. The point at which rupture will take place will evidently be where the particles are so far separated that the cohesive force is less than the tensile strain, but there is good ground for supposing that, even after rupture, the atoms of the iron would again cohere could they be again brought to within their original distance from each other. This, of course, would be impossible if we attempted merely to place the two fractured surfaces in contact. In the case, however, of two clean surfaces of lead, we may readily bring them so closely together as to restore cohesion, and in the same way iron journals running in steel bearings occasionally become welded to them when the pressure

is too great for ordinary work. In thus bringing two bodies within the range of cohesive force, we must remember that it is the ultimate atoms which cohere, and that the finest perceptible particle of matter may really consist of myriads of almost infinitely smaller atoms. In all welding we first employ a degree of heat sufficient to overcome so much of the cohesive force between the atoms of the iron as to allow of sufficient motion among themselves to bring all or most of the atoms forming one surface within cohesive range of those forming the opposite surface. Two cast-iron surfaces are readily welded together by running melted iron between them. Without heat the two surfaces, however well finished, would touch only at a great number of separate points, although there would be considerable cohesion even then, as every one knows who has brought two carefully scraped surfaces of iron or two sheets of plate-glass together; and although a part of the apparent cohesion is due to the pressure of the atmosphere, there would really be some cohesion in a vacuum. It is only from the fact that the atoms of matter will always cohere when once brought sufficiently near together, that we are enabled to make bricks and all articles of earthenware. Soluble bodies, like clay or salt, cohere after solution and evaporation simply because their atoms, having been first separated by the liquid solvent, are again left within cohesive range on the evaporation of that solvent. Bricks, but for the waste of fuel in burning them, might be made from the most dilute solution of clay, and the final baking is only necessary in order to so completely expel the moisture as to bring the clay atoms within the range of cohesion, but their own character is in no respect altered. Iron, where it possible to dissolve it without combining it with nitrogen, oxygen, sulphur, or other matters, would again return to its ordinary granular state, and would recover its cohesion, on the evaporation of the solvent. Great pressure, too, will effect cohesion in the cases of substances naturally having none at all. Thus in experiments made with the ballistic pendulum to ascertain the velocity of cannon shot, the sand packing against which the shot is fired is often converted into sandstone of considerable strength.

From the illustrations given the elasticity of solids appears to be no more than the range or play of the attractive and repulsive forces

of matter, as variably exerted, but within the limits of rupture or crushing. Thus elasticity is the same in kind whether the repulsive or separating force be externally applied, or whether it be that of heat acting between the molecules of the body. If a bar of good wrought-iron be stretched to the one-thousandth part of its length, corresponding, say, to a strain of ten tons per square inch, its elasticity will be fully excited or nearly so, and it will not support a much greater strain without taking a permanent set. It is true that, if the same bar of iron, when not under strain, be heated to from 150 to 200 deg. above its normal temperature, it will also elongate by one-thousandth part of its length, and that without injury. But if this elongation take place under a compressive strain, or, if the iron, first raised in temperature by 200 deg. and thereby elongated, be attached to two fixed points, and thus, while cooling, be made to contract under strain, it will be found that an elongation of not far from one-thousandth of the original length of the bar is the most that can be borne without injury, even when that elongation is due to heat alone. But if the iron be first heated sufficiently to soften it, as railway tyres and gun hoops are heated, the particles will be re-arranged and, within certain limits without injury, but after the metal has once cooled below the temperature at which the particles have the mobility necessary for this re-arrangement, any further contraction around an unyielding object will be attended with permanent and, there is reason to believe, injurious strain. Even in setting railway tyres, it is believed to be best to put them on cold, and under graduated pressure, and in the case of gun hoops, Captain Blakely and Mr. Mallet, who appear to be entitled to the credit of the modern system of the ringed construction of artillery, having always insisted upon the importance of a definite degree of shrinkage of each ring, so that the consequent strain shall not exceed the elastic limits of the material. Sir William Armstrong has stated that he does not consider any especial accuracy essential in the distribution of the strains imparted by shrinking his gun hoops upon each other, but it may be questioned how far the failures of so many of the Armstrong guns have been due to neglect in this respect.

It would be interesting to know the precise manner in which a separating strain acts upon the molecules of iron, or rather to know



the successive positions of the atoms during the application of the strain. We are, however, without any positive knowledge of the positions which the atoms assume in solidification and under subsequent forging, but the multifarious forms in which all atoms visibly crystallise serve to show us that they cannot all be at equal distances from each other throughout the whole body. If they were, the arrangement would be that of cannon balls in a triangular pyramidal pile. Could we visibly represent the atoms, as occupying the angles of an infinite number of equilateral triangles, we should understand that a linear strain acting to separate any two of the atoms would, at the same time, draw a third atom, if not a number of atoms partly between them. And when, from this intrusion, the repulsive force, or heat, always enveloping the intruding atom, had once overpowered the attractive or cohesive force existing between the two atoms thus strained apart, these would, in turn, cohere anew to the atom which had been drawn in between them, and thus we should have a permanent re-arrangement of the atoms, or, in other words, a permanent set, with permanent elongation in one direction, and permanent contraction in a plane at right angles thereto. That the atoms are thus drawn into parallel rows of straight lines, in many kinds of iron at least, seems evident from the appearance of fracture, which presents stringy collections of particles forming what is commonly called fibre, although there is great reason for doubting that anything like fibre existed in the iron before it was broken. Mr. Kirkaldy's recent extensive experiments appear to show, as many others have shown, that iron may be made to break short or to break with an appearance of fibre, just according as it is broken with a sudden blow or a gradual pull. Something like fibre may be imparted, on a coarse scale, by repeated rolling or in wire-drawing, but it is more probable that masses of atoms are thus drawn into strings than that any fibre is really imparted to the atomic arrangement itself.

It is commonly held that, within certain limits of strain, iron is perfectly elastic. No matter how often it may be stretched or deflected, up to a certain point, the general belief is that it will come back to its original form every time the load is taken off. There are high authorities, however, who maintain that iron takes a permanent

set under even very moderate strains. If we are to understand that the set is exceedingly small, this may be true. The late Mr. Hodgkinson, for example, remarked, on the 381st page of his "Experimental Researches," that two cast-iron beams took each a permanent set with weights respectively equal to one fifty-seventh and one eightieth of the breaking weight. In a discussion at the Institution of Civil Engineers, a Mr. Dines mentioned that he had tested upwards of 8000 cast-iron girders for the late Thomas Cubitt, and that he found it hardly possible to apply a weight so small as not to produce a permanent set, one-twentieth of the breaking weight producing a perceptible set. In the experiments of the Iron Commission at Portsmouth, a bar of annealed wrought-iron 50 ft. long, was said to have taken a perceptible set with a weight of less than $1\frac{1}{4}$ tons per square inch. After this weight had been doubled, however, the set was still only perceptible; and notwithstanding that the elasticity of annealed iron is known to be inferior to that of unannealed bars, the whole set of the 50 ft. bar was but the $\frac{1}{100}$ th part of one inch, after a strain of $8\frac{1}{2}$ tons per square inch had been borne; and the set was but the $\frac{1}{100}$ th of an inch in 50 ft. after a strain of 11.9 tons per square inch. Mr. Edwin Clark has experimented on a wrought-iron bar 10 ft. long and 1 in. square. Under a strain of 3 tons per square inch, he gives a permanent set of nearly the $\frac{1}{1000}$ th part of an inch in 10 ft. With 8 tons the permanent set is given as about the $\frac{1}{1250}$ th of an inch in 10 ft., and it was not until a strain of 13 tons per square inch had been applied that a set of $\frac{1}{4}$ in. in 10 ft. became apparent. With such exceedingly minute measurements, we may, perhaps, doubt if there was really any permanent set at all, with strains under 9 or 10 tons per square inch. An increase of temperature in the bar of perhaps a single degree, while the measurements were being made, would more than account for some of the reported sets, even under considerable strains. Thus, Mr. Edwin Clark gives the permanent set of his bar, after a strain of 8 tons per square inch, as the $\frac{1}{1250}$ th part of its length, and this is almost exactly what the extension of the bar would have been had its temperature been raised but a single degree between the observations. Iron is heated in the very act of straining it, and a sudden breaking strain will generally leave the broken ends too hot to be handled.

Such a slight apparent extension might also have occurred while the shackles by which the bar was strained were coming to their bearings. But even if such a microscopic permanent set really existed, it is one of which no engineer would take the slightest serious notice as affecting the strength of the bar in which it was observed. With the means of measurement commonly employed by engineers, ordinary wrought iron is seldom permanently stretched until after it has borne strains of upwards of 8 tons per square inch. In seven experiments by Professor Barlow, on wrought-iron bars 10 ft. long, two of them retained their full elasticity under a strain of 11 tons per square inch, three bars bore 10 tons without injury, while one bore $9\frac{1}{2}$ tons, and another, made from old furnace bars, did not retain its elasticity beyond a strain of $8\frac{1}{2}$ tons per square inch. All the links of Pesth Suspension Bridge, upwards of 5000 in number, and 12 ft. long from centre to centre, were tested without permanent set up to 9 tons per square inch, and those of Chelsea Suspension Bridge were tested, without permanent elongation, up to $13\frac{1}{2}$ tons per square inch. Mr. Edwin Clark, from the results of his experiments, considers that the limit of elasticity of wrought iron is 12 tons per square inch, and this appears to have been adopted by him both for bars having a breaking weight of 24 tons, and for plates having a breaking weight of 20 tons. Every chain cable purchased by the Admiralty is tested up to 11.46 tons per square inch of the metal in each side of the link, the standard test being 630 lb. for each circular $\frac{1}{2}$ in. of the diameter of the iron of which the cable is made, one half of this strain coming upon each side of the link. The iron of which the cables are made does not, as a rule, take any permanent set when strained to this amount, or to say $11\frac{1}{2}$ tons per square inch. Mr. Howard has stated that the best iron begins to stretch permanently under about 10 tons per square inch in 10 ft. lengths, although he occasionally tests up to 15 tons or 16 tons per square inch, the breaking weight being from 26 to 28 tons. Mr. Mallet, about four years ago, presented to the Institution of Civil Engineers, the results of a valuable series of experiments on wrought-iron and puddled steel, from which it appeared that the elastic limit and the breaking strain under tension were, in the case of certain samples, as follow :

	Elastic limit.		Breaking weight.
Hammered slab or bar, 12 in. $\times$ 4 in.....	15·312 tons	...	24·062 tons
Hammered bar.....	14·219	" ...	22·969 "
Rolled slab or bar, 12 in. $\times$ 4 in	10·937	" ...	22·969 "
Rolled bar.....	10·937	" ...	22·969 "
Fagotted forged slab, 4 ft. $\times$ 4 ft. $\times$ 1 ft...	8·750	" ...	18·594 "
Original fagot bars, Horsfall gun.....	12·031	" ...	21·875 "
Longitudinal cut, forged mass.....	9·844	" ...	19·688 "
" "	10·937	" ...	17·900 "
Circumferential "	6·562	" ...	16·406 "
" "	5·470	" ...	16·716 "
Transverse.....	3·281	" ...	6·562 "
Charcoal rolled bar from borings from } the Horsfall gun..... }	5·470	" ...	22·321 "

Sir Marc Brunel made a number of experiments on Yorkshire iron, hammered to small dimensions, or from $\frac{3}{4}$ in. to $\frac{1}{2}$ in. square. A very high elastic limit was obtained, as follows :

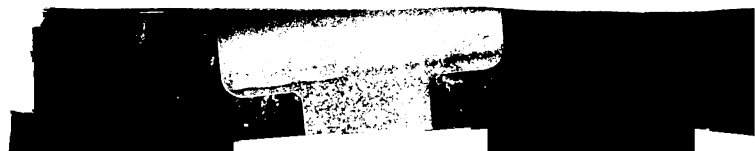
Mean of ten bars, began to stretch with 22·2 tons per square inch, the mean breaking weight being 30·4 tons per square inch. With ten other bars the mean strain at which they began to stretch was 24·4 tons, the breaking strength being 32·3 tons per square inch. It is to be borne in mind, however, that these bars were reduced by the hammer only at the centre of their length, and that, therefore, the stretching could be observed upon but a very small part of their length. Mr. John A. Roebling, the engineer of the Niagara Suspension Bridge, has made experiments upon bars similarly drawn down to $\frac{3}{4}$ in. square at the centre, the breaking weight being 33 tons per square inch; these bars bore a strain of 20 $\frac{1}{2}$ tons per square inch without visibly stretching, and when no jar was given to the bars they would support this strain for a week. Upon any vibration, however, the bars immediately took a permanent set.

Under strains, however, considerably within the elastic limit, a gradual re-arrangement of the particles of the iron commences; and if the strain be continued sufficiently long permanent set will after a while take place. The late M. Vicat, whose work on limes and mortars is so well known, began as early as 1830 to investigate the



effect of continued strains on unannealed iron wire. He applied various strains to similar wires of a known breaking strength, and continued these strains from July, 1830, to October, 1833. One wire was strained to one-fourth its breaking weight, but beyond the elongation which at once took place no additional stretching occurred in thirty-three months. A second wire was strained to one-third of its breaking weight, and in thirty-three months it stretched at the rate of $2\frac{1}{2}$ parts in every 1000 parts of its length, this stretching being additional to that which took place as soon as the weight was applied, but which, of itself, was not sufficient to immediately produce any permanent set. Under a strain of one-half of the breaking weight another wire stretched rather more than 4 parts in every 1000 parts of its length. Under a strain of three-fourths of the breaking weight a fourth wire stretched, in thirty-three months, $6\frac{1}{2}$ parts to every 1000 parts of its length, and then broke, which circumstance terminated the experiments. M. Vicat's account of them appeared in the 54th volume of the second series of the *Annales de Chimie et Physique*. It is to be regretted that, in place of the constantly recurring experiments upon the breaking strength of iron, and which, as is already beginning to be understood, give us but a very partial knowledge of its available working properties, we have not a larger experimental acquaintance with the continued supporting power of iron, as afforded by experiments similar to M. Vicat's. Mr. Fairbairn, it is true, made an extensive series of experiments between the years 1837 and 1842, to ascertain how long bars of cast-iron would support weights equal to about nineteen-twentieths of their breaking weight. By taking care to prevent any vibration in or about the bars, several of them continued, for five years and upwards, to support nearly their full breaking weight. Their deflection steadily increased, however, during the whole time, and Mr. Fairbairn has stated that some of these bars afterwards broke with but one-twentieth of their original breaking weight. As bearing upon the last-mentioned circumstance, it may be remarked that M. Vicat, writing in 1833, observed that M. Henri, an engineer serving in Russia, had already shown that iron which had once withstood a great proof strain often broke some time afterwards under a much less strain. This fact must indeed have been known to practical men even earlier than in 1830.

Now, in employing iron in any structure where it is subjected to strain, we seek to keep within its limit of elasticity. Yet not only have we but a comparatively small number of recorded experiments to show us what this limit is, even under a single and temporary strain, but we have at least the result of M. Vicat's experiments to show us that we cannot depend upon anything like this limit under a long-continued strain. What experimental knowledge we have goes to show that the original elastic limit of iron is greater when hammered than when rolled, but we are unable to count with any degree of certainty upon the ultimate superiority of hammered iron, in this respect, after long-continued strain. As a rule, also—the abundant evidence of which it is not, perhaps, necessary to introduce into the present paper—all harsh, hard crystalline irons have a higher elastic limit, in proportion to their breaking strength, than soft, ductile, highly fibrous irons, like Swedish bar, for example. That is to say, the harder irons will bear a greater strain, before taking a permanent set, although, as we shall presently have occasion to inquire, it may not follow that they are really superior to other irons which are more readily stretched, and which, indeed, may have an even less breaking weight. What information we have goes to show that there is no settled relation between the elastic limit and the breaking weight of iron; the former is much more variable than the latter, and can hardly be expressed at all as an average result, ranging, as it does, from less than one-fourth to more than two-thirds of the breaking weight; or if the elastic limit be taken irrespective of the breaking weight, the instances already cited show that the former varies from $3\frac{1}{4}$ tons up to $24\frac{1}{4}$ tons per square inch in different qualities of iron, although the range in ordinary bar iron and plate-iron is not nearly as great. Now, no engineer, in apportioning the strains in a structure, would think of working up to, or near to, the breaking strength of iron. His object is to keep within the elastic power of the material, not merely as ascertained by a single strain of a few minutes duration, but the continued elastic power of the iron as exerted through a very long series of years. We ought by this time to have hundreds of trustworthy experiments upon this point where there is now one. If the safe working strength of a metal is limited, as it would appear to be, by its measure of permanent per-



fect elasticity, we may say that we hardly know, even yet, what is the strength of the materials we are constantly dealing with, notwithstanding that not a year passes without some addition to our stock of knowledge of breaking weights.

While we are about considering the permanent injury which iron suffers when strained beyond its elastic limit, it is to be understood that iron may be strained for a short time almost up to the breaking point without in the least diminishing its strength under a breaking weight subsequently applied. Indeed, in gradually applying the breaking strain to any sample of iron, it is clear that it must have borne 10 tons before it is subjected to a strain of 15 tons, and that it must have borne 15 tons before it is strained to 20 tons, and so on. Not only is this the case, but after a bar of iron has been actually broken under a tensile strain, the two broken portions of the bar will almost always require a still higher strain to break them. The weakest spot in the bar will fail first, and although the breaking strain will at the same time permanently stretch the bar throughout its whole length, the iron on each side of the fracture will still have its original breaking strength. Professor Barlow's *Treatise on the Strength of Materials* contains the results of several experiments made at Woolwich Dockyard, as follows :

A bolt of Solly's patent iron was nicked at one place, and then broken by a strain of 24.7 tons per square inch. One of the pieces was then tried and broke at 29½ tons. In the next experiment a bar of the same iron was first broken with 23½ tons per square inch then again with 26½ tons, a third time with 26½ tons, and a fourth time with 25¾ tons.

Mr. Thomas Lloyd, engineer to the Admiralty, made a like series of experiments a few years ago, on ten bars of SC crown iron 1½ in. in diameter, and 4½ ft. long. The mean breaking weight at the first breakage was 23.94 tons per square inch. At the second breakage, with pieces 3 ft. long, the mean strength was 25.86 tons per square inch. At the third breakage, with pieces 2 ft. long, 27.06 tons per square inch, and at the fourth breakage, with 15 in. lengths 29.2 tons per square inch. Mr. Lloyd's experiments have been held to show that iron was actually strengthened by stretching it; or, in other words, that by destroying the cohesion at one point, the co-

hesion was everywhere else increased. A more obvious explanation is that the bars first broke at the weakest part, then again at the next weakest part, and so on. A variation of from 23·94 tons to 2·92 tons in the strength of the same bar is undoubtedly large, the greater strength being 22 per cent. more than the lesser : a difference which appeared to exist in each of the ten bars tried. It is well known, however, that hardly any two bars of iron have exactly the same strength, and Mr. William Roberts, manager of Messrs. Brown, Lenox, and Co.'s extensive chain cable works at Millwall, has cut a 12 ft. bar of iron into 2 ft. lengths, and found on testing that there was a difference of strength of 20 per cent. between the strongest and the weakest of these pieces. In the experiments of the Railway Iron Commission upon the extension of cast-iron, the strength of Lowmoor cast bars was 7·325 tons per square inch at the first, and 8·152 tons at the second breaking. Blaenavon iron broke with 6·551 tons per square inch at the first, and 6·738 tons at the second breakage. Gartsherrie iron broke with 7·567 tons per square inch at the first, and 8·475 tons at the second breakage. Other cast-iron bars of a certain mixture broke with 6·6125 tons per square inch at the first, and 6·777 tons at the second breakage, the latter being at an unsound place. Upon these results the commissioners remarked that "it would appear that iron, repeatedly broken, becomes more tenacious than it was originally. This erroneous conclusion may be obviated by considering that it would be very difficult, if not impracticable, to obtain cast-iron bars perfectly sound and 50 ft. long. Fracture may be supposed to take place the first time at the largest defect, and subsequently at those smaller, until, finally, none remain." It is not intended, however, in the present paper, to entirely deny that the breaking strength of iron may be actually increased by being stretched when cold ; and this point may be left as an open question. That iron is so strengthened derives some probability from the known fact that its strength is greatly increased by being drawn cold into wire, and also by cold rolling. When heated moderately, or to less than a dull red, and then stretched, iron is strengthened throughout. This treatment is known as thermo-tension, and in an extensive course of experiments made about twenty years ago by Professor Walter R. Johnson, for the United States Govern-

ment, a total gain of nearly 30 per cent. in strength and length, taken together, was estimated to have been obtained with a variety of irons. A bar of iron, having a strength of 60 tons, was heated to upwards of 500 deg., and then stretched by $6\frac{1}{4}$ per cent. of its length, when it acquired a strength of 72 tons. Captain Blakely has lately proposed the same treatment of iron, and his experiments, it is understood, corroborate those of Professor Johnson. All the links made for the four great pitch chains employed, with the steam-ship *Great Britain's* engines, for getting up the speed of the screw, were stretched $\frac{1}{8}$ in. while at a dull red heat.

But from what has been said, it is not to be supposed that iron is not injured by excessive strains, notwithstanding that the metal strained may, when tried immediately afterwards, still retain its full breaking strength. The injury will appear when a subsequent working strain is long continued; and even without waiting for this, it will be found that the strained iron has been deprived of a large part, if not the whole, of its natural elasticity. In a paper of great value, read nearly seven years ago before the Royal Irish Academy, and afterwards published in a quarto volume entitled, "On the Physical Conditions involved in the Construction of Artillery," Mr. Robert Mallet has laid down a useful measure of the working and ultimate strength of iron. Poncelet had already employed co-efficients which indirectly expressed, not merely the elastic limit and breaking strength of iron, but the range also through which the force acted in each case in reaching these limits. Mr. Mallet has adapted these co-efficients to the English standard of mechanical work, to wit: "foot-pounds," and he represents the structural value of different materials, or of various qualities of the same material, in one case by the product of the elastic load in pounds into half the range in feet or parts of a foot through which it acts, and in the other case by the breaking weight in pounds multiplied also by half the range, in feet or parts of a foot through which it acts. Mr. Mallet employs Poncelet's co-efficients, as follows:—

T_e	=	foot-pounds in reaching elastic limit of tension. •
T_r	=	" to produce rupture by tension.
T'_e	=	" in reaching elastic limit in compression.
T'_r	=	" to produce crushing.

E

One half the weight into the whole extension, or, what is the same thing, the whole weight into half the extension, is adopted because the force gradually applied to break a bar must increase from nothing to the breaking weight. Upon Dr. Hooke's law, *ut tensio sic vis*, the weight of a grain will in some minute degree deflect or extend the heaviest bar of iron, and the deflection or extension will increase progressively, with the weight applied, up to rupture. Therefore, if a bar be stretched 1 ft. and then broken with a weight of 33,000 lb., the work done will be the mean of zero and 33,000 lb. into 1 ft., or 16,500 foot-pounds. This, as has been said, is the work done in the case of a gradually applied strain. If, however, the weight be applied without impact, but at the same time instantaneously, upon the bar, it will, so long as the limit of elasticity is not exceeded, and supposing the bar to have no inertia, produce twice its former deflection, and, therefore, twice the ultimate strain. For the weight, in falling through the distance of the deflection due to the load at rest, will acquire momentum sufficient to carry it through an additional distance equal to that of the static deflection. This may be best demonstrated experimentally with the aid of a spring balance. If, upon the pan of a balance sufficiently strong to weigh up to 40 lb., a weight of 15 lb. be placed, and this be then lifted to zero on the scale and there released, it will descend, momentarily, to nearly 30 lb. on the scale; and if there were no opposing resistances and the spring had no inertia, it would descend to exactly 30 lb. In the actual application of strains in practice, a weight is never thus applied, but a consideration of what would occur under such circumstances is sufficient to show how important it is that vibratory action be not overlooked in considering the strains on bridges. It is to be remembered that this action of suddenly applied loads is only manifested in the case of the application of weights, for if the strain be produced by the sudden admission of steam or any other practically imponderable body, no additional deflection will take place beyond that due to the pressure acting statically. If steam pressure acted in the same manner in this respect as a weight, the steam indicator would show nearly or quite double the pressure acting effectively within the cylinder of the engine.

It will not be attempted in the present paper to enter fully into

the application of the coefficients adopted by Mr. Mallet, for there are objections against, as well as reasons in favour of, their application. It is evident that T_r may be the same in two cases, in one of which a high breaking weight is exerted through a very short distance, and in the other of which a low breaking weight produces stretching through a correspondingly greater distance. But this coefficient does possess a value in taking account of the combined cohesive force and extensibility of iron, instead of the breaking strength alone. As Mr. Mallet justly observes, glass has a high cohesive force, and is, nevertheless, useless under strain, owing to its brittleness, while caoutchouc has great extensibility or toughness with but slight cohesion. The products, therefore, expressed by the coefficients in question do not afford a complete notion of the practical value of a given material unless the factors whereby these products are obtained are also given. The elastic limit of iron, however low, is not to be exceeded in practical use, whatever its range of elasticity may be; nor does it appear prudent to work into the neighbourhood of a high elastic limit when the elastic range is known to be small. It is not to be understood that the coefficients in question are intended to apply otherwise than in comparison of bars of equal length, else it would result that the measure of T_e in a bar 50 ft. long, was one hundred times greater than that of a bolt 6 in. long, and of the same material and sectional area. For the purposes of the engineer a long bolt is not only no stronger than a short one, but, as it can be no stronger than its weakest part, it will follow that the average strength of 100 bolts 6 in. long, is likely to be greater than that of a single bolt of the same diameter and 50 ft. long. Every engineer must be aware of the importance of toughness in combination with cohesive strength in iron, but we need much more extensive and accurate information as to the former; and a consideration of Mr. Mallet's coefficients should lead to additional experiments being undertaken. It is but right to mention here that, in 1848, Mr. Homersham Cox contributed a paper to the *Civil Engineers' and Architects' Journal*, in which he represented the mechanical work, in foot-pounds, expended in deflecting a girder, the work being expressed by the product of half the weight into the range of deflection. Mr. Kirkaldy, proceeding upon an independent course of inquiry, but with the same

object as that pursued by Mr. Mallet, has lately published the results of a most important series of experiments, which are the first upon anything like an extensive scale, to take into account the combined cohesive force and extensibility of iron and steel. Mr. Kirkaldy experimented upon many hundreds of specimens, but he did not ascertain their limit of elasticity. He has given both the original dimensions and cross sectional area, and the dimensions and area after fracture, and he has also given the amount of elongation at fracture, although he did not ascertain the extension at the elastic limit. The reduction of diameter of a bar at the point of fracture serves to give a practical man a good idea of the quality of the iron, but it does not admit of an expression of the mechanical work done in producing fracture, as does the combined breaking weight and linear extension. In tearing a bar in two, also, we have to consider the permanent stretch communicated to all parts of the bar alike, and the additional stretch at and near the point of fracture. That part of the stretching which extends uniformly throughout the whole bar would, we may suppose, be exactly proportional to the length of the bar, while that part of the stretch which takes place close to the point of fracture would, we may also suppose, be a fixed quantity, whatever might be the length of the bar. Mr. Kirkaldy's specimens of iron and steel varied from 2·4 in. to 8·2 in. only in length, and with these the ultimate elongation at fracture varied from nearly nothing to 27 per cent. of the original length, whereas longer bars would have shown a proportionally less elongation. The samples which hardly elongated at all were of puddled steel ship plates. One sample, which bore 63,098 lb. per square inch of the original area, stretched before breaking but the $\frac{1}{16}$ th part of an inch in a length of 7·6 in., or less than $\frac{1}{16}$ ths of 1 per cent. of the length. Adopting Mr. Mallet's coefficient, the structural value of such a material would be almost nothing. In fact, Mr. Kirkaldy found the puddled steel plates throughout to have much less extensibility than cast-steel plates, while the former also were of very irregular breaking strength. The interest attached to the subject of puddled steel may possibly serve as a justification for mentioning this fact, although foreign to the object of the present paper, which has to deal with iron.

We have, up to this point, considered the elastic limit or perma-

nent supporting power of iron under a single application of strain, extending in some cases over a few minutes, and in others over some months. We have very little experimental knowledge as to the extent to which we may approach the elastic limit under continued strains, but there is a probability that a continuous strain, much exceeding one half of the elastic limit, would be attended with a gradually progressive permanent set. At the same time, with good iron possessing a fair amount of extensibility, a single temporary strain, producing a slight or even considerable permanent set, may not diminish materially the strength expressed by Mr. Mallet's coefficient T_r , and the iron may still hold up indefinitely with an ordinary load considerably within the limit of elasticity. But apart from the consideration of the time through which a strain acts, we have also that of the frequency with which the load is applied and removed. Mr. Fairbairn loaded cast iron bars to upwards of nine-tenths of their breaking weight, and this load remained on for from five to seven years before the bars broke down; but with a repeated application and removal of load, the Iron Commission experiments showed that iron bars, when repeatedly deflected, by impact, through one half the distance due to their breaking weight, broke down after from 130 to 4000 applications of the strain. Mr. Fairbairn communicated some of the results of an important series of experiments to the British Association at the Manchester meeting, 1861, from which it appeared that a large model of a wrought-iron plate girder withstood without injury 1,000,000 applications of a load equal to one-fourth its breaking weight, and afterwards 5175 applications of one-half its breaking weight, when it broke down. The model was then repaired, and 25,900 applications of two-fifths its breaking weight, and afterwards nearly 3,000,000 applications of one-third its breaking weight, were made, it was said, without injury, although neither the deflexions nor the permanent set were given. We know that iron alters its form, temporarily, during the application of very moderate strains, the elasticity of good iron being generally observable with strains of one ton per square inch. And we know that its form is permanently changed both immediately on exceeding the limit of elasticity, and gradually under strains nearly approaching that limit. There clearly must be a re-arrangement of the

particles of iron always going on where the strain is great; and as we know that when even a more moderate strain is eased off the iron tends to resume its original form, it appears incontestible that final injury must result under what may appear moderate although irregular loads, say one-third or even rather less of the breaking weight. Iron, it is still to be remembered, has not been employed long enough for purposes of construction to enable us to compare its endurance with masonry, of which there are abundant examples still perfect after many hundred years. At the same time it must not be forgotten that, while we can never know the absolute strength of a bar of iron without destroying it under strain, neither can we always infer its strength from its deflection, or apparent range of elasticity. For we are not yet secure against flaws or those other faults of molecular structure which Mr. Mallet so well describes as "planes of weakness." A bar of iron may have a general strength of 22 tons per square inch, except at a single point in its length, where for an almost inappreciable linear distance, the strength may not exceed 10 tons. If the bar be broken this fault will be detected, but hardly, if at all, otherwise. For under a strain of even 8 tons or 9 tons, the extension at the precise point of weakness would be so slight as to be quite overlooked in a general observation of the total deflection or extension.

In this fact, which was mentioned thirty years ago by M. Vicat, and in the known effects of suddenly applied strains, vibration, impact, &c., we have abundant reason for extreme caution in the use of iron for permanent structures of any kind. It can hardly be said that the Board of Trade limit of 5 tons, arbitrary as it is, is too moderate, although there should be some account taken of the known quality of the iron to which this strain is to be applied; and it may not be necessary to adopt an unlikely load as that which is to produce the maximum strain of 5 tons. The recent discussion as to the security of Chelsea Bridge affords much suggestive material upon these points.

The application of iron to bridges, especially to those of large span, necessarily requires the most careful consideration in apportioning the strains, since every pound of metal not brought into effective action is so much dead weight or useless load—being not only mis-

applied of itself, but requiring additional material to support it. In considering the strains upon iron, therefore, reference has been more particularly made in the present paper to its employment in bridges, but in the case of boilers, iron ships, cranes, ordnance, railway bars, warehouse girders and columns, roofs, engine beams, and in many other applications, the most careful distribution of material and adjustment of strains is of very great importance. Iron is, perhaps, more severely strained in steam boilers than in other structures. In the case of locomotive engines, there is a disposition to employ still larger boilers and to carry still greater pressures. With 50-inch boilers, formed of $\frac{7}{8}$ inch plates double riveted, and carrying, as is now not unusual, from 130 lb. to 150 lb. pressure, there is at the higher limit a circumferential strain of $5\frac{1}{2}$ tons per square inch at the joints and a longitudinal strain of nearly 2 tons per square inch along the whole length of the boiler: the resulting strain at the joints being nearly 6 tons per square inch. This strain is constantly maintained with plates ranging from 21 tons to 24 tons in strength and under all the contingencies of corrosion, incessant vibration, and occasional sudden exaltations of pressure due to the instantaneous production of steam upon overheated tubes or plates. In many cases we have 4 ft. boilers with $\frac{3}{4}$ -inch plates, single riveted and worked at 120 lb. corresponding to a strain of at least $6\frac{1}{2}$ tons per square inch at the joints of the boiler when new: the circumferential and longitudinal strains being both taken here into account. Put under this strain when new, many locomotive boilers are worked in all for from ten to twenty years, and often from three to seven years without any internal examination of the plates. It is not remarkable, therefore, that explosions are becoming so frequent.

We may regard with much hope the increasing use of steel in large masses, as produced by Krupp and by Mr. Bessemer, and others whose discoveries have already effected a great economy in the production of that material. Although a departure from the subject of the present paper it is interesting also to refer to the introduction of phosphorised copper, as now produced by the Birmingham and other coppermasters. It was announced, about three years ago, as a new discovery by Mr. Abel, chemist to the War department, that the addition of from 2 to 4 per cent. of phosphorus to copper greatly

increased its density and strength. There is no doubt of the large advantage of this combination, although it was discovered in the last century, and made publicly known sixty years ago. A French chemist, M. Sage, contributed a paper upon this subject to the *Journal de Physique*, and which was translated into the 20th volume of the *Philosophical Magazine*, for 1805. By combining the maximum quantity of phosphorus with copper, the latter acquired the hardness, grain and colour of steel, and although M. Sage had already kept the compound for fifteen years it had suffered no change from exposure to the air. It was easily turned and took a fine polish. It may yet be found that copper thus treated is the best material for many of the purposes of the mechanical engineer.

In conclusion, it must be regretted, if not indeed deplored, that our exact knowledge of the permanent supporting power of iron is so limited. The present paper, it is feared, will hardly fulfil its title, which may have led to the expectation that some definite relation would be assigned between the safe load and the ultimate strength of iron. Even at the risk, however, of leaving the subject as the author found it, he indulges the hope that the considerations which have been touched upon may induce closer observation and inquiry into the real working properties of iron, as distinguished from its ultimate strength. The question involved in this subject does not appear to be one which admits of solution by a reference to any known natural law, nor does it appear that it can be disposed of by mathematical investigation, but our only safe conclusions must be derived from the results of protracted, varied and repeated experiment. In the meantime common prudence requires that we ascertain, with as much accuracy as possible, the elastic powers of every given kind of iron proposed for permanent works, and that we then so proportion the parts that the working strain shall never much exceed one-half the elastic limit, a ratio which will correspond, according to the variable quality of iron, with a load of from one-eighth to one-fourth its breaking strength.

DISCUSSION.

Mr. W. ROBERTS agreed with the Chairman that the paper just read was of a most valuable and interesting character. It introduced many points which were deserving great consideration. He referred

more particularly to one, namely, the point at which the elasticity of iron terminates and where it commences. He had no doubt that where permanent set had been noticed at a very low strain, that it had occurred from an alteration in the temperature. He had observed that, in testing samples of iron, even a cloud passing upon a fine day would cause the iron to go back when it had apparently stretched, which certainly would account for these very low strains. During the whole of the experiments he had conducted, extending over a period of some years, he had never found permanent set in good iron under 9 to 10 tons to the inch. It was true the longest bars had been under 10 feet. He had not only found that iron bars, after a strain had been rapidly put upon them up to 9 tons, would go back, but when a second strain had been put upon them equally rapidly, and when apparently permanent extension had been established, the iron bars after a few hours go back again to their original length. He did not know that any gentleman present, who had been trying experiments had observed that, after good soft iron had attained a maximum strain, it had gone back, and ultimately broke at from 15 to 20 per cent. less than it had borne. He thought that was a point worthy of attention.

In reply to the Chairman, who inquired whether Mr. Roberts had tested any chains that had been a long time in use,

Mr. ROBERTS stated he had tested cables that had been 25 or 30 years in service, and found that they stood 20 per cent. more than the Admiralty proof. He had also found a chain (two inch) that had been used constantly for testing purposes, ultimately break at a much less strain than had been put upon them at different times. He had had some bars in their testing machine, exactly the same size as the back guys in Mr. Humphrey's sheers, and many of those bars maintained their elasticity up to 15 tons to the inch, and this was about the strain that *good* iron would bear without permanent set.

Mr. PARSEY said there were many collateral circumstances that had to be considered in connexion with the elasticity of iron. For instance, something must be known of the amount of inspection and supervision to be exercised during the manufacture of the work, before the proper limit could be determined. He had had opportunities of testing iron used for different purposes, and of applying a very accurate machine to test the extension under strain. The rule

adopted had been to first put a pressure of 10 tons to the square inch on the bar under strain, and he thought he could say that in good iron he had never found permanent set under 10 tons. He could point to some hundreds of experiments. In some cases there was an extension, or perhaps permanent set, with less than 12 tons per inch, but frequently there was some defect observed after fracture in the bar. It was his opinion that the apparent permanent set was due to the flaw. He had never in the whole course of the experiments found iron attain the high strains that many manufacturers talked about; some, for instance, said that their bars would bear a strain of 30 tons to the inch, but he had seldom found small bars bear more than $26\frac{1}{2}$ tons, and when the bars had a large sectional area of five or six inches, they will hardly bear 24 tons. The experiments he had referred to had induced him to come to the conclusion that good iron will bear a strain of 10 tons to the inch without injury. He thought if they adopted the coefficient of 5 tons to the inch, and the work was properly constructed, and no bars were allowed to be put into structures with flaws or defects, those structures would be perfectly safe. He had tested pieces of steel iron reputed to bear a strain of 40 tons to the inch, but had found them break under a pressure of 27 tons. He had also tested some pieces of Bessemer steel plates which bore a strain of over 50 tons to the inch; there was no doubt that that sort of stuff in large spans would be of great advantage. Referring to Chelsea Bridge, it had been stated that the bars would bear a strain of 30 tons to the inch, all he could say was, that he had had opportunities of breaking many similar bars, and had never found them to come up to 24 tons, the average not being above 22 tons—many had broken at 18 or 19 tons—but he must mention that the latter were made of single hammered scrap, and had been rejected. Therefore, in estimating the strain of those links they must not reckon to have 30 tons to the inch, for he believed it was perfectly fabulous, and the slightest defect in any portion of the bar would reduce the strength considerably. They must also consider the size of the bar and the diameter of the pin, and to see if they bore the proper proportions.

The Chairman inquired if Mr. Parsey had ascertained the elasticity of Messrs. Howard and Ravenhill's links?

Mr. PARSEY said it was about 12 tons to the inch. He had some-

times found them come up to $13\frac{1}{2}$ tons, but not as a rule. Generally the bars were very good indeed, but still not so good as had been talked about.

The Chairman inquired how Mr. Parsey allowed for any change of temperature?

Mr. PARSEY said the instrument was screwed on to the iron, so there was no chance of a variation of temperature affecting them. He sometimes found there was an apparent set, but it went back. That he thought had been observed by others, namely, that when the strain was put on and drawn off suddenly, the iron did not recover itself instantaneously. It was very frequently thought there was permanent set when it was due to a flaw in the bar.

The Chairman inquired whether the measuring-rod was made of iron or wood?

Mr. PARSEY replied that the rod was made of gun metal.

Mr. W. ROBERTS remarked that he always used yellow deal for his measuring-rods.

Mr. C. J. LIGHT said that the relation of the safe load to the ultimate strength of iron must vary considerably, according to the class of structure for which the iron was to be employed. For instance, if there was a dead permanent load that did not vary at all, it was clear that the relation between that load and the ultimate strain must be very different from that in the case of a strain on a railway bridge.

Mr. F. YOUNG stated that though Mr. Parsey thought no bars would bear a strain of 30 tons to the inch, such a strain was mentioned by Mr. Kirkaldy in his book on the strength of iron. He had himself seen some Shelton bars bear a strain of 30 tons to the inch before they broke. It was generally assumed that cast-iron should not be employed where much tension would result; but he knew a case where a cast-iron boiler had been in use for forty years with a pressure of 40 lb. to the inch. He referred to the mills of Messrs. Barrett at Ratcliff, where there was a cast-iron boiler still in use, which lately had had a wrought-iron patch put upon it. He might mention also the case of an American boiler made of $\frac{1}{4}$ -in. plate-iron, which was worked at a pressure of 200 lb. to the square inch. It showed that either the plate or workmanship must have been very good, far better than was generally found in this country. He quite

agreed with Mr. Roberts that measuring-rods for testing elongation, should be made of wood, and not of metal.

Mr. W. ROBERTS had never seen iron that stood 30 tons to the inch in any boiler plate. He had seen steel stand that pressure. He had just completed a boiler from an idea for which he was indebted to Mr. Colburn, and its working results he would be happy to give to the society upon a future occasion.

The Chairman asked Mr. Roberts the breaking weight of the iron used for his cables?

Mr. ROBERTS replied that the breaking weight of the best Trinity iron was about 27 to 27½ tons to the square inch, and their ordinary Admiralty iron about 26½ tons.

Mr. PARSEY thought that there was no doubt the first working might leave some defects in it, which might be improved by working over again, the parts would be better welded; but he thought after the second working, the strength of the iron would be reduced instead of advanced. He had had opportunities of breaking iron made by several makers, but he had never found the strength very much greater in the very high brands. The higher qualities were softer and more ductile, and certainly better for some purposes; but he did not think the ultimate strength was greater. He should not take the result which Mr. Young had cited as being correct, without he (Mr. Parsey), had first overhauled the whole of the apparatus; for then very different conclusions might be arrived at. A short time since, at the Institution of Civil Engineers, the subject of the lasting qualities of iron structures was discussed, and the general opinion was that iron structures would last for a great many years. In passing over Hungerford Bridge a short time since, he observed the heads of the tension-bars (which were being taken down) where they had been in contact—they were perfectly sound, which confirmed the opinion that the structure would have lasted many years. Therefore, if constructed within the elastic limit of the iron, it was to be supposed, if the iron was in close contact and the parts painted outside, that the bridge would last perhaps for five hundred years.

Mr. PERRY F. NURSEY was of opinion that the reduction of the area of pins, or weakening of joints by rust was not a matter for very serious consideration. Deterioration of metal by oxidation was reduced to a minimum in iron structures under disturbance, in some

cases oxidation did not occur at all. This was particularly observable on lines of railways where there were rails under traffic and others in disuse, the former would be found to be free from rust, while the latter would be coated with it. He believed the absence of oxidation to be due partly to the mechanical and partly to the electrical action arising from passing trains, and was not confined to the points of contact, but common to all parts of the rail and its iron fastenings.

Mr. COLBURN expressed his best thanks for the very favourable way in which his paper had been received. He felt that he had not arrived at any conclusion upon the subject, because, really there was but little information upon which to base a conclusion. From what had been recorded, there was no question that the breaking strength of iron was very variable, and that the elastic limit was very much more variable. The elastic limit seemed to be the limit of the useful strength of the iron. In seeking for information upon these points, it was really surprising how very little information had been recorded, and how few of the opinions of practical men got into print, which was much to be regretted, seeing that it was the only data that could guide engineers when they had to work close. In locomotive boilers there was a great strain, and proportionate allowance should be made. In cases of stationary loads there could be no doubt that the ultimate strain could be worked up to a closer point, than in a locomotive boiler. In cases of water-tanks supported by beams, the limit of the breaking strain could be much nearer approached than in cases where there was vibration: that was shown by Mr. Fairbairn's experiments. His (Mr. Colburn's) object in bringing all these considerations together was to induce a greater attention to the subject, more especially to the elastic limit of iron. He hoped that experiments would be much more frequently made and recorded. With regard to the case mentioned by Mr. Young, of the strength attained by an American boiler plate, he (Mr. Colburn) might mention that the plate referred to was rolled from cold blast charcoal iron. The experiment took place upon the Baltimore and Ohio Railway: the weight actually applied in several cases was over 2 tons to one sixteenth of the square inch. There was no doubt the result was attained; but all these higher strains must be looked upon as exceptional.

The CHAIRMAN said that Mr. Colburn's paper, and the remarks made upon it, had proved how very difficult it was to arrive at any satisfactory conclusion, either as to the amount of breaking strain of iron, without breaking it, or the elastic limit of iron without stretching it beyond that limit. A bar of iron might have a small flaw which could not be seen till the bar was broken, and therefore could not be measured in measuring the amount of elasticity. A great point would be gained if these flaws could be detected in testing. Mr. Parsey said he used a metal rod, or guage, for measuring the amount of elasticity in Messrs. Howard and Ravenhill's links; now, unless the same temperature was given to the measuring-rod as was given to the bar by the work done in stretching it, the amount of elasticity would not be measured correctly; and, again, the bar under test would appear to have a permanent set, unless it was allowed to arrive at the same temperature as when measured before testing. It had been stated during the evening that in testing bars up to, say, 10 tons, there was an extension, after the strain was taken off the bar, which gradually disappeared in one or two hours; this apparent permanent set was due only to the heat generated in stretching the bar, and as soon as the bar arrived at the same temperature as before testing, its length would be the same. If iron could be got for use that would stand 28 or 30 tons per square inch, instead of iron that broke at 18 or 20 tons per square inch, we certainly ought not to be bound not to exceed a strain of 5 tons per square inch, as the greatest strain. If a load giving 5 tons strain is good for 20-ton iron, certainly $7\frac{1}{2}$ tons strain is equally good for 30-ton iron; therefore, instead of being bound, as was the case, not to exceed 5 tons per square inch for all kinds of iron, we were allowed to take $\frac{1}{4}$ th of the breaking weight of whatever iron we used, we should be under a far more sensible restriction. On the continent they had the same restriction as here, always one strain for all kind of iron, no matter what the strength was. From what had been stated during the evening, it certainly impressed upon us the necessity of testing every part of an iron structure before use. The Chairman concluded by stating that the discussion upon Mr. Colburn's interesting paper would be resumed by Mr. Ordish at the next meeting.

April 6, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON THE RELATION BETWEEN THE SAFE LOAD AND
THE ULTIMATE STRENGTH OF IRON.

By ZERAH COLBURN.

ADJOURNED DISCUSSION.

Mr. R. M. ORDISH said the paper read by Mr. Colburn to the Society at their last meeting contained a very valuable abstract of experiments, facts, and opinions on this very important subject; but the main question was still left open to form deductions from the data supplied. From those and other data engineers would arrive at very different conclusions, as recently evidenced by the investigations, as to the strength of the Chelsea Suspension Bridge, the engineers' reports and opinions on the question varying from 5 to 8 tons per sectional inch as the proper strain to be put on the main chains of this bridge, when loaded with a distributed load of 80lb. per superficial foot. For the purpose of arriving at a result as to the proper quantity of metal to be used in the construction of wrought-iron bridges or other load bearers, he took the requirements of the Board of Trade as regards Railway Bridges; viz.—that the tensile strain should not exceed 5 tons per square inch of section. The strain appeared to be reasonable and proper, and consistent with economical structures, and provision for the safety of the public. For several years past railway bridges had been made according to this requirement, and in most instances the quality of the iron used had been of mean breaking weight of 20 tons per sectional inch; therefore engineers generally, and the Board of Trade, were content with such structures, the iron being of a mean breaking weight of 20 tons per inch, the edges of the plates for the flanges, &c., being sheared, and the holes in plates, angle and T irons being punched. It was generally admitted on the authority of Mr. Fairbairn, that more than 20 per cent. of the strength of the iron was lost by punching,

such loss being distinct from that of the metal actually punched out. It was also generally admitted that the breaking strain per square inch of wrought-iron bars was 20 per cent. more than wrought-iron plates, the statement being likewise in accordance with the experiments made by Mr. Kirkaldy on the strength of iron. Therefore many railway bridges when riveted up and in place, were composed of iron of an effective breaking weight of 16 tons per sectional inch. Should a bridge be constructed of the same quality of iron as assumed above, but with the holes for the rivets drilled instead of punched, there was no doubt that the bridge would be stronger, and in the absence of experiments on the loss by drilling, he would assume that the iron would be of an effective breaking weight of 18 tons per inch. Therefore by drilling the hole a stronger bridge was obtained, but at an increased cost, the weight of metal remaining the same. The conclusion sought to be arrived at and proved, was that by employing a less quantity of superior qualities of iron, and better workmanship in bridge building, works were obtained of greater stability than the majority of railway-bridge constructions which were generally approved of, and were no doubt quite sufficient for their purpose. Instead of employing plates and angle iron riveted together for forming the tensile members of railway-bridge girders, if bars with enlarged ends were used similar to the links used in suspension bridges, it was found from the data previously summarised that instead of requiring an amount of metal dependent on the strain of 5 tons per inch, a strain of 8 tons per inch could be calculated on for determining the necessary quantity of material required, so as to give the same result. Assuming that the tensile strain or the bottom flange of a girder was 250 tons, it would require 50 inches effective sectional area according to the 5 tons per inch requirement of the Board of Trade. Comparing the required sectional area of iron of the punched and riveted form of construction against bar-iron with enlarged ends, and the pin-holes drilled, there was first a loss of 20 per cent. by punching, thus reducing the 50 inch area of assumed good iron to 40, and a further loss, or difference, between plate-iron and bar-iron of 20 per cent., reduced the 40 inches sectional area to 32 inches sectional area, which with a strain of 250 tons gave very nearly 8 tons per sectional inch, when the iron was assumed to be of equal quality to bar-iron.

Mr. W. T. CARRINGTON stated, according to Mr. Fairbairn's experiments, more than 20 per cent. of the strength of the iron was destroyed by punching. A loss quite distinct from that of the metal punched out. That being the case, and taking plates—as Mr. Colburn said we sometimes did—of a breaking strain of only 18 tons per square inch before punching, we should have iron of a breaking strength of only 14.4 tons after being punched. The Board of Trade said that 5 tons per square inch should be the strain allowed in the case of wrought-iron bridges—thus allowing the working strain to be as much as one third of the breaking strain. Now, according to M. Vicat's experiment, iron being loaded to $\frac{1}{3}$ rd of its breaking weight stretched in 33 months at the rate of $2\frac{1}{2}$ parts in every 1000 parts of its length, in addition to that which took place as soon as the weight was applied. Therefore in the case of 18-ton iron in plate bridges, holes being punched, 5 tons was too much to be taken as the safe load if that strain remained on the bridge for a long time, it was therefore fortunate for the bridge, that $\frac{1}{3}$ rd the breaking strain was in action but a very short time, for if that permanent stretching continued to go on, as was to be inferred from M. Vicat, it was natural to conclude that the iron would break in time with $\frac{1}{3}$ rd its breaking strain. Such strain, according to M. Vicat, was too much to be taken as a safe continuous strain; but Mr. Fairbairn had said that 3,000,000 applications of $\frac{1}{3}$ rd of the breaking strain did not injure a large model of a wrought-iron plate girder. If both the above experiments were correct, they led to the belief that the experiments were made with different qualities of iron, and that the proportion of the safe strain varied considerably with various qualities of iron; such, no doubt, was the case, for the colder iron was when rolled, the greater strain it would bear without permanent set. Again, rolled bars of small section would bear a greater strain per inch, than the same kind of iron in bars of large sectional area. In fact, the closer the fibres of the iron were brought together, whether by the rolls or the hammer, the greater was the strain per square inch, required to give a permanent set.

In testing iron it had been noticed by Mr. Roberts, Mr. Parsey, Mr. Light, and others, that with a strain close upon the elastic limit, there had been an extension which gradually returned—some-

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times in two hours, sometimes more—this extension could not be permanent set, or the bar would never have returned to its original length, and it could not be elasticity only, for if it was the bar would have returned to its original length the instant it was relieved of the strain. The most probable cause of such extension was the heat generated by the work done in stretching, and after the bar was relieved of its strain it would take some time for the heat to disperse, that time depending upon the mass of the bar, &c., besides depending upon the state of the atmosphere. If the measuring bar was made of the same material, and could have precisely the same heat given to it as the testing bar, then the extension spoken of would not be noticed, and for all practical purposes it ought not to be noticed, for it had nothing to do with the strength of the iron, it being caused simply by the heat generated by the work done. It appeared to him that a deal measuring bar would be the best in all cases of testing, or measuring, the extensions from strains, because its length was nearly always perceptibly the same, it varied but little; any bar of metal, no matter what kind of metal, would naturally vary, sometimes, considerably in length—the heat of the hand, a ray of light, a current of air, &c., was quite sufficient cause. Mr. Roberts said, at the last meeting, that he had seen bars shrink when a cloud had been passing over. He (Mr. Carrington) supposed the case of testing a bar, and while making the first measurement before putting the strain on it was cloudy, and while putting on the strain, taking it off, and making the last measurement, the sun shone, what would be the effect? It would be that the bar would appear to have a permanent set, no matter how small a strain had been put on; of course the extension would be evident if no strain at all had been put on. He mentioned this to show how very carefully testing experiments should be carried out before the amount of permanent set should be given for various strains.

If M. Vicat's experiments were correct, it might be supposed that 9½ tons strain applied for an instant, and 7 tons for, say, five years, on a wrought-iron bar one inch square, and in each case no permanent set had been given, and the elastic limit had been attained, then there would be a difference of ¼th of the elastic limit of the 9½ tons (that ¼th very nearly agreed with M. Vicat's experiments);

it could be proved by experiment if that difference was the same in all kinds of iron, and what reason could there be why it should not be the same? For take 6 tons instant strain of the elastic limit, and it was evident but little could be taken on either side of the $\frac{1}{4}$ th of the 6 as the difference. If, therefore, it was correct to assume that there was always a proportionate difference, and that proportion once decided, then take the elastic limit strain of an instant load, deduct the above-mentioned difference, and the result would be the elastic limit of a permanent load, which would seem to be a strain that could always be safely worked to. It was such strain that might be called the "elastic limit" of iron. From what had been said there were two elastic limits: 1st, the elastic limit of instant load; 2nd, elastic limit of permanent load. The second had still to be decided by experiment, which must be very extensive and most carefully conducted. In the case of M. Vicat's experiments, the most probable cause of the gradual extension, or permanent set, was the action of heat, or variation in temperature, for if a wire was made to support one third of its breaking weight, heated, and then suddenly cooled, it was very certain that the wire in suddenly contracting must overcome the inertia of the hanging mass, and to do this it must exert a greater strain, which strain is over the elastic limit of the instant load, and would therefore give the permanent set. The more sudden the contraction the greater would be the extra strain, and therefore permanent extension. He thought M. Vicat did not attempt to keep his wires, while under experiment, at the same temperature for thirty-three months. If he had done so, it was most probable there would have been no gradual permanent extension of the wires from the permanent loads. In making the experiments required to decide the difference between the elastic limits of the instant load and the permanent load, great care would have to be taken that the iron should be subject to all the variations of temperature it would be subject to, as in the case of a bridge, otherwise the proper difference to be made use of would not be got. There was still that great difficulty of finding any flaw of infinitely small length. It was quite out of the question to expect that it could be measured, and if he could not find it by inspection and the elastic limit test, he would give it up as hopeless, and trust to providing against such small weaknesses by the number of parts

side by side, so that the chances would be always in favour of never having more than one flaw in the section of, say half a dozen pieces. When this provision could be made, no particular anxiety need be felt on the subject. In conclusion, Mr. Colburn thought his paper would hardly fulfil its title. He (Mr. Carrington) was sure no one could expect him to come nearer fulfilling it; if he could have given the data for arriving at the elastic limit of all kinds of iron, *i.e.* the elastic limit of iron strained for, say, five years or more, then he certainly would have conferred a great boon on all engineers; but if he had not given that data, he had shown very clearly how much it was required, and that it could be acquired by experiment, that was certainly the next thing to giving the data. He was sure many had no idea of the very great difficulty of arriving at the *useful* strength of iron, until Mr. Colburn read his most valuable paper.

Mr. PARSEY said he thought he mentioned at the last meeting that between ten or twelve tons to the inch in well-manufactured iron did not produce permanent set, and that in a great number of instances, where there was an apparent permanent set, it was owing to a flaw in the bar; that would sometimes make it appear that there was a permanent set, when it was only an extension in the defective part. It was necessary to be very careful in estimating the assumed loads. He thought five tons to the square inch might be taken as a general rule, for it was only a proper measure of caution to provide for defect in the iron, and deterioration of strength by punching and oxidation. As regards the bars used in the construction of Chelsea Bridge, a statement had gone forth to the effect that they would bear a tensile strain of thirty tons to the square inch; now that appeared to him erroneous, as he had broken many similar bars under twenty-four tons to the inch; he thought they should be very careful in giving iron a very high working strain, unless they were perfectly sure that the bars would be perfect, and thoroughly inspected during manufacture.

Mr. C. J. LIGHT said that, with the permission of the meeting, he would read the results of experiments on the strength of pieces of test iron that had been reduced to the square inch of section at one point only by two drilled holes, as compared with other pieces from

the same plates or bars cut to a parallel width for a length of twelve inches, as usual for trying extension. The results, in tons per square inch, in four pieces of plate iron, were as follows :

Cut parallel.....	21·0	24·21	24·1	22·5
Drilled.....	22·5	25·3	24·1	23·02
Difference per cent.....	5 $\frac{1}{2}$	4	7	2 $\frac{1}{2}$

Four pieces of T iron gave the following results :

Cut parallel.....	22·15	23·51	22·76	23·20
Drilled.....	23·14	25·6	25·98	24·58
Difference per cent.....	4 $\frac{1}{2}$	8 $\frac{3}{4}$	14	4

He explained the above differences on the supposition that in the parallel piece there was sure to be one point much weaker than the rest at which it would break, but the chances were greatly against the drilled hole coming at such a weak place.

There was also one point in connexion with plate-testing, which he had remarked and confirmed by several experiments, viz.—that in thin plates as $\frac{1}{4}$ th or $\frac{1}{8}$ ths, if the piece to be tested was fixed in the usual way by bolts or pins passing through its ends, the width of the reduced part in the centre should not much exceed the diameter of the pin, or an unfair strain would be put on the iron, the centre being strained to failing before the edges were able to aid it.

He then quoted some high tests of angle and bar iron from Rhymney Iron Works.

A piece of $4 \times 4 \times \frac{3}{8}$ angle broke at 27·19 tons per inch.

„	$5 \times 3 \times \frac{1}{2}$	„	25·96	„
„	$7 \times 4 \times \frac{3}{8}$	„	27·47	„
„	$9 \times \frac{1}{2}$	bar bore	28·2	„

As these results were obtained not from prepared samples, but from the bulk of a large delivery, he thought them worth recording.

He had also obtained, within the last few days, some particulars with respect to some five-inch links lately tested at Howard and Ravenhill's works. The results were tolerably satisfactory, because

two of them came up nearly to 24 tons, and the extensions and permanent sets on a five-foot length were given at frequent intervals. The results were as follow :

LINKS $5 \times \frac{3}{4}$ IN BODY.

No. 1.

Tons per inch.....	8	10	11	12	13	14	15	20	21·06
Extension.....	·05	·05	·06	·06	·07	·07	·67	3·82	broke.
Permanent set.....	·00	·00	·00	·00	·07	·00	·60	3·69	9·62

No. 2.

Tons per inch....	8	10	11	12	13	14	15	20	23·73
Extension.....	·04	·05	·05	·06	·075	·08	—	3·60	broke.
Permanent set....	·00	·00	·00	·00	·00	·01	—	—	11·0

LINKS $5 \times \frac{1}{2}$ IN BODY.

No. 3.

Tons per inch.	8	10	11	12	13	14	15	16	20	23
Extension.....	·04	·05	·06	·06	·07	·07	·08	·25	3·16	broke.
Permanent set.	·00	·00	·00	·00	·00	·00	·00	·17	3·08	6·75

No. 4.

Tons per inch.	8	10	11	12	13	14	15	20	23·92
Extension.....	·04	·05	·05	·06	·06	·08	0·9	1·79	broke.
Permanent set	·00	·00	·00	·00	·00	·00	·015	1·70	10·75

Mr. E. RILEY thought it would be satisfactory if Mr. Light would give some information as to the process of manufacture that the iron had undergone, and the materials from which it was made, as he understood Mr. Light to say it was common iron.

Mr. C. J. LIGHT stated that in the experiments he had given, the iron was cut and tested in the ordinary course of inspection.

Mr. E. RILEY said the iron appeared to have borne an extraordinary strain, and he thought it would be important to know the date at which the iron was manufactured, as he was aware that large quantities of Spanish ore, containing much manganese, had been used at Rhymney, and he thought that the presence of this metal in an iron ore had a most beneficial effect on the quality of the iron, and might probably account for its unusual strength.

Mr. C. J. LIGHT replied that the experiments on the Rhymney iron were made in September and November, 1861, and the iron was supplied three or four months previously. It was hardly an exceptional case, as several other pieces bore almost equally high strains.

Mr. E. RILEY inquired whether the iron mentioned was angle iron, and what means were employed to determine the permanent set?

Mr. C. J. LIGHT stated that the piece that bore twenty-eight tons was bar iron, and the others were angle iron. The permanent set and extension were ascertained by means of a fine scribe.

Mr. OLBICK wished to know if there had been any experiments with Bessemer's homogeneous metal. He (Mr. Olrick) called it metal because it was better than iron, and not exactly steel. He would also ask if any extensive experiments had been made with respect to the breaking or tensile strain of that metal? Where the breaking strain had been ascertained as well as the limit of elasticity, in the same kind of iron and under similar circumstances, he would ask whether it would not generally apply that a three-fifth part of the breaking weight would be a rough and ready rule for the limit of elasticity?

Mr. ROBERTS would make one remark with regard to punched plates. He believed that Mr. Fairburn and others contended that punched plates were more liable to contain flaws or defects than drilled plates. He (Mr. Roberts) agreed with them to a certain extent. In bridge building, boiler-making, &c., the joints were broken by one plate having its butt in the middle of the one above or below it; by this means one plate supported the other. When the edges of the plates were punched, he had no doubt there was as much as 20 per cent. difference up to a certain point in the plate. But he did not

think that could interfere very much with the middle of the plate. You must not take off 20 per cent. for the whole surface of the iron, because it was punched at the edges. He then gave the following table of results of testing four pieces of iron :

No. 1.

Piece of Lowmoor iron, $2 \times \frac{1}{8}$, cut from a plate longitudinally, or with the fibre ; distance between centre 3 in. ; commenced stretching at 10 tons.

At 11 tons stretched	·03
„ 12 „ „	·08
„ 13 „ „	·14
„ $13\frac{1}{2}$ tons broke ; size at fracture	$1\frac{1}{8} \times \frac{1}{4}$ full
Limit of elasticity	16 tons per inch
Breaking strain	22 „

No. 2.

Piece of Lowmoor iron, $2 \times \frac{1}{8}$, cut from same plate as above, but transversely ; distance between centre $3\frac{1}{2}$ in. ; commenced stretching at 10 tons.

At 11 tons stretched	·04
„ 12 „ „	·08
„ 13 „ „	·16 and broke ; fracture $1\frac{1}{8} \times \frac{1}{4}$ full
Limit of elasticity	16 tons per inch
Breaking strain	20·8 „

No. 3.

Piece of G. B. Thorneycroft and Co's. treble refined iron, $2 \times \frac{1}{8}$, bar cut longitudinally from a plate ; $3\frac{1}{2}$ in. between centres ; commenced stretching at $9\frac{1}{2}$ tons.

At 10 tons stretched	·04
„ 11 „ „	·09
„ 12 „ „	·155
„ 13 „ „	·25 and broke, size of fracture $1\frac{1}{8} \times \frac{1}{4}$
Limit of elasticity	15·2 tons per inch
Breaking strain	22·4 „

No. 4.

Piece of G. B. Thorneycroft's treble refined plate, $2 \times \frac{1}{8}$, cut transversely from the above; $3\frac{1}{4}$ in. between centres; commenced stretching at 11 tons.

At 12 tons stretched .05

„ 13 „ „ .10

„ 14 „ „ .135

„ $14\frac{1}{2}$ tons broke; size at fracture $1\frac{1}{8} \times \frac{1}{4}$

Limit of elasticity 17.6 tons per inch

Breaking strain 23.8 „

He thought these results for soft ductile iron, was very satisfactory. He had tested some Swedish iron, which, stretched longitudinally, bore a strain of $32\frac{1}{2}$ tons to the inch, and transversely it gave a breaking strain of $27\frac{1}{2}$ tons. As regards the liability of flaws he had examined closely into that subject, and he found there was a series of flaws in each piece. He did not think he had ever seen a small plate (say) six inches long without one or two flaws. It was very difficult to find plates without a flaw.

Mr. LIGHT inquired what means were employed for determining the permanent set.

Mr. ROBERTS said that the pieces were so short that he was obliged to use small dividers. During the last twelve months he had tested some hundreds of plates to be used in vessels for the Government, and he could assure the meeting that very few could stand 20 to 22 tons to the inch, others 21 to $21\frac{1}{2}$ tons, and sometimes it was difficult to obtain even that strength.

Mr. LE FEUVRE thought the tendency of the present discussion was in favour of greater tests and a superior quality of iron being employed; but it was to be remembered that the Board of Trade test was higher than the tests required by several foreign governments.

For example, in France the test was 4 tons 10 cwt., in Italy 4 tons, in Holland $3\frac{3}{4}$ tons.

These tests were very low; but they were low, he thought, in consequence of the inferior quality of the iron in use.

As regarded bridges, he thought there was no question that the tests ought to be higher when superior qualities of iron were employed, and the several parts of a bridge had been actually submitted to a much higher strain.

The bridges recently built, or in course of construction, over the Thames, such as the Charing Cross Railway bridges, the Lambeth and the London, Chatham and Dover Railway bridge, were so designed that every portion of the bridge could be tested, with the exception of the cross girders, and under those circumstances there could be no question a much greater strain could be put upon the iron than upon arch girder or plate girder bridges.

Mr. RILEY said it was obvious that flaws in plates would be more detrimental than in bars, as in bars the flaw was drawn out to an exceedingly fine thread, whilst in plates it was extended. In a case recently submitted to him respecting iron for tin plates, it was found that in the bad plates, which were much spotted all over with hard lumps, that very minute portions of charcoal were enclosed in the iron; this prevented the iron welding, and converted the iron contiguous to it into a hard steel, so that these lumps were transferred in doubling the plates, from one to the other; in the subsequent rolling of the plates this defect could never be remedied. Such defects as the above are only found where charcoal is used to refine the iron, and shows the great effect of any foreign matter in the iron when rolled into plates. The plates where these defects exist crack in bending, and make them almost useless.

In experiments on the tensile strength of iron, he thought that attention ought to be paid to the character of the ores used for manufacturing the iron, and also the process of manufacture employed, as it was only by these means that the smelter of the iron could obtain data upon which he would be able to improve the quality of the iron he produces.

Mr. COLBURN said he had heard nothing to induce him to alter his opinion that there was great variation in the strength of iron; and there was reason to believe that the working strength of iron for working purposes was rather over estimated. He had recently been very much struck with a letter which had been published,

written by Messrs. Napier, of Glasgow, to Lord C. Paget, of the Admiralty, which letter spoke of 22 tons being a very high strain indeed. The experiments that were made were generally upon small specimens. He thought they were too much disposed to assume the strength of plates to be higher than it really was in practice.

The Government were testing all iron supplied at Chatham. A contract had been offered to the Staffordshire makers to manufacture plates upon the condition that they shall stand 24 tons to the inch, and it appeared impossible for the manufacturers to comply with the condition.

With regard to Bessemer's metal, so far as he (Mr. Colburn) knew, he did not think its elastic limit had been tested; indeed, Mr. Bessemer made so many different kinds of metal. He made the strong highly carbonized metal, and a lower quality for rails. The amount of carbon varied very much indeed, and this regulated the tensile strain and the purpose to which it was to be applied. Consequently, the exact proportion between the elastic limit and the breaking weight, could not be assigned; in fact, there was no definite proportion whatever, either three-fifths, two-fifths, or anything else. He believed all the facts proved this. Take the Swedish iron for instance; the elastic limit of that was very much lower than hammered iron. Take, also, certain kinds of Yorkshire iron; the elastic limit of which was 16 tons, and the breaking strain 20 tons; so that the elastic limit was very nearly four-fifths of the breaking strain. If they referred to Barlow's work "On the Strength of Materials," they would find that the elastic limit of one bar was put at 32 tons, and the breaking strain at $32\frac{1}{2}$ tons.

The CHAIRMAN did not think it necessary to detain the meeting by any lengthened observations, although it was true that Mr. Colburn in his very able paper had not solved the difficult problem upon which it treated; yet, the question was so ably and clearly put before them that each could draw his own conclusion from it. From the extraordinary variation of iron in every possible way, it was difficult, if not impracticable, to arrive at any rule to guide them. There was no doubt that one of the first considerations in designing a bridge was to decide on the quality of the material with which it

was going to be built. He quite agreed with Mr. Colburn that iron varied in tensile strain from 15 tons to 30 tons to the inch, and that under such extraordinary range some strength must be assumed to which the parts would be tested. After the positive strength of the material to be used was decided on, they had next to consider the nature of the construction of that material. Messrs. Ordish and Carrington had touched upon that point. As regards the punching and drilling of plates, he (the Chairman) thought, before Mr. Roberts made his remarks, that the talented reader of the paper had left this point a little dubious. But it occurred to his (the Chairman's) mind, whether the extra cost of material necessarily required for punched work might not be less than the extra cost of labour in drilling and punched work preferable on this account; this view was quite overlooked by Mr. Ordish and others. He thought that Mr. Roberts's views were more correct with regard to punched plates, than those given by Mr. Light; that the strength could only be damaged by punching in proportion of the section punched, to the section left untouched in a girder, and that the loss of strength could not be the same for every construction of girder. Mr. Parsey had said that so long as the elasticity of the plate was not injured that it was safe. Mr. Parsey also said that he thought the elastic limit was put at rather too low an amount; but he (the Chairman) considered it was wise to do so. He understood Mr. Parsey to say that something like 11 tons, as the maximum strain, might safely be put on first-class iron; under these circumstances Mr. Parsey would be quite safe. He (the Chairman) thought that, as far as experiments were concerned, so long as the plate was undamaged, and the elastic properties were uninjured, there was no doubt they were perfectly safe; directly they got permanent set the iron was being damaged, and the strain was more than should have been applied. Another remark was made about the construction of bridges, all parts being designed so that every portion of the structure could be tested by itself before erection. Now that, no doubt, was thoroughly good in principle; for if they constructed a bridge of girders, and those girders were each carefully tested, they could be perfectly satisfied that the bridge would be safe, having not only the knowledge of the strength of the materials in the first place by calculation, but likewise an assurance of sta-

bility in the larger sections as separately tested. In conclusion, he might say that, although the present discussion had not enabled them to form any definite theory upon the subject, yet it had so ventilated it that the members of the Society generally would have a much clearer notion than hitherto of the important matter which Mr. Colburn had introduced in the elaborate paper read by him at their last meeting.

May 4th, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON THE CONSTRUCTION OF CHELSEA BRIDGE.

BY GEORGE GORDON PAGE.

THE Chelsea Suspension Bridge, which has been opened to the public for the last five years, is a bridge remarkable in many respects, and which, in point of design, mode of construction, and economy of cost, presents features of great interest.

In the year 1846 an Act of Parliament was obtained, and the necessary funds granted for the construction of this bridge, which forms a communication between Pimlico, Belgravia, and Chelsea on one side of the river, and Battersea Park and the surrounding neighbourhood on the other.

In addition to the design for the suspension bridge the engineer, Mr. Page, was instructed to prepare, for the consideration of the Metropolitan Improvement Commission, designs both for a bridge of seven arches, faced with stone, and one in cast-iron of five arches; but, ultimately, the Chief Commissioner of Her Majesty's Works decided to carry into execution the suspension bridge originally mentioned in the Act.

In point of construction this bridge deserves particular attention; the whole of the details having been worked out with great care. Not only were the latest improvements made available, but many important innovations, calculated to ensure the stability of the structure, were introduced. The foundations, which will presently be

described, are similar in principle to those of Westminster new bridge, which were the subject of much discussion in Parliament a few years back. By the method adopted coffer-dams are entirely dispensed with.

The use of suspension bridges has of late increased, and is still much on the increase. Within the last few years three metropolitan bridges on this principle have been erected over the Thames, and application has been made to Parliament to erect more. The advantages of a suspension bridge are—the economy with which it can be constructed, and generally its non-interference with the bed of the river. It offers less impediment to navigation than any other description of bridge, and can be thrown across openings where it is impracticable from the altitude of the banks, the rapidity of the current, and other conditions, to erect centering for the construction of stone and other bridges. It can be erected with greater ease and expedition, and generally at much less cost than bridges constructed on other principles.

As regards the comparison of a suspension bridge and an arch, with a girder of the same span, deflection (or depth) and strength, it is evident that while the arch will correspond with the upper member of a girder, the chains of a suspension bridge will correspond with the lower member or tie of the girder; and, therefore, a great advantage is possessed by the suspension bridge and arch bridge, in point of weight, over the girder—a girder being, as it were, a compound of the two constructions. The arch is a structure which requires so many additions to maintain its shape, that it cannot practically be looked upon as only being in a state of compression. The case of the suspension bridge is, however, quite different: the chains being in a state of equilibrium, preserve their form independently of any additional assistance; the only disadvantage of the suspension principle, as applied to railway purposes, is its flexibility: but even this has been successfully overcome in the case of the Niagara bridge, built under the direction of Mr. Roebling.

This bridge crosses the Niagara river at a height of 245 ft. above the level of the water by a single span of 821 ft.; its rigidity is owing to the deep trussing, 18 ft. deep between the upper and lower ways.

It should be borne in mind, that suspension bridges are incomparably slighter than bridges of stone and cast-iron. A bridge intended to be a great and perpetual thoroughfare, exposed to crowds and heavy goods' traffic—in short, a bridge in the position of London Bridge, should not be on the suspension principle. If the strength were increased to that of an arched bridge, such as London or Southwark, the extra weight, the difficulty of erecting the chains, and the increase of the foundations, would so raise the expense that it is doubtful whether it would not be more economical to build an arched bridge.

For large openings, where it is important to have a permanent passage, and where the traffic is not very great, as at Lambeth, suspension bridges are admirably fitted, as they can be constructed of almost any span, and at any height, with greater economy than bridges on any other principle.

As regards preservation, every link, every bolt, every bearing, from the moorings on one side to those on the other, are, or should be, in a properly constructed suspension bridge, open to inspection, and can at any time be examined, and preserved with paint or other material.

GENERAL DIMENSIONS.

The length of Chelsea Bridge is 704 ft. from face to face of abutments; and consists of a centre opening of 333 ft., with two side openings 166 ft. 6 in. each. The piers are 88 ft. long, and 19 ft. wide, terminating in curved cutwaters; the piers are carried to a height of 7 ft. 6 in. above high-water mark, the width of the bridge is 47 ft., the roadway at the centre of the bridge is 24 ft. 6 in. above high-water, and has a curve of 18 in. rise, commencing at the abutments. The towers and ornamental casings are of cast-iron. The girders and flooring of the platform of wrought-iron.

OF THE ABUTMENTS.

Too much attention cannot be bestowed on the abutments of a suspension bridge, as on their careful consideration and construction so much depends.

The abutment is the mass of masonry, or in some cases of natural

rock, to which the extreme ends of the chains are made fast, and by the weight of which the strain from the chains is resisted.

The principles of the stability of the abutment of a suspension bridge, are the same as those of the abutment of an arched bridge, but reversed.

In the former there is a tendency to upset or slide forward instead of backwards, as is the case in the latter. The weight or gravity of the abutment should always be sufficient to prevent it from sliding on its base, and its form and dimensions should be sufficient to prevent it from upsetting. The tendency to sliding forward may be considerably lessened by making the base of the abutment, or a portion of it, slope so as to be at right angles, or nearly so, to the resultant of pressure.

Of all parts of a suspension bridge, the abutments are the last in which solidity and stability should be sacrificed to motives of economy.

The weight of the abutment should be equal to resisting twice the utmost strain that can be brought upon the chains by dead weight, and the total power of resistance, combining the weight and the tendency of the abutment to slide from the ground on which it stands, should be at least equal to four times the utmost strain that can be brought upon it.

The resistance offered by the adhesion of the abutment to the ground on which it stands depends entirely upon the nature of that ground, and cannot by any general rule be accurately predetermined.

When piles are used in the foundation, they should be driven at an angle approaching as near as possible to the direction of the resultant of pressure.

With regard to the saddles on the abutment, by the aid of which the direction of the chains is changed, it is not always necessary to place rollers under them, but as they must be capable of sliding to a sufficient extent, other means are sometimes resorted to in bridges of short span, and the saddles are sometimes laid on a bed of asphalted felt. In large suspension bridges rollers are, however, universally used, to allow for the expansion and contraction of a necessarily large extent of chain.

As it is most important that the chains or wire cables of a suspension bridge should be kept free from rust, the tunnels in the abutment through which the chains pass down to their fastenings are generally constructed of such dimensions as will allow of space for access for the purposes of examination and repair if required.

The abutments of the Chelsea Bridge consist of a mass of brickwork and concrete, measuring at the base 112 ft. in length, by 56 ft. broad, and at the top 100 ft. by 46 ft. and 40 ft. deep.

The face of the abutment adjoining the river is composed of cast-iron piles and plates, somewhat similar to those of the pier, with the exception that the ironwork is not brought above the level of low water.

The portion of the abutment on which the land saddles and cradles bear, for changing the direction of the chains, rests upon timber piles, 14 in. square, driven deep into the bed of the river, and are from 3 ft. 2 in. to 4 ft. from centre to centre; these piles are cut off at the level of low water, 16 ft. below Trinity high-water mark, and the spaces between filled up with hydraulic concrete; the cast-iron and timber piles are tied together with wrought-iron ties 3 in. $\times$ $\frac{3}{4}$ in. On the top is bedded a series of landings, forming a table at the level of low water 53 ft. 6 in. $\times$ 27 ft. $\times$ 6 in., upon which a mass of brickwork is erected, up to a mean level of 3 ft. below the level of the roadway—upon this, 12 in. landings are bedded for the reception of the cradles which carry the saddles on rollers; the cradles are bedded in asphalted felt, and firmly secured by wrought-iron holding-down bolts, brought up through the masonry from below. An invert, springing from beneath each saddle, is built in the brickwork below, so as to distribute equally the pressure from the cradles over the whole area of the foundation.

The mooring chains are carried down tunnels to the moorings, the tunnels forming an angle of 155 deg. with a horizontal line. The chains are secured to massive cast-iron mooring plates, resting against three courses of 12 in. landings, respectively 12 ft. $\times$ 8 ft. 9 in., 16 ft. $\times$ 12 ft. 6 in., and 20 ft. $\times$ 16 ft. 3 in. The tunnels are contracted at the bottom by elliptical brick domes, thus affording a complete bearing for that portion of the landings at the end of the tunnel. These landings rest against a mass of brickwork, with inverts, to distribute the pressure over the whole area of the abutment.

This mass of brickwork rests on a series of timber piles, driven at an angle of 65 deg., with a horizontal line; the tops of the piles coming up above the level of the concrete, and having a good bond with the brickwork, by which means the tendency to slide is greatly diminished, the whole spaces between the masses of brickwork being filled up solid with concrete.

THE PIER FOUNDATIONS.

The construction of the foundations of the piers combines all the advantages of foundations on bearing piles, made by means of cofferdams, without the expense and obstruction to the waterway which they involve, and which would have rendered their use at Westminster Bridge all but impracticable.

The foundations of the piers consist of timber bearing piles 14 in. square, driven deep into the bed of the river at intervals of 3 ft. over the whole area of the pier, varying in depth from 40 ft. 6 in. to 25 ft. below the level of low-water, according to the resistance offered by the bed of the river.

The face or external surface of the piers consists of a cast iron casing of piles and plates driven alternately; the main piles are 12 in. in diameter and 27 ft. long, with longitudinal grooves on each side for the reception of the plates. These piles are driven to a uniform depth of 25 ft. below the level of low-water, and between them are driven cast iron plates or sheeting 7 ft. 2 in. wide, so that the pier is entirely cased from the foundations to the top, which is 7 ft. 6 in. above Trinity datum. The space enclosed by this casing is then dredged to the hard gravel above the clay, and filled in solid with concrete up to the level of the top of the timber piles. On this foundation a flooring of stone landings is bedded, and on this the cast iron plates, frames, &c., forming the base of the towers are placed.

The portion of the caisson situated above low-water is hollow, being so formed to avoid throwing useless weight on the foundation, and is merely lined with brickwork, strengthened by cross walls and iron ties.

The whole of the ironwork below the water was covered when hot with a protecting coating of tar. The thickness of metal in the caisson is 1 in.

OF THE TOWERS.

The towers which support the chains are entirely independent of the ornamental cast iron casing surrounding them, and consist of a cast iron columnar framing strongly braced both horizontally and vertically, carried to a height of 57 ft. above high-water.

The columns are cast in pairs and have a diameter of 10 in., and thickness of metal of 1 in. They are arranged in clusters of fours, and the whole are connected with six horizontal frames, occurring at intervals. The columns are not vertical, but incline towards each other upwards from either side of the pier, the columnar framing being 13 ft. 6 in. at the base, and 9 ft. 9 in. at the top. In the direction of the piers the columns are 4 ft. 3 in. apart, and rise parallel to each other. There are two towers on each pier, 32 ft. from centre to centre. The pressure from the chains coming directly from their centre, each tower carries therefore one-fourth of the whole weight of the bridge, or about 375 tons, or about 670 tons when the bridge is completely loaded; the sectional area of the columns is 284 square inches, and there is, therefore, a pressure upon them when the bridge is loaded of 2.36 tons per square inch of section. The weight of the towers, exclusive of the ornamental cast iron casing, is 350 tons.

On the towers are fixed massive cast iron cradles upon which the saddles rest.

OF THE PLATFORM AND ROADWAY.

The roadway platform is carried by two longitudinal trellis girders, running the whole length of the bridge from abutment to abutment, immediately beneath the chains, by which they are supported at intervals of 8 ft. These girders are suspended from the chains by wrought iron rods 2 in. in diameter.

The weight of the roadway is distributed over the whole of the four chains by the coupling plates to which the rods are attached; the rods are jointed at the chains and at the roadway to accommodate any lateral motion that may occur, and are provided with screw coupling boxes for their adjustment; the suspension rods pass through the longitudinal trellis girders, and support them from beneath.

The transverse girders which support the roadway are placed 8 ft. apart from centre to centre, immediately under the suspension rods, and bear upon the bottom flange of the longitudinal girder, are 31 ft. 10 in. long, 2 ft. $2\frac{3}{4}$ in. deep at the centre, and 1 ft. 11 in. at the ends, where they are connected by a system of rivetting with cantilevers 7 ft. long, which practically form a continuation of them, and serve to support the overhanging footpaths; the sectional area of the top and bottom flanges is 10 in., and the vertical rib $\frac{1}{4}$ in. thick stiffened with T-iron.

The small roadway bearers between the transverse girders are from 3 ft. 3 in. to 3 ft. 10 in. apart, 8 ft. long, and vary in depth from 1 ft. $5\frac{1}{2}$ in. to 1 ft. $9\frac{1}{2}$ in. to suit the cambered surface of the roadway.

The several girders that support the roadway thus form a series of rectangular cells, which are covered with arched plates of wrought iron, stiffened with angle iron.

The haunches of the plates are filled in with a light concrete, composed of cork and bitumen.

Previous to laying the bitumen concrete, the plates and girders are coated with asphalt.

The roadway is paved with oak blocks 6 in. $\times$ 3 in. $\times$ 4 in., bedded in bitumen, and trams of timber, flush with the roadway, with wrought-iron strips bolted down on the top for durability.

The preference was given to the cork and bitumen concrete as a bedding for the roadway blocks, on account of lightness compared with ordinary concrete. Concrete, moreover, in such a position, and in so thin a layer, is liable to crack, and become in time pulverised, and (then no better than loose gravel) liable to be deranged by passing traffic.

The footpaths are paved in the same way, only the blocks are of smaller dimensions. This pavement rests on planking placed on joists running longitudinally, resting on the cantilevers. The available breadth of the carriage-way is 29 ft., and footpaths 14 ft. 4 in.

The longitudinal trellis girder is 6 ft. deep, and its flanges are composed of a top plate 10 in. $+1\frac{1}{4}$ in., and two angle irons $3\frac{1}{4}$ in. $\times$ $3\frac{1}{4}$ in. $\times$ $\frac{3}{8}$ in. thick, the effective area of the top and bottom flanges is $12\frac{1}{4}$ square inches.

This girder materially stiffens the roadway and prevents, in a great degree, that undulation to which suspension bridges are liable.

The handrail is of wrought iron, secured to the cantilevers at every 8 ft. by brackets. The ornamental bosses and stays for supporting the railing are of cast iron.

THE CHAINS AND SADDLES.

The chains of the Chelsea Bridge are four in number, two being placed on either side, at a distance apart of 32 ft. They consist of links of seven and eight bars alternately, 8 in. wide, and of lengths varying from 16.55 ft. at the towers to 16 ft. at the centre of the span, so as to admit of an uniform horizontal distance of 16 ft. from centre to centre of the pin-holes of each link, and are connected by pins $4\frac{1}{4}$ in. in diameter. The aggregate section of the four chains at the towers is 230 square inches, and at the centre $217\frac{1}{4}$ square inches. The span of the centre opening is 348 ft., and the deflection of the chain is 29 ft. The semi-span of the back chains is 183 ft., and the deflection 30 ft. 6 in. The length of the chain for the centre opening is 354 ft. 5 in., and the length of each of the back chains 186 ft. The mooring chains are placed at an angle of 25 deg., and are 95 ft. long, and have an aggregate section of 235 square inches. The total weight of the chains is 340 tons. The chains are carried over the towers by means of saddles formed of No. 8 1 in. wrought iron rectangular plates, 5 ft. 8 in. long, and 2 ft. 10 in. wide, placed at intervals of 1 in. apart, and bolted together by No. 10 bolts. The bottom edges of the plates are planed, and are let into a cast-iron plate 4 in. thick, also planed on its top and bottom surface, and which moves on ten 6 in. diameter steeled rollers, working on the cast-iron bed-plate fixed at the top of the towers. The chains are connected to the saddles in the same way as the links of the chains are connected together. At the abutments the chains are diverted down the tunnels by means of saddles of similar construction to those on the towers, based on cast-iron cradles, and placed at right angles to the resultant of the strains.

For mooring the chains the following means were adopted:—As has been observed in the description of the abutments, the tunnels for the mooring chains are closed at the bottom by elliptical-shaped

brick domes, against which the York landings are placed at right angles to the angle of inclination of the mooring chains. The chains pass through holes formed in the centre of the landings (the dimensions of the landings were stated in the description of the abutments). A brick semi-circular arch or invert springs from the outer face of the landings, and connects the two sets of landings of each abutment together, by which means the whole weight of the middle portion of the abutment, it will be seen, is made to resist the pull of the chains. The chains are secured by means of castings 21 in. deep, abutting against the landings, and are divided, each into four compartments, rather more than two inches wide, through which the chain bars (here put two and two together) pass, and are moored by keys driven through the heads of the bars, and bearing against the mooring castings. Keys were here used instead of pins to allow of an adjustment in the length of the chains. Similar means for adjusting the lengths of the chains were made at the saddles on the towers, but were not needed.

In calculating the length for the chains the curve may be assumed to represent a parabola, though, strictly speaking, the curve of the chains is peculiar to the construction; but deduction being made for the stretch due to the tension, caused by the appended weight, the weight so deducted will be found practically correct. Care should be taken to ascertain the exact distance of the span, as a small error in the horizontal distance will cause a serious error in the amount of deflection. It is well to provide for any discrepancy of this kind by leaving the centre links of the chains the last to be rolled; when the error being known it can be rectified without any serious interference with the rest of the construction.

For the erection of the chains four temporary chains were thrown across made of 2 in. round bar iron, and placed one on each side of the line of the chain to be erected. Upon these temporary chains travelling purchases worked; by which the bridge chains were hoisted and put in place. Four other and similar chains were thrown across beneath the former mentioned ones to which timber platforms were suspended, and which served to carry the bars of the chains until the connection of the links was complete. In the hope that the descrip-

tion may be acceptable, a few observations are subjoined respecting the manufacture of the bars.

The bars for the chains of the Chelsea Bridge were manufactured by the process patented by Messrs. Howard and Ravenhill, by which the head and body of the bars are rolled of one piece, and was effected as follows:—Piles or, as they are technically called, balls of cleansed scrap iron, of about $\frac{3}{4}$ cwt. each, were heated (eighteen balls being the usual charge) in a reverberatory furnace of ordinary construction, and afterwards hammered into slabs about 2 in. thick by a 4-ton wrought iron hammer. The slabs, while still hot, were then piled in sets, of the weight required for the respective bars, and again heated and hammered into oblong masses of iron called shingle, somewhat wider than the width for the bars, and about 2 ft. 9 in. long. The time required for heating the balls of scrap was one hour and a quarter, that is, so much time elapsed from the time of charging the furnace to the withdrawal of the first ball; and the time required for hammering the eighteen balls into slabs was three-quarters of an hour. It may, therefore, be observed that the last ball withdrawn was nearly twice as long in the furnace as the first ball was; and it may, consequently, be supposed that some of the balls of scrap were too much and others too little heated, but the precautions adopted in the management of the furnace prevent any great irregularity in this respect. The balls first withdrawn were placed nearest the furnace, and, as withdrawn, the remaining balls were pushed nearer the furnace, or otherwise, as their state required. The time for hammering a pile of slabs into shingle was about five minutes. By the two heats and hammerings the loss of iron was about 13 per cent.; and after the shingle was rolled into bars the total loss of iron was 20 per cent., that is, the bar weighed one-fifth less than the scrap iron weighed from which it was manufactured. For converting the shingle into bars of the required form the shingle was heated to the required temperature in the furnace of the rolling mills, and was then passed longitudinally through rollers till reduced to a width of 8 in., and to a thickness of $2\frac{1}{2}$ in. It was then transferred to other rollers, and passed through sideways—these rollers being so constructed as to act only on the extremities of the bar, which, by this means, were

spread out to the width required for the heads. The bars were then passed again longitudinally through ordinary rollers, till reduced to the length and thickness required; after which, while still hot, they were straightened by being beaten with wooden mallets. The time required for rolling a shingle into a bar was eight minutes.

The next process was boring the pin-holes. In doing this the bars composing each link were placed one on another, and bored by one operation, by which means uniformity of length was obtained. Shearing the heads of the bars to the proper form was the next operation. To do this the bars were fixed eccentrically on a table revolving in contact with shears, which, as the table turned, cut off the superfluous portions of the heads.

Every bar of the chains at this stage was tested with a strain of $13\frac{1}{4}$ tons per square inch, the contract requiring, in order to insure material of the best quality, that the iron used should stand this strain without a permanent elongation of more than one-fortieth of an inch in a ten-foot length. It having been found from experiments made that up to this strain the best commercial iron did not extend more than the very best iron that could be manufactured. It may be observed that notwithstanding this amount of strain very few of the bars had to be rejected.

The last process in the manufacture of the chains was numbering the bars and lettering the links, that there should be no mistake in erecting the chains, as to every bar being in its proper place. A few words will suffice to explain how this was carried out. The chains were divided into eight portions and named A, B, C, D, E, F, G, H, respectively. The chain A extended from the moorings on one side to the centre of the bridge, where it was joined by the chain B, which continued to the moorings on the other side, and so of the other three remaining chains. The heads of every bar of every link were then stamped with the letter of the chain to which it belonged and numbered—the heads of the first links at the moorings being numbered 0, and the heads at the other extremity of these links 1. The heads of the second series of links were numbered 1 and 2; of the third series 2 and 3, and so on throughout the whole length of the chains. The bars of every link were also numbered 1, 2, 3, 4,

5, 6, 7, 8, showing the position they occupied in the link during the operation of boring.

The engineer considered it highly advantageous to the successful completion of this part of the bridge that the chains were prepared by Messrs. Howard, Ravenhill, and Co., who spared no pains and no expense to carry out his instructions to produce a perfect structure; and so far from their making any attempt to evade any condition of the contract for their own advantage, the perfection of the work was their chief consideration.

It will show the excellence of the iron they produced to state that whereas the late Mr. Barlow deduced that the stretch of iron was at the rate of one-ten-thousandth part of its length for each ton, the iron which Messrs. Howard, Ravenhill, and Co. produced for the chains of the bridge only stretched from one-fifteenth-thousandth to one-fourteen-thousandth part of the length per ton, being above 50 per cent. less than Mr. Barlow's.

As so much depends upon an honourable contractor in the execution of a work, Mr. Page authorised me to make these observations in justice to Messrs. Howard, Ravenhill, and Co.

OF THE PROBABLE LOAD.

Before considering the degree of strain to which the chains are liable, it would be well to investigate the amount of load to which a bridge may be subjected.

M. Navier, a great authority on suspension bridges, calculated the load likely to occur on a bridge at 42 lb. per square foot. The standard proof for suspension bridges in France is 200 kilogrammes per square mètre, which amounts to 41 lb. per square foot, the proof load required by the French Government.

For troops on march 21 in. in rank and 30 in. in pace are allowed, giving 4.37 superficial feet per man, which, at 11 stone each, would be 35½ lb. per square foot.

The load taken in the calculations for the Menai Bridge was 43 lb. per foot super.

An experiment was made by the engineer of the Chelsea Bridge by packing picked men on a weigh bridge, with a result of 84 lb. per

superficial foot, but it is not within the limits of probability that such a crowd could accumulate on any bridge.

Seventy pounds per square foot of platform are assumed as a standard for the load that may come on a bridge, as being the utmost load that the platform could hold, supposing it, in fact, quite filled with people crowded as close together as they could be. This, it is true, is not often likely to happen, but it may do so on a public occasion, and needs, therefore, to be provided for.

The march of cavalry, or the passage of cattle, is not so productive of dangerous effects as troops on the march, inasmuch as cavalry take up more room in proportion to their weight, and do not preserve a uniform pace.

As regards the greatest moving load or crowd, it is an acknowledged fact that it is impossible for a body of people on the move to occupy per man less space than trained troops, and as I have before shown that troops on the march do not produce a greater dead weight than $35\frac{1}{2}$ lb., one may safely assume that the dead weight due to a moving crowd will not amount to so much.

OF THE STRAIN ON THE CHAINS.

Having described the various loads that may come upon a bridge, it may be useful to show the strain produced on the chains of the Chelsea Bridge under the several circumstances.

The strain on the chains from their weight alone is 1.1 tons. The strain from the weight of the platform and road alone is 3.32 tons, giving a total strain produced by the structure alone of 4.42 tons, or 9.08 tons below the proof strain.

The strain on the chains from the weight of the structure and a load of 70 lb. (being the weight per square foot of a dense crowd), is 7.60 tons—or 5.9, nearly 6 tons, below the proof strain; so that the chains will carry in addition to the weight of the structure nearly three times the greatest crowd that can come upon the bridge, before the proof strain is arrived at. Taking the breaking strain of the chains at 28 tons, we should require $7\frac{1}{2}$ times the greatest possible load to be brought on the bridge to produce that strain.

Before concluding these observations on the Chelsea Bridge it may be interesting, without taking into consideration the high quality of

the iron, to compare the strain on the chains with other suspension bridges; and for this purpose I may refer to the Hammersmith and Pesth Bridges as fine examples of bridge engineering, both being built by the same engineer, Mr. Tierney Clarke, at distant intervals—the Hammersmith Bridge having been open thirty-six years, and the Pesth fourteen years.

The Hammersmith Bridge is 710 ft. 8 in. between abutments, the span of the main opening is 442 ft. 6 in., the deflection is 29 ft. 6 in., the useful width of platform is 30 ft., the sectional area of the chains is 180 square inches, the weight of a square foot of road 63 lb., and the strain per sectional inch upon the chains from a load of 70 lb. is 8·86 tons; the chains were proved up to 9 tons, leaving a margin of ·14 tons between the proof strain and the strain from the greatest load.

The Pesth Bridge is 1262 ft. between abutments, the central span is 666 ft., the deflection of the chains is 47 ft. 6 in., or $\frac{1}{14}$ th of the span, the available width of roadway is 36 ft. 3 in., the weight of a square foot of suspended roadway is 74 lb., and the chains have a sectional area of 510 square inches.

The strain produced on the chains with a load of 70 lb. per square foot is 7·72 tons, or 1·28 tons below the proof strain, all the bars having been proved up to 9 tons.

The margin or allowance between the strain from the greatest load and the proof strain is therefore as follows :

Hammersmith Bridge		·14 tons.
Pesth Bridge		1·28 tons.
Chelsea Bridge		5·9 tons.

DISCUSSION.

Mr. CARRINGTON said he was very much pleased with the plan that had been adopted for the abutments, and with the arrangement of the piles. Driving the piles on the slant, he believed, was advantageous. As regarded the load on a bridge, he considered that 70 to 80 lbs. per square foot was certainly as much as could fairly be put upon the roadway. It could scarcely be imagined that there ever would be a crowd over the whole of the bridge so great as to produce a load of much more than 70 lbs. to the square foot. He thought 70 lbs. was a very fair allowance, but there was no harm in taking 80 lbs. to the square foot, that load would certainly give a greater margin for safety. He understood the author of the paper to say that, in France 200 kilogrammes per square metre was taken as the working load, and 400 kilogrammes as the test load; those loads were taken by both French and Italian engineers, the load of 400 kilogrammes was simply a test load to be put upon the bridge for a short time, in order to make it quite certain that the bridge was strong enough for the continuous working load of 200 kilogrammes per square metre, equal 41 lbs. per square foot. Referring to Chelsea Bridge, he should like to ask how it was that the road plates had corroded so rapidly?

Mr. C. J. LIGHT asked for information as to the connection of the suspension rods with the chain.

Mr. F. C. REYNOLDS said that in all structures where links of an uniform thickness were used, connected together by round pins, attention was requisite to make sure that there should be a sufficient bearing surface at the back of the pin, and if, as was frequently the case, the pin had a sectional area equal to the section of the bar, it would very often happen that the bearing surface before referred to would be far from sufficient; for instance, assume a bar 10 inches by 1 inch = 10 in. area with a round pin of the same area—*i. e.*, about $3\frac{1}{8}$ in. diameter—then, even on the assumption that the half circumference would be effective, the bearing surface would only be about $5\frac{7}{8} \times 1 = 5\frac{7}{8}$ th area to sustain the pressure that could be put on by a bar of 10 in. $\times$ 1 in. = 10 in. area; the bearing surface was, therefore, evidently inadequate. The remedy was obvious enough,

viz., either to increase the diameter of the pin beyond what was required for the strength of the pin itself, or else to thicken the head of the bar so as to give more surface for the same diameter of pin. He believed this was a matter of the greatest importance, but one that was very frequently overlooked; it came prominently forward in the case of suspension bridges, but had often to be considered in designing warren and other trussed girders.

Mr. ZERAH COLBURN had come quite unprepared to take any part in the discussion of the paper just read. The paper was of a very interesting character, and very complete in point of description. There was one point in connexion with Chelsea Bridge which the author had not, he (Mr. Colburn) thought, sufficiently adverted to, namely, the ornamental character of that bridge. That certainly was a point which should not be forgotten, although it had not been mentioned by Mr. Page; and the more especially, when so many bridges were being constructed, which certainly did not possess any merit of that description.

Mr. PARSEY said that, at the Institution of Civil Engineers the previous week, the question of strains and the measure of the load was introduced. Mr. Cowper stated that he had placed a number of men together, and they weighed 140 lbs. to the square foot, and from that time he had adopted that measurement. Another gentleman said that he always took 100 lbs., and another that he always took 80 lbs. Admitting every possible variety in the weight of men he (Mr. Parsey) thought that, upon an average, when they were put together closely, that they would weigh, at least, 112 lbs. per square foot. But in ordinary crowds of people, he thought, 80 lbs. might be taken as being sufficient. Upon the continent it was not usual to take quite so great a load. As regards the strain per square inch on the parts of a bridge, $7\frac{1}{2}$ tons was the greatest that could be applied. The point which had been alluded to by Mr. Reynolds, namely, the size of the connecting pins, was one worthy of a considerable amount of attention. It had always appeared to him (Mr. Parsey) that the view generally taken of the case was erroneous. The pressure acted on a surface equal to the diameter of the pin multiplied by the thickness of the bar, so that if the pin was two-thirds of the width of the bar in diameter, and a strain of five tons

per square inch on the body of the bar, the pressure at the back of the pin hole would be $7\frac{1}{2}$ tons. He had seen instances where the back of the pin hole had been upset the 50th part of an inch, although the other parts of the bar were uninjured. He would ask Mr. G. Page whether he had tested any of the links, and if so, whether he found them equal to 28 tons to the inch.

Mr. F. YOUNG, referring to the difference in the weight of men, said that he reckoned fifteen average sized people to the ton as a fair average. He did not agree with Mr. Cowper, for he (Mr. Young) thought 80 lbs. to the square foot was quite outside the mark, and safe in practice.

Mr. G. PAGE, said that a question had been asked concerning the roadway plates, and allusion had been made to the rusty condition in which they were. All he (Mr. Page) could say in reply was, that they had not been painted for a considerable time; indeed, the bridge had not been painted for five years. With respect to the question of the connection of the suspension roads with a chains, this was effected in a somewhat similar way to the plan adopted at Hungerford-bridge. The suspension rod was divided into two at the chains by means of a cross head, or short bar attached at the centre to the suspension rod, and at the ends to the two short rods, one being attached to the upper chain and the other to the lower, by which means the leverage is the same on each side, and consequently the strain is the same. As regards the size of the pins, they were of the general proportion of the pins used in suspension bridges. The diameter of the pin was $4\frac{1}{4}$ and the depth of the bar 8 inches. Very great attention was paid to this point, and it was not until after several experiments had been made that the size of the pin was determined upon. As regards not having made any observations with reference to the ornamental parts of the bridge, he did not consider that was a question of engineering. He had confined his paper strictly to engineering points, and left people to judge for themselves as to the ornamental portion of the bridge. As to weight, it was considered by Mr. Page that the greatest load that could be produced would be 70 lb. to the inch. They had tested bars made in a similar manner, and 27 or 28 tons was a very good average of the

breaking weight. It was not till after several experiments were made to prove the load that it was determined upon.

The CHAIRMAN said he was afraid the very quality of Mr. Page's paper was one great reason why there had not been such a lengthened discussion. He was sure every member would heartily thank Mr. Page for the truly practical paper he had submitted. It was a paper which, when published in the Society's Transactions, would be even more appreciated than it was now, for the members had not yet had an opportunity of fully considering these practical details. For the first time in the history of this Society, the committee had determined to publish the whole of the Transactions, and the first book would be ready for issue in the course of a few weeks. As regards Mr. Page's paper, he had already expressed his opinion as to its merits; and he (the Chairman) thought with Mr. Page that 70 to 80 lbs. must be taken as a fair load, for Mr. Cowper's 140 lbs. could only be an exceptional case. Mr. Page in his paper had entirely omitted the very important part of the structure, its ornamentation. That certainly might be called an engineering portion of the structure; the more especially was this so in connexion with Chelsea Bridge. Its ornamentation was quite an innovation, something new in Bridge Architecture about London, and he thought they should be all obliged to Mr. Page for having taken such a decided stride. Mr. Colburn had referred to the want of this important point in the bridges at present being constructed about London, and he (the Chairman) thought it was a pity that such ugly structures were allowed. It was singular that the legislature, or those Local Boards who had authority, had not taken this matter in hand, so that these bridges could have been made pleasing to the eye, and at the same time meet the great exigencies of the public traffic.

CHELSEA BRIDGE.

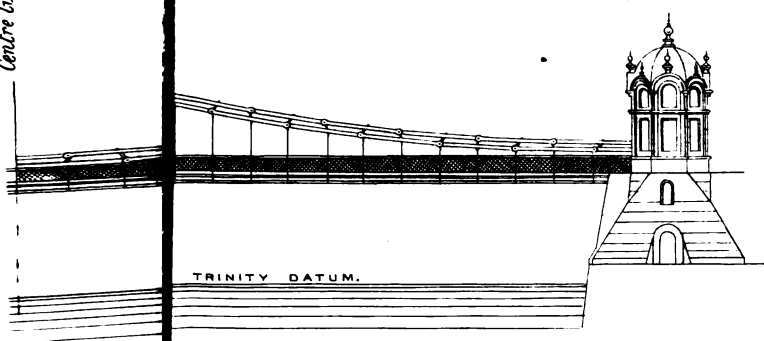
REFERENCE TO PLATES.

PLATE I.—HALF ELEVATION OF BRIDGE.

- „ II.—LONGITUDINAL SECTION OF ABUTMENT ON THE LINE OF CHAINS.
- „ III.—PART PLAN, SHOWING TOP OF ABUTMENT, PART SECTION ALONG LINE OF CHAINS, AND HALF-PLANS OF FOUNDATIONS OF ABUTMENT.
- „ IV.—PLANS, ELEVATION, AND SECTIONS OF CAST IRON PILES AND PLATES USED IN THE PIER FOUNDATIONS.
- „ V.—ELEVATION OF TOWERS.
- „ VI.—PLANS AND ELEVATION OF HORIZONTAL FRAMES IN TOWERS.
- „ VII.—HALF-TRANSVERSE SECTION AND PART PLAN OF THE PLATFORM AND ROADWAY.



Centre line of Bridge.



G. G. Page

Upfield Green, Litho.

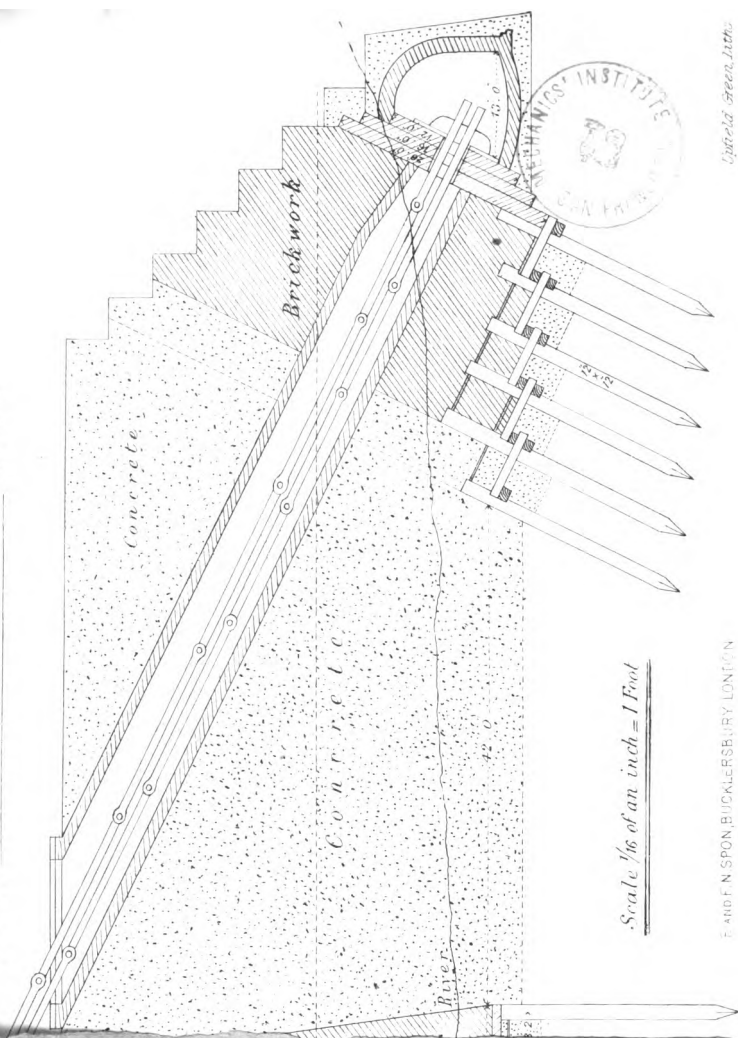


CHelsea BRIDGE.

Plate 2.

ABUTMENTS.

LONGITUDINAL SECTION ON LINE OF CHAINS.



E. AND N. SPON, BUCKLESBURY LONDON

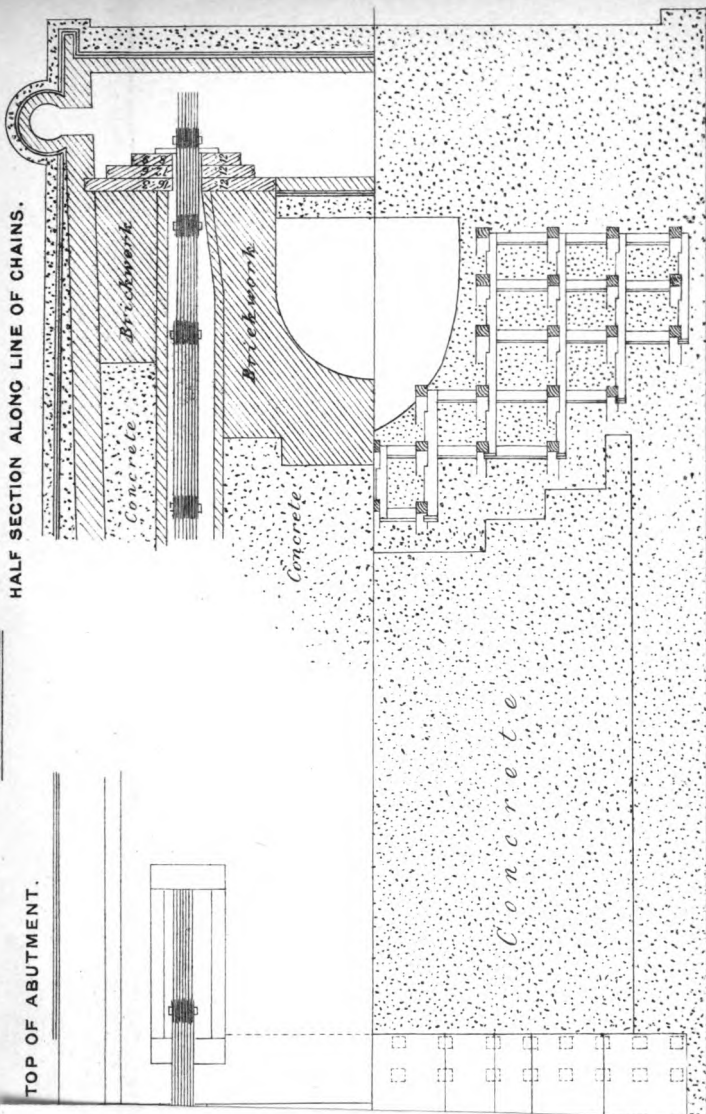
Copyright 1862, 1863



CHELSEA BRIDGE.

Plate 3.

ABUTMENTS.



HALF PLAN OF FOUNDATIONS.

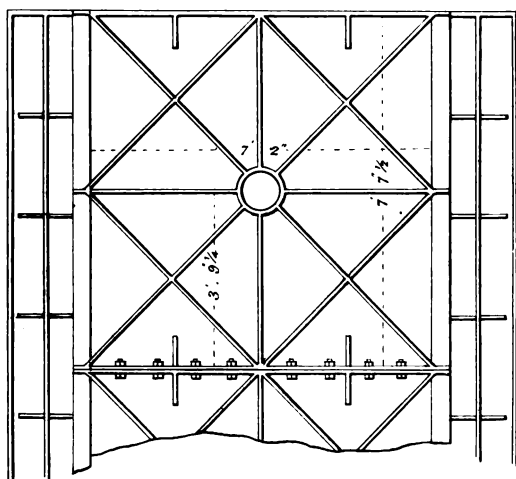
Scale $\frac{1}{16}$ of an inch = 1 foot.

NON

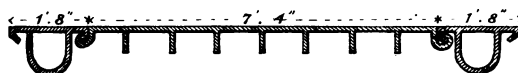
Copyrighted Green Edition

CHELSEA BRIDGE. **DETAILS OF PILES & PLATES IN FOUNDATIONS.** SCALE $\frac{1}{4}$ INCH = 1 FOOT.

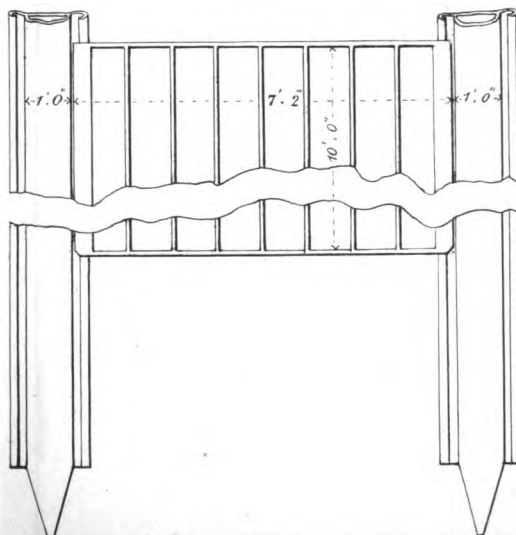
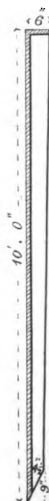
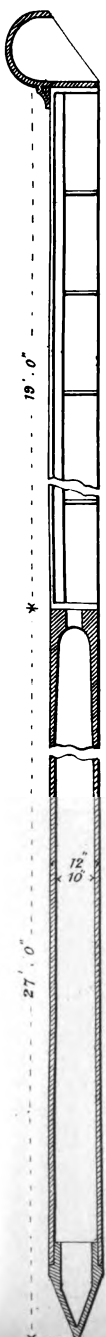
Plate 4.

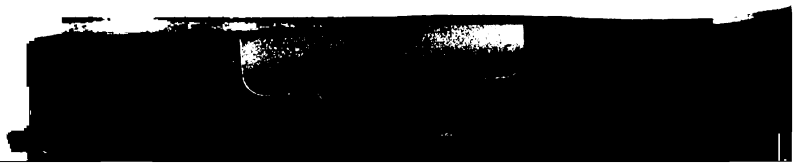


PILES & PLATES ABOVE LOW WATER.



PILES & PLATES BELOW LOW WATER.



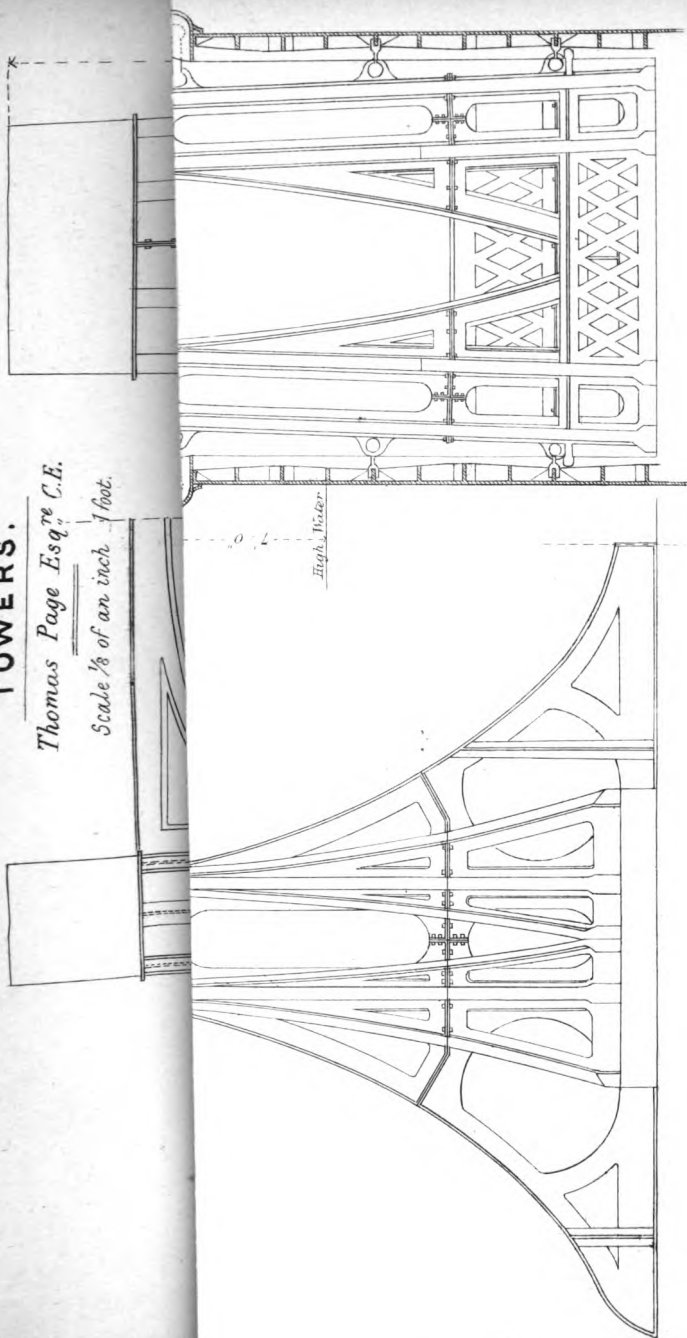


CHELSEA BRIDGE.

TOWERS.

Thomas Page Esq^r C.E.

Scale $\frac{1}{8}$ of an inch = 1 foot.



G. G. Page, del.

E. AND F. N. SPON, BUCKLESBURY, LONDON.

Upfield Green, Litho.

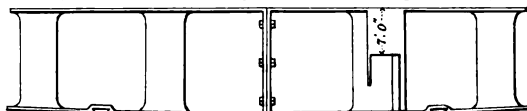
CHELSEA BRIDGE.

HORIZONTAL FRAMES.

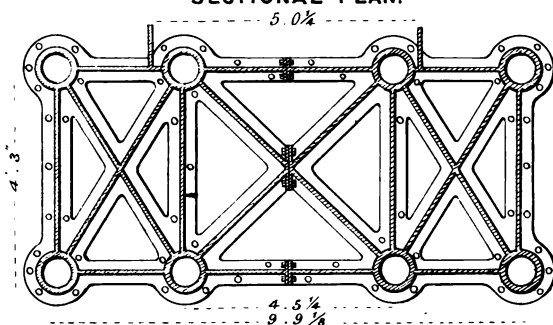
SCALE $\frac{1}{4}$ OF AN INCH 1 FOOT.

TOP FRAME.

OUTSIDE ELEVATION. INSIDE ELEVATION.



SECTIONAL PLAN.



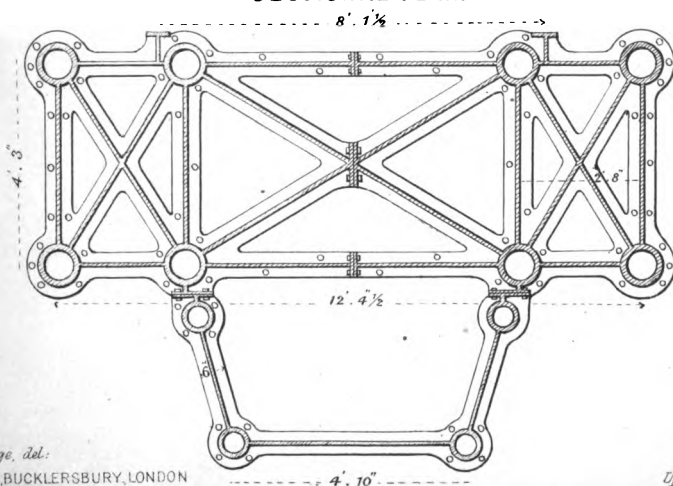
5TH FRAME.

INSIDE ELEVATION.

OUTSIDE ELEVATION.



SECTIONAL PLAN.



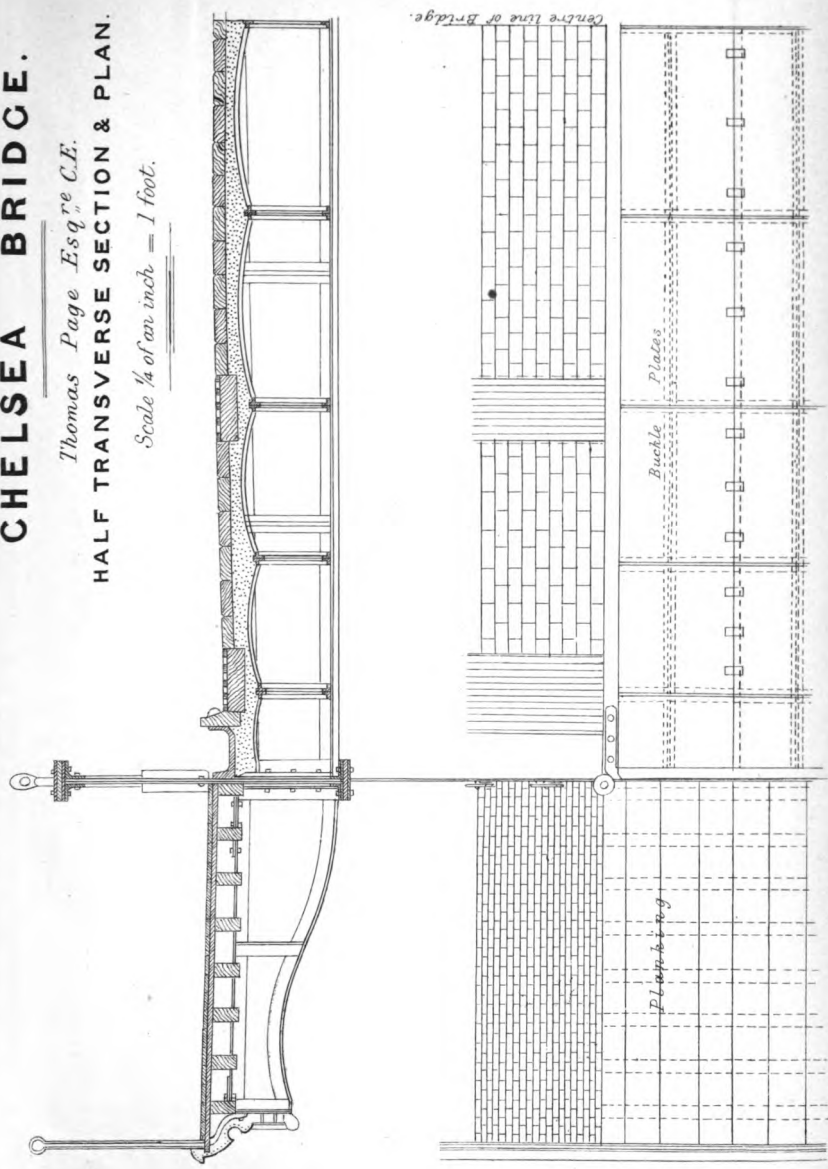
G. G. Page, del.

F. N. SPON, BUCKLESBURY, LONDON

Upfield Green

CHELSEA BRIDGE.

Thomas Page Esq^r C.E.
HALF TRANSVERSE SECTION & PLAN.
Scale $\frac{1}{4}$ of an inch = 1 foot.



June 1st, 1863.

E. RILEY IN THE CHAIR.

ON THE BOMBAY, BARODA, AND CENTRAL INDIA
RAILWAY.

BY CHARLES SANDERSON.

(Communicated by W. H. LE FEUVRE.)

THE late and much lamented Mr. James Berkley having read a paper in 1860, before the Institution of Civil Engineers, upon Indian railways generally, and which, at the time, excited much interesting discussion as to the best mode of conducting railway works in the three Presidencies, it is not the author's intention to do more in this paper than to describe briefly :

First. The general features of the country through which the Bombay, Baroda, and Central India Railway passes.

Second. Some of the characteristics and estimates of the works as designed.

Third. Some of those "makeshifts" which the resident engineer is compelled to adopt in a tropical climate, where time, and difficulty in procuring materials, are elements involved.

Lastly. Some observations as to rolling stock, and traffic working.

First. The Bombay, Baroda, and Central India Railway commences at Bombay Fort, and passing through the Islands of Bombay and Salsette (both of which are connected by causeways), crosses the Bassein Creek, by a bridge of one mile in length, to the mainland of the Tannah Collectorate. It then skirts the coast by Damann, Bulsar, and Surat, to Broach, at a tolerable even distance of only a few miles. Thence, still keeping northward to Baroda (the chief city of the Guikwar), points a little westward to Ahmedabad, a total distance of 310 miles from Bombay.

In its course it crosses the following chief rivers and creeks :

H

			Length.
Bassein Creek	. . 86 spans of 60 feet	. .	5160 feet
Vyturnee River	. . 41	„ . .	2460 „
Damann River	. . 14	„ . .	840 „
Par River	. . . 10	„ . .	600 „
Orunga River	. . 15	„ . .	900 „
Cavery River	. . 21	„ . .	1260 „
Beelemora River	. . 14	„ . .	840 „
Poorna River	. . 19	„ . .	1140 „
Mendola River	. . 12	„ . .	720 „
Taptee River	. . 30	„ . .	1800 „
Narbudda River	. . 59	„ . .	3600 „
(Two of the spans are 90 feet)			
Mhye River	. . . 27	„ . .	1620 „
Watruck River	. . 10	„ . .	600 „

besides seventy-five smaller rivers and nullahs varying from 600 feet and under in width, all crossed by iron screw pile bridges, the total length of which is $6\frac{1}{2}$ miles.

In addition to these, there are several brick and stone bridges of 3 spans and less, numbering 155 including 20-foot spans.

All the rivers, with the exception of the Mhye and Watruck, are affected by the daily tides; at the point of crossing, the greatest range of tide is 10 feet, and the maximum known flood-level of the Narbudda is 51 feet above the summer level. The waters of the Narbudda and Taptee attain a velocity of 8 miles an hour, both in monsoon floods and high spring tides.

The creeks southward, and the rivers northward, have a substratum of hard clay and kunker. Whilst the intermediate rivers, such as Damann, Sinjun, Par, and even Orunga, have beds of trap rock, underlying a few feet of alluvial or clay.

It has frequently been observed of this railway, that apart from the numerous rivers crossed, the country admitted of a very easily constructed and cheap line, on account of its extreme flatness. This, however, is by no means true in all respects; for, notwithstanding the fact that the southern part of the line passes over immense flat tracts of land flooded by spring tides; or, that the northern portion traverses many miles of flat cotton soil district, yet, in the former

case, the difficulty of procuring sufficient material to make up the embankments, and in the latter, the porous nature of the soil and the difficulty of getting good foundations, have added largely to the cost of the railway.

The stone which has been obtained from the 10-foot cuttings about Sinjun or Kelvee Mahim, proved to be useless for building purposes. So that quarries had to be opened at several points between the Vyturnee and Dongree (about 130 miles north of Bombay), the cost of getting, dressing, and carriage, being about eight annas (1s.) per cubic foot. Fever generally prevails to a large extent in these quarries, even more so than in the marshy districts. It may be accounted for, in the fact of jungle and timber being adjacent to the quarries.

On the whole, Guzerat is tolerably healthy, and Europeans may live in the flat districts overlying the sea coast with perfect safety, it being simply a question of temperate habits. The amount of rain varies from 100 inches in Bombay to 30 in Surat and 20 in Ahmedabad. The whole falls generally between June and October. No earthworks can be carried on during those four months; but where bridges and other works are accessible by roads, the rains do not offer very serious impediments to bridge works on tidal rivers not subject to monsoon floods.

In the northern rivers, during monsoons, large quantities of sand and organic matter are brought down from the uplands and forced into the Gulf of Cambay. Much of it contributes to alter the beds of rivers; scooping out in some places, and filling up in others. This rendered it necessary that the foundations, whether masonry or screw piles, should go below the sands, although in some cases 30 feet deep. In the Narbudda, many of the piles went 45 feet deep.

The population of the districts through which the line runs, averages 257 to the square mile, with about 13 per cent. of males over females. A large number of the latter were employed upon the railway in forming earthworks, their wages being two to three annas per day. The male coolie earning from four annas to six. The above average of 257 does not include Bombay and the island.

When required to do so, the coolies, both male and female, will work in the rains; but it was only in very pressing cases such work could possibly be profitable.

The superstitious habits of the people often militated against the quick progress of the works; for instance, in forming a temporary bank across the Mhye, in summer time, many of them ran away rather than close up their goddess. The same applied to the work-people of the Narbudda, they could only believe that the bridge could be safely opened when four Europeans had been accidentally drowned in the rapid currents of the river, who being thus immolated upon the altar of their favourite goddess, propitiated her.

Second. The surveys of the railway were ordered by the Honourable Court of Directors of the East India Company on the 10th of August, 1853, and sanction was obtained on the 16th of April, 1855, to commence the works of the northern section from Surat to Ahmedabad, a length of 143 miles. The southern portion of the line was not sanctioned until the 20th of November, 1857, and even that did not embrace the last 7 miles of the terminal part into Bombay Harbour. Considerable delay had occurred in the settlement of the southern portion of the line, in consequence of varying opinions as to where the railway should join the Great Indian Peninsula Railway, and whether the terminus should be common to both. In November, of 1861, final sanction was given to the above mentioned 7 miles, and also for an independent terminus; but that arrangement has since been disturbed by the agent of the company, so that at the present moment it is believed that no works for a permanent station are yet commenced.

In December, 1860, the author having been appointed chief engineer in India, proceeded to Bombay; and in February, presented his first report of the middle portion of the line, between Brehsar and Baroda. The remaining portions of the line, north and south, being reported on in the following month of March. The author found that his predecessor, Mr. Forde, had had great difficulties to contend with; not so much in the construction of works as in the organization of the departmental system. The author found 80 miles open for traffic, between Surat and Baroda, half of which had been open only a few days, on his arrival January 16th, 1861. For many miles the permanent way was laid on the bare cotton soil. The Narbudda bridge not being complete caused a break in the continuous traffic, the river and transhipment of goods requiring one hour and a half

to two hours, besides being highly dangerous in the rapid tidal currents.

By the 12th of March, 9 miles more were opened south of Surat, and on the 7th of April, 9 miles northward of Baroda. A farther opening, southwards, of 34 miles, took place on the 2nd of September, 1861, making a total of 132 miles, or 52 miles during the author's first half-year of service. In addition to this, there was the completion and opening of the Narbudda bridge (3,600 feet in length) on the 20th of June, after the most strenuous exertions of Mr. King (the bridge engineer), and just in time to save the monsoon.

The general works of the line are of the ordinary class, the greatest cutting being 26 feet, and the greatest embankment 29 feet. The gradients have been laid down originally to have nothing sharper than 1 in 500; that, however, is not the ruling gradient, for there are several of 1 in 100. One of these the author would have been glad to have dispensed with, between the Umbudda bridge and Broach station, but for the fact of the expense and the want of time. A proposal was made, in the author's early reports, to adopt, here and there, sharper gradients of 1 in 200, or 1 in 250, with a view to lessen earthworks and lower river bridges, but the proposition met with such strong opposition from the agent and others, that it was abandoned, excepting in some cases, where sanction was obtained to continue falling gradients and lowering the long embankments over flat and flooded districts. The general plan and section (Drawing No. 1), together with the tabular form B, will show the general gradients. With regard to the sharper gradients of 1 in 200 and 1 in 250 (not exceeding three-quarters of a mile in length), the author's agreement was, that momentum alone would have prevented any serious retardation to a maximum load, or drawback to the designs of Colonel Kennedy (the consulting engineer), who contemplated flat gradients and one monster train a day. It was never intended by the author that such sharpened gradients should be placed in a position where a heavy train would be required to start upon them.

The cotton soil of Guzerat makes very bad embankments. Being exceedingly porous, and capable of immense expansion and contraction, such material must ever be a source of anxiety to the engineer,

especially if at all exposed to monsoon floods, in addition to 50 or 60 inches of rain fall. Indeed, without a good covering of turf, babul plants, or other thickly growing shrubs, it cannot stand heavy rain. In one case, about seventeen miles north of Surat, a whole embankment, standing above the ground level about 12 feet, was not able to stand the brunt of a 10-foot flood, for, being composed of the cotton soil, it got soaked and pushed over 1 foot 6 inches (permanent way and all) before the water subsided. The earthworks on the whole of the line have been accomplished by coolie labour with baskets. An experiment was once tried with wheelbarrows, but utterly failed, the coolie always preferring head-work. The ordinary way of executing earthworks, both on our own lines at home and some in India, viz. by cutting and tipped embankment, would certainly have obviated a difficulty arising out of side pits, and which promises to be a very formidable objection to basket-made embankments. In nearly all the cases of rivers having old channels, or blind rivers, the flood waters are apt to take the direction of the side pits (which are excavated parallel to the railway), and wasting in their course the toe of the slopes. A diagram (No. 2) showing the Narbudda River and its obsolete channel, or "blind river," will explain the foregoing remarks. The channel has a 28-foot embankment put over it, instead of a viaduct of fifteen 60-foot spans (as originally proposed by Mr. Forde, the previous engineer), and along both sides of the embankment are the holes excavated for material. Now when the mighty 50-foot floods come trending from the north-west down the valley of the Narbudda, they have not time to turn the corner on the east of the railway, so they naturally take their old course, and plumping up against the embankment, get driven down the side holes on the east side, and rounding the abutment of the bridge, take the lower level again among the side pits on the west side, until the flood reaches the old channel again, proving the necessity of keeping such channels open by viaducts. What is termed the "blind river bank" has not only been a fruitful source of much correspondence, discussion, and delay, but, in a pecuniary point of view, will be costly in maintenance. The dotted lines in the diagram No. 2 show the direction of the flood waters. There is another case of a similar character at Ambulsar (twenty-nine miles south of Surat), where the "blind" part

of the river Umbicka, finding itself suddenly arrested by the railway embankment, took to the side pits, and finally through a 20 feet arch-way, burrowing a hole 20 feet deep close to the foundations. The argument of the engineers who designed these works is, that once the dam is formed, the water will cease to flow that way, at the same time providing neither breast-walls or puddle to meet the pressure and impetus. On the contrary, the side pits offer a new channel, which in every case seriously wasted the works of the railway. It is true that the consulting engineer endeavoured to meet this contingency by directing that the holes should not join each other, but that partitions should be left. These, however, are soon washed down in the under currents. The author has always been an advocate for proper openings, even at old channels of rivers, and in cases where traffic is actually passing over such embankments, it was proposed to build the usual iron screw bridges, as if for double line, and if nature insisted on the destruction of the embankment, it would soon be accomplished without much labour. Nevertheless, in some of the worst cases it would absolutely pay to refill the side holes with the spare embankment when the bridge is completed. The earthworks throughout the whole line are only formed for single road. North of Baroda the soil is very sandy, and the monsoon rains waste the embankments composed of it very much. From the river Mhye, northwards, most of the earthworks were completed in 1859, and waited over two monsoons for the permanent way. In order to preserve the tops and slopes, "bends" were made, *i.e.*, the tops were converted into little reservoirs of about 1 foot deep, somewhat after the fashion of "paddy bunds." This tended in a great measure to prevent the washing away or serrating of the banks.

In the cuttings, there is a very useful limestone, familiarly known as "kunker." This makes most excellent lime and chunam. Indeed, it appears to have compensated for much loose and careless work. The kunker burnt with wood costs about 11 rupees per cube yard; whilst the inferior sort, burnt with cow-dung, sold at 8 or 9 rupees. The lime is hydraulic, and takes 2 parts of sand.

The fencing used on the line has been various. There is the teak-post and jungle teak-rafters, at about 9 annas (14d.) per yard; also a rougher kind of jungle timber-fence at 6 annas per yard, and a

slight bamboo-fence tied with coir rope, at $4\frac{1}{2}$ annas. The two last have been used only for temporary purposes. The wire-strand fences with teak-posts, have been too inferior to command success. There has generally been an extra side width of land allowed by Government, say 50 feet from the toe of slopes. This would be useful, planted with babul, as that wood after seven years' growth makes excellent fuel for locomotives. Lands outside the allowance of 50 feet, are charged to the railway company at a rental of one rupee per acre.

The natives very frequently break down fences in passing over the line; and not unfrequently, cattle also stray on to the line, and get destroyed by passing trains. The white ant is very destructive to the unseasoned, sappy woods. Tarring and charring is absolutely necessary, even with the better woods used in fencing.

The permanent way of the railway is of three kinds, viz. the ordinary cross-sleeper road, with rails 65 lb. to the yard. 2. The "sandwich" permanent way, invented by Mr. B. W. Adams; and 3. A suspended girder-rail, by the same inventor. Of the two latter kinds of road, there were not more than 40 miles on the line, out of 310 miles. The sleepers for the ordinary road are creosoted Baltic timber, at a cost, delivered on the ground, of 10s. each. There were enough to lay 60 miles of road. But a considerable portion of them proved so useful in the staging for iron-bridge erection, that some 30,000 of them got absorbed in that way. The remaining supply of sleepers was of teak, sadra, khair, or pine. The teak costing about 4 rupees, and the inferior woods 3 rupees per sleeper. The creosote and the vibration of passing trains together, entirely prevented the ravages of the white ant. Sometimes when native sleepers, or even Malabar teak, remained piled long together on the bare ground, the white ant was sure to have attacked them largely.

With regard to the Adams's "sandwich" road, it was too light for embankments, with only a limited supply of ballast and heavy 50-ton engines. The cross-ties, 5×5 creosoted timber, broke by dozens. The principal objection to the introduction of that class of road, was the difficulty of grooving and boring the longitudinal timbers, the native woods spoiling the tools sent out, from sheer hardness. The author was compelled to send both the "sandwich"

road and the suspended girder-rail into cuttings, where a good firm base could be obtained. When the "sandwich" road was nicely packed and settled, the smoothness of the running was very remarkable at speeds under 20 miles an hours.

The suspended girder-rail was laid for about 4 miles on the Narbudda embankment, and was altogether too weak for heavy traffic. The speed was always slackened over that portion of the line.

There can be no doubt but that the double-headed rail is the best for India. It can be turned four times, and even then be useful in sidings or light branch traffic. The rail for the "sandwich" road is 6 inches deep and light in the web, and in the absence of longitudinal sleepers, the author cast a few suspension chairs (Ordish's patent), and adapted them very successfully. It will be an economical plan to absorb broken piles in this way, if it should be found that the suspension chairs answer with so great a depth of rail.

The ballast question of the northern portion of the railway was one of very difficult solution. North of Surat, there was nothing to be had but the sand from the beds of rivers, or moorum, which underlaid the black cotton soil, and often got largely mixed with it. So long as the dry weather lasted, the packing might have been done with the sun-hardened lumps of cotton soil; but no sooner had the rain penetrated the ground, than the whole road dropped into disorder, and often stopping the traffic for hours. Indeed, so great were the difficulties and anxieties involved in attempting to work any portion of line without ballast during the rains, that the author was strongly opposed to the repetition of premature openings similar to that of Broach to Baroda, 44 miles, 30 of which were unballasted, and the opening of which occurred on the 9th of January, in the absence of a chief engineer.

The bridges of the line, as before stated, are nearly all built on Mitchell's screw-pile system, with Warren girders. This class of bridge is so well known to the engineering world, that it will only be necessary to give a few details explanatory of the accompanying diagrams. Nos. 3 and 4.

The diameter of the cylindrical piles is 2 feet 6 inches, the thickness of metal 1 inch. Length of piles uniform, and 9 feet, weight averaging $1\frac{1}{2}$ tons. With the exception of the bottom piles (which have

their flanges inwards), the flanges are all on the outside, and held together with twelve 1-in. bolts. The fracture of piles generally occurred at the flanges, during the progress of screwing. Large 40-foot bullock arms have been tried for screwing; but it was found so difficult a matter to control the strain, that the old method of capstans and coolies was resorted to. The staging required for the erection of the three-piled tiers, was generally 50 feet wide, composed of jungle teak round timbers, averaging 20 inches girth. The decking of the staging being composed of European creosoted sleepers, which afterwards came in for the permanent way.

The average rate of actual screwing per day was 1 foot, at a cost of 43 rupees in deep rivers to 15 rupees in dry beds; 5-inch rope (Manilla) was used with the capstans, which were adapted for the use of double-headed rails inserted into the cap. The spokes of the screwing-collar were also pieces of double-headed rail. A very large amount of Manilla rope was used, in some cases, a ton to a span.

The piles were screwed for a double line, but the girders erected for a single one only. The spans are 60 feet. Depth of girders, 7 feet 8 inches. Width of compression beam at top, 1 foot 5 inches. Thickness of compression-beam plates, 1 inch. Thickness of tension bars, $\frac{1}{2}$ inch. These are two in number in the centre bays, and increasing to 6 at the ends. It was originally intended that the compression beams (which were shipped in three parts), should be bolted together with the lap-plates with $1\frac{1}{4}$ bolts; but riveting was found more complete and secure.

The tiers of three piles were originally intended to have strut piles of the same dimensions, both up and down stream, of all important bridges; but in practice they have not been applied (excepting on the Taptee and partially on the Narbudda) on both sides. It was found difficult to screw them at an angle of 35 deg., and, indeed, they were not necessary, excepting on the down stream.

The author, in his early report, recommended the use of timber fenders instead of the up-stream strut piles, in the case of the Narbudda. There would be a danger of piles being fractured by floating timber or native pattimars, in rapid, tidal, or flood currents on that river. Sanction was given to put these fenders in, and 30 were completed by October of last year. They were composed of teak

timbers 14×14 and shod. They raked up at a batter of 1 foot in 20, and were tarred. The tiers of piles were braced together by horizontal T and diagonal L bracing. No longitudinal bracing or ties were used, excepting some $1\frac{1}{2}$ -inch rod, connecting the tops of the unused piles, as they were observed to oscillate during very high floods.

It was never intended that there should be decking on the bridges, only side planking for mere gangways. The longitudinal teak balks, carrying the permanent way, were bolted down to the wrought-iron cross-girders, which occurred at each standard, or 7 feet 6 in. apart. The author having witnessed the effects of a serious accident, when he had just arrived in the country, felt the necessity of having either decking or some kind of fender, to guard the double-headed rail.

Accordingly asked for 7-inch decking, and (that being refused) subsequently for 4-inch, also for timber fenders or curb, as a stop to the decking. These arrangements might even prevent an engine from doing serious damage to a Warren girder, and certainly would be sufficient to check a carriage when getting off the rails. In the case of the Wishwametree-bridge accident, a native, or natives, having a design against the life of the Guikwar of Baroda, placed a double-headed rail across the line, only a few yards from the bridge. The train was running about fifteen miles an hour, and was thrown off the rails. The engine and tender bounded from one cross-girder to another in a singular succession of jumps, finally hanging by the axles on the permanent way which was severed from the bridge. If that permanent way had not been fished the whole train must have fallen fifty feet below. As it happened the Guikwar was not in the train, and the passengers escaped by jumping on to the compression beam.

In addition to the decking and fenders asked for, the author indented for flat-bottom bridge rails, as being safer and better adapted for bridges. Unfortunately those rails are not yet on the line, and the ordinary rail and chair continue to be used.

Drawing (No. 3) shows the construction of the 60-foot girders, together with sizes and weight of the parts. No other spans have been used on the line except the "makeshift" girders, which will be described hereafter.

The stations of the railways are for the most part temporary or "cutcha." Only three had been completed in brick, viz. at Broach, Unklesur, and Keem. The bricks are burnt in kilns with babul wood, and cost from 15 to 20 rupees per thousand. The temporary stations are simply upright jungle-teak posts (or sotas), filled in either with brick nogging or lath and plaster. When the author left India several of the northern stations had been commenced with brick, but which would require an outer surface of chunam. Materials were difficult to procure, on account of long distances and no roads—so that the most temporary stations were expensive.

Platforms, paving, and nosing was done with Gogo stone, from the other side of the Gulf of Cambay.

There are some very good specimens of brick bridges on the northern part of the line. The Mhaynee, of 3 arches, for instance, with 40-feet spans, and 40 feet high, looks very well. But many of the single 20-feet brick arches and culverts are cracked at the crown. The common-sense conclusion was, that for want of backing and counter forts the abutments had followed the direction of the shrinking earthwork. The popular cause was, however, assigned to an earthquake. Whether there is, or is not, any ground for this assertion, there are large temples at Ahmedabad rent from top to bottom by earthquakes of recent date.

The "Swindon" of the railway is at Amrolee, on the north bank of the river Taptee, and about two miles from Surat. The position is not considered a healthy one, partly from being surrounded by native villages, and partly from native bodies being burnt on the opposite bank.

The engine-repairing shed is small and only temporary. The carriage building is conducted on a large scale—the iron work being obtained from England. A small foundry is at work, also vertical and circular saws. The permanent workshops were designed and submitted for sanction in the autumn of 1861. The locality is proposed to be forty-three miles south of Surat, at a place called Bulsar, where the sea breeze has full play. Those works are to cost 50,000*l.*, and are now about being commenced. One great advantage of the locality is the closeness to a river and the sea. Water carriage is

very useful and economical where the transit of an engine over 100 miles of ground to its destination is concerned.

Turning now to the third point, viz. the "makeshifts" of an Indian railway while under construction, the author is reminded of a maxim of a late and much esteemed president of the Institution of Civil Engineers—Mr. Robert Stephenson—that he who could tell of some failures as well as successes in engineering, was the greatest benefactor to the Institution. The screw-pile bridge with facility of erection is certainly a success in the alluvial rivers of India. The failure of much masonry, both brick and stone, along the whole course of the line, has proved beyond doubt that the cotton soil, clay and moorum, are, at the best, unsafe materials to deal with, and so far the screw pile has been the only remedy for such a country.

The Narbudda bridge has been very difficult of erection, even after the engineers had had the experience of the Taptee. In 1859, a high spring tide got such a strong hold upon the staging of the Narbudda, that it broke off short many of the iron piles, and the whole was carried away. The consequence was, when Mr. King, the principal bridge engineer, took to the works, he found that many pile stumps, some 30 or 40 feet down, must be extracted before the sites would be available for fresh screwing. In order to effect this, European divers, with Heincke's patent dress, were employed; but the work was so tedious that the author had to devise a "makeshift" in order to open before the monsoon burst upon him in 1861. That "makeshift" consisted in adapting such 60-feet iron bridge parts, as were in the country, to make up 90-feet spans; the increased depth of the compression beam by 8 inches; tension bars increased in number from 2 to 4 in the middle, and from 6 to 12 at the end. Also the substitution of $2\frac{1}{2}$ in. main pins, instead of $2\frac{1}{4}$ in., enabled the author to get a safe girder, capable of bearing a rolling load of 144 tons with only $\frac{3}{4}$ inch deflection. Sanction was obtained from government to erect the spans in two places on the bridge. The home authorities were written to as to the alternative which had been adopted, and only just before the burst of the monsoon in June, was that difficult bridge open in time to convey troops northward.

The suggestions from the board at home to put 45-feet spans, or a

combination of 15 ft., 60 ft., and 15 ft. unfortunately came too late, for they arrived only six days after the bridge had been opened. It is intended, of course, to restore the 60-ft. members to their proper sphere, and send out new 90-ft. girders from England. The bridge has now been opened sixteen months.

Another "makeshift" adopted was that of increasing the base of the screw piles by building round them 5-foot wells, and filling them with concrete. Persons unacquainted with large Indian rivers, have little conception of what immense changes occur in the beds during one monsoon. Foundations of bridges are often undermined where least expected. For instance, the Mhaynee bridge of three arches, before alluded to, had its foundations laid bare; while the Keem bridge, with a 60-ft. Warren girder and brick abutments, had ten cubic yards of masonry washed clean out of the foundation, causing the abutment to crack and heel over. This latter case was remedied by making it into two spans of 60-foot iron piles.

The wells at the Mhye River (see drawing No. 4), were adopted on two grounds. First, that they afforded a better base than 2 ft. 6 in., and where, with a height of 100 feet above the bed of river, and a flood rise of 54 feet, it seemed highly desirable to get something more than the mere 2-ft. pile, as was designed for it in England; and second, the sinking of wells much cheapened the cost. While screwing cost 20 rupees per foot, sinking in wells came to about 4 rupees per foot. The wells were simply sunk in the ordinary way. The objection to using lime arose out of the fact of the divers getting it into their eyes. When the pile was screwed into the bottom of the well, to the requisite depth, both the pile and the well were filled up with concrete, and the latter domed over with brick-work and chunam. These wells were sunk only at 26 places at the Mhye bridge, instead of 100, owing to the consulting engineer having put a stop to them under an impression that they were to serve as *bases* for piles instead of mere casings. Out of the 517 spans of 60 feet, at least half of them have their piles driven into the dry sand or clay, and such a method of sinking, would have been a great saving in construction, when contrasted with the enormous wear and tear of the capstan, involving as it did 5 per cent. of piles in destruction.

The cost of screwing in the Narbudda River (taking the part covered by water) was as follows :

	Average per foot. Rupees.		
Screwing at the rate of $1\frac{1}{2}$ feet per day for each capstan.	25	5	0
Cost of extracting broken piles and diving (Heincke's dress and Europeans)	15	8	0
Cost of broken piles per foot of screwing	1	2	0
Unwatering by leathern buckets	1	14	0
	<hr/> 43 13 0		

This amount may appear heavy, but there were no pumps to unwater the piles. Although often indented for, it is unfortunate that such useful auxiliaries never found their way on to the line.

The diving department was a very expensive one. The dresses and pumps were costly, and require great care in preserving them from injury by climate and use. The men suffered very much by the air being heated and tasting of oil. The author hit on an expedient of building cells containing saltpetre round the pump cylinders, and this greatly relieved the divers. The expansion of the Taptee bridge girders was only 10-16ths of an inch in 1890 feet. There was no corrosion of the pile cylinders where salt water came into contact with the concreted piles.

Amongst the other "makeshifts" adopted, there were those of using Adams's rails to build portions of the machine shops, fractured screw piles to build engine-lifts, and common railway spikes to make rivets for the iron bridges. Fortunately the latter were of good material, or the author would not have permitted the continuance of a practice which had been in vogue some time before he arrived. It was either stopping the works or using the spikes, for native iron would not do.

Fourth. The rolling-stock question is one about which a few remarks may not be inopportune. On the Bombay and Baroda Railway there are at present 18 engines ; 14 of these were working the 132 miles when the author left in February last ; 4 others (tank engines) had been sent out and erected subsequently. Out of the 14 engines,

10 are coupled goods engines (4 wheels out of the 6 coupled), 16-inch cylinders, 22-inch stroke, and 5-feet driving-wheel. The average speed (according to the time-table), 17 miles an hour; weight of engine, light, 30 tons; loaded, nearly 34 tons; tender, 13 tons; loaded, 19½ tons; total weight of engine and tender, loaded, 53½ tons. The tractive power at the rails, 6000 lb. The small tank engines built by England were used for ballasting. Their water capacity was sufficient to run only 9 miles. They had 4-feet driving-wheels, 17-inch stroke, and 14-inch cylinders. The larger engines were built by Neilson, of Glasgow. Wood fuel had been used to a great extent, so much so, in fact, as to have cleared the surrounding country of babul wood. About 7 years' growth will give a circumference to the stem of the babul of about 15 or 19 inches. It will not bear to be kept long after cutting, as the white ant gets into it and soon reduces it to powder. The cost of this, cut up in 12-inch lengths, and delivered to the railway, is about 13 rupees per 100 maunds of 37½ lb., or 26s. per 33 cwt. The quantity of this used with the large engines was about 80 lb. per mile (running at 17 miles an hour). Coal from Newcastle has been bought in large quantities in Bombay Harbour at 16 rupees up to 20 rupees per ton, but delivered on to the line at Surat would be more like 50s. per ton, or 25 rupees. The quantity of this (used with coke) was about 30 lb. per mile as compared with the wood, or at a cost per mile of ¼th more for coal. The wood fuel of India contrasts favourably with that of America, the latter costing, according to Mr. Zerah Colburn, 9d., and the Bombay, Barodo, and Central India Railway only 4½d. per train mile. The babul wood is much denser, and throws out a stronger heat than the American woods.

The average load of trains (exclusive of engine) is 150 tons on the Bombay and Baroda Railway, generally consisting of an average number of 22 carriages and goods waggons. The ballast waggons of the line were often used to get the ordinary traffic out of hand. Cotton waggons, capable of holding 20 bales, were much needed, but notwithstanding much had been said and designed on the subject, not waggons had been placed on the line worth speaking of when the author left, so that cotton bales, after lying at the up-country stations, were obliged to go by ordinary road. The bale, as pressed

in the Mengaum or Surat districts, averaged 4 cwt., and the size was 4 feet 9 inches $\times$ 4 feet $\times$ 3 feet. The pressing was by no means as complete as it might have been. The usual plan was to half press them up country, and complete them in Bombay.

The question of third-class carriages had been occupying the authorities in India. The result of the frequent discussions was a carriage with two floors, not requiring that any passengers should squat, but that there should be accommodation for sitting in both upper and lower floors, the passengers on the former placing their feet in a trough-like space, where the lamps usually are. The cost was about two-thirds that of the ordinary teak carriages sent out from England. The natives had a great dislike to the squatting on floors, especially the Parsee class, so that the original carriage with two floors and bars like a sheep-truck remained unused in a siding.

The traffic of the line averaged 7% per mile per week. This no doubt would have been much more between two great cities, Baroda and Surat, had there been sufficient rolling stock, for Baroda is the centre of a thriving kingdom under the Guikwar, and Surat a flourishing port, with at least a dozen steamers in almost daily communication with Bombay. When the line is opened southwards to Bombay, and northwards to the thriving city of Ahmedabad, and with a sufficiency of rolling stock, the receipts will no doubt reach those of the Great Indian Peninsular Railway, say 20% per mile per week. This, on 13,000% per mile, and working expenses at 55 per cent., would give 4 per cent. dividend. There will always be a large amount of competition by sea.

In January last (the height of the season) the goods traffic was $\frac{1}{4}$ th of the whole, and even this consisted principally of railway materials. The Baroda and Surat length of 80 miles had been open a full year. This result was due to the want of rolling stock and circumlocution combined, for there are all the elements for as good a traffic, at least, as the eastern lines opened only between provincial cities.

The per-centage of receipts in the passenger traffic for January last was :

1st Class, 3.81 ; 2nd Class, 7.09 ; 3rd Class, 89.10.

The comparison with other lines in India for the same month was as follows :

East Indian (Howrah to Burdwan, 320 miles), 45*l.* per mile per week.
 East Indian (Allahabad to Cawnpore, 24 miles), 14*l.* 10*s.* per mile per week.

Great Indian Peninsular Railway (part open to Bombay, 437 miles), 12*l.* 10*s.* per mile per week.

Bombay and Baroda (Bulsar to Dolia, 132½ miles), 7*l.* per mile per week.

The third-class accommodation, it will be seen, is of the utmost importance, the traffic from which forming, as it does, 90 per cent. of the passenger traffic. Longer platforms, pure water laid on for the convenience of travellers, and shelter from heat and rain, together with comfortable carriages, are points for attention and consideration.

DISCUSSION.

MR. LE FEUVRE begged permission to make a few remarks before the discussion commenced. He, Mr. Le Feuvre, had taken upon himself the duty of reading this paper from the fact that he knew Mr. Sanderson, and had been requested by him to bring it before the Society. When Mr. Sanderson wrote this paper, it was with the view that he should read it himself, but he, Mr. Le Feuvre, need hardly say that was impracticable, as Mr. Sanderson was still in India. He might also mention that the paper was entirely due to Mr. Sanderson, and that the whole of the diagrams were drawn by him.

MR. C. G. WILSON said there could be no doubt as to the interesting character of the paper, and he hoped some members of the Society would be able to give some important facts as to the costs of the line.

MR. WELLS said during the time he was in Bombay, Colonel Kennedy wished him to go to Surat to give Mr. Sanderson some advice relative to the screw piles, but his stay was so short in Bombay that he had not time to do so. The system adopted was the ordinary one, viz. that of screwing down the pile as far as possible, and then by taking out the core the pile was readily fixed to any depth. In some cases they were screwed down as far as 35 to 40 feet, according to the nature of the ground. He had at the present time one of the assistant engineers fixing similar piles in Oxford, and if business had permitted he would have attended and given every information necessary. As to the system of screw piles, that was so well known as

not to require any remarks from him. All he could say was that they had been entirely successful in India, and there had been no bridges erected in India of such length or so rapidly as those where screw piles had been used.

Mr. C. L. LIGHT had used screw piles very extensively, and as to their breaking at the flange, he could say that he had never known an instance of the kind, and on the Bahia and San Francisco Railway he had used piles of 10 and 12 inches diameter, with $1\frac{1}{2}$ inch flange. The bolt-holes should be all properly drilled. He did not recollect a single mishap where the faces of the flanges were faced, and holes drilled and equi-distant from each other. If occasionally a pile broke, it arose from bad casting. There was one point in reference to these piles that he did not consider at all correct, and that was *casting the lugs on* for the bracings. He had generally used wrought iron clips, which could be attached at any point, and made much better work. He had bought good screw piles in very large quantities, faced, drilled, and delivered for 6*l.* per ton. The piles he referred to were but 2-feet piles, which were quite sufficient to carry a 60 feet span.

Mr. WELLS agreed with Mr. Light that all the flanges ought to be faced. All the screws used for the Bahia railway might have been made of good iron, but those for the Bombay line were so bad that if a hammer were dropped on some of them, they would break, and this was owing to the iron being run from the furnace instead of the cupola. Engineers, he was sorry to say, would not give more than 5*l.* to 6*l.* per ton for the piles, at which price good iron could not be expected.

Mr. PARSEY said the paper presented many points of interest as matters of evidence that occurred during the construction of the line. He did not see there was anything novel in the permanent way. The section of the rail was very light for a line of that nature, although no complaint was mentioned; yet it appeared that where the Adams's rail was laid down it shook when a train was passing at 17 or 20 miles an hour. Screw piles and light girders for the bridges over the numerous rivers was no doubt a judicious plan, if properly executed; but there appeared to be several matters in connection with the mode of carrying out the designs that were not so satisfactory. The size of

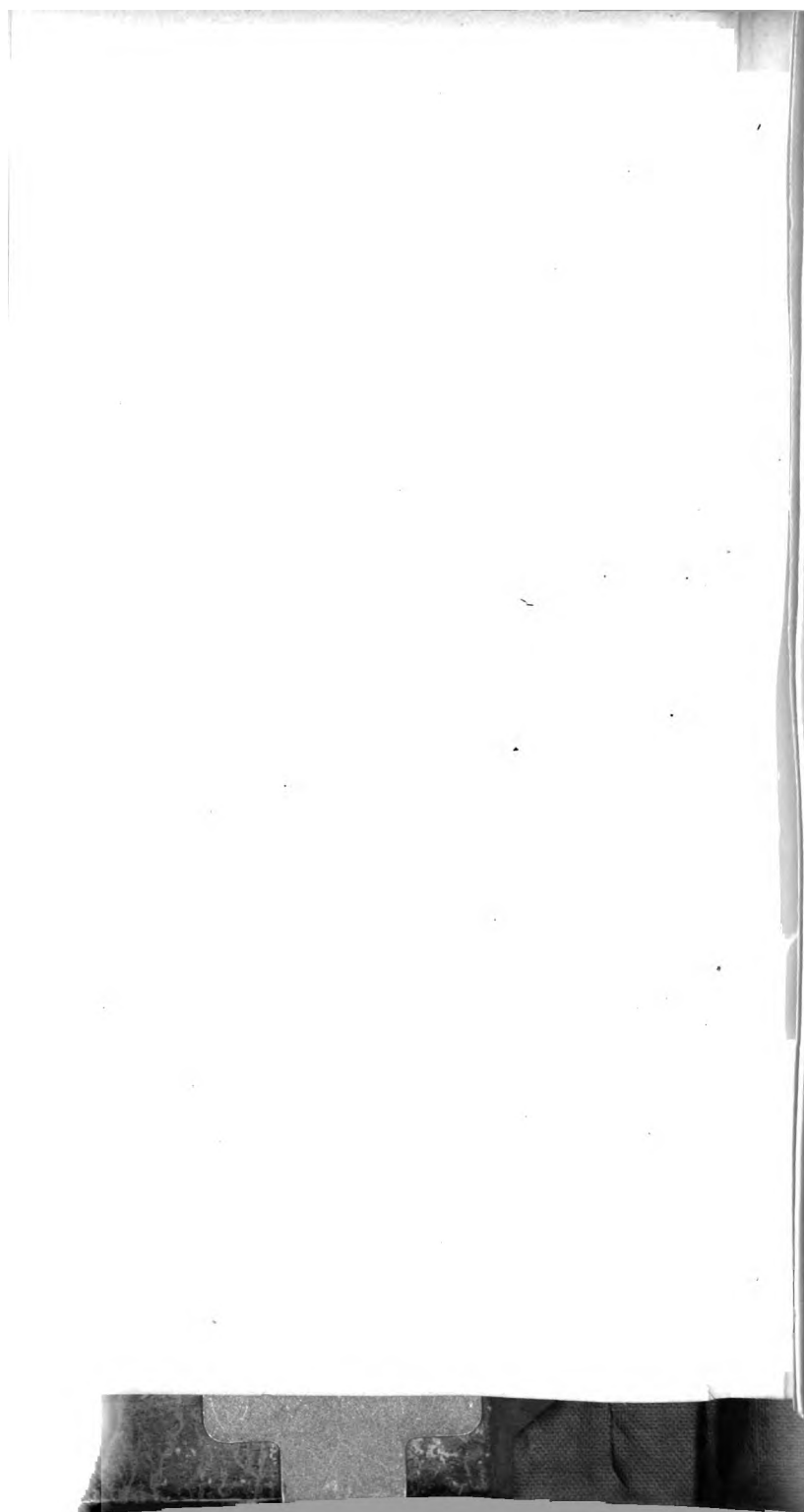
the connecting pins in the girders he thought were very small for the spans.

Mr. LE FEUVRE regretted that the discussion had been somewhat limited, but in answer to the points raised he might mention that it was originally intended to have a 60-foot span, but it was afterwards arranged to have a 90-foot span, and that the depth the piles were driven into the ground was, upon an average, about 30 feet. As to the breaking of the screws the average was about 5 per cent. Mr. Sanderson's argument was not so much against screw piles as to the difficulty of sinking them in India. As regards the breaking, there was no doubt if a screw pile broke it would be at the flange: these flanges were generally broken in screwing the piles in, several lugs were cast on, which were not used, some had four lugs where only two were required. The piles Mr. Light referred to were only 11 in. in diameter. Mr. Sanderson had to deal with piles 2 ft. 6 in. in diameter. As regarded the boring he thought the plan adopted by Mr. Sanderson was a good one, as it prevented the shifting of sands and the material being washed away. He thought now that the line had been completed the side pits should be fitted up, the objection to doing so before was probably the expense of carriage. As regards the failure of Mr. Brydges Adams's permanent way, he Mr. Le Feuvre mentioned the matter to Mr. Bridges Adams, and it was only due to that gentleman to state that he accounted for the failure by not having sufficient material. He, Mr. Le Feuvre, thought Mr. Brydges Adams's system of permanent way altogether a fallacy. As regards a 60 lb. rail, he thought they would have to renew the line with a heavier rail. He quite agreed with the opinion expressed, that where a Warren girder was used, the load should be upon the top.

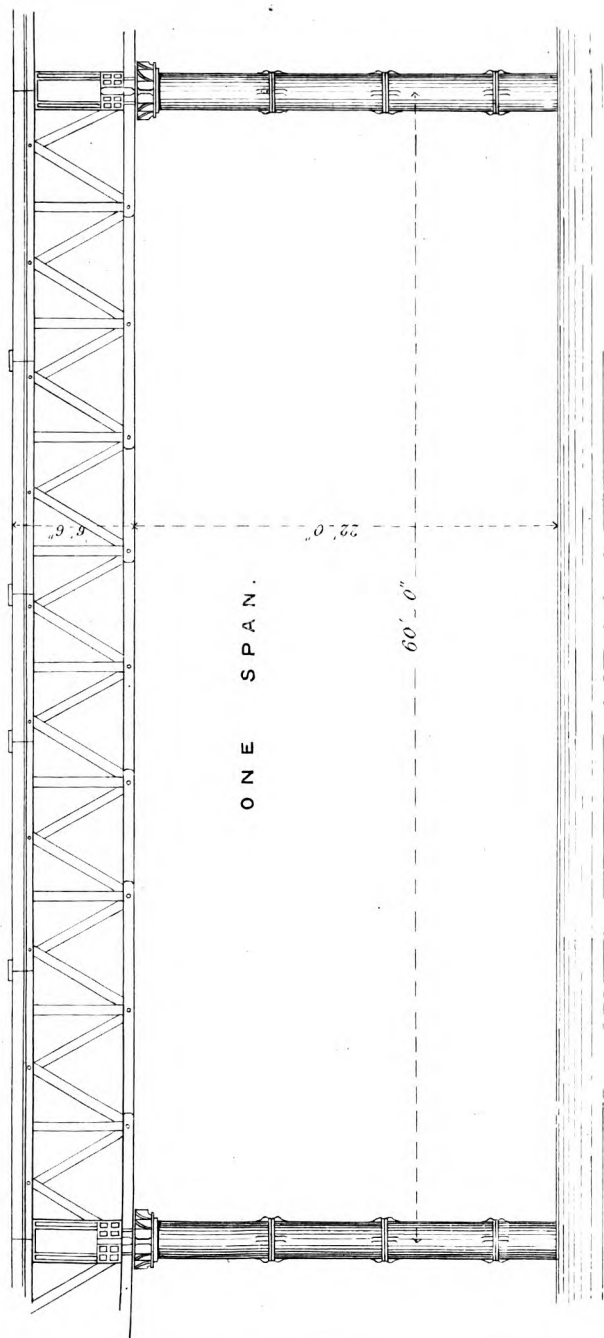
The CHAIRMAN said he was sorry there were not more present who had been in India, so as to have furnished the Society with some further details in connexion with the system carried out in the different railways. It seemed that the screw piles were a great thing for Indian railways, for without them there would have been very great difficulties in constructing the lines arising from the peculiar nature of the soil. It appeared to be the general opinion that the rails were a great deal too light.

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that the discussion had been somewhat



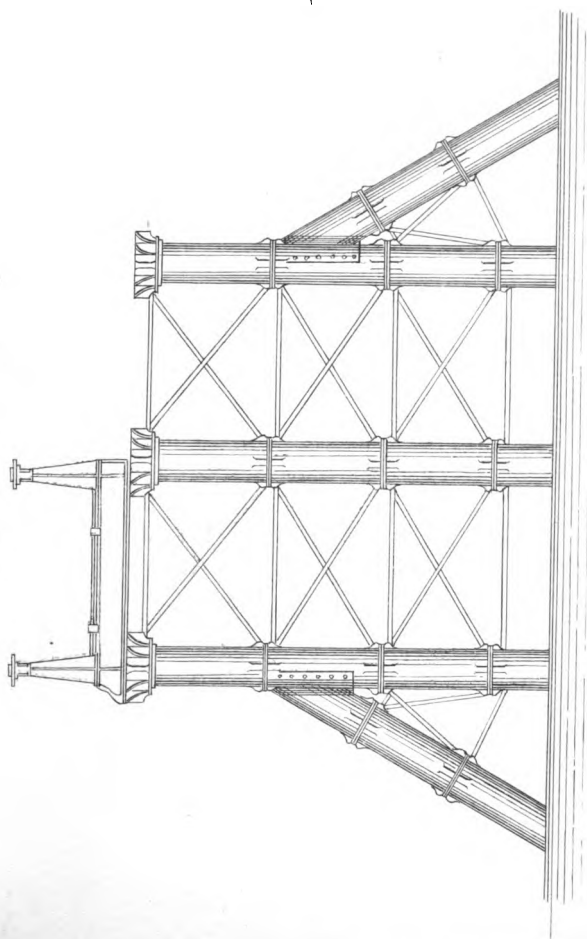
WARREN GIRDER BRIDGE.



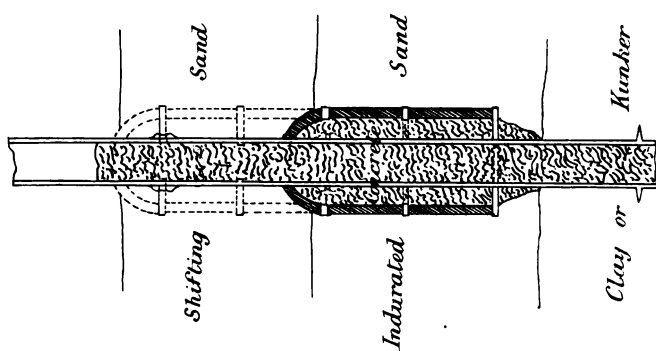
E AND F N SPON, BUCKLESBURY, LONDON.

Tipfield Green, Lith.

WARREN GIRDER BRIDGE.
TRANSVERSE ELEVATION.



E. AND F. N. SPON, BUCKLESBURY, LONDON



E. AND F. N. SPON, BUCKLESBURY, LONDON

September 7th, 1863.

G. W. ALLAN IN THE CHAIR.

ON SIGNALLING FOR LAND AND NAVAL PURPOSES.

BY FRANCIS WISE.

THE subject of signalling from one position to another where circumstances did not permit of a permanent connexion by conducting wires between the points, although most vital in importance to our naval and military services, was evidently one well deserving the attention of the engineer, inasmuch as there were frequent cases in which it would be highly advantageous to him to have the means of communicating with ease and rapidity to distant positions, and of receiving communications from them. The author might instance especially the case in which several parties were engaged on the survey of a large tract of uncivilised country, and who by their isolation from each other, were likely to be frequently placed in circumstances of difficulty and danger, which such means of communication would more or less enable them to avoid. Not only was this so, but those conversant with surveying operations would, the author thought, agree with him in believing that in cases of that kind, a considerable economy of time would also result from a simple and well-arranged system of signalling. There were numerous other cases in which the engineer would find such a system of the greatest service, but it was unnecessary to do more than indicate the fact, and leave those who might have the opportunity, to consider its applicability to operations in which they might happen to be engaged.

The use of signalling from one moveable position to another had hitherto been so much, the author might say almost entirely, confined to the naval and military services, that in treating the subject he was of necessity compelled to refer principally to them, although he looked forward to the time when it should be much better known, and its importance more generally appreciated among the members

of the engineering profession, than it was at present. He trusted also that those present would bear in mind that he in common with many of his brethren in the profession, had had little opportunity of practically learning the facts in connexion with the subject, and would therefore treat with something like mercy, the rather extensive drafts he had been compelled to make on the information and experience of others. If the members would only give him credit for a wish (however ill-carried out), to direct their attention to what he believed to be an important subject for them, and would accept the little information he was able to afford, regarding it as an incentive to further inquiry, his end would have been served, and he should receive the criticisms which his temerity might call down, without complaint, and in a proper spirit of resignation.

The subject of signalling had been well and ably treated by his friend Lieutenant Colomb, of the Royal Navy, to whose researches he was indebted for the majority of the facts referred to in his paper, and who had kindly allowed him the use of numerous highly interesting illustrative models which elucidated the various systems. In treating the historical part of his subject, he should endeavour to occupy as little time as possible, and merely touch upon some of the most salient points in connection with it.

More than 2000 years ago, the importance of the subject of Military signals, was seen and maintained by a Greek historian, who was himself the inventor of a system stated by competent authorities to be of considerable value. At the period referred to, Naval signals appeared to have been of the simplest possible character, and to have been made by a red flag or a brazen shield, hung out from the admiral's galley, and moved in various directions to indicate the movements to be made by the squadrons under his orders. This was, beyond doubt, a very imperfect plan, but it must be remembered, that at that time, vessels were so small, and from their mode of propulsion were so readily manageable, that it was a matter of comparatively little difficulty to convey verbal orders from one vessel to another throughout an entire fleet. Taking this into consideration, and bearing in mind the limited range at which they were effective in their assaults upon each other, it could readily be understood how the fleets of those days were manœuvred by such simple means.

The greater difficulties, arising from accidents of ground, &c., which had at that time to be encountered in the land service, led, as was usual in most cases where difficulties presented themselves, to the invention for it of more perfect arrangements, and caused that service at this period to be considerably in advance of the other, in the matter of signalling. As, however, the vessels increased in size, and propulsion by oars was abandoned, it became necessary to make use of means entirely independent of verbal communication, for conveying orders or information; and about James the Second's time, coloured flags were beginning to be employed for the purpose, without, however, taking a systematic form until probably a century later.

The author would not enter into all the details of the various stages of progress in signalling, but would at once come down to the period at which the art might be considered as possessing a practical value to ourselves; merely remarking that Lord Howe, on the celebrated 1st of June, had only the means of making one hundred and eighty-three signals, and that four years later, it had progressed so that flag ships were able to make three hundred and ten signals. At the present time, the vessels of the Royal Navy could display about fourteen thousand distinct signals, and those of the merchant service seventy thousand.

It was thus seen that Naval signalling had made important progress. The same could not, however, be said of signalling in the sister service, where, although its importance was obvious, there had been until lately a very manifest want of some simple system which would enable commanding officers to rapidly and with certainty communicate their orders to those under their command, and thus to make the movements required at any particular time, without trusting to the transmission of orders by word of mouth, and taking all the consequent chances of delay and miscarriage.

The author did not propose in the confined space of this paper to touch upon all the systems of signals that had been proposed; for their name was legion, and a vast number of them were evidently so well deserving of the obscurity in which they had remained, that it would neither be pleasant nor profitable to occupy the time of the Society in detailing them.

He might, however, briefly refer to one or two, which, if not

thoroughly practical in character, had at least some redeeming features either in the way of mechanical ingenuity, or of arrangement indicative of improved ideas upon the subject.

Thus, in the year 1801, a patent was taken out by James Boaz, a manufacturer of Glasgow, in which an attempt was made, by the use of mechanical contrivances, to render the making of signals less liable to error than it had hitherto been. This inventor did not seem to have had the idea of any code or system of signals; but to have depended upon the spelling of the words he wished to communicate, letter by letter, not merely by symboling each letter; but by actually producing the form of the letter itself. His contrivances, shown in diagrams 1 and 2, displayed considerable ingenuity, and consisted, for night signalling, of a frame or case, containing twenty-five lamps in the form of a square, and each capable of being obscured when necessary by a cover, which, when at liberty, fell over the orifice, through which the lamp was displayed. These covers were connected by cords or chains, with a series of boards working (at each end) on pivots, and arranged beneath the lamp-frame. Now, presuming that it was required to signal the letter H. In order to do this, a certain number of the lamps had to be obscured, leaving only those visible which would convey to the observer the idea of that letter, as shown in the diagram. For this purpose the inventor made use of a slide marked H, and provided with certain projections which, when the slide was pushed in, came in contact with so many of the boards referred to, as were connected with the covers of the lamps required to be exposed, thus instantly giving them a tilt, drawing down the required cords or chains, and simultaneously exposing the whole of the lamps necessary. Similar slides were provided for signalling the other letters; and numbers, &c., if required.

For day signalling Boaz proposed to employ a similar arrangement (diagram 2) merely substituting hinged shutters for the lamps, and turning them edgeways towards the observer when they were not required to be seen.

Lieutenant-Colonel the Hon. John Lindsay, in the year 1809, patented a system (diagram 3) which appeared to possess considerable merit. Lindsay's system for day signalling depended on the different positions of a triangular object suspended on a centre, and the combi-

nations of those positions with the positions of a circular body capable of being revolved around the triangular one. For night signalling this inventor made use of lanterns placed in various positions, as shown in the diagram. This system, owing to its comparative simplicity, contrasted very favourably with some of later date.

Rear-Admiral Henry Raper, in the year 1827, made a further step in advance, when, at that date, he patented "A new and improved system of signals; first, for communicating by day by means of flags and pendants, between ships at sea or other objects far distant from each other; in which system the colours of the flags and pendants, which have heretofore served to distinguish the signals from one another, and which by distance or other causes, are extremely subject to be mistaken, may be dispensed with altogether; and, secondly, for communicating by night between ships at sea, and other objects far distant from each other, by means of lights, and which system of signals is more conspicuous, expeditious, and certain than any which has hitherto been employed for the like purposes." (Illustrated by diagram 4.)

The principal peculiarities of this system were thus stated by the Patentee:—"They consist, first, in classing the signals according to their significations, and distinguishing the classes from each other, by combinations of flags and pendants; and, second, when colours are not visible, the employment of the distant signals by which they are made to express every signal, and even the telegraph. These signals are numeral, and for the first time, classed under distinct heads, such as movements of the fleet, signals *to* chasing ships, signals *by* chasing ships, engaging, compass signals, distress, ship's numbers, telegraph, &c.; and every signal is numbered in its respective class. Each class is distinguished by an appropriate combination of flags and pendants, by which means, the most conspicuous combinations are adapted to the signals of most importance, such as signals for engaging, chasing, distress; and the colours of the flags and pendants, which compose each combination, point out the number each signal bears in its particular class. Eleven flags and eleven pendants are required." "Experience," said the Patentee, "may improve these forms, but the colours are of little importance." He relied on the distinction between flags and pendants for determining

the general nature of his signals, and said accordingly that, whenever a flag could be distinguished from a pendant, a signal to a chasing ship could not be mistaken for a movement of the fleet; nor could either of these be mistaken for a signal made by a chasing ship, or for the number of a ship on the list of the navy; and the same reasoning applies to all the other combinations. By this it would be understood, that according to Rear-Admiral Raper's system, the character of the combination of flags and signals determined the class to which the signal belonged, and enabled this class to be identified without its being necessary to distinguish colour for that purpose. This appeared to have been an important step in the right direction, and to have greatly lessened the chances of error previously existing. It would, however, occupy too great an amount of the limited time at the author's disposal to enter into all the details of Admiral Raper's system, which, however imperfect it might be, as compared with existing systems, was evidently in advance of any system then in use.

It would appear, in so far as the author was able to form a judgment, that both for naval and military purposes any system of signalling employed should be capable of communicating with either one or many points or positions at once. Thus, that an admiral might either communicate with one ship only, or simultaneously with several or all the vessels under his orders. A general ought, in like manner, to be able to signalize any one or number of the regiments constituting his army. Hence, it appeared to be a matter of necessity that the signals made should present the same appearance all around. Farther, it must be borne in mind, that as these signals were *most* vital in their importance, at the very times when the minds of those who had to work and to interpret them were agitated either by the storm of battle or that of the elements, they ought to be of the simplest character, and as independent as possible of the skill and coolness of those who had to make and to receive them; and although for the purposes of engineering these requirements might, at first sight, seem less essential in their character, the author could not but think that numerous cases were likely to occur, in which the agitation consequent on imminent danger might, even in connection with engineering operations, be rather necessary to be taken into account.

He would take, for example, a little party of surveyors, laying out a line, and temporarily isolated from their companions. How uncommonly cool and collected these gentlemen would feel themselves, on discovering a party of painted and grinning savages in their immediate neighbourhood! He thought that under these circumstances a simple means of "telegraphic dispatch" would be wished for by most of them. But taking even a less romantic and more comfortable case, such as the floating of the great tubes of the Britannia and Conway Bridges, he apprehended that none of his hearers would deny the utility of a simple system of signalling, whereby the operation could be thoroughly and completely controlled without danger of misinterpretation; and he trusted, ere the conclusion of his paper, to bring to notice a system whereby this could be effected. It was unnecessary, however, to enlarge upon this point; as, if signalling was capable of useful application to the purposes of the engineering profession, he was confident of their being sufficiently progressive to avail themselves of it.

Systems of signalling had been divided into two classes; namely, the "General" system, wherein the symbols were displayed in groups, and the "Telegraphic," wherein the system of grouping was not used. Each of these had their acknowledged advantages and objections. The former was objectionable owing to the number of objects usually required to be displayed at one time, or, as it was technically termed, "at one hoist;" but was advantageous on account of the amount of meaning conveyed at one time. The telegraphic system was acknowledged to be good where only one position had to be addressed, inasmuch as it admitted in that case of very rapid change of objects; more than proportional to the greater amount of meaning conveyed at once by the general system.

Form, colour, sound, and motion had either separately, or in combination one with another, all been enlisted for signalling purposes.

The first systems to which he should refer, as belonging to the present or recent state of the subject, were those which were based upon the use of flags, variously grouped, and made use of to indicate the numbers affixed to the various significations of a pre-arranged code. Of such systems, it was obvious that those would be best which depended least upon colour, inasmuch as at certain distances,

and in certain states of the atmosphere, all colours resolved themselves into either dark or light, and it became impossible to farther distinguish their character. The ranges, too, at which the several colours might be seen varied considerably, so that if colour was an essential feature in a system, its effective range must be taken as that at which the least penetrative colour employed in it was visible. Beyond this range, that was, when a colour ceases to be distinguishable, there was obviously a liability that any two flags of similar form and pattern might be mistaken one for the other, and that in some cases serious consequences might result.

If these remarks were true of day signals, how much more so were they of night signals, as hitherto used? In considering this part of the subject, it must be borne in mind, that in using coloured flags for *day* signalling, the signal reached the observer by means of reflected light; but that in the case of night signalling, where lanterns were employed, a part of the light generated within the lanterns themselves had to be transmitted to the observer, and that every coloured glass interposed between the light and the observer greatly diminished the range. Of the colours employed, *red*, which absorbed the least amount of light, had the effect of diminishing the range one-third; *green*, being still worse; and *blue*, almost entirely obscuring a light. Then again, the difficulties of hoisting lanterns in various forms, and ensuring their being visible in their proper relative positions, on every side, on ship-board in a heavy sea, were such as to amount almost to impossibility. Under these circumstances, it was scarcely to be wondered at, that while day signals, as used in the Navy, numbered some fourteen thousand, it had only been considered that fifteen could with certainty be made at night. That such a vast disproportion existed between the requirements of a fleet in the day, and its necessities at night, it was obviously absurd to suppose; yet, such had been the proportion of communicating power hitherto relatively possessed during day and night. That such a state of things was lamentably bad, especially in the case of a nation having such vast naval interests at stake as our own, could not for a moment be denied; but to find a cure for the evil was apparently a matter of such difficulty as almost to give it the appearance of

impossibility. The author should remark that, *nominally*, the night system of the Navy was capable of making one hundred and three signals, but the number he had previously given was the limit of those which could be depended upon. The modes proposed for increasing the number of signals by means of what were called multipliers, consisting of mast-head-lights, rockets, blue-lights, guns, &c., were very objectionable, and had been condemned by our most experienced and best informed signal officers. In some cases, indeed, serious accidents had been caused by their failure to answer the end required.

Returning, however, to the subject of day signals. It had been justly observed that the most obvious and simple way of making a signal—say, of number—would be to display the number painted or written upon a card or other surface, or otherwise to display the corresponding number of flags or other objects; but the impracticability of such a system was too obvious to need demonstration, and it was hence necessary to make use of symbols which should act as a kind of short-hand, and enable us, with comparatively few objects variously placed or exposed, to represent a great range of numbers or other characters.

Now, in employing flags in which form, colour, number, and position constituted the elements of distinction of one signal from another, numerous systems had been suggested, and many had had their share of use; but those which had been, properly speaking, generally employed, and their use continued in its integrity for any length of time, were very few in number; if, indeed, any really original system could be said to have been employed since the earliest days of the art of signalling. The fact was, that in this, as in all other arts, improvement had gone on, not by great strides and sensation innovations, but progressively and by gradually striking out the bad and combining the good points of the various proposed systems.

Flags were usually employed with reference to a recognised code, which consisted of a collection of words or sentences, to each of which a number was affixed, which number was, therefore, symbolized whenever it was desired that the observer should act upon the word or sentence to which it was affixed, and which it consequently repre-

sented. This being understood, the author would proceed to call attention to one or two of the most prominent systems of flag signals.

Firstly, then, there were the "Numeral" and "Vocabulary" flags, made use of in the Royal Navy. In this system, five colours were employed, namely, white, black, red, yellow, and blue. Now, from what the author had previously said concerning colour, it would naturally occur to his hearers that such a variety was most objectionable, and that the liability to error in such a system must of necessity be very great. But it must be borne in mind, that in speaking of colours he had said that, under certain circumstances, all colours resolved themselves into *dark* and *light*, and that no other distinction between them could be depended upon. With this fact, then, before them, they would find on analysis that red, black, and blue, resolved themselves into dark, and yellow and white into light, colours. Now, on looking into the system, they would find that, for its purposes this distinction of dark and light colour was all sufficient, and that in the whole series of forty-seven flags and pendants, there were only two cases in which the dark and light colours assumed the same shape in different flags. From this it would be seen that, except in the cases referred to, colour might be entirely dispensed with, and the distinction of black and white only employed.

To show this more clearly he would take four of the numeral flags, represented in their proper colours, and the same four flags in black and white only. A comparison, would he thought, show the plain black and white to be the clearer of the two. The same fact might be observed with reference to the two cards exhibited, the uncoloured one being clearly visible at a greater distance than the one which was coloured.

With reference to this point of coloured and uncoloured flags, signal officers of experience stated that the latter were decidedly superior in distinctness to the former, and that where errors in reading occurred, the proportion was three, and even four, to one, in favour of the uncoloured.

America, a country from which, of all others, one would expect to derive improved ideas upon this important subject, had produced two sets of flags, which, like Hogarth's well known picture of per-

spective, were only likely to effect good by pointing out what ought to be avoided. These two strikingly bad systems were those of Mr. Rogers and Mr. Ward, and their faultiness was not only apparent to naval men of experience, but would, the author thought, at once be clear to the members. To illustrate this, he would take five flags of Rogers's system properly coloured, and a corresponding set of five flags in black and white only, and would ask whether they did not appear to have been specially designed with a view to enable the three upper and two lower flags to be mistaken one for another. These flags however, bad as they were, were greatly superior to Mr. Ward's, in which the principles to which the author had referred regarding dark and light colours seemed to have been completely ignored, and, as had been remarked by a naval officer conversant with the subject, "It really would seem as if, in devising this arrangement of flags, special preparation had been made for as large a number of errors as possible; as seven or eight flags done in black and white would represent everyone of the twenty-seven used in the set, and a simple black flag would stand for no less than eight of them." The consequences of carrying faults such as these into practice, were likely to be of the gravest character, and suggested that none but those who were practically acquainted with the requirements and difficulties of naval signalling were competent to design such systems of signals as could with safety be employed.

The author would not enlarge farther upon the subject of flag signals, which, however good and useful for the purposes to which they were applied, were scarcely likely to be largely available for engineering uses, inasmuch as the system was necessarily somewhat cumbrous, and required an amount of skill and practice for its efficient working far beyond what was ever likely to be devoted to it by engineers or their employés. But, before leaving the flag system, he would call attention to a method which had been suggested for displaying flags for army signalling, and seemed likely to be very useful. It consisted in sending up a small balloon, to which was attached a line of any required length, having upon it the necessary flags, which, in this manner, were enabled to be displayed from positions otherwise shut out from signalling.

Turning, then, to the subject of *night* signalling; it had been seen