

elevations they had any advantage, in point of range, over the larger projectiles which he had fired from rifled service guns. In order that this important point should be fully understood, he had prepared a Table, giving the results of his experiments, and he had added the results, as published in the newspapers, obtained with the Armstrong and the Whitworth guns:—

	Bore.		Charge of Powder.	Projectile.		Elevation.	Range.	Mean Velocity per Second.
	Diameter	Area.		Weight.	Capacity			
	Inches.	Inches.	lbs.	lbs.	lbs.	Degrees.	Yards.	Feet.
Rifled 9-pounder Service Gun, Cast Iron, 17 cwt.	4·2	13·1	1½	14	6 oz.	5	2,000	Not observed.
	"	"	"	"	"	10	3,200	
Rifled 32-pounder Service Gun, Cast Iron, 56 cwt.	6·41	32·2	6	49	3½	3	1,600	1,122
	"	"	"	"	"	4½	2,100	1,016
	"	"	"	"	"	8½	3,100	930
	"	"	"	"	"	10	3,600	900
Similar Gun	6·57	31·9	5	41	solid¹	"	3,700	740
Rifled 68-pounder Service Gun, Cast Iron, 95 cwt.	8·12	51·7	6½	90	7½	10	3,150	850
	"	"	8	"	"	"	3,560	920
Rifled 32-pounder, Cast Iron, 95 cwt.	6·37	31·9	7	56	2	"	3,700	955
Rifled 18-pounder, Cast Iron, 58 cwt.	5·29	22	6½	34	1½	"	3,900	948
Smooth Bore 68-pounder Service Gun, Cast Iron, 95 cwt.	8·12	51·7	16	68	solid	0·30'	340	2,040
	"	"	"	"	"	1	640	1,280
	"	"	"	"	"	5	1,960	939
	"	"	"	"	"	14	3,480	714
Armstrong Breech loader, Field Gun²	8	7	1·6oz	12	½ oz.	3	1,200	923
	"	"	"	"	"	5	1,820	900
	"	"	"	"	"	10	3,030	826
Ditto Large Gun	6	28·2	9	80	solid	10	3,900	-
Whitworth Breech loader, Field Gun	3	7	1½	12	solid	2	1,250	Initial velocity about 1,300 feet per second.
	"	"	"	"	"	5	2,300	
	"	"	"	"	"	10	3,780	
	"	"	"	"	"	5	2,600	
Ditto, Large Gun, Weight 80 cwt.	5·2	21	12	80	"	7	3,490	feet per second.
	"	"	"	"	"	10	4,400	

It would be seen from these figures, that up to about 10° elevation, the rifled cast-iron guns had at least as long a range as the wrought-iron breech-loaders with equal charges; and that at less than 5° elevation, the rifled service guns had a positive superiority in this respect. Nor was this all. The velocity with which the rifled service guns projected their shot, even with smaller charges of powder, was much greater than was the case with the breech-loaders. In the Official Reports of Mr. Britten's experiments, the time of flight of each shot was carefully recorded, so that there was no difficulty in ascertaining the mean velocities at the different

¹ Service round shot, prepared by Mr. Britten to suit rifled guns.

² *Vide* Range Tables in Horse Guards Manual, published by authority.

ranges. The mean velocity of his 49-lbs. shells, fired from the 32-pounder rifled service gun, was thus shown to be 1,120 feet per second, in a range of 1,600 yards; the 56-lbs. shell, with 7 lbs. of powder, had a mean velocity of 955 feet per second, in a range of 3,700 yards; and the 90-lbs. shell, 8 inches in diameter, with only 8 lbs. of powder, or one-eleventh of the weight of the projectile, had a mean velocity of 920 feet per second, in a range of 3,560 yards. When, therefore, it was stated, that the velocity of the Armstrong projectiles, on leaving the gun, with charges of one-eighth of the weight of the shot, was only 1,080 feet per second, and that of the Whitworth shot, with a charge of one-sixth, was under 1,300 feet per second, he thought it might safely be asserted, that the muzzle-loaders did more work with the power applied than the breech-loaders.

In order to show the great effect of the resistance of the air in diminishing the velocity of large bodies during flight, the mean velocities, at different ranges, of the 68-pounder service solid shot, with full service charges, were given in the Table. These figures were officially determined, from practice on board the 'Excellent' gunnery ship. It would be seen, that at 340 yards, the mean velocity of the service solid 68-pounder shot was 2,040 feet per second; but this mean speed fell off to 714 feet per second at the range of 3,480 yards. The same gun, when rifled, threw a 90-lbs. shell, 3,560 yards, with a mean velocity of 920 feet; it was therefore probable, that the initial velocity, in this case, must be very much more than was obtained by the breech-loaders. This was remarkable, when it was remembered, that the 8-inch shells had the resistance of the air upon 51 square inches, the sectional area of the shell; while the Armstrong and the Whitworth projectiles had a sectional area of only 28 and 21 square inches respectively, and were fired with much heavier charges. From these facts he inferred, that for horizontal fire up to 2,000 yards range, which was the service most required, his large-bore guns were in no respect inferior to the new small bores, while in many points they were far more serviceable.

There was one other point to which he would advert. The cost of an 8-inch cast-iron gun was about £100, while it was doubtful whether either of the new rifled breech-loaders of the same size, which had been so much spoken of, could be made for much less than £1,000. Then the projectiles for these breech-loaders would be costly. At present, he believed every shot for the Armstrong gun had first to be turned accurately cylindrical, then a considerable number of grooves had to be cut round it in order to hold on the lead, which had afterwards to be turned again. The shot for the Whitworth gun would be also very expensive, if, as was asserted, it had to be put into a planing-machine of

curious construction, and each of its six sides planed with the utmost nicety into the form of a screw, to fit the rifling. On the other hand, Mr. Britten's shells were plain castings, and would cost so little more than the common spherical shells, that the saving of powder, required to project them, would be an equivalent. This was a point of great importance, for the expense incurred in the cost of guns was trifling, in comparison with the cost of the projectiles to be thrown.

The system proposed by Mr. Britten would, in his opinion, not only enable the present service guns to be utilised, but he had shown, that the round shot and shell of all kinds, now in store, could be adapted, with great advantage, to suit the rifled service guns. The common 32-pounder round shot, when prepared with lead, so as to expand in the gun, and when fired at 10° elevation, with only half the usual service charge of powder, had nearly 1,000 yards longer range, than on the old system. The 32-pounder shot, when thus adapted, weighed about 41 lbs., and at 10° elevation, it attained a range of 3,750 yards, with 5 lbs. of powder; while the ordinary 32-pounder service shot, at the same elevation, only had a range of 2,800 yards, with 10 lbs. of powder. The cost of preparing the shot and shells with lead, in the way proposed, would be considerably less than the value of the powder saved; so that this advantage might be obtained, and a positive saving of expense be effected at the same time.

General Sir JOHN FOX BURGOYNE observed, that the subject before the Meeting, was one requiring the deep study of professional men, having a knowledge of the application of iron to bear different trials. His principal researches had been as to the practice with the gun after it was made. Some of the observations which had been offered did not quite accord with his views. It had been said, that it would scarcely be possible to make fortifications, to withstand the effects of these heavy guns. He thought, however, they would not affect fortifications in so great a degree. Supposing, for instance, a fortification to be thoroughly covered with a ditch and counterscarp, the escarp being thus entirely covered, the besiegers must get close to it before a breach could be made. Long-range guns might help to destroy the parapet somewhat earlier; but if the place was provided with casemates, it must be closed with, and then the old service guns were as good as the new rifled ordnance. In fact, they were better, as the velocity for the first 400 yards, or 500 yards was greater, although it was not maintained as in the rifled gun. These guns would also be available for defence, as well as offence. A remark had been made, as to the comparatively small value of long ranges, such as the one of between five miles and six miles obtained by Sir William Armstrong, and Mr. Whitworth. That great length of range was, perhaps, not of so

much importance in itself, but it was an indication of other qualities. It was a proof of superior accuracy and greater power of penetration. It likewise gave the power of firing at lower angles of elevation, which was of great value. The Armstrong and the Whitworth guns were very perfect, light, of beautiful workmanship, and of great strength. If better could be devised, it ought, by all means, to be done; and he hoped that the Members of this Institution would assist in bringing about still greater improvements. With regard to the expense of the manufacture of the Whitworth shot, he might state, on the authority of that gentleman, that the additional expense in shaping was not more than a penny per shot.

Mr. JOHN ANDERSON said, he would endeavour to confine himself to the ostensible object of the Paper,—the best principle of constructing a gun, so as to afford the artillerist the full development of the gunpowder used to throw the projectile. The Author argued, that the system of constructing a gun by binding it round with wire, was the true one. He fully coincided in that opinion, on theoretical grounds; but at the same time, he agreed with the remarks which had been made, as to the difficulty of applying that material. The great objection to the wire system was, that it had no end-hold, and thus it could not be depended upon, for forming a good breech, and producing a substantial and thoroughly good serviceable gun. He believed that Fig. 18, (page 318,) represented the construction of a gun, which the Author thought would be most likely to secure the object upon that principle. While agreeing with the principle, Mr. Anderson thought there were many better ways of carrying it out. Before stating his views on this point, however, he would allude briefly to what had been said in favour of cast iron. He did not think the great results expected by the artillerist, would be secured by the use of a homogeneous material like cast iron. It was most uncertain in character; one day a gun stood well, while on another day, it burst, without giving any warning, even when the same amount of care had been taken by the founder. Great expectations had been raised, of late years, by the system of strengthening cast iron by the employment of wrought-iron hoops. Without entering into the question of the priority of invention, he might say, that great results had already been obtained by the use of wrought iron in that manner. Still, taking into consideration the risk of failure, which experiments made within the last few days showed was possible, together with the expense, he thought another direction held out a better prospect of success. A few years ago, it was believed, that the proper gun would be obtained by forging. In 1854, when Mr. Nasmyth was at work, the country expected great results.

The end of that gun might be said to have been a national disappointment. Since then, there had been the Liverpool gun, a monster mortar, which was referred to in the Paper. It was a magnificent forging,—the finest he had ever seen,—yet it was not a perfect gun. The bore of that gun would never have passed the proof of the artillerist. There were defects in it, and that would always be, more, or less, the case, in the heart of all such large structures when forged. At the present moment, there were at Woolwich some apparently very fine forgings, which were defective, owing to fissures at the core, and more especially, in the chamber at the breech. Therefore, he did not think the good gun, which all were aiming at, would be obtained by the system of forging. So far as his own experience went, the best result was to be obtained by building up, on the principle of hooping. When he spoke of the best result, he did not mean the mere conditions of supplying the wants of the artillerist, but he had also in view the important question of cost. In reference to the cost of Sir William Armstrong's gun, which had been alluded to, he might state, that the 12-pounder gun, complete, was now being made at Woolwich at a cost of under £90; this was about one half the cost of the old 12-pounder brass gun, the value of which was £175. 10s. When it was remembered, that in the one case the material cost about £19 per ton, and in the other about £125 per ton, together with the great difference in weight, there was a large margin to work upon. Having had considerable experience in this question for many years, he might say, that it appeared to him, that the gun of the future must be a built structure. He was not now prepared to say precisely how the detail of the structure was to be carried out, whether with a wrought-iron, or a steel lining to the bore, supported with a covering of wrought iron under compression; but he maintained that it must be a built gun. From the readiness with which wrought iron could be worked into the handy form of a bar of any section, by means of the rolls of the iron-maker, and the equal facility with which this bar might be wound upon a mandril, in a sort of lathe, and then welded into a tube, or drum, a degree of cheapness, combined with extraordinary strength, was secured, in a manner not equalled by any other method yet introduced into the iron manufacture. When the system of tools was adapted to manipulate the material into the right form, the greatest amount of strength would be obtained by the built gun, consisting of such hoops, or drums, shrunk, or forced together. This was the principle, which Sir William Armstrong and others were now working out, and it appeared to be that which would afford the greatest strength, and would constitute the cheapest description of structure for guns. He thought, that the cost of obtaining

this peculiarity of construction had been over-rated in the Paper. In olden times, there was a difficulty in working to the $\frac{1}{16}$ th of an inch; but the $\frac{1}{1000}$ th of an inch was now measured with as much ease as was formerly the case with the $\frac{1}{16}$ th, or $\frac{1}{32}$ nd of an inch. Although it might not be possible to bore to the $\frac{1}{1000}$ th of an inch, yet by means of turning, a gun could be most accurately and economically put together. He had said that the wire system was defective, inasmuch as there was no end-hold. Looking at Fig. 18, (page 318,) it would be seen, that a band was passed round the breech. To his mind, that was not the right principle of construction; nor did he think that was the kind of gun which was likely to come into use.

In reference to the complaint which had been made in the Paper, as to the treatment of the Author's invention by the Ordnance Select Committee, Mr. Anderson, having had the honour of being a member of the Committee at the time, would like to make a few remarks. He would ask whether any ten independent men, having undertaken the responsibility of recommending a gun which involved the expenditure of hundreds of thousands of pounds, would not even now hesitate, before recommending the wire system of construction? He thought there were few persons who, having to make a selection, would advise the construction of a gun upon the wire system, after knowing what Sir William Armstrong and Mr. Whitworth had done, with built wrought-iron and steel guns, on the principle of hoops shrunk, or forced on.

Mr. CLAY said, that having designed and manufactured the large gun which had been made at the Mersey Company's Works, he might, perhaps, be permitted to say a few words on this subject. The Author had called attention to the difference in the tensile strength of the material forming the inside and the outside of large forged ordnance, and had observed, that the same thing had been noticed in the gun made at the Mersey Works. Moreover, it had been stated, that large masses of metal, forged in that way, must be subject to inequalities of structure, which rendered them unfit for heavy ordnance; and that the gun in question was, on that account, untrustworthy. Now the gun was tried at Liverpool, before it was delivered to the Government, and was then pronounced by the officers from Woolwich to be fit for Her Majesty's service. After that, it was subjected, at Shoeburyness, to nearly double the charges which were used at Liverpool. Hundreds of rounds had been fired from that gun, yet it was still as good as on the day it was first made. With regard to the systems of hooping guns, and of covering them with wire in a state of tension, one thing had escaped notice; that was, the variation in the amount of expansion and contraction in different qualities of iron. It had been stated, that an error of $\frac{1}{300}$ th of an inch would cause a

serious difference, in regard to the tensile strain which would be brought upon the several parts. Now he believed it would be found, if careful experiments were made, that different descriptions of iron would vary more than $\frac{1}{500}$ th part of an inch, in the rate of expansion and contraction. He knew that iron and steel differed much in their expansion and contraction, and he thought it would be found to be the case with iron generally, according as the crystallized, or fibrous structure predominated. He considered it was a dangerous system to pursue, to build up guns with concentric hoops, as no dependence could be placed upon the degree of initial tension with which they were shrunk on. Some remarks had been made as to the liability of hydraulic presses to burst. He had made many large cylinders, and was not aware of one having so failed. The presses used in the manufacture of lead pipes were, perhaps, subjected to the severest test to which cylinders could be exposed. It had been said, that the process of forging was not the proper one for the manufacture of ordnance. It so happened, that at the present time, he had before the Select Committee, a plan to make ordnance by a different method. He was not at liberty to describe the method, but he might say, generally, it was by the rolling process. In several guns made in that way, he had not found a single defect, either in the bore, or in any other part. He had recently fired, in the presence of a number of Artillery Officers, twenty-two balls, with a cylinder projecting 12 inches from the muzzle, with $1\frac{1}{2}$ lb. of powder, out of an iron gun with a bore of $2\frac{1}{2}$ inches. A steel gun was also tried in the same manner. He considered that the manufacture of ordnance was still in its infancy; but he trusted, that as the public mind was now directed to that particular science, important improvements would be introduced.

Mr. VIGNOLES observed,—through the Secretary,—that the gunpowder experiments of his grandfather, the late Dr. Charles Hutton, had been very carefully made and often repeated. The calculations and deductions from these experiments, showed results equal to a pressure of 17 tons per square inch on the metal of the gun. This had appeared, to the President, when first mentioned, as very high, but he had since observed, that his own calculations brought out even 20 tons. Mr. Vignoles remarked, that from data recorded, his calculations induced the conviction, that the pressure was often nearer to 30 tons, than to 20 tons on the square inch. On mentioning the subject to Professor Airy, (Hon. M. Inst. C.E.,) that distinguished mathematician and philosopher expressed the opinion, that under certain circumstances, the force of gunpowder was much greater than had been stated. Mr. Vignoles thought, that attention should at once be directed to this point, of so much importance, and so easy to be determined, in order to put the matter beyond dispute; and thus the principles on which this enormous pressure was to be counteracted, would be clear of all doubt.

Mr. HADDAN stated, that his operations had been limited to the existing service gun. He started with this axiom, that for a cast-iron gun the rifle twist should be as slow as possible, so as to establish only such an amount of rotation of the projectile, as would insure its travelling point foremost throughout its flight, and that any greater extent of rotation was useless. Another essential point was, that the rifling should weaken the gun as little as possible. He assumed, that if the rifle cuts, or grooves, were angular, or square, they would have the same effect, as nicking a bar of iron intended to be broken; whilst broad and shallow grooves, or cuts, would not destroy the elasticity of the gun. An ordinary 68-pounder cast-iron gun had stood firing, with shells 115 lbs. in weight, and considerable charges of powder, without bursting. He believed the plans of Mr. Whitworth and Sir W. Armstrong were inapplicable to cast-iron guns, and hence resort was had to built guns of malleable metal. The large cast-iron guns which had been bored on the Whitworth system, and the calibre of which was small, had burst, owing, as Mr. Haddan believed, to the quickness of the rifle twist. He had fired a shell, (of which a specimen was exhibited,) weighing 90 lbs., with the rifle twist, only about one turn in 40 feet, whilst the Whitworth 3-pounder had a twist of one turn in 40 inches. The Government report upon the experiments with a service 68-pounder cast-iron gun, rifled upon the principle he had adopted, showed that he had obtained equal ranges, charge for charge, to the Armstrong gun. He believed, that the length of range depended upon the charge of powder used; and on that ground, he invited comparison with the results of Mr. Whitworth's and Sir W. Armstrong's experiments. In the generality of cases, he understood the Armstrong gun had been tested with a charge of powder equal to one-eighth the weight of the projectile; whilst in Mr. Whitworth's experiments the charge of powder appeared to be only one-fifth the weight of the projectile. Again, in considering the flight of the projectile, it was important to know whether it was hollow, or solid. The large shell weighing 90 lbs., which he exhibited, was 8 inches in diameter, and was made to contain a bursting charge of nearly 6 lbs. of powder. Mr. Whitworth's 90-lbs. shot was solid and was only about 5 inches in diameter. With the same charge, therefore, Mr. Whitworth's would necessarily have a longer range.

In a letter to "The Times,"¹ Sir W. Armstrong said:—

"With respect to ranges exceeding 2,000 yards, I may state, that on previous occasions the gun had been tried up to 3,000

¹ Vide "The Times," January 3rd, 1857: (page 5). Reprinted in "The Mechanics' Magazine," April 22nd, 1859: (page 262).

yards,—a distance which was reached with an elevation of 11° ,—and the usual charge of 10 ounces of powder, or one-eighth the weight of the projectile. By augmenting the charge the range is increased, but the accuracy is impaired, and I therefore adhere to the 10-ounce charge, which gives ample penetration, as the experiments at the butt will testify.”

The shells adopted by Mr. Haddan were fit for use, without any manipulation, after they came from the founder's hands; and he could obtain a long range, with a small charge of powder, as compared with the weight of the projectile, and as he believed, at a very small cost. In corroboration of this remark, he would state, that the two shells exhibited, had been cast at the Royal Foundry, at Woolwich, and Mr. Davison had informed him those shells, fit for service, would not cost more than the ordinary service shells, or not exceeding £5. 6s. per ton. A common service gun, rifled for him by Messrs. Napier, York Road, Lambeth, was tried at Shoeburyness on the 23rd of February, 1859. From the Government Report it would be seen, that the charge was 10 lbs., the elevation 10° , and the range 2,900 yards. The projectiles weighed on an average 90 lbs., and were capable of containing about $5\frac{1}{2}$ lbs. of powder. Both the deflection and the range were very uniform. If, therefore, cast-iron guns were not all that was desired, yet for many practical purposes, it was evident they might be made largely available as rifled ordnance. He thought it would be generally agreed, that before the Government incurred the serious expense of making wrought-iron guns of large size, it should be placed beyond doubt, that the present service guns were not available. At present there was no proof, that cast-iron guns could not be used. A statement had been made, that every cast-iron gun that had been rifled, had failed, but that was utterly untrue. Mr. Britten had rifled several service guns, and not one of them had burst. The guns experimented upon by Captain Scott, R.N., Mr. Jefferies, and others, had not burst, nor had the 68-pounder, rifled by himself, been injured.

Referring to the employment of lead, in the construction of the projectile, he said that he had always thought it objectionable, and therefore, did not make use of it. He preferred a wad of wood, or other material, and placed it at the conical back of the projectile. The first blow from the explosion of the powder drove the wad tight round the projectile, and caused it to expand so as to fit the gun. There was thus no loss from windage. This projectile possessed all the advantages of the lead-coated one, and in addition, the wad was cheaper and cleansed the gun; there was no difficulty as to transport, and it was immaterial how rusty either the guns, or the shells might be. By means of the broad shallow grooves, or cuts, he did not materially weaken the gun, and by

giving to the rifling a slow twist, of only about one turn in 40 feet, there was little strain on the gun when discharged, and therefore, little, or no tendency to burst it. He had lately had two 32-pounder service guns rifled by Messrs. Napier. They had been tested with 56-lbs. shots and a charge of 7 lbs. of powder, and neither of them had burst.

When his proposition was brought before the Government authorities, there were already eighteen, or twenty different plans for rifling the service guns. It was suggested, that each should rifle a gun upon his own principle, so as to prove practically which method would answer best. That appeared to be a perfectly fair mode of proceeding. Whether the experiments had been carried out by the others he could not say; but he understood, that Sir William Armstrong had made two attempts with service guns, both of which had burst, and that Mr. Whitworth had been equally unsuccessful. Mr. Haddan was prepared to take his two rifled 32-pounder service guns, with the shells, which required no labour to finish after they came from the founder, and to fire them against those of Sir W. Armstrong and Mr. Whitworth, with the same relative charge of powder, and thus to let the results show which system of rifling was the best.

Mr. BRAMWELL said the Author had stated, that it was a bad practice to cast guns solid; and in Fig. 1, (page 290,) he showed the assumed lines of contraction of the metal in the interior of a gun so cast. Every practical man must agree, that a solid casting was objectionable, and that a gun would be stronger if it were cast hollow, so that the contraction should commence simultaneously in the inside and at the exterior. No doubt, formerly, from the want of the appliances of the present day, difficulties were experienced, which caused the plan of casting guns hollow to be abandoned, and they were, therefore, made solid. He had upon a previous occasion mentioned that, in the American service, Lieutenant Rodman had introduced the principle of casting guns hollow. To obtain the full benefit from the interior cooling before the outside, water was passed through the core of the gun, equal to sixty times the weight of the casting; the operation being continued for seventy hours, and the outside being kept heated by fires. The result of comparative experiments with a solid and with a hollow gun, both run from the same furnace, and made from precisely the same metal, was that the 8-inch gun, cast solid, burst at the seventy-third discharge, whilst the 8-inch gun, cast hollow, with water passed through the core, endured fifteen hundred discharges, and did not burst. He thought that went a great way to show, that the principle of casting a gun solid was wrong; and that when it was cast hollow, the metal was in the best condition to resist the strains put upon it. In the Paper it was stated, that the tensile strength

of cast iron, in a gun cast solid and then bored out, was 8 tons to the square inch, at the outside of the gun, and 4 tons to the square inch, at the side of the bore. In the solid gun cast by Lieutenant Rodman, a sample cut out near the trunnion showed a tensile strength of 44,000 lbs. for the outside, and 31,000 lbs. for the inside. That also was another indication, that a gun ought to be cast hollow, if cast iron was used at all. In the account of the experiments made by the American Government, it was stated, that guns had been tried which had been kept a long time after being cast, and before being tested. A gun which had been so kept for six years, endured eight hundred discharges before it burst; while another gun endured two thousand five hundred and eighty-two discharges, and did not burst. Guns of the same description, tried thirty days after casting, burst, one at the eighty-fourth, and the other at the seventy-second discharge. This result showed, it was not impossible, that the superior manner in which guns cast some years ago, but recently used, had stood their work, was due to their having been cast a long time, rather than, as was commonly supposed, to their being of a better quality of metal; and to the strains that existed in them, from unequal contraction, when originally cast, having ceased, while the strains in the new castings were still exerting a prejudicial effect. It was proved, in the case of the two guns to which he had alluded, that the gun which burst after eight hundred discharges had a tensile strength of 23,000 lbs., and that which endured upwards of two thousand five hundred discharges, without bursting, had a tensile strength of 29,000 lbs. to the square inch. Of the guns which were tested thirty days after being cast, the one had a tensile strength of 27,000 lbs., and the other a tensile strength of 37,000 lbs. per square inch of section. Both these recently cast guns endured a less number of rounds, than those which had been cast some years, although the metal of these latter was much weaker than that of the former. It was a general opinion, that castings ought not to be put to work too soon after being made, and in tilting ingots of steel, it was preferred that they should stand some days after being cast, before being subjected to the tilt hammer. It had been the practice to keep wire for some time between the 'drawings,' in order that its particles might recover their relative condition, before being again subjected to strain. He, therefore, considered there was a fair prospect, that if good cast iron was used, and guns were cast hollow and kept some time, or even if cast solid and kept, cast-iron guns might be made successfully. As to the tensile strength of cast iron, he had a sample, which was broken at the testing machine at Woolwich, that bore $19\frac{1}{2}$ tons to the square inch of section, before it gave way. He thought that 8 tons was too low an estimate to take for cast iron, in a gun.

There was another point which had been rather overlooked. The explosive force of gunpowder had been variously estimated, as amounting to from 7 tons to 70 tons per square inch. It had also been stated, that in a cast-iron gun, the average strength available to resist the pressure that was brought to bear upon it, could not be taken at more than $2\frac{1}{2}$ tons to the square inch. It was quite clear, that these statements of the force of powder and of the strength of iron could not be reconciled. But one thing had been neglected, the area of the section of the gun which bore the strain. If a gun of infinite length were loaded, the metal which bore the strain would be only the metal at the sides of the gun, and all the discussion had been based on this assumption. If that metal were equal on each side to the bore of the gun, it followed that for every inch subjected to the pressure of the gunpowder, there were two inches of iron in tension, or of metal to be burst through. If there were, as in a 68-pounder with a service-charge, a length of about 15 inches, where the greatest pressure was exerted, there would be the same metal at the sides to burst through, and in addition, a portion of the annular section of the gun, making $3\frac{1}{2}$ inches instead of 2 inches of metal to be broken through, for each inch of area pressed on. This appeared to be the reason why a comparatively weak metal was capable of enduring a pressure, which, at first sight, it seemed unequal to bear. Regarding the metal which had to resist the strain in this way, decreased windage might operate favourably; for although it might increase the pressure per square inch in the gun, yet nearly the whole power of the powder was employed in propelling the projectile, therefore the amount was diminished, and the part subjected to pressure was shorter; and thus the annular section to be torn through, bore a greater ratio to the area pressed, than it would with a longer charge. If this view was correct, it was an argument against building a gun with hoops, and still more against the ingenious plan proposed by the Author, of covering the gun with wire, which only gave radial strength. He thought, however, that the plan of binding with wire, enabled an accuracy of strain to be obtained, which could not be obtained with hoops. The varying qualities of the metal and the difficulty of shrinking on hoops to the $\frac{1}{300}$ th part of an inch, which was said to be necessary to insure success, were insuperable objections to that system. He did not agree in the opinion which had been expressed, that although a barrel covered with wire might be made sound, yet a strong breech could not be formed with wire. In some experiments he had made, in conjunction with Mr. E. A. Cowper, (M. Inst. C.E.,) with steel wire of No. 22 music-gauge, in which, it should be remembered that the numbers ran in the reverse direction to wire gauge, the tensile power was found to be as high as 142 tons per square inch. There

was another matter to be borne in mind. Assuming that hooped guns could be made with the accuracy shown in Fig. 5, (page 298,) when the explosion took place, the whole gun was in equal tension; but in time, the hoops in tension would stretch, and those in compression would give way. Therefore lapse of time, as regarded those guns, would do injury instead of good, as was the case with cast-iron guns.

Captain SCOTT, R.N., believed, that the ordinary cast-iron service gun would do all that could be fairly required, and would stand rifling, if too heavy shot were not attempted to be fired, and too sharp a turn were not given to the rifling. The shot he used were plain castings. In making them, he had endeavoured to obtain the form for permitting the greatest velocity through the air, and at the same time, for keeping up the rotation as perfectly as possible. His shot were cast so as to bear on three grooves in the gun, and were so shaped as to carry round little, or no air. In this respect they had a great advantage over polygonal and lead-coated shot, with which a large quantity of air must be carried round in rotating. This defect he had endeavoured to avoid, by deviating as little as possible from a cylindrical form. When that, or a circular form was not adopted, as, for example, if the shot was polygonal, a greater amount of initial rotation was required, than if the shot were of a figure adapted to keep up the rotatory movement. Hence, those who had tried the polygonal form, or who had fired a lead-coated shot out of a many-grooved gun, had been obliged to give a greater amount of rotation to the shot, than would have been necessary with fewer projections. His plan had also these advantages. The cast shot could be cheaply and easily made in any number, while there was a difficulty in the manufacture of compound shot, and in time of war, he did not see how they could be carried, in large quantities, without damage. On the other hand, the common cast-iron shot could be carried in bulk, and be stacked in the open air, without injury. The guns rifled for elongated projectiles by Mr. Haddan, Mr. Britten, and himself, would also fire round shot, and he felt certain that round shot would always finish naval actions. The elongated shot had this special disadvantage: it would not, like a round ball, at once run home in charging the gun; besides which, at close quarters, no projectile was more destructive than the round shot. A remark had been made as to the greater initial velocity of round shot, and the greater injury caused by them to parapets, than by any elongated shot which had yet been tried. He considered, therefore, that in rifling the present cast-iron guns, it should be done in such a way as to admit of round shot being fired, without damaging the grooving, and yet with accuracy. There seemed to be a general feeling against the

use of cast-iron guns, from their fancied insecurity. It had been said, that cast-iron guns were almost as brittle as glass, and that they would not safely stand firing. He was astonished to hear such statements, for after twenty years' experience, as a gunnery officer, he had never seen an accident from the bursting of a gun. He believed that in those instances in which guns had burst, they had done so either through extremely careless loading, or else from being exposed to the air for years, and becoming honey-combed by oxidation. He did not agree with the statement, that a gun required a long period of rest to gain strength. He thought a few months' rest, after casting, might be necessary for the particles to set; but if a gun was left longer exposed to oxidation, it became weaker, from the moment oxidation commenced.

With regard to the mode in which new guns should be cast, he considered there was a great advantage in casting them with a core, because then the inside skin of the gun would be strong and hard, and the metal generally, from its decreased thickness, would be more homogeneous. All that was necessary to convert the present gun boxes, for casting with a core, was to plane their tops, and to have the core spindle made to pass through a properly-fitted cover, so as to expand in guides when heated by the hot metal. With guns thus cast, the wearing of the bore, by the elongated ball, would be very small. Even with the ordinary 32-pounder gun he had tried at Shoeburyness, which was cast solid, and therefore, had a comparatively soft bore, he could scarcely detect a scratch upon the rifling, after firing his cast-iron elongated shot. He was astonished to find that the friction, and consequently, the strain due to the rifling, was so little. With respect to the mode of casting iron guns, he believed another great improvement might be effected. At present, from being cast solid, they were made with a degree of hardness which was injurious to tenacity, in order that the centre of the gun might not be worn away by the rubbing of the shot. Some of the guns cast at Woolwich, which were known to be too hard, were not tested in the first instance. On being recently proved, they burst as was expected; but those which had been made subsequently, stood the highest proof-charge perfectly. Nothing definite could be deduced from the bursting of a few cast-iron guns, under such circumstances. The factory at Woolwich was in its infancy, when the hard guns were cast, and only about two hundred guns had been made before the establishment was broken up. All new factories were liable to make similar mistakes. The failure in this case was owing, in part, to the iron used, which was of inferior quality and of extreme hardness; but when better iron was substituted, good guns were made. After guns were cast, they were frequently subjected to a great deal of rough treatment. The longer a gun was allowed to cool, in the position in which it was cast,

the better ; and if it were cast with a core, toughness of the mass might be attained, with sufficient hardness for the rifle grooves. The bore, after casting, should be carefully made straight, and be smoothed, without taking away much of the skin. As to the outer part, the gun, when it came from the foundry, was at present turned down ; so that the skin, or that part of the gun which would perform all that the wrought-iron bands were expected to accomplish, —keep the iron in its natural and perfect state of compression,—was taken off. The gun was thus not only much weakened, but was also rendered very liable to rust, which, after long exposure, destroyed the tenacity of the metal. Not content, however, with merely removing the skin from the gun, it was taken off unequally, leaving rings and patches, and these inequalities checked the proper vibration and expansion of the particles of the metal on discharge. The whole force of the explosion, and the rapid rubbing of the shot, caused a strong vibration through the gun. This vibration was greatest after the ball had moved, and before it had acquired sufficient velocity to compensate for the augmented volume and increased compression of the gunpowder gases. In the ordinary service gun there was a projection round the muzzle, and another round the breech, while at the trunnions there was a great patch, which checked the expansion and vibration.

Another disadvantage of the projection at the muzzle was, that on board ship, it caused the loss of 1° in elevation and 1° in depression, as well as 1° in training right and left. Now that long ranges were demanded, to cut off 1° of elevation and depression, for the sake of retaining the useless muzzle swell, did seem absurd. He mentioned these matters merely to show, that this question had not yet been thoroughly examined. No Select Committee had sufficient time to go into the details of the wide and important subject of the improvement of cast-iron ordnance ; there was such a pressure of other business upon their hands, that it appeared to be impossible.

Captain BOXER, R.A., said, the principal subject dealt with, in the Paper, referred to the construction of artillery, in allusion to which, the Author said, this was a question, with which, in his opinion, no one was more able to deal, than the Civil Engineer of the present day. Captain Boxer could not concur in that opinion. Although one particular Civil Engineer might be better fitted to deal effectually with a subject of this sort, than one particular Officer of the Royal Artillery, that was a personal question ; but it did not affect the general proposition. It must be borne in mind, that it was not only the business and the duty of the artillery officer to make himself thoroughly acquainted with the properties of various materials, including metals, woods, and other substances ; but he was also bound to investigate the subject, with

reference to the peculiar action and effect of fired gunpowder, under various conditions, upon those materials. He thought it would be admitted, that those specialties could not be considered as an essential part of the professional acquirements of the Civil Engineer.

The Author had not been guilty of flattery in speaking of the Ordnance Select Committee. Having been a member of that Committee at the time this invention was referred to it, he might state, in answer to the remarks which had been made, that if it was merely intended to convey, that the proposal was not accepted, or rather was not recommended for adoption, then the Author was quite correct; because out of a hundred plans which were laid before the Committee, possibly not one was recommended for adoption. But if it was desired that the inference should be drawn, that the subject which was brought before the Government, and referred to the Select Committee, was not properly dealt with, or duly considered, but was rejected without due investigation, that was a matter of opinion, which was open to fair argument. Captain Boxer did not, however, think any good would result from making insinuations against the Committee; nor did he think it right to make remarks of this sort, when the Committee had no opportunity of meeting the objections.

The Author stated he had ascertained, from his own experiments, that the strength of gunpowder was equivalent to 17 tons per square inch. Not being aware of the method adopted for determining this pressure, Captain Boxer could not say whether there was any error in the manner of making the experiments, or in the deductions from the trials. But he might observe, that the subject of the explosive force of gunpowder had engaged the attention of the most eminent philosophers; and notwithstanding this,—notwithstanding the most careful calculations, involving the highest order of mathematics, made by those practically acquainted with the subject, no satisfactory results had yet been obtained. In considering the force of gunpowder, there were two main points to be regarded. First, there was a certain definite pressure, which might be termed the statical force, or the force which was effectual in giving motion to the projectile in the bore of the gun. Secondly, there was a percussive force, in addition to that already mentioned, and of a different nature from it, which principally tended to cause destruction to the material of the gun. With regard to the first,—the initial force, or pressure of gas,—although the average pressure which was exerted upon the bore of the gun, during the passage of the shot through the bore, might be determined, by finding out the initial velocity of a shot of a certain weight, when projected from a gun of a known length; yet when it was attempted to determine the initial force, or pressure exerted by the gases

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when first generated, and when filling that portion of the bore originally occupied by the charge of powder, the question would be found to be involved in the greatest possible difficulty. He had never been able to satisfy himself, that any right solution had been arrived at; and he thought the wide difference of opinion, which existed upon the subject, confirmed the view he had expressed. With reference to the second point, namely, the percussive force of the powder which was exerted, principally tending to cause rupture, over and above the statical pressure, and which did little towards giving motion to the shot, he would observe that, upon the explosion of gunpowder, there was, no doubt, a force exerted similar in character to that of percussion, or an effect was produced like that caused by a body in motion coming into contact with a body at rest. This force, which was generated at the time of the explosion, could not be measured by, or be compared with, any statical pressure, and he believed it was owing to this force not being taken into account, or to erroneous ideas of its nature, that so much discrepancy of opinion existed.

It appeared to be the fashion, at present, to condemn cast iron as a material for guns. He thought it was premature to do so. First, it must be remembered, that cast iron had done good service, and had, for many years, been found a valuable material for guns. It should not, then, be condemned without careful investigation. No doubt, since the introduction of the system of rifling, and of large calibre for firing solid shot, the strain upon the gun had been much increased; that was to say, the strain in those guns was greater than in ordnance of smaller calibre formerly used. It was clear then, that if cast iron was employed, a stronger quality of metal was required, than was before found to answer. He was of opinion, however, that by careful investigation, a material might be obtained, and a mode of treatment of the metal might be discovered, which would enable cast iron to be used, with advantage, for rifled guns constructed upon true principles. Cast-iron guns had been made, possessing an enormous amount of enduring power; but it was essential to insure uniformity in this respect. He believed that by close investigation, a mixture of cast iron might be obtained, possessing all the necessary properties of a good gun metal. He thought, for these reasons, that cast iron ought not to be altogether condemned, and he was of opinion, that at some future time, cast iron would again be brought into use for artillery. At the same time it was not clear, that cast steel, or some preparation of cast iron, might not answer the purpose better than the systems of hooping, or of covering with wire, or even of building up, as in the manufacture of Sir William Armstrong's guns.

Mr. LONGRIDGE remarked, that he had not said anything dero-

gatory to the Ordnance Select Committee. He had merely stated facts, and if they conveyed the impression just intimated, he regretted it, but he was not answerable for it.

Captain BLAKELY, R.A., remarked, that as the name of Lieut. Rodman had been mentioned, it might interest the Meeting to know, that a gun had been lately cast on a hollow core, with perfect success, by that distinguished officer, although the weight of the gun was 39 tons. He might add, that Lieut. Rodman's plan was another application of the principle brought forward by the Author of the Paper, and had first been proposed in the United States, in 1851.

Sir CHARLES FOX said, some years ago he expressed the opinion, that it was important to arrive at that condition of things in a gun, which should, when an explosion took place, make available the whole thickness of the gun. This, he believed, had never, hitherto, been effected. At one time, he thought he had discovered a mode of doing it; but at the present moment, he confessed he saw great difficulties in arriving at such a result. If a gun was built in separate parts, without being cemented together by soldering, or welding, he thought it would at some time, and that not a distant one, prove to be of no use. Although guns might be made by using a tube for the internal portion, turning the outside to an accurate cone, and then, by hydraulic power, pressing on it, with proper degrees of force, various layers of hollow cylinders, so as to insure every part having its proper initial strain, still he thought a very little use of that gun would so change the condition of its parts, as to destroy all the advantages of so expensive a mode of construction. The difficulty of making a gun, by encircling it with wire, especially in forming the breech, was so well known, that it was unnecessary for him to refer to it. He believed it would, eventually, be found, that the best gun could be constructed with some extremely dense and homogeneous alloy, cast and used without being drawn under the hammer. If a gun was made of an alloy possessing very great density, the detonating force of the powder would be resisted by a greater quantity of the metal employed, than it could be by making use of one with greater elasticity. He thought, therefore, the best guns would be made of iron mixed with some other metals, such as wolfram and titanium, so as to insure the greatest strength and density. Mr. Mushet had obtained great density, by mixing with iron a small percentage of wolfram, and great strength by the use of titanium. Therefore, he was inclined to believe, that guns cast of the densest alloys would have greater effect, in proportion to their thickness, than could be obtained by any complicated and expensive mode of construction.

Mr. F. A. ABEL said, the discussion seemed to have regard chiefly to mechanical details,—to the manufacture of the metal and its conversion into the form most suitable to withstand repeated heavy strains. As the Chemist to the War Department, any observations he might make would refer, more particularly, to the comparative chemical qualities of the materials for cannon. He had heard the remarks which had been made, both by Engineers and by Artillery Officers, with reference to the best system of constructing guns, and the materials of which they should be manufactured. He agreed, to some extent, both with the advocates, and with the opponents of cast iron. He did not think, however, that cast iron could be considered as altogether untrustworthy, as a material for rifled cannon. He entertained this opinion, because the chemical examination of a large number of samples of cast iron, from different sources, either as obtained from the blast furnace, or after repeated re-meltings, had led him to the conclusion, that the uniformity of this material was, to a great extent, under control. He had examined specimens obtained from some of the best iron-works, and on comparing with them samples made, at intervals of two, or three years, at the same works, he found them, from a chemical point of view, almost identical in their nature. There might be a variation in the density, and other physical properties, resulting from the temperature at which the metal was cast, and from other circumstances, but the regulation of such differences was under the control of founders and engineers. If, therefore, it was found that cast iron might, with proper attention to its manufacture, be made almost perfectly uniform, some faith ought to be placed in that material. At the same time, the important results obtained by the further treatment of cast iron, should not be lost sight of. By progressive decarbonisation, it might be made to approach to perfect steel in its nature, or to acquire the characteristics of malleable iron. Such conversions could, a few years ago, only be carried out upon a small scale, or by most laborious processes; now they could be effected upon a very large scale, so that masses of the products, of great size, could be produced. Amongst others, Mr. Bessemer had obtained results which should not be passed over. He thought they might prove most important, particularly when it was remembered what had already been done in this direction, by Mr. Krüpp, in Prussia. A gun of cast steel manufactured at his works at Essen, had been tested in this country, and it was found almost impossible to burst it. That gun was cast in one mass, and the system of manufacture had been pursued in Prussia with such success, that rifled field-guns were now being constructed in that country, exclusively of this material, with every expectation that they would prove equal to the guns made in France and in England. He thought the results already obtained with those

materials should not be neglected ; but that the proper authorities should embrace, as far as possible, the opportunity afforded to try fairly what guns, of that material, would do, as compared with guns of cast iron and with built guns of wrought iron. It appeared to him, that if a material suitable for the construction of a light and durable gun could be obtained in one mass, a vast number of possible sources of failure would be avoided. Nor would reliance have to be placed upon a number of details, which, although determined on the most correct theoretical principles, could only be perfectly carried into practice with great difficulty. It was questionable, in his opinion, how far the various suggestions relative to the construction of built guns should be entertained, until the question of the possible construction of durable rifle guns, in one mass, had been fully and fairly gone into. He anticipated that materials such as cast steel, or homogeneous iron, cast in large masses, must, independently of cost, furnish better results than other materials which had been proposed, on account of their greater uniformity.

He stated, in reply to an inquiry, that he had been induced to make some experiments upon the combinations of phosphorus with copper ; and had found that, by the introduction of a small proportion of that substance, say from 2 to 4 per cent. of phosphorus into copper, a metal was produced, remarkable for its density and tenacity, and superior in every respect to ordinary gun metal, (the alloy of copper and tin known by that name). He believed the average strain borne by gun metal might be represented by 31,000 lbs. upon the square inch ; whilst the material obtained by adding phosphorus to copper, bore a strain of from 48,000 lbs. to 50,000 lbs. But the increased tenacity was not the only beneficial result obtained by this treatment of copper. The material was uniform throughout, which was scarcely ever the case with gun metal. The experiments alluded to were merely preliminary, and had been, to a certain extent checked, by the improvements since introduced in the construction of field guns, which had led to a discontinuance of the employment of gun metal.

Mr. E. A. COWPER explained the construction of the Swedish percussion shell, which had not yet been referred to. It was an ordinary round shell, and in the interior there was a small percussion ball, having tubes to be furnished with caps. The end of one of the tubes, which pointed towards the breech, had no cap on it ; so that the blow, or shock, caused by the powder when fired, did not explode the shell. But as soon as the shell came in contact with any object, the caps caused the shell to explode by the percussive action. That was a good thing in itself, but when rifled guns were used, it was sufficient to put one cap at the end of the projectile. An elastic ring, or wad, well tallowed, was fitted behind the shell, so that when the blast of powder took place, the ring was

forced between the shell and the gun, thus saving windage, and insuring greater accuracy in firing. By this system the charge of powder could be reduced, and the rattling, or rebounding of the shell from side to side of the gun, was prevented.

Some of the more marked distinctions, which had been pointed out, between the different plans of guns, alluded to in the Paper, or in the discussion, might be thus tabulated :—

Designation.	GUN.		PROJECTILE.		WINDAGE.		Strain due to Projectile.	Service converted.
	For.	Against.	For.	Against.	For.	Against.		
Service . .	Cheap ; strong.	Aim bad .	Simple ; cheap.	Too light	..	Bad . .	Small .	..
Enfield . .	Ditto .	Aim better	Ditto .	Ditto .	Very little	..	Ditto .	..
	Strong ; good aim.	..	Simple ; heavy.	..	Ditto .	..	Ditto .	..
Lancaster .	Good aim ; moderate cost.	Weaker form.	Simple ; wrought iron.	Costly ; wrought iron.	Little, if with good wad.	..	Great .	Yes.
Armstrong.	Good aim ; strong.	Costly ; breech loader.	Heavy .	Costly ; lead and iron.	Little .	Cannot load at muzzle.	Ditto .	..
Whitworth	Good aim	Costly ; weak form.	Ditto .	Very costly fitted up.	Little .	Cost of fitting.	Ditto .	..
Britten . .	Ditto ; cheap.	..	Ditto .	Compound lead and iron.	Little .	Cost of lead.	Moderate	Yes.
Hadden . .	Ditto, ditto	..	Heavy ; cheap.	..	Very little good wad.	..	Ditto .	Ditto.
Ditto, improved bore.	Ditto, ditto, strong form.	..	Ditto, ditto	..	Ditto, ditto	..	Small .	Ditto.

The ordinary service gun was of a strong and cheap form ; but the aim was inaccurate. The projectile was cheap and simple in construction, but was too light, and there was too much windage. The strain upon the gun, due to the projectile, was small, but a heavy charge of powder was required, to make up for the loss by windage. There was no tendency to twist, or to burst open the gun, from the projectile acting as a cam. The Swedish gun was strong and cheap, the aim was better, the projectile was also cheap and simple, but was too light. There was little windage, and it was a great improvement upon the common gun. The Enfield gun, (small arm,) was strong, the aim was good, the projectile was long, and the form was simple. There was scarcely any windage, and but little strain due to the projectile. The Lancaster gun possessed good aim, and was constructed at a moderate cost. The form of the gun was simple, but it was weakened by the oval bore. The projectile was simple, being composed of only one material,—wrought iron,—but it was costly. There was but little windage, if a good wad was used. The projectile, had, however, a tendency to burst the gun. One advantage of the plan was, that the ordinary service gun could be converted into the Lancaster rifled gun. In the Armstrong gun the

aim was good, the projectile was heavy, but costly, on account of the fitting, and of its being composed of lead, antimony, and iron. The windage was small, from the projecting rings of lead and antimony on the projectile being larger than the bore of the gun; the projectile being introduced at the breech. The gun was very costly. In the Whitworth gun, the aim was good, the projectile was heavy, but it was costly in fitting up. In some cases the projectiles fitted the bore so closely, that upon putting a finger upon the touch hole, and pushing the projectile down the gun, it would spring back again on being released. The gun was very costly, and of rather a weak form. The material of which it was made was very expensive, but was admirably adapted for the purpose, being, in fact, a kind of mild steel. Mr. Britten's gun was of simple form, the aim was good, the projectile was heavy, and so far good; but being compounded of lead and iron, if a number were packed together, they would be liable to injure each other. The windage was small, from the lead expanding with the blast of the powder and filling the bore. The cost of soldering-on, or belting the projectile with lead, was not very great. Mr. Haddan's gun was cheap, the projectiles were a little cheaper than common shells, and were heavy,—weighing 92 lbs. for a 68-pounder gun. The windage was small, owing to the expanding wad, which filled the bore of the gun, and at the same time, kept the axis of the projectile in the centre of the bore, and quite steady during its passage out. The ordinary service guns were capable of being converted to this plan, for about twenty-five shillings each, and it could be done on the deck of a ship, as in the case of the Britten gun. The charge of powder was reduced from 16 lbs., the ordinary service charge, to 10 lbs., and the range was increased to 3,000 yards with excellent aim. In the improved form of bore, the tendency of the projectile to burst the gun was very small, and the form itself was strong, being nearly a circle.

With regard to the wire-bound gun, it remained to be shown, how the breech could be made of the same strength as the barrel, which could, undoubtedly, be made in that way. The strain upon the breech, at the time of explosion, was as much as upon the projectile and the sides of the gun. As to iron guns, cast, or forged in a solid mass, of mild steel, or of steely iron, he would propose to forge them hollow, so as to admit of their being cooled from the inside. He would use a stream of water, air, steam, or water and air, or water and steam, in the form of rain, and at the same time, thoroughly compress the outside upon the inside, as it cooled, by a heavy steam hammer; so that when the gun was quite cooled, the inside would be in considerable compression, and the outside in decided tension. The different circles, or rings of metal, between the outside and the inside, would then be in a

regularly graduated series. He believed that in this manner, the idea of various strains throughout the homogeneous mass, might be perfectly carried out; and he might say, more scientifically, than by any system of hoops, or wire.

He thought the question of attaining long ranges depended far more upon the strength of the metal of which the gun was composed, than upon the section of the rifle cuts, the speed of the twist, or the plan of breech loading. It was, no doubt, imperative, that the projectile should pass along the gun steadily, with some twist, and should have very little windage, in order that it might receive the full effect of the quantity of powder used. It was very much a question of the charge of powder and of the strength of the gun; as it was evident, that a charge of one-fifth the weight of the projectile, would send it further than a charge of one-ninth, or one-eighth.

Mr. W. STRODE said, he noticed that in the Table, (page 374,) the Enfield rifle was stated to be strong and of good aim. Now he believed, that on the contrary, the metal of the barrel was so weak, as to render it liable to injury from external causes, such as the fixing, or the taking off the bayonet. The projectile being too small, the aim was not good, and he thought the bullets often did not take the rifling at all. He had in his possession one of the regular service bullets, obtained from Aldershot, which was quite loose in the Enfield barrels.

Captain JERVIS, R.A., said, he was at a loss to understand the remarks just made. There was no such thing as an Enfield ball. The diameter of the service bullet had been slightly reduced, on account of the fouling caused by the powder, but the accuracy of the shooting had not been affected by that reduction.

Mr. LANCASTER thought the discussion upon this subject, elaborate as it had been, ought not to conclude, without some notice of what had been done by experimenters in other countries. Professor Daniel Treadwell, of the United States, had, for many years, directed close attention to this question. The results were given in a Paper, read before the American Academy,¹ remarkable for the acumen, learning, and deep research it displayed. The principle therein advocated, was that of using a cast-iron matrix, forming a screw on its outer circumference from end to end, and then applying wrought-iron rings, put on hot. On the outer circle of these rings, another screw was turned, when a second series of rings was added. Professor Treadwell obtained

¹ *Vide* "On the Practicability of constructing Cannon of Great Calibre, capable of enduring long-continued use under Full Charges." By Daniel Treadwell. From the Memoirs of the American Academy. (Mr. Lancaster presented a copy of this pamphlet to the Institution.)

the following remarkable results. He said, "My observations upon the lodgement have been made upon wrought-iron cannon. Between the years 1841 and 1845, I made upwards of twenty cannons of this material. They were all made up of rings, or short hollow cylinders, welded together endwise. Each ring was made of bars wound upon an arbor spirally, like winding a ribbon upon a block, and, being welded and shaped in dies, were joined endwise, when in the furnace and at a welding heat, and afterwards pressed together in a mould, by a hydrostatic press of 1,000 tons force. Finding in the early stage of the manufacture, that the softness of the wrought iron was a serious defect, I formed those made afterwards, with a lining of steel, the wrought-iron bars being wound upon a previously formed steel ring. Eight of these guns were 6-pounders of the common United States bronze pattern, and eleven were 32-pounders of about 80 inches length of bore, and 1,800 pounds weight. Six of the 6-pounders, and four of the 32-pounders, were made for the United States. They have all been subjected to the most severe tests. One of the 6-pounders has borne 1,560 discharges, beginning with service charges and ending with ten charges of 6 lbs. of powder and seven shot, without essential injury. It required to destroy one of the 32-pounders, a succession of charges ending with 14 lbs. of powder and five shot, although the weight of the gun was but sixty times the weight of the proper shot. If any of these guns are ever destroyed by firing them, the destruction will commence in the lodgement.

* * * * *

"I cannot leave this subject, without observing, that I regard the late and still continued attempts to make wrought-iron cannon in Europe, by the process of fagoting, or piling, as a strange engineering delusion."

With respect to the Lancaster guns, it was true that three were burst at the muzzle in the Crimea; but it must be understood, that those which failed were the service guns, bored oval on his system, and not the guns specially made for the purpose. It was generally supposed, that the action of the shell, in passing out of the bore, burst the gun. The fact was simply, that the shells were originally made in two pieces; the base of the shell being welded to the upper portion. In practice, this weld was often imperfect. Hence, at the moment of the explosion of the service charge, the flame penetrated through the defective weld, to the charge within the shell. The charge, amounting to 12 lbs. of powder, was fully ignited, just at the moment the shell was in the act of leaving the muzzle of the gun. It could, therefore, only be a question of time, whether the shell burst within, or outside the gun. If the shell burst within the

muzzle of the gun, the destruction of the gun followed as a matter of course. Directly this failure in the manufacture of the shells was discovered, steps were taken to rectify it. Instead of being made of two pieces, the shells were now constructed of one piece of iron, and the muzzle of the gun had been strengthened, by the addition of a ring of wrought iron. Should, therefore, any of the old store of shells be used, by any chance, the gun would be strong enough to resist the contingency. It was found that these guns were now thoroughly equal to the requirements of the service.

Mr. C. W. SIEMENS said, many years ago he had some slight practical experience in the use of guns, and had watched, with great interest, the progress which had since been made in their construction. Addressing himself to the subject of the Paper, it had been objected, that a gun constructed upon the plan proposed by the Author, would not have sufficient longitudinal strength. It had occurred to him, that the longitudinal strength of the gun might be much increased, if instead of winding wire upon it, it was bound with corrugated bands of steel, put on spirally. He estimated, that two-thirds of the whole tensile strength of these bands would thus be made available for longitudinal strength. He proposed, that the core of the gun should be turned with spiral grooves, extending backward beyond the bore, and fitting the longitudinal ribs, or corrugation of the strips. The strips should be put on under varying tension, while the gun rotated in a bath of solder, in order to unite the several layers. He thought the core of the gun ought to be of equally hard and tough material, and he had no doubt, that the most serviceable gun would be one made of solid, but mild, cast steel, well solidified by hammering. Such guns were manufactured by Mr. Krüpp, of Essen. From a report made to the Prussian government by Colonel Orges, it appeared, that the German cast-steel gun had given the most satisfactory results, as regarded strength. A bar, 1 inch square, of this material, had borne a weight of 50 tons, whereas a bar of wrought iron of the same dimensions broke with 33 tons. Mr. Krüpp's gun bore five and a half times the internal pressure of an ordinary cast-iron gun of the same internal and external diameters, and three times the internal pressure which burst a bronze cylinder of the same dimensions. Mr. Krüpp was now making three hundred guns for the Prussian government. The weight of his 12-pounder breech-loading gun was 825 lbs. The cost of the forging was about £93, and that of the gun complete was £150. These figures would enable a comparison to be made with the cost of guns of other constructions. With regard to composite guns, he would suggest, that although they might possess greater strength against internal pressure, than a gun of homogeneous metal, yet such a gun would be more liable to injury, when hit by a hostile shot. A

composite gun would, he thought, suffer more from that cause, than a gun of homogeneous metal, which would only be indented by a shot; whereas the composite gun would probably be disabled.

Hitherto, comparatively few experiments had been made, to determine what the pressure upon the interior of a gun really was in firing, and also what was the resistance of the atmosphere to shot, at different velocities. In 1846, his Brother, Mr. Werner Siemens, suggested a plan to a commission appointed by the Prussian Government, the results of which had been published. He determined the velocity of the shot by making it pass insulated wires, in connection with a Leyden jar. The electrical discharge passing through the shot, caused a spark to go from a point, upon the polished surface of a steel cylinder revolving at high velocity, which it marked by a speck of burnt metal. The shot in striking other wires, at a given distance, would make another speck upon the polished steel cylinder, and the angular distance between those two points, would represent the time that was occupied by the ball in passing from the one place to the other. The results that had been obtained by this apparatus were, however, not quite satisfactory; and it had occurred to Mr. C. W. Siemens, that in order accurately to ascertain the forces acting in a gun, and also the resistance of the atmosphere to the passage of the projectile, an apparatus of a more simple nature might be constructed, which should record those facts, in the same way that the exact pressure of steam in a steam cylinder, at every portion of the stroke, was arrived at. His object was, in fact, to indicate the forces acting upon the projectile throughout its flight. For this purpose he proposed to employ a hollow shot with open ends, closed by strong doubly-dished steel plates, laid one upon another, with lead plates between. When the shot was fired, the gases of the powder would act upon the end diaphragm, the pressure upon which would, in fact, urge the shot along. It was important to reduce the inertia of the elastic medium to the lowest possible amount, in order that it might instantly obey a change of pressure. The motion of the centre of the diaphragm was imparted to a scribing point in contact with a disc, made to rotate, during the flight, with a given velocity. It appeared difficult, at first sight, to obtain a uniform velocity of this disc without clockwork, which was, evidently, inadmissible; but an arrangement had occurred to him, by which he expected to effect that purpose. He fixed upon the disc two small fuses, or rockets, acting in opposite directions. If both these rockets were made of equal power, it was evident, that no rotating motion would ensue; but the one being made equal to only about two-thirds of the other, the more powerful jet would accelerate the wheel, until it was balanced by the lesser jet, on account of the negative motion imparted to it. A moderate and remark-

ably uniform rotation might thus be produced, for the power of the larger jet would diminish, as the square of the diminished relative velocity between the escaping gases and the wheel; whereas the power of this counter jet would increase, as the square of the increased relative velocity between the gases and the wheel. A small retardation of the wheel, by friction, or otherwise, would, consequently, produce a great change in the relative power of the two jets. These fuses were lighted, the instant the shot was dropped into the gun. Cards of zinc plate were fixed to the sides of the rotating disc, covered first with a black and then with a white varnish, whereupon the scribing point would trace a very clear line. Whilst this wheel revolved, a circular line would be obtained, until the pressure upon the steel disc caused the scribing point to ascend, producing a spiral indication of the pressure at all intervals of time. The disc in the front of the projectile was much lighter, being intended to indicate the resisting pressure of the atmosphere, by a line upon the other side of the rotating wheel. The negative pressure of the atmosphere against the back of the projectile might, also, be recorded by a similar arrangement. The diaphragm behind, should, in that case, be made very slight, and be covered by a strong metallic plate, to resist the force of the gunpowder, which plate would separate from the projectile at the mouth of the gun. In the same way, the pressure upon any portion of the curvilinear front surface of the projectile might be indicated, by making, instead of one opening in the centre, several openings in a circle around it. In order to maintain atmospheric pressure inside the projectile, its sides were perforated by a number of small holes. The weight of the moving mechanism need not exceed 6 ounces, and considering its strength and simplicity of arrangement, he did not apprehend any force, less than that which would destroy the shell itself, would interfere with its proper action. The advantages that would be obtained by such a complete record of the forces acting upon projectiles, under different circumstances of charge, form and speed, would, he thought, be very great, not only with regard to the construction of ordnance, and to ballistic laws, but to science generally, in affording useful information regarding the nature of fluid resistance. The experiment could be tried with any gun, and at a small expense; and if the proper authorities should think his proposal worth the trial, he should most readily give his services in the matter.

Mr. CONYBEARE said, it appeared to him, that the first step in the present inquiry should be, to determine the maximum amount and the *modus operandi* of those forces tending to disruption, to which guns were subjected in service, and which it was required they should endure, without injury to their fabric. These forces were; first, the disruptive force of the powder, which was depen-

dent on its quality, on the amount of the charge, on the diameter of the cylinder in which it acted, and on the amount of windage; and secondly, the inertia and friction of the projectile, the inertia being a function of the weight and elongation of the bullet, and the friction being dependent on the angle of the twist, and on whether the projectile was made with projections to fit the grooves, or had to be forced into them by the explosion.

The total mechanical effect of powder, exploded in rigidly closed chambers, might be readily determined, by experiments on the minimum bursting charge of shells, varying in diameter, or in thickness of metal. For example, the 12-lbs. shrapnel shells used in the United States service, were 4·6 inches in external, and 3·6 inches in internal diameter. When one of these was split into two pieces, by internal pressure, the area of fracture was rather less than $6\frac{1}{2}$ square inches; and taking the tensile strength of the iron at 25,000 lbs. per square inch, it would show, that a force of about 72 tons must be exerted, at the moment of disruption. If the action of the powder was diffused, by its being commingled with the balls, the requisite bursting charge was 4 oz. In Captain Dahlgren's new arrangement, where the balls were imbedded in two hemispherical masses of sulphur, leaving a cylindrical cavity for the bursting charge, in the axis of the shell, even three-quarters of an ounce of powder sufficed to generate the requisite disruptive force of 72 tons. But no large gun could be manufactured, to endure a disruptive force so enormous, in proportion to its charge, as 72 tons for each three-quarters of an ounce of powder exploded. Nor could any gun be subjected to such a stress in service; for so large a ratio of pressure, to weight of powder, could only be obtained, in the case of explosion in a chamber absolutely and rigidly closed. The smallest vent would act as a safety valve, and would reduce the explosive force to a mere fraction of its amount, in a rigidly closed chamber. This was demonstrated by the experiment, originally tried at Woolwich, and described in Mr. Greener's treatise on "The Gun."¹ He said:—"screw into each end of the breech part of a gun-barrel a well-fitted plug, then having filled the barrel with powder, drill a communication and screw in a nipple, in one of the plugs, fire a cap on it, and the explosive fluid will stream out from the small orifice like steam from a pipe, till it is expended." Mr. Greener repeatedly tried the experiment, and had thus fired 2 oz. of the best powder, without any violent motion being communicated to the barrel. Now, if a charge of three-quarters of an ounce sufficed to exert a disruptive force of 72 tons, in the case of the shell, 2 oz. under similar

¹ *Vide* "The Gun; or, a Treatise on the various descriptions of Small Arms." By W. Greener. 8vo. Sunderland, 1835. Page 36.

circumstances, would exert a disruptive force of 4,313,000 lbs., or upwards of 141,000 lbs. per square inch on the area of pressure in Mr. Greener's experiment, taking the area of pressure in the interior of the breech at 3 square inches. It was known, from the United States experiments, that the breech end of the strongest musket barrel was permanently bulged, by a hydrostatic pressure of about 13,000 lbs. to the inch. Therefore, assuming the pressure in Mr. Greener's experiment to have been within three-thirteenths of the ultimate strength of the barrel, it would only be one-tenth of the pressure that would have been exerted, had not the vent used for firing the powder been left open. In gunnery practice, in addition to this vent being always open, one end of the explosion chamber was only closed by a movable projectile. It appeared to him, that the indications of the ballistic pendulum were fully adequate to the determination of the stress that guns were subjected to, in actual service. But if any more direct evidence was required, it would be afforded by the United States experiments, on the extreme proof of cannon and musket barrels under hydrostatic pressure. These were tried, to determine the relative power of iron to resist a direct tensile force, hydrostatic pressure, and the fire of gunpowder. They led to the conclusion, that for cast-iron ordnance, the hydrostatic proof could not be safely extended, without permanent injury to the gun, beyond from 3,000 lbs. to 4,000 lbs. to the square inch. This pressure, the experimenters would thus seem to have considered, as equivalent to the stress to which the guns were subjected, under the ordinary proof discharges of powder and ball.

Some high authorities were, at present, disposed to decry cast iron, as a material unfitted for the construction of ordnance of any description. But he thought such a conclusion was based on wholly insufficient grounds. Considering the enormous number of proof discharges that really good cast-iron guns would withstand, and the great improvement that had been effected in America in their material, in their form, and in the method of casting them, he was convinced, that the gun of the future, for naval purposes and for land batteries, would be of cast iron. In short, that cast iron, in the present improved method of preparing it, combined the requisite hardness and tenacity, with cheapness, more perfectly than any other material. Many of the improvements, to which he alluded, were recorded in the volume of United States Reports, a copy of which he begged to present to the library of the Institution.¹ In addition to these experiments, ever since 1841, by the

¹ *Vide* "Reports of Experiments on the Strength and other Properties of Metals for Cannon. With a Description of the Machines for testing Metals, and of the Classification of Cannon in Service." By the Officers of the Ordnance Department, U. S. Army. 4to. Philadelphia. 1856.

regulations of the U. S. Ordnance Department, an officer had been required to be in constant attendance at the foundries, while the cannon were being made, to examine and test the metal before it was used, as also in the first gun made, before another was cast from the metal. This system of inspection had resulted in increasing the average tensile strength of iron cannon, cast in the United States, from 23,638 lbs. to 37,774 lbs. per square inch, and its maximum tensile strength to 45,970 lbs., with a density of 7,304. This high average result was conclusive, as regarded the adaptation of cast iron, when properly treated, to the manufacture of ordnance; for it must be remembered, that the tensile strength of bronze gun-metal could scarcely be taken at more than 30,000 lbs. The second result of these experiments had been to show, that iron guns cast hollow, according to the plan of Lieut. Rodman, of the United States Artillery, had from eleven to twenty times the duration of guns of the same pattern cast solid, at the same time and from the same metal. Another improvement, which was due to Captain Dahlgren, consisted in the exact adaptation of the thickness of metal, in each segment of the length of the gun, to the amount of the disruptive force to which such particular segment was subjected, on the discharge of the gun. The mode by which these forces were measured might be thus described:—At rather more than one calibre in the rear of the muzzle, a transverse bore was drilled, from the exterior surface of the experimental gun into the chase. Into this transverse bore, a projectile of ascertained weight was exactly fitted. The gun was then loaded and discharged. As soon as the ball had passed, and the exploding gases had reached the transverse bore, its projectile was propelled against a ballistic pendulum, by which the force, with which it was discharged, was exactly measured. A series of similar transverse bores were then drilled, consecutively and at equal intervals, from the muzzle end to the chamber; each being, in succession, closed with a projectile of precisely the same weight as that of the first, and the force with which each successive projectile was discharged, being similarly measured and recorded. The relative force of these successive discharges was a measure of the relative force of the explosion, in each corresponding segment of the chase;¹ and consequently, of the relative thickness of metal required in each such segment. These experiments proved, that in guns of the common model, the weight of metal, between the trunnions and the muzzle, was greatly in excess. In the model deduced from these experi-

¹ In the United States Reports, an instrument is mentioned, invented by Lieutenant Rodman, and authorised by the Washington Board of Ordnance, for determining the successive increments of velocity communicated to the ball, in its passage through the bore. This instrument is stated to have satisfactorily answered the intended purpose.

ments, and which, under the designation of the Dahlgren gun, had superseded the old patterns in the United States service, the weight of metal, between the trunnions and the muzzle, was greatly reduced. The metal, thus saved, was placed about the breech, where strength was chiefly needed. The result of these improvements had been, to enable the United States to replace the 32-pounders, weighing 57 cwt., previously used in their vessels of war, by Dahlgren guns, which, though not materially differing from the old 32-pounders, either in weight, length, or range at equal elevations, had the great advantage of throwing an 8-inch ball, in place of a 32-lbs ball. Dahlgren guns, 12 inches in diameter, had withstood every proof, and were now adopted as pivot guns in the United States Navy.

Of the experiments recorded in the United States Reports, those on the extreme proof of cast-iron ordnance, under hydrostatic pressure, and of casting guns hollow, on chilled cores, were so instructive, as to the molecular arrangement of the particles, in heavy masses of cast iron, and so suggestive, in respect to the modes of treatment that were to be avoided, and of those that were to be adopted, in the construction of a cast-iron cylinder intended to resist internal pressure, that he would briefly describe them.

In the experiments tried to ascertain the relative capability of cast-iron ordnance, to resist a direct tensile force, the fire of gun-powder, and hydrostatic pressure, four guns were subjected to the extreme hydrostatic proof. With the first, an 18-pounder, when the pressure reached 7,000 lbs. to the inch, the water came through the pores of the metal, in several places, forming small mounds of white froth, which afterwards collected in drops, and fell off in small streams. When the pressure was extended to 9,000 lbs., the gun burst with unexpected violence, one fragment weighing 170 lbs. being projected 21 feet. The second gun was a 24-pounder, of a very heavy model. Proof bars cut from the trunnion exhibited a good quality of iron. The water began to exude at 9,000 lbs.; and with a pressure of 9,500 lbs., the water was projected in several rills through the metal, to such an extent as to reduce the pressure to 5,000 lbs. and thus to terminate the experiment. The third gun experimented on, was another 18-pounder. This burst, when the pressure reached 9,860 lbs. on the square inch. The fourth gun was a 6-pounder. This had endured, without injury, thirty-six proof fires of the usual ascending series; fourteen being of the maximum charge of 3 lbs. of powder and sixteen balls, and then two extra charges of 6 lbs. of powder and seven balls. The thin portion, near the muzzle, separated from the thicker portion by a groove turned in the metal, for the purpose of the proof, was burst with a pressure of 12,400 lbs., and the thicker portion of the chase was burst by a pressure of

20,000 lbs. on the square inch. The metal appeared impermeable, and no water traversed the pores, at any stage of the operation. A circumstance attending these trials, strikingly illustrated the porous character of the interior of guns cast solid, the impermeability attained by the rapid cooling of the surface of a casting, and the consequent advantage of casting guns hollow, and of cooling them from the inside. The experimenters reported, that hydrostatic proof could only be applied to guns which had been turned after casting, "as the hardened crust of chilled iron on their exterior surfaces, arising from the coldness of the mould, would prevent the penetration of the water, and thus afford no indication of the existence of imperfections in the body of the metal." Cases occurred during the experiments, where the force of water collected under this crust, through a fissure in the mass of the metal beneath, had raised it in the form of a spherical segment of an inch in projection above the surface of the gun; and not until this blister was broken through, by an increased pressure, was there a leak of any kind to indicate the flaw.

Surely the canon, in Hydraulic Engineering, "that every structure intended to resist the impact of water, should be impermeable by water," applied with tenfold force, to a fabric intended to resist the intense force of the gases set free by the ignition of powder, calculated as they were, by their extreme tension, to search out every fissure and to permeate every pore, in the material of the gun chase. Such repeated permeation of the pores of the metal, by incandescent gases acting injuriously on the iron, chemically as well as mechanically, and by their heat, must, necessarily, destroy the gun in a much less number of rounds, than it would endure, if its interior surface was impermeable. As the practicability and advantage of giving impermeability to the interior surface, by casting it hollow, on a chilled core, and of cooling the metal from the interior, instead of from the exterior, had been fully established, it appeared to be a very unscientific proceeding to continue to cast iron guns solid. The mode, hitherto adopted, of casting iron ordnance solid, and cooling them from the exterior was, moreover, by subjecting the interior of the metal to extension, and the exterior to compression, exactly the converse of the conditions of maximum efficiency. It was admitted, that the material, of which a gun was composed, should be so disposed, as to present a sound and impermeable surface to the ignited powder, and to have the metal, adjoining the interior surface of the tube, in a state of compression, and that adjoining the exterior, in a state of extension. These conditions were precisely the converse of those actually obtaining, in the case of a gun cast solid, and cooled from the exterior; but they were fully complied with, when the gun was cast hollow, and was cooled from the interior. No one would think of commencing the construction of a gun built up of a number of concentric layers

of wrought-iron rings, by heating the second ring, and placing it within the exterior ring already cooled and shrunk; and when the second ring had cooled, repeating the operation with a third red-hot ring. It was manifest, that a gun so formed, would have air spaces between each layer of rings, and would be wholly destitute of coherence and strength. The failure of such a gun could not, however, be fairly attributed to defects in its material. Yet this was precisely the mode of proceeding adopted, in the construction of cast-iron ordnance, cast solid and cooled from the exterior. A gun so cast and cooled might be considered as composed, or built up, of a series of thin concentric layers, or rings, formed by successive congelations, one within another; the exterior ring being congealed by contact with the cold surface of the mould; the second ring forming at the cooling point of the metal, upon and within its exterior predecessor, and so on. The exterior of the casting would thus have cooled down to a temperature at which contraction had ceased, while the interior remained at the melting point. The result would be, as stated by Mr. Mallet, that "in a casting of 2, or 3 feet, or more in diameter, it is not unusual, (with the founder's best care,) to find a central portion of from 6 to 8 or more inches in diameter, consisting of a spongy mass of scarcely coherent crystals of cast iron, usually in arborescent masses, made up of octohedral crystals; the whole so loose, that upon a newly-cut section, dark cavities can be seen by the naked eye in all directions, out of which often, single, or grouped crystals can be picked with the hand, and so soft, that a sharp-pointed chisel of steel may be easily driven into the mass some inches, as if into lead, or soft stone."¹ It was true, that a large portion of the defective core of spongy metal was usually removed, by the process of boring out; but still, soft spots were left, which were easily bruised and burnt away, by the shock and blaze of the powder. And even if free from such flagrant defects, a gun, so cast and cooled, must, inevitably, have the texture of its metal loosest, where it ought to be closest; and that portion of its material in extension, which ought to be in compression, and *vice versâ*. On the other hand, when a gun was cast hollow and was cooled from the interior, the concentric layers, or rings, formed by successive congelations, were each, so to speak, shrunk on, outside its congealed and contracted predecessor, just as in a built wrought-iron gun; the interior was thus put into a state of compression, and the exterior into that of extension, which were the conditions of maximum efficiency.

The United States experiments had demonstrated, both the practicability of casting guns hollow, on a chilled core, and also the immense superiority in durability, possessed by guns so cast, over

¹ Vide "On the Physical Conditions involved in the Construction of Artillery, &c." By R. Mallet. 4to. London. 1856. Pages 20 and 21.

those cast solid and cooled from the exterior. In these experiments, three pairs of guns were cast; two pairs of 8 inches, and one pair of 10 inches calibre. Each pair was cast from the same iron, melted in two furnaces, which discharged, by separate streams, into a common reservoir, whence it issued in a single stream, and dividing, filled both moulds simultaneously. One of each pair was cast solid, in the usual manner, and the other, according to the plan invented by Lieutenant Rodman, was cast hollow, by means of a core, formed on a tube of cast iron, through which a stream of water constantly circulated, while the iron was cooling. The core tube was water tight, and was closed at the lower end. The water was admitted through an interior tube, descending nearly to the bottom of the core tube, and the water then ascended through the annular space between the two tubes, and was discharged from the core, at a point above the casting, flowing off in a heated state. In the first 8-inch guns so cast, the flow of water through the core, was continued for forty hours. The core was then withdrawn, and the water was passed through the interior cavity formed by the core, for twenty hours more. The total quantity of water passed through, was 6,000 cubic feet. In the case of the second 8-inch gun, 10,000 cubic feet of water were passed through, in sixty-five hours; the core having been withdrawn after the first twenty-five hours. In the third case,—a 10-inch gun,—22,560 cubic feet were passed through in ninety-four hours, when an unsuccessful attempt was made to withdraw the core. In each case a fire was kindled at the bottom of the pit directly after casting, and was continued during sixty hours. The pit was covered, and the iron case, containing the gun mould, was kept at as high a temperature as it would safely bear, being nearly at a red heat all the time. Each pair of guns, thus cast simultaneously, and from the same metal, the one hollow, and the other solid, were simultaneously proved. The results, which were given in detail, proved, that the endurance of the gun which was cast hollow, greatly surpassed that which was cast solid. The difference, in one case, was more than twenty to one, in which case the gun which was cast hollow remained uninjured, after fifteen hundred discharges, and one thousand additional discharges had been, subsequently, recorded, whilst that which was cast solid, burst at the seventy-third round. In the aggregate of all the cases, the difference was as eleven to one, in favour of guns which were cast hollow. From the precautions taken to insure identity of material in both guns, of each pair, and for preserving an exact uniformity in the proof of each, the great difference of endurance must be ascribed solely to the different methods of cooling the castings.

Now considering the proofs recorded to have been borne with impunity, by certain old-fashioned iron guns, notwithstanding the disadvantage they laboured under, in having been cast solid, which

guns had been fired repeatedly, with a charge of powder equal to the weight of the ball and with seven balls, or with a charge half the weight of the ball and sixteen balls, it might reasonably be expected, that when cast hollow, and with the additional advantage of the diminished calibre and charge of the modern rifled cannon, the same material, if judiciously treated, would be found adequate to any strain to which it could possibly be subjected in service.

The method of casting hollow, on a chilled core, might be obviously applied, with increased facility, to a breech-loading cannon; for the continuity of the bore would allow of the more exact centering of the core. Indeed the core might be so accurately fitted to the calibre, as to permit the gun to be bored out and rifled, without penetrating the crust of chilled metal formed on the core. Such a gun, by presenting an absolutely impermeable surface to the exploding gases, would prove almost indestructible. It would further conduce to strength and durability, and to preserve the hardened crust formed on the exterior surface by the contact with the mould. This was done with the iron guns cast at the Belgian Government foundry, at Liège, where instead of turning off the crust, as was the practice with guns manufactured in this country, the guns were only bored and finished, without removing any part of the outer skin. He believed the cast-iron gun of the future would be cast hollow, on a chilled core, and that the exterior crust would be preserved. The exterior should be plain, and devoid of the ornamental astragals, reinforces and sudden changes of thickness, which, owing to the re-entering angles they produced, and the planes of weakness they created, by their effect on the interior crystallisation of the iron, so frequently originated and were coincident with, the lines of fracture. The thickness of metal in the chamber, and thence to the muzzle, should likewise be proportioned by the Dahlgren formula. He thought it should be a breech-loader; and he believed the Whitworth arrangement would, probably, be found better adapted to cast-iron guns, than the Armstrong system, in which the cylinder of the gun was weakened by the slot required for the moveable breech. The rifling should be free from re-entering angles, which were a source of weakness in cast iron, and it should be such as to weaken the cylinder, in the least

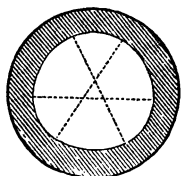


Fig. 27.

possible degree. The form that would best answer these conditions, and which was shown in Fig. 27, would be one that would bear the same relation to the three-grooved rifle, that the Lancaster oval did to the two-grooved rifle. Hexagonal rifling would be inapplicable to cast-iron guns, on account of the re-entering angles it would present to the exploding gases. Projectiles exactly fitted to the form of the rifling, now suggested, might be cheaply manufactured by self-acting shaping machinery.

The present tendency of naval armament appeared to be, to place heavy guns, intended to fire at extreme ranges, and consequently, with high trajectories, on board comparatively small and unsteady craft. Under such conditions, the most practised marksman would seldom succeed in hitting a distant object, unless he had the means of instantaneously discharging the piece, at the exact moment, in the roll of the vessel, when the object was covered. To secure this result, it was desirable that some arrangement should be devised, by which the marksman, looking through a telescope exactly parallel with the line of aim, might discharge the piece at the lucky instant, as if he fired from the shoulder, and with a hair trigger. For coast defences, it would be advisable to establish a telegraph communication with two posts of observation, two, or three miles distant to the right and left of the battery. From these observing stations the exact bearing, or angle subtended by the direction of the battery, and that of an enemy's vessel in sight, might be instantaneously and coteremporaneously telegraphed to the battery, where, by means of the intersection of two rules, on a graduated plane table, the exact distance of the vessel from the battery might be ascertained at any moment, without any process of calculation.

In recording experiments on various descriptions of rifled arms, it appeared desirable to adopt a common standard of ratios, in registering the elements of each, as by this means, alone, could a fair comparison be instituted between them. These elements were four in number, of which the first three were the more important. First, the ratio of the weight of the charge to the weight of the projectile; which ratio, supposing the bore of sufficient length to burn the powder, determined the 'initial velocity.' Secondly, the ratio of the weight of the projectile in pounds, to each square inch in the area of its cross section; on which ratio depended the loss of velocity of the projectile during its flight. Thirdly, the ratio of the twist to the calibre, as one turn in so many calibres; on which depended the accuracy of flight and the amount of elongation that might be given to the projectile. And fourthly, the length of the chase, or bore in calibres; as unless this was sufficiently long, the powder would not produce its full effect. This last element would be affected by the description of powder used, whether fine, or coarse-grained, and on the ratio of the space occupied by the powder to the capacity of the chase. The ratio of the charge of powder to the weight of the projectile, ranged from one-tenth to one-fourth; and the ratio of length of bore to the calibre, from 25 to 1, to 88 to 1. As regarded the rate of twist, measured in terms of the calibre, according to Major Croquillet, of the Belgian artillery, the turn of the grooves should be to each other, in all rifled arms, as their calibres, provided the projectiles were similar. Colonel Theroux of the French artillery,

gave a similar formula, for determining the proper twist of grooves, for firing elongated balls. H (the helix, or twist) = $56.8 D$; D being the diameter, or calibre. This rate of twist was also found to be the best, in General Jacob's experiments, and was adopted in his pattern rifle. The ratio of twist to calibre, ranged from 1 turn in 20 calibres, to 1 turn in 136 calibres. In the case of each particular gun, or rifle, all four ratios,—weight of powder to that of projectile,—weight of projectile to cross section,—rate of twist,—and length of bore,—must be considered together, and in connection with each other. For there were different means for effecting the same ends; and in many cases, a deficiency in one of the four ratios might be made up by an excess in another. Thus, in the case of the twist, its object was to give a certain amount of rotation to the ball, as it left the muzzle. This requisite amount of rotation might be obtained, either by means of a rapid twist combined with a low initial velocity, or by a slower twist, combined with a high initial velocity. The first mode was adopted in the rifle of the Sardinian Bersaglieri, where, with a projectile 0.65 in diameter, not a diameter and a half in length, and weighing 530 grains, the charge was only 54 grains, or little more than one-tenth. The initial velocity was, consequently, exceptionally low, and a very rapid twist was required, to establish the necessary rotation in the balls. The twist was, accordingly, 1 turn in 17 inches, or in 26 calibres. The second method was adopted in the Swiss Federation rifle. In this, instead of one-tenth, the charge of powder was one-fourth of the weight of the ball. This produced so high an initial velocity, that 1 turn in 77 calibres sufficed to establish the requisite amount of rotation in the ball. Each of these two methods would secure equal accuracy, in firing at a mark, at a known distance; but the Sardinian rifle ball would have a much higher trajectory, and much less penetration, than the Swiss. Rapid rotation could not be combined, beyond a certain extent, with a high initial velocity, unless the projectile was made with projections to fit the grooves. Without such projections, at high initial velocities, the ball would 'strip,' or be driven out without taking the rifling. Hence those experimenters who, like General Jacob and Mr. Whitworth, had obtained the greatest results, by discerning clearly the advantage of combining the accuracy of rapid rotation, with high initial velocity, and its consequence, a flat trajectory and great penetration, had adopted projectiles made with projections to fit the grooves; and he believed, such projectiles were destined to supersede those which were forced into the guns by the explosion of the powder. There was one disadvantage attendant on an increase of twist. It increased the 'drift,' or tendency of the ball to carry to the right of the line of aim, when the turn of the rifling was from left to right, and to carry to the left when the rifling was in

the contrary direction. Thus, though 1 turn in 57 calibres was considered the most advantageous in other respects, the French Commission adopted a twist of 1 turn in 135 calibres, to reduce this drift. The drift might be diminished by grooving the projectile transversely. It was found that the drift of cylindro-conic balls, like the Enfield, without grooves, fired from rifles, but with the same twist as the Enfield, was 10 feet in 872 yards; with grooves, the drift was inappreciable, at the same distance. It was probable, that the projecting angles of the Whitworth hexagonal projectile answered the purpose, as well as grooves.

It was also found, in the experiments tried by the French Commission, that when the centre of gravity of an elongated projectile was near the front, the point of such projectile drooped below the trajectory, in its flight; that when the centre of gravity was near the rear, the tail drooped; but that when the centre of gravity was in the centre of the length of the projectile, the axis of such projectile remained coincident with the line of trajectory, throughout its flight. It was obvious, that the resistance of the air would be at a minimum, in the last case, and this explained the improvement that was effected in the range of the Whitworth projectiles, by tapering them in the rear as well as in the front. By increasing the twist, it became practicable to increase the elongation of the projectile to the extent of seven diameters, if such a projectile was annularly grooved. But the elongation of the projectile was limited by other considerations; and it was now established, that from two and a half to three diameters would be the utmost amount of elongation adopted, save in exceptional cases.

The loss of velocity in the projectile, from atmospheric resistance, during each second of its flight, depended on the ratio of its weight, to the area of its cross section. This ratio might be conveniently expressed in terms of the weight of the projectile, in pounds for each square inch of the area of its cross section. For example, in the Armstrong 3-inch gun, the area of the cross section of the bore, or projectile, would be about 7 square inches; and the weight of the projectile being 12 lbs., the ratio of weight to atmospheric resistance would be $\frac{12 \text{ lbs.}}{7 \text{ square inches.}} = 1.71 \text{ lb. per square inch.}$ In the case of a rifle ball $\frac{1}{2}$ inch in diameter and weighing 1 oz., the area of its cross section would be about 0.2 square inch, and its weight, 0.0625 lb. The ratio of weight to resistance would, consequently, be 0.313 lb. per square inch. The appended Table, (page 392,) gave the elements of twelve of the most approved rifled arms.

He had already observed, that equal rotation, and consequently, equal accuracy might be obtained, either by combining a small charge in proportion to the weight of the ball, that was to say, a low initial velocity with a very rapid twist, or by combining a

NAME OF GUN.	DIMENSIONS AND QUANTITIES.							RATIOS.					
	1	2	3	4	5	6	7	8	9	10	11	12	13
	Mode of Rifling. Number of Grooves.	Twist, one Turn, in Inches.	Length of Bore, in Inches.	Calibre, in Inches.	Area of Calibre, in Square Inches.	Length of Projectile, in Inches.	Weight of Projectile, in lbs.	Charge of Powder, lbs.	Weight of Projectile in terms of Weight of Powder. $\frac{\text{Projectile}}{\text{Powder}} = \text{ratio}$.	Weight of Ball to resistance, in lbs. per Square Inch.	Turn, in Calibre.	Length of Bore, in Calibre.	Length of Projectile, in Calibre.
RIFLED CANNON.													
Whitworth's 80-pounder Gun	Hexagon	120	118	5	16.125	..	80	10	0.125	5	24	25	..
Whitworth's 12-pounder Gun	Ditto	60	93	3.25	6.72	..	12	2	0.166	1.76	20	30	..
Armstrong's 12-pounder Gun	32	114	75	3	7.07	..	12	1½	0.166	1.69	38	25	2.25
Whitworth's 3-pounder Gun	Hexagon	40	72	1.5	1.462	..	2.5	5	0.200	1.71	26.6	48	..
RIFLED SMALL ARMS.													
Sardinian Rifle (Bersagliere)	8	17	..	0.65	0.332	.9	580.	54	0.102	0.228	26.1	..	1.803
Whitworth's Rifle	Hexagon	20	39	Major axis, 0.49	0.139	1.45	520	68	0.132	0.534	46	88	3.00
Jacob's Rifle	4	30	24	0.529	0.22	1.31	754	94	0.125	0.490	56.7	46	2.50
Lancaster Rifle	..	32	32	0.498	0.196	1.12	520	68	0.132	0.379	64	64	2.25
Swiss Federal Rifle	8	36	33	0.41	0.132	1	257	62	0.243	0.278	76.8	80.6	2.44
Enfield Rifle	3	78	39	0.568	0.254	0.96	520	68	0.132	0.292	135.5	68.5	1.69
French Line Rifle.. ..	4	78	38	0.68	0.363	0.9	494	77	0.156	0.195	114.7	56	1.32
Belgian Rifle	..	78	38	0.66	0.342	Powder ball, 1.19	725	85	0.118	0.32	118	57.5	1.80

higher initial velocity with a slower twist; but that though the accuracy was the same in both cases, the latter method gave a flatter trajectory and greater penetration. Similarly, there were two methods for obtaining equal ranges; first, by combining the use of low initial velocity with that of a projectile of high 'maintaining power,'—or, one whose weight bore a high proportion to its surface of resistance; or, secondly, by combining a higher initial velocity with a ball of lower 'maintaining power.' The latter principle, which was best carried out in the Swiss rifle, had the advantage of attaining the same results, with a smaller weight of projectile; thereby diminishing the recoil, and enabling the soldier to carry a larger number of rounds of ammunition.

The most important ratios in the Table were those given in columns 9 and 10, which determined the "initial velocity," and the "maintaining power" of the projectile. The comparative initial velocity, in the various cases, would be directly as the numbers set opposite to them, in column 9, and the maintaining power of the ball, directly as the number set opposite each, in column 10. The maintaining power was due to diminishing the loss of the ball's velocity, during its flight, by reducing the ratio of its resistance, or cross sectional area, to its weight. This might be done, either by increasing the weight of the ball, without altering its proportion of length to diameter, or by reducing its diameter and cross section, and increasing its length, without altering its weight. The latter method was the best, and was carried to its utmost extent in the Whitworth rifle. It must be remembered, that the ratios of maintaining power, given in column 10, assumed that the ball was so balanced in the centre, as to fly with the whole length of its axis coincident with the line of trajectory, throughout its flight; but if the centre of gravity was not in the centre of its length, the ball would droop either in the front, or the rear. The axis being then inclined to the line of trajectory, the resisting surface would be commensurately increased, and the maintaining power would be diminished.

The three great principles of effect, in rifled projectiles, were :—initial velocity, maintaining power, and rotation. Neither should be carried to such an extent as to interfere with the due application of either of the other two, and the perfection of a rifled arm consisted in the happy combination and due relative proportionment of all three conditions.

The conditions that should be fulfilled by the form of rifling, both in respect to the projectile and in respect to the gun, would appear to be, that the projectile should be of such a profile, as to admit of its being economically constructed and shaped by machinery, so as to fit the grooves of the bore with great accuracy. It should be simple and not liable to injury in carriage, and it should take the rifling without friction. The form of rifling should

be such, as should avoid re-entering angles, and should weaken the gun in the least possible degree. The projectiles used for rifled ordnance were divisible into two great classes:—Those made with projections to fit the grooves; and those without such projections, but which being of lead, or coated with lead, were forced into the grooves, by the explosion. In the infancy of rifled cannon, it was assumed, that the only efficient method of eliminating windage, was by the adoption of a lead-coated ball, forced into the grooves, by the explosion. But experience had since shown, that by the use of a simple cast-iron shot, shaped to the rifling by machinery, and provided with a greased wad, the windage might be reduced equally well, and without subjecting the gun to the great increase of strain and to the friction incidental to the use of the 'forced ball.' As such simple shot, made with projections to fit the rifling, could be fired with a quicker twist, without the possibility of stripping, were cheaper to manufacture, less injurious to the gun, and much less liable to injury, in carriage, there was little doubt, in his opinion, that ere long, they would supersede the forced balls.

Major-General ANSTRUTHER said, there was an apparent error in speaking of cast iron as being of uniform strength, and of gunpowder as having a fixed force. He had seen an English 8-inch cast-iron shell, which could not carry its own weight, filled with lead, and thrown with 2 ounces of powder, to a distance of 150 yards. He had known the strength of powder to vary so much, as with the same weight of charge, in one case to throw a given projectile to a distance of 300 yards, and in another to 1,200 yards. In the former instance the powder was of Chinese, and in the latter of English, manufacture.

Mr. PEARSON proposed to confine the few remarks which he should make to the construction of a gun to withstand the greatest amount of internal pressure; as, although he had some opinions with respect to a particular kind of shell, and to the principle upon which a gun ought to be rifled, he thought those questions deserving of separate and distinct discussion. Previous to the Great Exhibition of 1851, he had made a gun, on a principle of his own, but was persuaded not to bring it forward on that occasion. He thought the gun should be made of wrought iron, and that the grain, or fibre of the iron should be placed in such a direction, as to enable the whole of its strength to be employed in resisting internal pressure. In welding the gun, care should be taken to heat the iron properly, and to work and use the material to the greatest advantage, when so heated. He believed, that a wrought-iron gun could be made of one-half the weight of a cast-iron gun, to carry the same weight of shot, and to do the same amount of execution. He then proceeded to describe two ways of making guns. In the first, a bar of iron was coiled

spirally round a steel tube, which formed the bore. The outer case was to be welded together, so as to become one piece when finished. A square thread was then to be cut on the outside of the steel tube, and a corresponding one on the inside of the iron case. These should be so arranged, that when the iron case was heated to a blood-red heat, and the steel tube was screwed in, proper appliances being provided for the purpose, there should be a sufficient amount of contraction to cause the iron case to bind the steel tube firmly. The thread of the screw need not be more than $\frac{1}{16}$ th of an inch deep. Supposing there happened to be a defect in the welding of any joint in the outer case, the thread on the steel tube would help to keep it firmly together. In fact, he did not think one could separate from the other, without the bursting of the gun itself. When the steel tube was worn out by service, by heating the iron case, and unscrewing the old tube, it could be replaced with a new one; and thus a perfect gun be obtained for the cost of a new tube. The iron gun, or case would answer for a series of steel tubes. The second plan was to coil two bars of iron of a V shape, one over the other. When rolled together, they would represent a kind of double V, the solid part of the outer bar embracing the joint of the inner coil, and *vice versâ*, so that there was no joint through the metal. Guns might be made upon that principle of any size, from a pistol up to an 80-pounder. Musket, or rifle barrels could be made, either of iron, or steel, on the same principle, by machinery, quite as readily as common barrels, or gas tubes were now made.

With reference to the various forms of guns, which had been noticed, he was prepared to admit, that the plan of shrinking a series of hoops upon an old cast-iron gun, would add considerably to its strength. But he did not think that plan ought to be adopted, in the case of new guns, because they might be made on a cheaper and better method. Notwithstanding the favourable results obtained with a gun on that system, he thought too high an estimate had been entertained of the strength of such a gun. The fact appeared to have been overlooked, that a hoop was weakened just in proportion to the amount of contraction put upon it. He contended, that a hoop in its usual state, was stronger than when it was put into a state of tension. Supposing a weight was lifted 15 feet high, and a hoop was placed underneath, with a tapered mandril in it. If five blows were sufficient to burst the hoop, it would, after it had received one blow, be one-fifth part weaker than it was previously; and he considered, that shrinking a hoop on to a solid body was tantamount to giving it one blow. He knew from experience, that if a hoop was made so small, that it had to be considerably heated, and it was then forced on to its place, it would frequently break by the power of its own contraction. He had no hesitation in saying, that many accidents

on railways had been occasioned by the tyres of the wheels of the carriages having been shrunk on too tight; and when worn, they had not sufficient strength to resist the tension to which they were subjected by the contraction.

Mr. WHITWORTH exhibited a model of his rifled cannon, which he had brought to the Meeting, at the request of the President, through whom a number of questions were addressed by the Members to Mr. Whitworth, who explained, that the metal used in the construction of his guns, was the material called 'homogeneous iron,' which partook largely of the nature of mild steel. It was made chiefly from bars of Swedish iron, cut into short lengths, melted in crucibles, and cast into a large cylindrical ingot, which was subsequently forged, under a tilt hammer, into the required form. The guns were forged solid, bored out in the usual manner, and rifled uniformly throughout their length, without leaving any chamber at the breech end. The pitch of the rifling in a 12-pounder gun was one turn in 60 inches.

The operation of loading was very simple; the breech, which consisted of a screwed cap, suspended in a hinged collar, so as to swing freely to the right hand of the gun, was unscrewed and turned aside, the rifled projectile was introduced, and followed by the metallic case cartridge, with its lubricating wad at the forward end, the breech cap was swung home and screwed up, the fuse was introduced through the centre of the breech, and the gun was fired. This operation was so simple, that great rapidity in firing could be attained, if it was considered desirable.

One of the projectiles exhibited was a $2\frac{1}{2}$ -lbs. shot, which had been thrown the distance of $5\frac{1}{2}$ miles, by a charge of 8 oz. of powder, when fired from a small 3-pounder gun, weighing 208 lbs. The gun was fired at an elevation of 35° .

The cartridge used was made of tinned iron, shaped to fit the rifled bore; the powder was retained in it by the lubricating wad, which was placed in the open end. This wad was composed of wax and tallow, and when the explosion took place, it was melted and was driven through the gun, lubricating the bore so thoroughly, that with a good quality of powder, the gun might be fired, for almost any period, without requiring to be cleaned by sponging it out. Some of the projectiles were cast in the proper form, the hexagonal bearing surfaces being cut, or shaped, by self-acting machinery. The cost of shaping a 12-lbs. shot did not exceed the expense of a penny per shot, for wages only. Very long experimental shells had been fired from the 24-pounder guns; and as there was not any chamber in the gun, a large charge of powder and any length of projectile might be used; it was, therefore, easy to try various charges and weights of shot, and thus to find out what was the proper charge of powder and what was the best length of shot for the gun, for a particular object. The 3-pounder

gun would, with a suitable charge of powder, throw a shot weighing $2\frac{1}{2}$ lbs., further than it would throw a projectile weighing 2 lbs., or 3 lbs. A shot weighing $2\frac{1}{2}$ lbs. was, therefore, shown to be the kind of projectile best suited to a gun of $1\frac{1}{2}$ inch calibre. In the same way, the proper weight and form of projectile best suited for the 80-pounder gun, was determined by a series of experiments, which showed that the gun called a 68-pounder, was that best adapted to an 80-lbs. shot. With this weight of projectile the best momentum was obtained, and it was, therefore, the most effective. The various forms of projectiles exhibited, had been arrived at by very careful investigation, and were not the result of *à priori* reasoning alone; they were, in fact, the results of many experiments, and as facts were obtained, further experiments were instituted, and thus the ultimate results were attained. The flat-fronted projectile was a model of that fired through the iron plate, 4 inches thick, and the side of the ship 'Alfred' at Portsmouth, from a distance of 450 yards. That projectile was fired from a cast-iron gun, such as was ordinarily used in the service, except that it was rifled: it was too weak to bear the strain, and it burst the day after the experiment, above referred to, was made. Flat-fronted projectiles weighing 24 lbs. were fired from brass howitzers rifled by Mr. Whitworth in 1856. They were fired during a series of experiments, made by Captain Hewlett on board the 'Excellent.' The shot passed through 30 feet of water diagonally, and 8 inches thickness of oak, piercing the timber 3 feet below the surface of the water, and burying itself many feet in the earth, or mud. This was believed to be the first instance of a shot having passed through water, with such an effect. When spherical shot, or long projectiles, with rounded fronts, were fired into the water, they would not penetrate below the surface, but they turned and came out again. The flat-fronted shot was the best form for traversing iron plates. If the round-fronted form of shot was fired against a thick wrought-iron plate, it displaced the particles of the iron plate, in a lateral direction, and had to overcome the great lateral resistance they offered to its passage. In the case of the flat-fronted shot, the resistance was confined to the exact spot on which the flat front of the shot struck; the resistance was direct and not lateral; and if the momentum of the shot was sufficient, and its particles cohered together, it punched a hole in the plate. In the experiment made on board the 'Excellent,' the gun was depressed, and the shot, fired at an angle, passed through 30 feet of water, striking the oak target and piercing it, 3 feet below the surface. The shot weighed 24 lbs., and the charge of powder was $2\frac{1}{2}$ lbs. The gun was a brass howitzer 52 inches long, such as was used in the service, except that it was rifled. From that

[1859-60. N.S.]

2 R

howitzer an experimental shell, 40 inches long, had been fired, which, with the charge of powder, nearly filled the bore of the piece.

In reply to a question relating to the strength of the material used for the rifle, Mr. Whitworth observed, that he had put into a rifle barrel, 1 inch in diameter at the breech, with a bore of 0.49 inch, a leaden plug 18 inches long, as tightly as it could be driven home upon the charge; it was fired with an ordinary charge of powder, and the long leaden plug being expanded by the explosion, remained in the barrel, the gases generated by the gunpowder all passing out through the touch hole, which was small and lined with platina. The lead was then melted out of the barrel, and the same experiment was repeated four times, with the same results. He was, therefore, satisfied, that the homogeneous metal was sufficiently strong, to resist the greatest strain to which, in practice, it could ever be subjected.

Much had been said as to the alleged great cost of the Whitworth rifles. But cost should be considered relatively to the material and workmanship, which should be of the best quality for the best weapons. In the first place, the best material that could be obtained was employed. It cost £60 per ton; it was very hard, very tough, and very difficult to be worked, but then it was very good, very strong, and very durable. It was unreasonable to compare the price of a rifle made of such material, with that of a rifle made of the softer and more easily worked, but far less durable material used at Enfield, for the ordinary service musket. Some time since, Mr. Whitworth had recommended, that rifles should be made, at Enfield, with his bore and twist in rifling, of about two turns in the length of the barrel of 39 inches, or one turn in 20 inches, instead of only half a turn, as in the Enfield rifle. He recommended, that rifles should be made, having that comparatively quick turn and the hexagonal bore of the diameter, which he employed; and he had no doubt whatever, if that had been done, the effectiveness of the rifles would have been doubled, and they would have been made at the same expense. If a rifle was made of a material more expensive and harder to work, it could not be done at the same cost; it would, however, be better, and in reality, it would be more economical, to make a more effective and more durable weapon, as the increased cost would be recovered in the extended wear. Mr. Whitworth stated, that in the competitive trials made before an official Committee, between the Whitworth and the Enfield rifles in April 1857, the results were pretty nearly about three to one in favour of the Whitworth rifle, in accuracy of firing and length of range. A target was made with the Whitworth rifle at 2,000 yards, whilst the longest range of the Enfield rifle was 1,400 yards.

Reverting to cannon, Mr. Whitworth explained, that the 80-

pounder gun was purposely made so strong, that it could be submitted to any proof, without injury. It was made of homogeneous iron. Upon a tube, having an external taper of about 1 inch, a series of hoops, each about 20 inches long, were forced by hydraulic pressure. Experiments enabled him to determine accurately what amount of pressure each hoop would bear. All the hoops were put on with the greatest amount of pressure they would withstand, without being injured. A second series was forced over those first fixed, and thus the 80-pounder gun was constructed. At the present time, it was not necessary to have the second series of hoops, but it was thought better, in the case of the first large rifled cannon, to have it stronger than was practically necessary; in fact, to make it so strong, that gunpowder could not burst it. In the 12-pounder there were not any hoops at all, and Mr. Whitworth believed, that a large-sized gun could be made with this homogeneous metal, without hooping it. The inside of the 80-pounder gun was made of a tube of homogeneous metal; and the external rings were of fibrous iron, except two hoops next to the breech, which were made by Mr. Clay of puddled steel. The hoops were put on cold. The rifle twist in the 80-pounder gun was one turn in 100 inches; in the 12-pounder it was one turn in 60 inches; and in the small 3-pounder, it was one turn in 40 inches. With respect to the degree of rifling adopted in the Whitworth guns, enabling the powder to be consumed more effectually, the following experiment was mentioned. Two barrels alike in diameter and bore were prepared; all the conditions were identical, except the difference of twist in the rifling. One barrel had two turns, and the other had four turns. It was found, on placing them both at an elevation of $1^{\circ} 20'$, and firing them with 50 grains of powder, that they each carried the shot to about the same height on the target. Mr. Whitworth then fired them with an increased charge of powder, and the barrel with two turns sent the shot considerably higher upon the target, while the barrel with four turns sent its shot but very little higher than with the small charge. A length of 10 inches was then cut off the latter barrel, leaving only three turns, and it was fired again with the increased charge. The result was, that, the elevation remaining the same, it threw its shot higher on the target, than the other barrel. This showed, that rotation must bear a due proportion to the length of the barrel. It was desirable to have as much rotation as possible, taking into consideration the length of the gun. With a very long gun, it was not advisable to have very rapid rotation, as the quick turn of the projectile was most felt at the muzzle.

It was further explained, that the experiments of which Mr. Whitworth had spoken, were made at Portsmouth, against the side of the ship 'Alfred,' covered with iron plates 4 inches thick. At a range of 450 yards, the shot traversed the iron plate, and also

pierced the side of the ship on which it was bolted; the shot was picked up inside the vessel. Experiments were about to be made, to try whether with the Whitworth 80-pounder gun, the flat-fronted shot would traverse the wrought-iron plates, $4\frac{1}{2}$ inches in thickness.

Mr. WHEATLEY remarked, that in the course of experiments in the United States, it was found, that round shot glanced off the target at an angle of 15° . He thought it very likely, that Mr. Whitworth's shot would rip open a plate at that angle. It would be worth while to try the experiment.

Mr. CONYBEARE inquired, whether Mr. Whitworth had not found, that an increased twist, increased the tendency of the shot to be thrown on one side of the line of fire. Rifles, when fired from fixed rests, did not throw the shot in one line of sight, but either to the right hand, or to the left, according to the rifling. The French had reduced their rifling, in order to get rid of the amount of divergence.

Mr. WHITWORTH said the shots did diverge in the direction in which the barrel was rifled; but there was no practical difficulty in the matter, as the deviation could be easily allowed for in sighting the gun. In the long ranges, very much depended upon the influence of the wind. Sometimes it would carry the shot 200 feet to the left; at other times 400 feet to the right, from the line of fire, supposing that no allowance was made for it.

The value of the self-acting machinery for shaping the rifled-cannon projectiles, would be about £500, to enable a workman to produce the shot at such a rate, as that the cost should not exceed one penny per shot, for wages only.

Mr. BIDDER,—President,—remarked, it was a most material point that, continuing to use the same material, a service rifle might be made at Enfield, upon Mr. Whitworth's principle of rifling and loading, at about the same cost as that at which the Enfield rifles were now supplied. He understood that to be the opinion of Mr. Whitworth. He also presumed, that Mr. Whitworth still retained the opinion, that the bore he adopted for his rifle, was better than that which had been selected for the Enfield rifle.

Mr. WHITWORTH replied, that his opinion was founded upon experiments, which showed that the diameter of bore he used, was more suitable for ordinary men than a larger bore, and for this reason;—if men were by nature three times stronger than they actually were, then the bore of the Enfield rifle would be right. There was for each bore a proper charge, to give the best trajectory; the charge that would give the best trajectory with the Enfield bore, would be far too great, and would produce too much recoil, for a man to bear. He had, therefore, adopted a bore so reduced, that the proper charge for it would only produce such a recoil, as was not too great for a man to bear.

Captain W. S. MOORSOM said, it seemed to him, that the observa-

tions, as to the size of the bore which especially affected the soldier, touched upon the line of demarcation which should be drawn between the Civil Engineer and those whose business it was to use these weapons; and he should like to know, how far it might be considered as trenching upon a subject, which could hardly be supposed to be properly understood by the Members of the Institution. It was to be borne in mind, that at a recent period in the military history of this country, one of the most valuable corps in the service could not fire twenty rounds continuously from the shoulder, without being put 'hors de combat,' in consequence of the improper construction of the arm used. He was by no means prepared to say, that it was within the province of the Civil Engineer to discuss that question. It struck him as being singular, that whereas it had been said by one who, he was told, was a scientific and practical man, and who was placed at the head of one of the largest establishments of the Government, that this subject could only be handled by practical Officers of Artillery; yet after that preamble, not a single practical fact had been adduced, by that gentleman, to illustrate the question under discussion, or to throw any additional light upon it. If, therefore, there was anything which showed the necessity, and the judiciousness of carrying out the suggestion of the President, that in the present Session, subjects of the most vital importance to the country should be entertained, in a way which belonged only to this Institution, not as going beyond what they knew as Civil Engineers, it was afforded by the circumstance to which he had alluded. He thought the President, and the Members of the Institution generally, had already conferred great benefit upon the country, and would continue to do so, by handling these subjects, with mutual temper, forbearance, and knowledge; so as to bring the military and the civil services into the best possible combination, for the improvement of the national resources and defences. There was now present one of the most practical Officers of the Musketry Instruction of the Army, and he hoped General Hay would give his opinion to the meeting, on the reduction of the bore proposed by Mr. Whitworth.

General HAY remarked, that the trials he had made had been, to a great extent, of a private character; and he was not at liberty to divulge the exact issue of those experiments. But, if he was called upon to give an opinion, as to the relative merits of the Enfield and the Whitworth rifles, that was a matter upon which any man, who had carefully considered the subject, was competent to come to a conclusion. The small bore would of course, in accurate shooting, beat the large bore. There was a peculiarity about the Whitworth small-bore rifles which no other similar arms had yet produced; they not only gave greater accuracy of firing, but treble power of penetration. For special purposes,

any description of bullet could be used, from lead to steel. The Whitworth rifle, with a bullet composed of one-tenth of tin, penetrated thirty-five planks; whereas the Enfield rifle, with which a soft bullet was necessary, only penetrated twelve planks. He had found, that at a range of 800 yards, the velocity added to the hardened bullet gave a power of penetration, in the proportion of 17 to 4, in favour of the Whitworth rifle. Velocity might be taken as a certain test, *cæteris paribus*, of penetration. The penetration of the Whitworth rifle was enormous, and this in a military weapon was of the highest importance, in firing through sand bags, gabions, etc. The Whitworth projectile would penetrate through a sand bag and a-half, or through a three-foot gabion; whereas the Enfield would only go through one bag, or would only reach the middle of a gabion. He thought the merits of the small bore had not yet been sufficiently understood. He was quite aware that it had been stated recently, that the small-bore Enfield had beaten the small-bore Whitworth; but nothing of the kind had ever taken place. Hitherto, the subject, he did not hesitate to say, had been entirely misunderstood, and it was only by such discussions as these, that the public could learn the real facts of the case. It was proper also to state, that the exact bore of the Whitworth rifle had been adopted at Enfield, without acknowledgment; that even the same twist had been given to the rifling, 1 turn in 20 inches; and therefore, it would not be very remarkable, if the same accuracy of fire was obtained. But he had shown, that there were other things to be considered besides accuracy. Supposing, for instance, that the same accuracy of fire was obtained with the small-bore Enfield, as with the Whitworth rifle, there was still the fact of the penetration of the latter being two-thirds more than that of the former. These points were not sufficiently considered. Mr. Whitworth had solved the problem he undertook,—how to project, to the best advantage, a given quantity of lead with a given quantity of gunpowder; and there was no gun in England, at this moment, which would fulfil that condition to the same extent as the Whitworth rifle.

In reply to a question from the President, General Hay said, every gun, in which a certain amount of powder was exploded, would become foul. But if the bullet expanded properly, it left the gun, as it were, sponged out, after every shot. Should the bullet not expand, the gun, of course, would not be sponged out. He ventured to say, that the Whitworth small-bore rifle, fired with common sporting powder, would never foul, so as to render loading difficult. If the lubrication was correct, there could not be any such fouling, even when the rifle was used constantly for a month. He had himself fired one hundred rounds one day, sixty rounds the next, then forty rounds, and so on, and left the gun without being cleaned for

ten days, when it fired as well as it did on the first day. But if the conditions, here insisted on, could not be carried out; if it was intended that bullets should be fired, without proper expansion and lubrication, then fouling would take place and accumulate in any gun. It had been asserted, that the Whitworth small-bore must of necessity foul, because that diameter of bore could not consume 70 grains of powder; but it was well known, that it had consumed 100 grains of powder with advantage. He had great pleasure in thus bearing his testimony to the merits of Mr. Whitworth's invention.

Colonel EARDLEY WILMOT, R.A., said, there was one remark in the Paper, the precise meaning of which it was difficult to understand. He apprehended, however, that the Author intended to convey the impression, that the Select Committee on Ordnance had treated his invention with contempt. He would just remark, that the Government had added to that Committee a Civil Engineer, Mr. C. H. Gregory, (V.P. Inst. C.E.,) a very proper compliment to the civil engineering profession; Professor Wheatstone; and one of the most distinguished mathematicians in Europe, Professor Sylvester. It was not correct to say, that the principle advocated by the Author was pronounced defective. The experiment, or practice, was certainly found to be so, for by firing the gun with a charge much less, he believed, than an ordinary cast-iron gun of the same size would have been tested with, the breech was blown out, and the wire uncoiled. The original objection taken to this arrangement, was the difficulty of making a sound breech in a wire gun; and also that if the gun were struck by a shot, it would, probably, be rendered unserviceable, owing to the cutting of the wire, or at all events, the extreme accuracy of tension, said to be necessary, would be affected. There was no reason, perhaps, why guns should not be made upon that plan; and it might be the case, that the Author had improved his gun, and had been more successful on each of those two points than formerly. The difficulty of securing the breech was very great. When a certain number of rounds had been fired from Captain Blakely's gun, (Fig. 9, page 304,) the breech was blown out. That would be a difficulty with all muzzle-loading built-up guns; but with breech loaders it would be more simple. He had witnessed the first experiments with the Whitworth gun, and could bear his testimony to the remarkable power of the howitzer gun, which had been mentioned. He had also been present at the early experiments with the Armstrong gun, at Newcastle. The accuracy of the firing was as remarkable then, as it was at the present time. At a distance of 1,500 yards, he could stand within 20 yards of the target, without cover, with perfect confidence. That gun was not now made of the same material, nor in the same manner; the rifling was different, and the form as well as the mode of manufacture of the shot, to what was then the case; so also,

Mr. Whitworth now appeared with a breech-loading gun of a different material. If Mr. Longridge had made modifications in his gun, in like manner, and if it was brought forward in a tangible form, Colonel Wilmot had no doubt it would meet with as much attention as the guns of Sir W. Armstrong and Mr. Whitworth. In regard to cost, in the days he spoke of, money was not so plentiful as it was now. The charge for Sir W. Armstrong's gun, in the first instance was, he thought, £850, and £400 for personal expenses, of which only one-half was asked to be paid. It was now stated, that a larger gun could be turned out complete for a much smaller sum. In reference to the material of which guns should be composed, he knew that many minds were directed to the subject, and he was satisfied that, before long, a good result would be obtained. At the same time, he thought it was a pity, that any conclusion should yet be arrived at, as to the absolute unfitness of cast iron for guns. At the present moment, experiments were being made in Woolwich Arsenal, with a gun which had stood the following discharges; 10 rounds with a cylinder weighing 68 lbs.; 10 rounds with a cylinder weighing twice 68 lbs.; 10 rounds with a cylinder weighing three times 68 lbs.; and so on to four times, five times, six times, and seven times, so that the weight of the cylinder with the last 10 rounds was 476 lbs.; the charge of powder being in all cases 16 lbs.; yet the gun was uninjured. Five rounds had since been fired, with the same charge of powder, and a cylinder weighing 544 lbs., which had the effect of destroying the carriage of the gun. This was repaired, and another round was fired of the same proportions of charge and weight of cylinder, when the gun burst.

He had been furnished with the results of experiments made with a Spanish cast metal 32-pounder, 8 feet 9 inches long, and weighing 45 cwt. That gun was fired, first with 21 lbs. of powder, 2 shots, and 2 wads; then with 9 lbs. of powder, 2 shots, and 3 wads, at an elevation of 10°. He need hardly say, that as the elevation was increased, the strain upon the gun became greater. It was then fired 827 times without injury, with 9 lbs. of powder, 2 shots, and 3 wads; next with 9 lbs. of powder, 3 shots, and 2 wads; then with 9 lbs. of powder, 4 shots, and 2 wads; continuing with the same charge of powder, and the same number of wads, up to 11 and 12 shots, when the gun was full to the muzzle. Subsequently, it was tried with 12 lbs. of powder and 10 shots; 15 lbs. of powder and 9 shots; 18 lbs. of powder and 8 shots; 21 lbs. of powder and 7 shots; 24 lbs. of powder and 6 shots; 27 lbs. of powder and 5 shots, when the gun was again filled to the muzzle, and then it burst. It thus took, to burst that gun, an aggregate of 3 tons 13 cwt. of powder, 25 tons 8 cwt. of shot, and 2 tons 19 cwt. of wads.

An American shell-gun, 9 feet long, 9 inches in diameter, and weighing 81½ cwt., had been fired with the following results:—

Number of Rounds.	Charge of Powder.	Number of Shot and Shell.	Weight of Shot and Shell.
	lbs.		
2	15	1 shot	90
1,500	10	1 shell	72
5	15	1 shot	90
5	15	2 shot	180
2	15	3 shot	270
3	15	4 shell	288
1	20	{ 3 shot 1 shell }	342
1	20	{ 2 shot 4 shell }	468
1	20	{ 2 shot 6 shell }	612
1	20	7 shot	630
1	20	8 shot	720
1	20	9 shot	810
1	20	10 shot	900
When the gun burst.			

He might also mention, that a British 32-pounder was known to have fired, at the siege of Sevastopol, three thousand rounds; and though the vent was much enlarged, the bore was perfectly smooth, sound, and serviceable.

If cast-iron guns could be made to withstand such a test, they should not be altogether discarded. He did not mean to say, that cast iron was the proper material for rifled cannon, but simply, that it was a metal which was not unworthy of consideration. It might be supposed, from the remarks he had made, that he was an advocate for cast-metal guns. For economy and simplicity of manufacture, a cast metal was preferable to a wrought one. He certainly saw no reason to despise even cast-iron guns, although several guns which he had made had burst, and others very lately, which he knew would burst, and had put aside as unworthy. It was for the purpose of improving the character of cast iron,—not eventually excluding steel,—that the Royal Gun Foundry, at Woolwich, had been established. It was not proposed to stop the operations of private firms; but to make experiments, in order to arrive at a certain method of making guns, so that the work, which this description of gun had been known to do, might be relied on. Although that establishment was commenced, without the means of getting information from those who were capable of giving it, but had to be worked out, step by step, he was nevertheless satisfied, that it would have accomplished its purpose successfully and satisfactorily. He made this remark, because he considered that establishment was not a Government work only, but a national undertaking, about which there should be no misunderstanding. All were equally interested, in finding out, if it was possible, how to make cast-iron guns possessing all the qualities which were required.

Allusion had been made to the rifling of the old service guns. The use of elongated projectiles increased the strain upon the guns, but not to the enormous extent which had been supposed, because the charge of powder was proportionately diminished. It had been said, that if the windage was diminished, the strain upon the gun would be increased. That might be true to a limited extent. The great injury in firing round-shot guns was from the windage. If the shot was made completely to fit the gun, it might be fired repeatedly without injury, as there would then be an absence of the bounding motion of the shot, whilst passing through it. He believed, in such cases as those brought forward by Mr. Britten, that with shot weighing from 50 lbs. to 55 lbs., and the windage prevented by a soft material like lead, it was not a necessary consequence, that the gun should burst with the charge requisite to get a good result. The old service guns might, he thought, be easily rifled for ordinary use, and be then as good as most of the foreign rifled guns.

A statement had lately been made relative to brass guns. It was wished to ascertain, whether brass guns might be used either as muzzle-loaders, or as breech-loaders; and whether the action of the iron projectile would have an injurious effect upon the brass. In actual trials, no injurious impression seemed to be made; indeed, as far as was at present known, a rifled cannon could be cast of brass, as well as of any other material, and with soft metal shot, it would last for a long time. It had also been stated, that such a gun would only stand eight hundred rounds, and had then to be disposed of for one-half its value. This was not the case. Brass guns were re-melted into new ones; and this metal possessed the advantage of being capable of conversion into any new form required. A brass gun was lately fired more than eleven hundred rounds, with a slightly diminished windage, and was still perfectly fit for service. Time, labour, and influence were required, to put this subject on a proper footing. He felt convinced, that whatever was done for the service of the country, would be duly considered, with proper regard to the public good.

Mr. WHITWORTH stated, with respect to the brass guns spoken of, that they had been rifled for the Government at the request of the late Lord Hardinge, in 1856, and upon exactly the same principle as he now adopted. They only differed from the guns fired during the late experiments, in being muzzle-loaders. It was a pity that they, and similarly-rifled brass guns, had not been employed, as they might have been, on actual service, in India and China. By means of the lubricating wad, all difficulty arising from the fouling was obviated, and if those brass rifled guns were mounted on proper carriages, and provided with proper sights, they would, probably, give as good results as any he had hitherto obtained from his breech-loading guns.

Sir WILLIAM ARMSTRONG said, the subject of the present discussion was one upon which he had hitherto observed almost entire silence, and it was only because the public attention had recently been so vigorously roused, in reference to competing systems of gunnery, that he was induced to deviate from that reserve, which it would have been more consistent with his official position to have maintained. It was, however, a satisfaction to him, that owing to existing circumstances, the first distinct and detailed description of his rifled ordnance and projectiles, would be communicated to an Institution composed of his professional brethren, and comprising so many persons with whom he had long been on terms of intimacy and friendship.

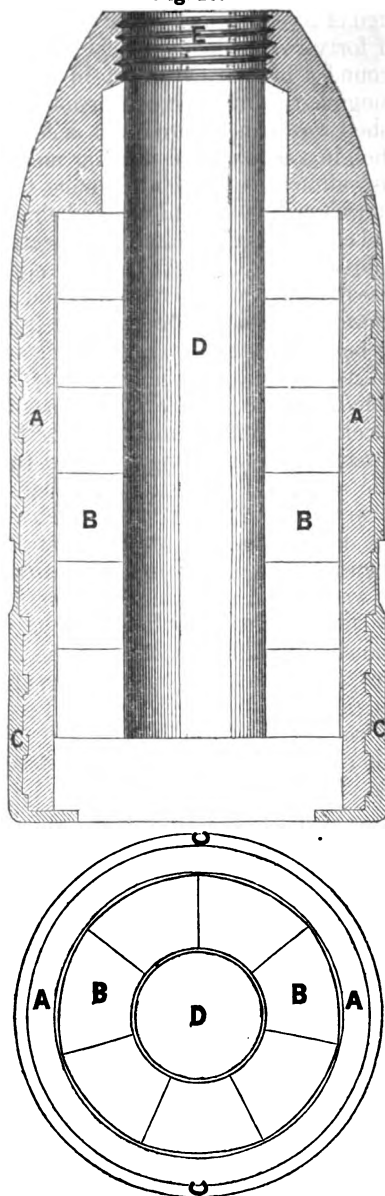
He had brought, for inspection, a specimen of his 12-pounder field gun, in the exact form in which it had been recently introduced into Her Majesty's service. But before he proceeded to give a description of it, he thought it would better conduce to a clear understanding of the gun, if he first directed his observations to the projectile; because he considered, that in all cases, the projectile should rule the gun, and not the gun the projectile. The projectile had to do the work, and it must, evidently, be the proper course, first to determine what kind of projectile was required, and then to devise the form of gun most suitable for throwing it.

It had, hitherto, been the practice to take into the field, three kinds of projectiles, viz. :—

- 1st. Solid round shot ;
- 2nd. Shrapnel shell ;
- 3rd. Canister shot.

The first of these was, as its name implied, a simple solid sphere of cast iron. The second was a shell, containing leaden bullets, with a small bursting charge; and fitted with a time fuze, ignited by the flash in the gun, so as to explode the shell a short distance in front of the object aimed at. The third consisted of a number of small iron balls, enclosed in a thin canister, and designed for firing upon an enemy, at very short distances. Both the round shot and the shell were very uncertain in their action, and could scarcely be used with effect, at more than a thousand yards. But apart from the defects of all these projectiles, there was an obvious inconvenience in having three varieties to carry, because, after exhausting the kind most suitable for any particular service, it became necessary to resort to a kind not adapted for the purpose. Sir W. Armstrong, therefore, conceived the idea of constructing a projectile, which might be used, at pleasure, as solid shot, as shrapnel shell, or as canister shot; and which should also have the advantage of exploding by impact, as well as by the action of a time fuze. Specimens of this projectile, both entire, and broken

Fig. 28.



12-Pounder Segment Shell.

A A. The Cast-iron Case, or Shell.
B B. The Segment Shot in layers.
C C. The Lead Covering.

D. The Central Cavity for the Bursting
 Tube and Concussion Fuze.
E. The Screw for the Time Fuze.

open to show the interior, were exhibited. (Fig. 28, page 408.) The projectile consisted of a very thin cast-iron shell, the interior of which was composed of forty-two segment-shaped pieces of cast iron, built up in layers around a cylindrical cavity in the centre, which contained the bursting charge, and the concussion arrangement. The exterior of the shell was thinly coated with lead, which was applied by placing the shell in a mould, and pouring melted lead around it. The lead was also allowed to percolate among the segments, so as to fill up the interstices, the central cavity being kept open by the insertion of a steel core. In this state the projectile was so compact, that it might be fired through 6 feet of hard timber, without injury; while its resistance to a bursting force was so small, that less than one ounce of powder was sufficient to break it in pieces. When this projectile was to be used as a shot, it required no preparation, but the expediency of using it in any case, otherwise than as a shell, was much to be doubted. To make it available as a shell, the bursting tube, the concussion arrangement, and the time fuze, were all to be inserted; the bursting tube entering first and the time fuze being screwed in at the apex. If then, the time fuze was correctly adjusted, the shell would burst, when it reached within a few yards of the object; or failing that, it would burst by the concussion arrangement, when it struck the object, or grazed the ground near it. Again, if it was to act as 'canister,' upon an enemy close to the gun, the regulator of the time fuze must be turned to zero on the scale, and the shell would then burst at the instant of quitting the gun. In every case the shell, on bursting, spread into a cloud of pieces, each having a forward velocity equal to that of the shell, at the instant of fracture.

One of these shells had been burst in a closed chamber, where the pieces could be collected. It would be seen that they consisted of:—

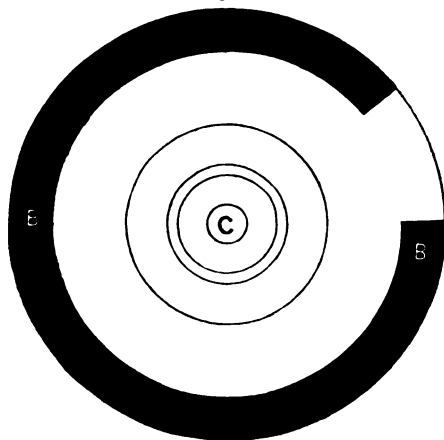
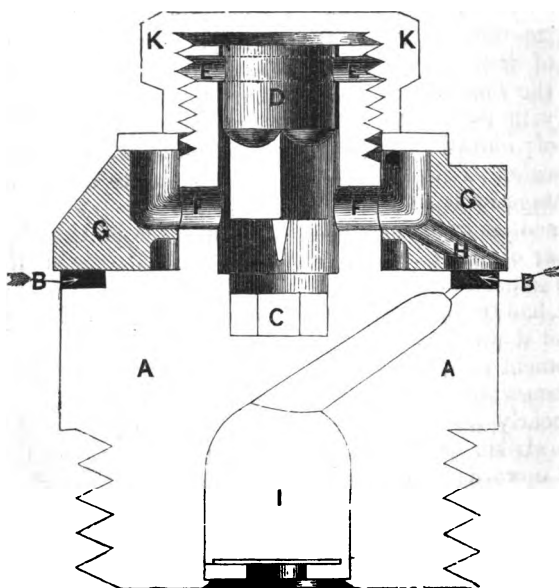
106 pieces of cast iron;—99 pieces of lead;—and 12 pieces of fuze, &c.; making in all 217 pieces.

Now it was no uncommon thing for one of these shells to make 100 holes in a column of targets, at a distance of 3,000 yards, and as a large proportion of the pieces was derived from the lead, it would be seen, that the lead added greatly to the efficiency of the shell.

The time fuze and the concussion arrangement demanded careful explanation.

The body of the time fuze, (Fig. 29, page 409,) was made of a mixture of lead and tin, cast to the required form, in a mould. The fuze composition was stamped into a channel, forming nearly an entire circle round the body of the fuze, and was afterwards papered and varnished on the external surfaces. As the shell fitted accurately into the gun, there was no passage of flame, by which the fuze could be ignited. That effect was, therefore, produced in the fol-

Fig. 29.



Time Fuze.

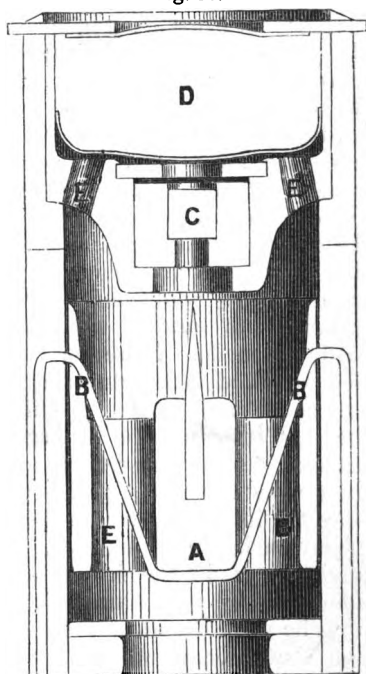
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|---|---|
| A. The Body of the Fuze. | F. The Flame Passage. |
| B. The Groove containing the Fuze Composition. | G. The Revolving Cover, or Regulator. |
| C. The Detonator. | H. The Igniting Aperture. |
| D. The Striker. | I. The Chamber for the Priming Powder. |
| E. The Holding Pin. | K. The Tightening Cap. |

lowing manner. A small quantity of detonating composition was deposited at the bottom of the cylindrical cavity in the centre of the fuze, and above this was placed a small weight, or striker, terminating in a sharp point presented downwards. This striker was secured in its place by a pin, which when the gun was fired was broken, by reason of the vis inertiae of the striker. The detonator was then instantly pierced by the point and was thus fired. The flame, thus produced, passed into an annular space, formed within the revolving cover, which rested on the upper surface of the fuze composition, and from this annular space, it was directed outwards, through an opening, so as to impinge on and to ignite the fuze composition, at any required part of the circle. The fuze, thus ignited, burnt in both directions, but only took effect at one extremity, where it communicated with a small magazine of powder in the centre.

Fig. 30.

The fuze was surrounded by a scale paper, graduated to accord with the elevation of the gun, so that when the range of a distant object was found by trial, it was only necessary to turn the igniting aperture of the cover, to the point on the fuze scale, corresponding with the degrees and minutes of elevation on the tangent scale. This fuze had the advantage of being capable of adjustment and re-adjustment any number of times, before entering the gun, and the officer in command, had the opportunity of seeing that it was correctly set, at the moment of being used.

The concussion fuze, (Fig. 30,) was on nearly the same principle. A striker with a point, presented upwards, was secured in a tube by a wire fastening, which was broken on the firing of the gun; the striker being then liberated, receded through a small space, and rested at the bottom of the tube, but as soon as the shell met with any check in its motion, the striker ran forward and pierced the detonator in front, by which means, the bursting charge was ignited.



Concussion Fuze.

- A. The Striker.
- B. The Holding Wire.
- C. The Detonator.
- D. The Chamber for the Priming Powder.
- E. The Flame Passages.

It would be seen, that the projectile, when thus complete, with its time fuze attached, was far from being of that form and proportion which ought to be selected, if length of range were the only consideration. Comparisons, therefore, ought not to be drawn between a shell thus designed for effect, and a solid shot designed for range. Its range, however, was enormous, in comparison with that of round shot, and as far as could be collected from the published reports of Mr. Whitworth's practice, it was not much short of that obtained with his 12-pounder projectile, when equal charges were used. It would be absurd, however, to contend that the form of this shell should be modified, at the expense of its efficiency, merely to obtain a small increase of range, when all military authorities agreed, that the range already attained was even more than sufficient for all field operations.

The public was always captivated by the attainment of long ranges; but great delusion prevailed on that subject. To hit an object upon a measured range where the distance was accurately known, was a very different matter from hitting the same object in the open country, where the distance had to be discovered. At 4,000 yards, a man on horseback, (taking man and horse as one object,) subtended an angle of less than 3 minutes of a degree, and 3 minutes of elevation was equivalent to only 15 yards in range. Hence therefore, assuming the gun to be absolutely perfect, unless the distance could be determined to such a nicety as 15 yards, a shot would either fall short of, or go over the object. But so far from its being possible, by any available means, to determine the distance with such precision, an error of even 500 yards might easily be made. Moreover, the variation of atmospheric refraction would, at such a distance, generally falsify the elevation, to a very embarrassing extent; whilst the wind blowing with, or against the shot introduced a further complication, in itself sufficient to baffle all attempts at refined adjustment of the angle. Even trial shots were unavailing to determine elevations for long distances, since the intervals between the graze and the object could not be appreciated. The fact was, that however perfect the weapons might be made, the fate of a battle would never be materially influenced by very distant firing. The real struggle would always lie within a distance of 2,000 yards, and the first consideration should be, to make the weapons as destructive as possible within that limit. As to firing solid bolts, at an elevation of 35° , in order that they might fall amongst an enemy five miles distant, it would be mere waste of ammunition. Certainly what went up, must come down, but according to the doctrine of probabilities, and to all experience of what was called 'vertical fire,' the chance of any one being injured by a projectile falling, as it were, from the clouds, was too remote to be worth consideration.

The gun was composed wholly of wrought iron, and the prominent feature in its manufacture was the application of the material in the form of long bars, which were coiled into spiral tubes, and then welded by forging. For the convenience of manufacture, these tubes were made in lengths of from 2 feet to 3 feet, which were united together, when necessary, by welded joints. From the muzzle to the trunnions, the gun was made in one thickness, and was, therefore, so far as that portion was concerned, strictly analogous to the barrel of a fowling-piece. Behind the trunnions two additional layers of material were applied. The external layer consisted, like the inner tube, of spiral coils, but the intermediate layer was composed of iron slabs bent into a cylindrical form and welded at the edges. The reason for this distinction was, that the intermediate layer had chiefly to sustain the thrust on the breech, and it was, therefore, desirable, that the fibre of the iron should be in the direction of the length, while elsewhere in the gun, it was more advantageously applied in the transverse direction. The back end of the gun received the breech screw, which pressed against a movable plug, or stopper, for closing the bore. This screw was hollow, and when the stopper was removed, the passage through the screw might be regarded as a prolongation of the bore. The screw was turned by means of a handle, which was free to move through half a circle, before it began to turn the screw. It had thus a certain amount of run, which enabled it to act as a hammer, both in tightening and slackening the screw. The bore was 3 inches in diameter, and it was rifled with thirty-four small grooves, having the driving side rectangular and radial, and the opposite side rounded. The bore was widened at the breech end $\frac{1}{4}$ th of an inch, so that the shot might enter freely, and choke at the commencement of the grooves.

The sights were peculiar. The eye hole of the tangent sight was in the form of a cross, which admitted of the vertical and horizontal adjustment being determined with great accuracy. There was a traversing movement at the top, to give correction for a side wind, as well as for the constant deviation to the right, which affected rifled projectiles of nearly all shapes, in a greater, or less degree. A vernier was applied both to the vertical and the horizontal scale, to enable the elevation and the correction to be adjusted to minutes of a degree. This tangent sight was designed chiefly for long distances, where great deliberation could be used, in adjusting the gun; but for rapid firing, at close quarters, a simpler and readier sight was provided. In this the elevation was given in short steps, each corresponding with 50 yards in range, and defined by a ratchet action, which indicated the successive steps by a very audible click.

The process of loading was effected by placing the projectile, with the cartridge and a greased wad, in the hollow of the breech screw, and thrusting them either separately, or collectively, by a

rammer, into the bore. The stopper was then dropped into its place and secured by half a turn of the screw. The gun was fired by the ordinary friction tube, the vent being contained in the stopper. It would be seen, that the whole operation was simple, and could be very rapidly performed.

In the early guns, it was necessary, that the portion of the bore which was occupied by the shot, should be perfectly clean, otherwise, the shot would not always enter its place. A wet sponge had, therefore, to be used ; but in the new guns, now issued for service, a slight alteration in the bore had enabled a greased wad to be employed with perfect effect, in substitution of the wet sponge. The gun could now be fired with great rapidity, and apparently, for any length of time, without being sponged at all.

The reason for making the vent in the stopper was, that since the chief wear of a gun always took place at the vent, it was better to make it in a part which could be easily replaced, than in the body of the gun itself.

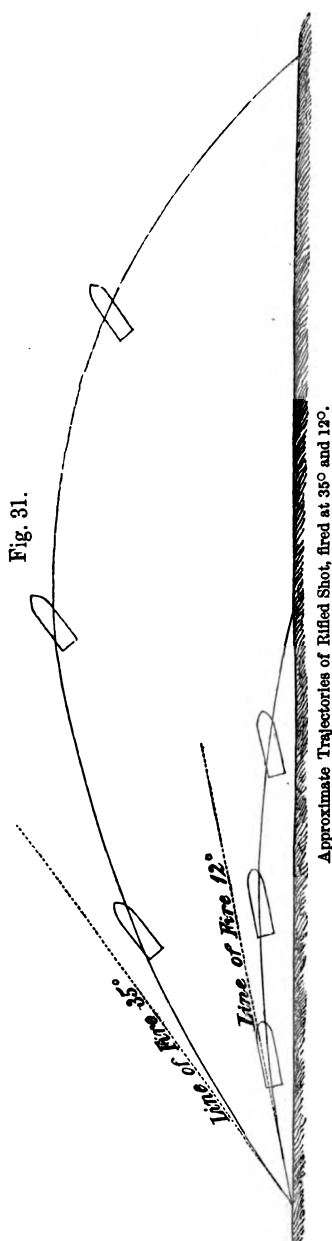
The breech screw, being internal, was never exposed to injury, nor could drifting sand, or dust, ever reach the oiled surfaces, so as to impede the action of the screw, by adhering to the lubrication.

The screw was of small diameter, and the few inches of extra length in the gun, required for its reception, could not be of any importance, considering that any further reduction of weight was prohibited by recoil.

The stopper had been secured from falling by a chain, but in practice, it was preferred to leave it loose. The man who fired the gun, lifted the stopper after each round, and in so doing, only occupied time that would otherwise be vacant. A duplicate stopper accompanied each gun.

The form of carriage, which was originally used, was represented by the model exhibited. It was fitted with a recoil slide, which was afterwards abandoned for field guns ; but it was now decided that the principle should be retained in the ship guns. It was a point of great importance, that a breech-loading gun should be self-acting, in recovering its position after recoil, so as to obviate the employment of so many men to run out the gun. A traversing movement had been originally applied to the field carriages, as shown in the model, and was found to afford great facility in laying the gun. A very neat modification of this traversing movement had recently been contrived in the Royal Carriage Department, and adopted for the field carriages.

Referring to heavy artillery, it was necessary to commence, as before, by considering what effects were required to be produced, and what species of projectile would best answer the purpose. For destroying ships, stone and brick forts, earthworks, and buildings, as well as for operating against hostile troops, it must be admitted, that the shell was the most effective projectile. In



fact, with the exception of iron-plated ships and granite batteries, there was nothing that could not be more effectually attacked by shell, than by shot. This must be evident, when it was considered, that a shell, arranged to act concussively, always penetrated before it burst. It, therefore, produced the effect of a shot, before it acted as a shell, and its explosive effect was superadded to that of a shot. Moreover, for high angle firing, the shell was the only projectile to be considered. But the power of a shell depended upon the magnitude of its bursting charge, and a large bursting charge involved a large diameter. It was in vain to say, that increase of length would compensate for smallness of diameter. If a shell for a small bore, could be lengthened, so could a shell for a large bore. Length for length, or weight for weight, the large diameter must, necessarily, accommodate a larger bursting charge, than the small diameter. In the construction, therefore, of heavy guns, Sir W. Armstrong, in contrast with Mr. Whitworth, inclined to the larger diameters, although by so doing, he might sacrifice somewhat in range.

Here it was necessary to say a few words respecting the conditions, upon which the attainment of long range chiefly depended. Everybody would understand, that a long taper projectile would cleave the air more freely, than one of short obtuse proportions; but there was another condition, not generally recognised, which had also a most important influence; viz., the sustaining effect of the air, consequent upon the obliquity of the shot to the path of its flight. It would be seen by reference to the diagram,

(Fig. 31,) that since the axis of the projectile in its flight remained

parallel with its original direction, it constantly acquired an increasing obliquity to the direction of its flight. Hence, therefore, the air acting on the inclined surface presented by the under side of the projectile, pressed it upwards like a kite, and although the shot was not actually lifted, its descent was retarded, and it had, therefore, time to reach to a greater distance, than it would otherwise attain. It would be observed, that this sustaining action was chiefly operative at low angles. At high angles the shot would, eventually, become transverse to the curve, and the lifting action would then cease. Sir W. Armstrong had long since pointed out, though he never did so without meeting with incredulity, that owing to this buoyant influence, a lengthened projectile from a rifled gun, actually attained at low angles, a greater range in a resisting atmosphere, than it would 'in vacuo.' This he knew to be the case, with some of his own projectiles, and he had no doubt, that any one who would investigate the matter, would find that the same effect was produced with Mr. Whitworth's projectile also. It was very generally supposed, that the axis of the projectile did not thus preserve its parallelism, but that it continued to move throughout its course, with its point in the direction of the curve. It was impossible, however, to reconcile the observed phenomena of the case, with such a supposition, nor was it possible to conceive, that any sympathy could exist between the direction of the axis, and the curve of the flight. Experiments had been quoted, of rifled projectiles having been fired with such small charges, as to allow of the projectile being distinctly seen in its course through the air, and it was said to have been clearly perceived, in such cases, that the axis followed the curve; but in all Sir W. Armstrong's own experiments, the indications attending the graze marks, and the form of the holes made in distant targets, led to a contrary conclusion. In fact, it was easy to understand, that the eye might be deceived, by the impression left on the retina, by an object thus rapidly moving, and producing the illusion of a sort of tail following the direction of the curve.

The slight difference in range, between a small bolt and a large one, was another circumstance, which could only be explained upon the assumption of the axis being diagonal to the curve. Although the longer time of flight, in the case of the smaller projectile, indicated a much less sustained velocity, yet the distance reached was almost identical. This could only be accounted for, by the larger proportion of surface in the smaller bolt, and the greater floatation, (as it might be called,) due to that surface. But that species of floatation implied obliquity, and could not have any existence without it.

The constant deviation to the right, was also in accordance with what had been stated. Owing to the obliquity of the shot, the

air would operate with more effect, in its upward pressure, upon the tapered front, than upon the back, and any tendency to raise one extremity of the axis of a rotating body, resulted in a lateral deflection, as was exhibited in the gyroscope. This cause of the deviation had already been pointed out by Dr. Magnus, a Continental writer, whose conclusions were now generally concurred in.

Now in applying this reasoning to the question of diameter of bore, it would be seen, that although a solid cylinder of small diameter, had a decided advantage, as regarded penetration of the air, over a hollow cylinder of large diameter, yet the hollow cylinder had the advantage in floatation, and the consequence was, that the difference in range was not nearly so considerable as might, otherwise, be supposed.

After a full and careful consideration of all the bearings of the case, Sir W. Armstrong had come to the conclusion, that a bore of 7 inches was about the most generally advantageous, for a 100-pounder gun, not only in reference to the shell, but also as regarded the solid projectile; because a shot of large diameter, by making a larger hole and producing more splinters and greater displacement, was, in general, more efficacious, than a long narrow bolt. A specimen of the cast-iron shell designed for the 100-pounder gun was exhibited. It was cast very thick, in order that it might bear violent impact, without premature fracture, for when a shell broke on striking, the force of the explosion was almost entirely lost. In its present form it contained upwards of 8 lbs. of powder, but if it was cast thinner and longer, the charge could be increased to 12 lbs. The fuze which was used with this shell, was one on the concussion principle, but it differed materially from that which had been already described. From what had been stated, it would be inferred, that a cylindrical shell, fired at a high angle, would fall upon its side, and the fuse must, therefore, be arranged to take effect, whether the blow was upon the point, or on the side.

It was, also, proposed to use for the 100-pounder gun, a built-up shell, similar to that already explained, in connection with the field gun, and this shell, when used against ships so as to burst concussively the instant it had passed through the side, would, probably, be the most formidable projectile ever used in naval warfare.

Although a bore of 7 inches had been adopted for the 100-pounder gun, it might be desirable, for special purposes, when extreme penetrating power was required, to employ a gun of smaller bore. It was manifest, for example, that a shot of 100 lbs. in weight, which delivered its blow upon an area of 20 superficial inches, would exert a greater punching effect, than one that operated upon double that area. A small bore was, therefore, obviously the right thing for piercing thick iron plates, pro-

vided shot of steel, or homogeneous iron were used, but it remained to be seen, whether the expense of such projectiles would not almost prohibit their use. Mr. Whitworth had fallen into an involuntary error, by stating that a shot from one of his guns was the only one that had ever pierced a wrought-iron plate 4 inches thick; but even if such were the case he would not be justified in the inference he had drawn, in the absence of experiments with rival guns of equal weight and bore.

Sir W. Armstrong trusted he had said enough to explain the views by which he had been guided, in the construction of his rifled ordnance, and to show how fallacious it was, to judge of a cannon, without reference to the destructive character of the projectile it was adapted to throw. He did all honour to Mr. Whitworth, for the energy, perseverance, and skill, which he had displayed in this matter, but he denied that his results had proved the superiority of his system. There was, in fact, much less difference between the two systems than was commonly supposed. Both Mr. Whitworth and Sir W. Armstrong adopted loading at the breech, and both used a fitting shot. Mr. Whitworth closed the breech of his gun by an external screw, while Sir W. Armstrong used an internal screw for the same purpose. The former swung back the entire breech on a hinge, while the latter withdrew a stopper. Then as regarded construction, Sir W. Armstrong's original gun, made more than five years ago, and which was still in existence, consisted of an internal tube of steel, jacketed with wrought-iron coils. Mr. Whitworth's 80-pounder gun, now brought forward, was understood to consist of a tube of homogeneous iron, (differing from steel more in name than in reality,) jacketed in the same manner with wrought-iron coils. Mr. Whitworth would correct this statement if it was erroneous. The greatest difference between the two guns was in the rifling; but whatever might be the practical considerations affecting the two methods, theoretically speaking, so long as the requisite rotation was given to preserve the stability of the projectile, it was a matter of indifference what form of rifling was employed. Sir W. Armstrong conceded nothing to Mr. Whitworth even in regard to range. If the question at issue was one of range, let it be so understood, and let each party make a gun and projectile for the attainment of that object; but the real question was, or ought to be, which system would inflict the greatest amount of injury upon an enemy, and upon this ground Sir W. Armstrong was ready to meet Mr. Whitworth in friendly contest. If it should be replied, that Mr. Whitworth did not contend with Sir W. Armstrong in regard to projectiles and fuzes, then he would say, Mr. Whitworth was at liberty to adapt to his guns, if he pleased, the shells and fuzes then exhibited. When that had been done, let them meet at Shoeburyness, and fight the

matter out in a quiet business-like way. The targets there would represent enemies, both at long and short distances, and they could have earthworks, and masonry, iron plates, and timber-framing, to operate against. It would then be seen which system was best adapted for the defence of the country; and whatever the public might be induced to believe, Sir W. Armstrong trusted that, in the mean time, the Institution of Civil Engineers would withhold its judgment, until such a trial had been made.

In answer to questions from various Members, put through the President :—

Sir W. Armstrong stated, that the weight of the 12-pounder gun was 8 cwt., and the pitch of the rifling was one turn in thirty-eight diameters. The ordinary charge of powder was one-eighth the weight of the projectile, or $1\frac{1}{2}$ lb. of powder for the projectile now used in that gun. The chamber was made a little larger than was sufficient for that charge; in fact, it was of a capacity to admit of the largest charge, compatible with the permissible recoil of the gun. No inconvenience had been experienced from having a little vacancy in the powder chamber.

The index of the short-range sight was graduated for distances of 50 yards, by actual trials of the range at two, or three points, and interpolating the other divisions.

The initial velocity of the shot was about 1,100 feet per second.

The weight of the 100-pounder gun would be about 70 cwt.

The outer layers and rings of metal were not put on with any calculated degree of tension; they were simply applied with a sufficient difference of diameter to secure effective shrinkage.

The grounds for concluding that the range, through a resisting atmosphere, was greater than 'in vacuo,' were, that at a low elevation the actual range exceeded the parabolic range.

The ship guns, now in progress of construction, were on the same principle as the 12-pounder exhibited.

The longest range, hitherto obtained, was upwards of 9,000 yards, with a 32-lbs shot and a charge of nearly 6 lbs. of powder, from a gun, 4 inches in bore. In cases of extreme range, the state of the wind was an important consideration. The time of flight of that shot was not ascertained, as the graze was too distant from the gun to be seen. That distance must not be looked upon as the maximum range attainable with guns of this construction.

The bore was not of uniform diameter throughout the gun. There was a slight contraction immediately in front of that part, where the shot was lodged, the object of which was to mould the lead covering into the grooves to a sufficient depth, at the first instant of motion, to give the shot freedom in passing through the remainder of the bore.

The lead covering had not been observed to be stripped from the shot, by using very heavy charges of powder.

With trained men the 12-pounder gun could be fired three times in ninety-five seconds, with such accuracy as to strike a target each time, at a distance of 1,200 yards. Without taking aim, the rapidity of firing would, of course, be considerably greater.

A pressure of several tons was required, to force the shot through the contracted part of the bore; but after it had passed that part it was free. It was a mistake to suppose, that any loss of velocity was occasioned by that resistance; because the more the shot was held back, the more the pressure accumulated behind it. To obtain the maximum effect from the charge, the shot would require to be held back, until the combustion of the powder was perfected. The resistance had been varied, by increasing and diminishing the thickness of the lead coating upon the projectile, and it was found that the range was rather increased by augmenting the resistance. No difference in the recoil could result from the increased resistance. The recoil was due, simply and solely, to the weight and velocity of the shot from the gun. Nothing affecting the shot, whilst in the bore, could increase the recoil, unless it augmented the velocity. It was merely a question of action and reaction; and the quantity of momentum, generated in the shot, must have its exact equivalent in the recoil of the gun.

As far as experience at present demonstrated, a length equal to about twenty-five diameters of the bore, appeared to be the best proportion; that length might, under certain circumstances, be somewhat reduced, without much sacrifice of velocity. Whether that rule held good for all diameters, remained yet to be proved.

No inconvenience had, hitherto, been observed from heating. About a year ago, when the wet sponge was used, one hundred rounds were fired in succession from an 18-pounder gun; beginning at the rate of one round in two minutes, and gradually accelerating to one round in forty seconds. At the end of the experiment the gun was not objectionably heated. There would not appear to be any reasons for a difference in the heating of these guns, and of those of ordinary construction. The smaller charges would compensate for the diminished quantity of material in the gun.

In a recent trial, at Shoeburyness, a plate of wrought iron $4\frac{1}{2}$ inches thick, 6 feet long and 3 feet wide, had been entirely broken up and destroyed, by eight shots of 80 lbs. weight, fired from one of Sir W. Armstrong's guns, having a bore, 6 inches in diameter; the weight of that gun was 63 cwt., and the charge of powder was 10 lbs. Six of these projectiles were of cast iron, and the other two were of puddled steel. Only two of them actually pierced the plate, and none of them penetrated both the plate and the timber back-

ing. On a former occasion, on firing against wrought-iron plates 4 inches thick, upon a floating battery, two of the shot traversed both the plate and the timber, in addition to which, one of them cut through some strong ironwork within the ship, and drove two pieces of the iron plate across the deck, lodging them in the timber on the opposite side. That was done at a very short range, less than 400 yards, and with flat-ended shot, such as Mr. Whitworth had previously used. With rifled projectiles of heavy weight, the velocity was so well maintained, that there appeared to be no sensible difference in penetrating power, between 50 yards and 500 yards. Still Sir W. Armstrong would admit, that up to the present time, no gun of his construction satisfied the conditions requisite for thoroughly and completely punching through thick wrought-iron plates. There was, however, no doubt that it could be done with a special gun. It was only a question of bore, and of a hard material, such as steel, or homogeneous iron for the shot. But the expense of such materials would almost prohibit their ordinary use.

Sir W. Armstrong concluded by describing the principle of the Nictoscope, an ingenious instrument designed by him for enabling the gunners to maintain a fire upon any given object after night-fall. The principle of the instrument was to render a false object in the rear, or at one side, visible upon a vertical line in a mirror, when the gun was laid upon the true object. A lamp attached at night to the false object, became visible upon the same mark in the mirror, when the gun was in line with the true object. The vertical adjustment for elevation was effected by a spirit-level clinometer, forming part of the instrument.

Captain NOBLE, R.A., corroborated the statements of Sir W. Armstrong. The computed initial velocity of the projectile was 1,080 feet per second, with a charge of powder one-eighth of the weight of the shot. With a 32-lbs. shot and a charge of powder of $5\frac{3}{4}$ lbs., at an elevation of 35° , the range from a gun with a bore of 4 inches, was 9,175 yards.

Mr. BIDDER,—President,—suggested experiments for accurately ascertaining the velocity of flight of the shot; the electric telegraph afforded means for making them.

Mr. J. LOCKE, M.P., (Past-President,) thought the Meeting would lose a great advantage, if they did not ask Mr. Whitworth to repeat some of the explanations as to the experiments he had made, in order to contrast the results with those which had just been stated by Sir William Armstrong. Mr. Whitworth had given a very interesting account of his experiments; but there were still others of not inferior interest which had not been given, and he thought the present would be a fitting opportunity for hearing them. There were many apparent points of difference between the two guns,

which required explanation. Those differences seemed to consist, chiefly, in the mode of constructing the guns, in the system of rifling, in the form and dimensions of the chamber, as compared with those of the gun itself, and also in the different modes of closing the breech. All these were matters of importance and value, and being himself a non-professional man, in the art of war, he should like to hear those matters explained by the two gentlemen who had invented the systems. There was supposed to be a certain amount of friction in the passage of a projectile sheathed with a leaden coating, through the rifle grooves; and these latter were also understood to be liable to be, if not stopped up, at least, to become foul. All these points had occupied attention. Mr. Whitworth had stated, that there was no fouling in his gun, because with two metal surfaces working against each other, and a lubricating wad, no such effect could be produced. These were extremely interesting questions, and he thought this was a suitable occasion for Mr. Whitworth to give some explanation of the points to which he had alluded.

Mr. WHITWORTH said, he was ready to answer any questions that might be put to him, but as he had already explained the principle of his gun, he submitted that it would scarcely be in good taste to interfere with the explanations which had been given by Sir W. Armstrong, or to interrupt the lucid replies which he had made to the questions that had been asked. Still if it was desired, he would extend his remarks, describing his own gun, and pointing out the difference between it and Sir W. Armstrong's gun. With reference to a trial of their respective guns at Shoeburyness, he should be most happy to meet Sir W. Armstrong there, and should only stipulate, that some members of the Civil Engineering profession should be present at the trial, which he hoped would result, in securing for the country the best and most effective gun that could be manufactured.

Colonel EARDLEY WILMOT, R.A., said, he might perhaps be allowed, as an Artillery Officer, to express his acknowledgments to Sir W. Armstrong, for the able and lucid explanations just given of the new breech-loading gun. He was sure his brother officers could not be otherwise than highly gratified with those explanations. Upon one point especially, Sir W. Armstrong had done good service, by urging so strongly, that, provided a good form of rifled projectile was used, the particular form of the grooves in the gun was immaterial, as regarded any marked difference in the accuracy of the weapons, with shot of the same material. It appeared now, that the great points to be attended to were, simplicity of manufacture and cheapness; because the day would come, when cost would form a more important element than it appeared to do at present.

Sir WILLIAM ARMSTRONG mentioned, that the cost of the 12-pounder gun now exhibited, complete in all respects, was less than that of the old 9-pounder brass gun.

Colonel WILMOT explained, that whilst the cost of labour upon a 9-pounder, or 12-pounder brass gun was almost nominal, it must be remembered that, after it was unserviceable, or obsolete, it was almost as valuable to sell, as old metal, as it was before.

Sir WILLIAM ARMSTRONG trusted that his guns would be almost everlasting.

Colonel TULLOH, R.A., called attention to the important advantage possessed by the Armstrong gun, in having a muzzle of so small a diameter, as to allow of the port holes of vessels being much reduced in size, probably to one-half what they were at present.

Mr. WHITWORTH, in compliance with the request of the President and many of the Members, exhibited one of his 12-pounder breech-loading guns, and specimens of the different projectiles, solid and hollow, which he used with his rifled guns of different calibres. He observed, in reference to his system of projectiles, that it had been said, he obtained long range by excessively reducing the diameter of the shot. To show that this was incorrect, he exhibited one of his 12-pounder shot which had been fired. The minor diameter of this shot, measured across the flats, was $\frac{1}{10}$ th less than 3 inches, while its major diameter, measured across the angles, was $\frac{2}{10}$ ths greater than 3 inches. This was larger than Sir William Armstrong's 12-pounder shot, the diameter of which was only 3 inches. It was, therefore, incorrect to state that he sacrificed capacity to obtain range, and that his gun was more suited for shot, than for shell. The contrary of this was the case. His 12-pounder shell was 12 inches long, and the diameter, as before stated, was $3\frac{2}{10}$ ths inches. Now it was obvious, comparing the solid and the hollow projectiles, that the increased length of the hollow projectile must add to its capacity and effectiveness. The shell might be built up inside, on the segmental plan, or shrapnel might be used; but he should prefer a simple hollow shell, such as he exhibited, as the minor diameter being slightly smaller than the major, it would, necessarily, split up along the centre of the flats, as they were the weakest parts. When the shell was fired, it was broken into the desired number of fragments, according to the grooves inside, and the size of the fragments might be regulated, so as to make them very effective projectiles. The number of fragments into which a shell was broken up, was not a correct test of its destructive power, as very many of the pieces might be too small to be effective. With regard to the ignition of this shell, the percussion system and a moveable striker could be employed, on the plan described in the specification of Mr. Tucker, in 1829, afterwards adopted by

Mr. Lancaster, and subsequently patented, with improvements, by Sir W. Armstrong. Mr. Whitworth could use the more simple and certain time fuze, which was placed in front of the shell, and was ignited by the flame of the powder charge. By removing a portion of the outside of the shell on two opposite edges, the flame passed by the projectile, and readily ignited the time fuze in front. This shell was, therefore, very simple, and its cost did not much exceed that of the solid shot.

Before proceeding with further remarks, induced by the President's request that he should exhibit one of his large guns, Mr. Whitworth requested the Members to excuse any repetition of the observations he had previously made.

Mr. Whitworth then explained, that he had used a tubular shot for penetrating wood and elastic materials, by which a core, or cylinder of wood, nearly 2 feet long, and $1\frac{1}{2}$ inch in diameter, had been cut out of a solid oak block, through which the tubular projectile was fired. The tubular was also the best form for penetrating masonry. The flat-fronted shot, which he showed, was like those that were fired through 30 feet of water and 8 inches of oak, at Portsmouth, from a brass howitzer rifled by him in 1856. He employed the same form, with malleable, or homogeneous iron shot, for piercing thick iron plates. He might mention, that having been the first to use the flat-fronted shot, he, for self-protection, included it under a patent, but had placed it, with other improvements, at the disposal of the Government. Sir W. Armstrong had told the Meeting that he had now adopted this form of shot, for a similar purpose.

Another form of shot was the spherical, rifled hexagonally. It was thought, that spherical shot would be best for ricochet; but Mr. Whitworth's experiments showed, that the improved elongated form of shot, he now used, was superior in that respect. These different projectiles, both of shot and shell, though of various lengths, could all be fired from the same gun, as occasion required.

Mr. Whitworth then described some projectiles, which were merely intended as experimental shot. The first was a shell, ten diameters in length, which was fired from a 24-pounder howitzer, rifled by him in 1856. The diameter of the howitzer was 4 inches, and the twist was one turn in 40 inches. The spherical shot and this experimental shell, were instances of two extremes, with regard to length; the one being one calibre, the other ten calibres long. Both could be fired from the same gun, as it had no chamber. The best practical length of shot was between the two extremes. By actual trial of various proportions, he had ascertained which length gave the greatest momentum, and that he adopted, not because it gave the greatest range, but because it produced the best general effect.

The metallic cartridge, which he showed, was made of tin plate, and had a rifled shape to fit the bore. When it was inserted in the gun, it formed a lining within which the charge was fired. The powder, therefore, instead of acting against the sides of the gun, acted against the inside of the cartridge. This saved the gun; and moreover, when the cartridge was withdrawn after the discharge, it brought away with it the fouling deposit. A small hole was made in the rear of the cartridge case, through which the fire from the friction fuze was flashed to the powder. The case was filled with powder to within about $\frac{1}{2}$ an inch of the open end. It was then closed by a wad, of lubricating material, which, when the charge was fired, was distributed over the interior of the gun. This obviated the necessity of sponging out, which had always been a great inconvenience in working guns. He believed this plan of obviating the necessity of sponging, by the use of the wad of lubricating material, had not been used, previously to his adopting it.

Mr. Whitworth proceeded to show the mode of working the gun. The gun was loaded by unscrewing the breech cap, and inserting first the shot, and then the metallic cartridge, containing the powder and lubricating wad. The breech cap was then screwed on the gun, by a few turns of a handle. Supposing the gun to have been discharged, the cartridge case was withdrawn, by a small and ingenious instrument, bringing with it the fouling residuum. Besides the convenience and cleanliness of this plan, it, as before stated, preserved the gun from the action of the powder. Those who had witnessed the operation of loading could form their own opinion, as to the conditions under which a simple, hard metal shot was propelled through the six-sided bore, when well lubricated, and could compare them with those, under which a lead-coated projectile was driven through the fluted bore, and had to cut its way through the thirty-four grooves. It had been stated, that there was not much difference between the Whitworth hexagonal gun and the Armstrong fluted gun. In that statement Mr. Whitworth did not agree; and in order to enable the Members to judge for themselves, he would point out some of the differences between the two systems. As to material. The hexagonal gun was made out of a single ingot of homogeneous iron, forged under the ordinary tilt hammer. This material partook of the toughness of copper and the hardness of steel. The Armstrong fluted gun was a compound piece of forging, made by welding up separate bars of iron, requiring special furnaces of very great length, and special forging machinery. The iron was fibrous, and comparatively soft, and could not be expected to last as long as the tougher and harder homogeneous iron. Of the admirable workmanship of the Armstrong gun, which had been made at Woolwich, there could be but one opinion; and the greatest

credit was due to Mr. Anderson, for the skill with which he had, in so short a time, organised an establishment capable of turning out such work. In weight, the guns were equal. In bore, the major diameter of the hexagonal gun was $3\frac{2}{10}$ inches, that of the fluted gun was $3\frac{1}{10}$ inches, the rifle twist being one in 60 inches in the former, and one in 114 inches in the latter. Consequently, twice as much rotation was given to the Whitworth projectile, which the results of repeated experiments had shown to be highly desirable. As to form of bore, the Whitworth was a six-sided spiral, while the Armstrong was a spiral with thirty-four fluted grooves. The hexagonal gun was rifled uniformly throughout its entire length, and required no chamber. The fluted bore was not rifled uniformly, having a contracted part near the chamber; and the chamber itself being of a fixed length, limited the length of the projectile and the charge of powder that could be used. In the Whitworth gun, the breech was closed by a cap, working in a hinged hoop, and fitting on the end of the gun, with an external screw. In the Armstrong gun, a slotted recess received the moveable breech-piece, and behind that, a considerable length of the gun was taken up by an internal screw.

As to the projectiles, the Whitworth hexagonal gun fired both shot and shell, of various lengths and forms, to suit special purposes. The shells were of great capacity, and might be ignited, either by the simple time fuze, like the ordinary shell, or by employing the percussion system. From the Armstrong chambered fluted gun, shot, or shell, of a fixed length only, could be fired; and as the lead was forcibly driven through the grooves, there was no windage, and the flame could not pass by to the front. Hence the percussion system of strikers was required. It was a mistake to suppose, that any serious loss arose, from allowing a small amount of windage. Mr. Whitworth had fired, from the same gun, an iron shot, rifled on his plan, (in which a small amount of windage was purposely allowed,) and leaden shot of the same shape and size. The leaden shot was, necessarily, expanded by the explosion, until it filled the bore, and was propelled without there being any windage at all. But, although its specific gravity was greater than that of the iron shot, and it had no windage, its range was not nearly so good as that of the iron shot. Again, in the hexagonal gun, the resistance offered to the projectile at starting, was only that due to its 'vis inertię;' and the only retardation, being that caused by the rifling, imparted an increased efficiency to the projectile. In the fluted gun, it appeared, that a pressure of several tons was required to start the shot; it had, moreover, been said, that this did not cause any loss in range, it being, on the contrary, supposed to be advantageous, to retain the shot and to give the gases of explosion a longer time to act. If that were the case, it would seem preferable to

retard the shot, by giving it increased rotation, and therefore increased power, rather than to expend a pressure of several tons, in forcibly rifling the shot. This brought under consideration another important difference between the two systems. The mechanically fitting hexagonal projectile was rifled by machinery in the workshop; whereas the lead-coated projectile, used with the fluted gun, was rifled by the explosion in the gun itself. On the economy and advantage of the former plan, it was not necessary to dwell. In the hexagonal gun a metallic cartridge was employed, which, both in practical use, and for carriage and storage, had many advantages over the old flannel bag, which was still retained with the fluted gun.

As to practical results, Mr. Whitworth did not now propose to carry out the comparison. But something ought to be said as to range, which he was surprised to hear undervalued. Without attaching too great importance to mere range, it must be admitted to be a very good measure of what the gun could do. If at an elevation of 7° , the range of the fluted gun was 2,495 yards, and the range of the hexagonal gun was 3,107 yards, the trajectory of the latter was flatter, and the errors in judging distance were of less importance, as during a greater portion of its flight, the hexagonal projectile was nearer the ground. This perhaps would appear more plainly, by comparing the range of the fluted 12-pounder gun at 9° , which was stated, on good authority, to be 3,000 yards and upwards, with the range of the hexagonal 12-pounder at 7° , which was 3,100 yards and upwards; now considering the ranges as about equal at these different elevations, the advantage of firing the hexagonal gun at 7° , as compared with another gun, which, to attain a like range, required to be elevated to 9° , was obvious. The gun which had the longer range and the flatter trajectory was more likely to hit a distant object, than another gun which had one-fifth less range, for the same elevation.

In constructing the rifled cannon, Mr. Whitworth had only applied the same principles which were found to be successful in the rifled musket. The results of the late experiments at Southport, on the Lancashire coast, though many of them were made without any previous trial, were such as he was justified in expecting, knowing how certain and simple were the principles on which reliance was placed. In gunnery, as well as in other things, especially those depending on mechanical principles, to simplify was to improve. The special object he had in view, in carrying out the system he had endeavoured to explain, was to reduce the principles, on which it was based, to their simplest elements.

Mr. Whitworth stated, in reply to a question from the President, that the charge of powder for the 12-pounder gun was $1\frac{1}{4}$ lb.

Sir WILLIAM ARMSTRONG said, the comparison as to ranges, which had just been brought forward, was with $1\frac{1}{4}$ lb. of powder in

the Whitworth gun, against $1\frac{1}{2}$ lb. in the Armstrong gun. As a general observation, he objected to comparisons of ranges and results, unless the experiments were made at the same time, under the same circumstances, and with the same object in view.

Mr. W. B. ADAMS,—through the Secretary,—said, the proposition of Mr. Longridge, to make a gun that could not be burst, was, evidently, the true basis on which to work, in the improvement of artillery. It must be clear, that all power generators, or receptacles, must be strong enough to hold and to maintain the power, while applying it. To make the strongest gun, required the strongest material, if it was practicable to use it, and drawn wire was the strongest at present known. Mr. Longridge had succeeded in making the lightest gun yet produced, of great transverse strength, by an endless skein of wire; and the problem of longitudinal strength by endless skeins of wire was, if needed, not difficult of solution. Nor need there be any fear of the wire becoming loose, for it might be of tempered steel, giving constant reaction against strain, and it might, moreover, be soft-soldered together to prevent it from unwinding. For the purpose of transport, it was evident, that lightness was a most important quality, but for use in the field another quality was required, weight to induce enough inertia, so that the whole of the force of the powder might be expended in moving the projectile, while the gun remained unmoved. And this weight was best applied over the whole surface of the gun, and not merely at the breech; otherwise, the jar on the light muzzle would disturb the accuracy of aim. For this reason it had been, that English duelling pistols, and American rifles, had usually been constructed with heavy and nearly parallel barrels. As regarded the question between built guns and solid forgings, the present practical condition of the art of forging made the former mode preferable; but it was probable, that ultimately, a mode of welding by jets of intense gas flame, instead of by furnace heat, would enable the manufacturer to pile any mass of iron together in perfect welds, without any oxidation of the surfaces internally, or, which amounted to the same thing, the malleable iron could be cast, by the Bessemer process, to any size, and subsequently hammered. Still, if a large gun, easily transportable by land, was required, it was a problem whether a gun of given lightness could be made stronger of wire, or of solid metal; and this was an important problem, for the projectiles of enormous bulk and weight, which would, henceforth, be required for siege purposes.

In the modern use of guns a circumstance had arisen, varying from the old practice. The powder was so highly explosive, that it became a violent sudden shock, instead of an expansive force, like steam in a cylinder. While loosely fitting round shot were used, the strain in the gun was relieved by windage. With

the modern elongated shot, expanding lead into rifle grooves, windage was prevented, and the strain was at its maximum. The powder of ancient times was slow burning, and gradually expansive, and for that reason the guns were made of great length, in order to expend the whole power on the shot, previous to its leaving the gun. The advantage said to be obtained, by what was called a 'gaining twist,' in rifle guns, afforded a strong inference, that sudden shocks were considered mischievous. It was, therefore, worth while to consider how powder might be used with an increasing pressure, without a sudden shock; possibly, by firing it from the front portion, next the projectile, as in the Prussian needle gun, instead of at the rear, as was usual with all cannon.

The next question was, as to the best mode of stopping windage. At present, the approved methods were by violently and explosively moulding a leaden plug into the bore of the barrel, or by fitting a long bolt by machine tools. Now a rigid fit was not used in a steam cylinder, which was analogous to a gun in its operation; elastic steam being used instead of elastic gas. The piston of a steam cylinder had an elastic packing, involving little friction, without suffering any steam to escape past it. An important element in the construction of an elongated shot was, to keep its longitudinal axis parallel to the bore of the barrel, and this was by no means certain with the leaden plug, which might yield more on one side than on the other, with the sudden explosion. A solid iron shot would not be subject to this difficulty, but the fit would vary and the windage would increase as the gun became heated.

In regard to the question of rifling the bores of guns, which seemed to be generally assumed as a principle not to be dispensed with, it was still doubtful whether further experiments would not dispose of it altogether. In truth, rifling was simply a contrivance for correcting the defects of badly constructed projectiles, at a considerable waste of propelling power. If the centre of gravity of a spherical shot was not in the exact centre, it would, when put in motion, move in a curved line, instead of a straight line. As it was a mere matter of chance, where the bias of a shot would be in the gun, it was uncertain where the projectile would fly to. The spinning motion given by the rifling, to some extent corrected this. But if the centre of gravity was exactly in the centre of the shot, there seemed to be no apparent reason, why it should not take the exact direction given by a true propelling cylinder. With elongated shot, as commonly made, not only might the centre of gravity be out of the line of the central axis, but almost invariably, when made with its sides parallel and with a conical point, it would, when placed in the gun, have the centre of gravity behind the middle of the length. Therefore, on leaving the gun, it would try to turn over, to get the heaviest end foremost, and to

prevent this, a large quantity of power was expended on rifle-spinning it.

But if the centre of gravity was far in advance of the middle of the length, there would not be any tendency to turn over, and if the centre of gravity was in the central axis of the gun, and the chase of the gun was long enough to determine the line of flight, there seemed no probable reason why the projectile should fly astray.

There was some analogy in the form of the arrow, or 'cloth-yard shaft,' used with the long bow. The diameter was to the length less than 1 to 72. The centre of gravity was, forwards, two-thirds of the length, and the steel head gave it a slight additional weight. The longitudinal axis was perfectly straight, and in the centre of the diameter all through, and two, or sometimes, three feathers at the hinder end, corrected all tendency to aberration in the flight. The muscular power with which it was propelled was from 50 lbs. to 60 lbs., and it was stated to have a range of 600 yards, which could be repeated twelve times in a minute. But all depended on the accuracy of construction. If the arrow was crooked, or the feathers were badly applied, the accuracy of flight and the length of range were impaired.

An arrow could not, for many obvious reasons, be fired from a gun. The problem therefore, was, how to make a metallic arrow which should carry its weight chiefly in the head, which should keep its longitudinal axis in the longitudinal axis of the gun, which should fit the bore elastically, with sufficient tightness to prevent all windage, without involving unnecessary friction, and with the propelling power applied near the head, drawing it, as it were, along the chase of the gun, instead of propelling it from behind. When this should be accomplished, and it did not seem a very difficult problem, the length of the gun should be so proportioned, as to consume the whole elastic power in propelling the shot, and not wasting it externally, as in carronades. And the longer the gun, the truer would be the aim and direction.

The advantages of getting rid of the rifling process would be, a gun more easily made, more easily kept in order, and considerably stronger, with a given weight of metal. But it was also important, that the shot should be in a convenient shape for stowage and transport, and moreover, be cheaply and readily produced.

It was not necessary to allude, particularly, to the construction of the Armstrong and the Whitworth guns. Both, if exceeding 3 inches in bore, were of wrought metal, formed by shrinking reinforce rings, or tubes on to a central tube. Both were breech loaders; both were rifled; both carried elongated shot; both were provided with a lubricator in front of the powder; both had their vent on a moveable part, capable of replacement when worn; and both were combinations of parts.

Now, as to the respective merits of the two systems. It was claimed for the Whitworth gun, that it was both a breech and a muzzle-loader, with its one prepared shot, whilst the Armstrong gun was only a breech-loader. This was not important, as muzzle-loading would only be resorted to, in case of something going wrong with the breech, which would, probably, render the gun useless. But the Armstrong shot could very easily be converted to muzzle-loading, by driving it through an iron collar, to strip off the lead covering. As regarded the rifling, a certain amount of surface was essential, to prevent the lead from slipping its threads by the twisting action. With few grooves they would require great depth, whilst by multiplying them, a less depth sufficed, and there was less risk of bursting. With the flat grooves of Whitworth, there was a tendency to draw them into a true circle, when the powder exploded. Theoretically, the outside of the gun should correspond to the inside in the number of the flats.

With respect to projectiles, Mr. Whitworth had the advantage in a hard material, which might be thrown down like ordinary shot without much damage, unless when exposed to rust, which, however, might be provided against by galvanising. The lead-covered Armstrong shot required to be treated carefully, and each would have to be packed in a separate box, to save the lead from damage. The Whitworth elongated shot, tapered at both ends, was much more in conformity with the established rules for passing through fluids with the least resistance, than were the parallel shots with conical heads. The statement of Sir William Armstrong, that the shot always advanced along the trajectory curve parallel to the axis of the gun, with its tail hanging down, simply proved, that it was not in balance, and that the heaviest end hung down in the tendency to get forwards, which was impeded by the spinning movement. An arrow in balance always fled parallel to the trajectory curve.

Mr. Whitworth, in his specification, claimed the original arrangement of a tallow box in front of the powder. Sir William Armstrong, after experience of the disadvantages of washing out the gun, enclosed the tallow in a ball of hemp.

It could not be denied, that with great outlay and a series of long-conducted careful experiments, both these gentlemen had accomplished an amount of precision and length of range never before known with cannon. But the principle of the whole was to be found in the elongated shot, the absence of windage, and the greased projectile, a combination common to all well-made and well-used shoulder rifles; and they had produced gigantic rifles, as perfect in all their arrangements as the best-made philosophical instruments.

Other things being equal, there could be no doubt, that length of range was an important advantage; but for practical fighting, in

the field, on ship board, and in forts, simplicity in the fighting machinery was very important. Breech-loading was important for many reasons, and both Whitworth and Armstrong had done good service, in showing that efficient breech-loading cannon could be made. But there did not appear to be any absolute reason why these breech-loaders should not be simplified. If the chamber where the powder was placed was reinforced externally, to a sufficient size, it might be pierced, at the back of the powder, transversely to the bore, with a slightly conical hole, larger than the bore, and this hole being stopped with a pin, with a slot in the small end with a key to tighten it, and a slot in the large end to loosen it, a perfectly practicable breech-loader would be obtained, without needing any screw whatever, the whole workmanship being of the simplest kind, and suited to the habits of working artillerymen. Screws of accurate workmanship constantly moved, and being liable to the sprinkling of sand, or gravel, would be easily rendered useless. The vent might be made through the coned pin, but it would be better to make it through the gun, near the front of the powder, so as to burn it backwards. Much stress had been laid on the importance of preventing the escape of gas, at the back of the breech. Sir William Armstrong applied a copper cone for this purpose, and Mr. Whitworth was very careful in the fit of the screw cap. But the effectual stoppage of gas, or windage, behind the powder, could not be more difficult than in front of the powder. If a wad of lead, or of papier mâché, were applied behind the powder, it would have precisely the same effect as behind the shot, and with a breech-loader might be more easily moved, after discharge, than the ordinary wad in a muzzle-loader.

No comparative experiments had yet been made, between the results of one of these new rifled cannon, with all its appliances, and elongated shot, and the best possible construction of a smooth-bored cannon, and appropriate shot; considering the range, force, accuracy, and cost. With the rifled cannon, it was said, that the armour plates, $4\frac{1}{2}$ inches thick, of a vessel had been pierced. There was little doubt, that a smooth bore driving the projectile with equal force, would better punch a hole, by reason of the absence of the spinning movement. If a revolving punch was applied to an ordinary punching machine, it was more than probable, that the punch would be broken.

In naval warfare there were, up to the present time, two modes of fighting; 'long-bowls,' and 'yard-arm and yard-arm.' For the latter purpose, breech-loading guns of great weight, ending at the muzzle in a large ball fitted to a close socket in the ship's side, with a wheel behind, on which to traverse about and depress, would be a good arrangement, leaving no opening, except at the front sight of the gun. There would be no smoke aboard, and by means

of water hose, the guns could be rapidly cooled down when required. Means being taken to prevent the recoil, two men could work a gun as easily as a dozen, without being exposed to shot from the enemy.

For 'long-bowls,' heavy non-recoiling artillery, probably mounted with a central ball, in a socket, instead of trunnions, and capable of moving vertically and horizontally, would best serve; and when that was accomplished with a gun of ordinary calibre, attention might be given to the idea of Mr. Mallet, of throwing into fortresses shells large enough to be termed 'portable mines,' only, in order to get the best result, cylinders must be used instead of spheres, for projectiles.

Objections had been taken to the great cost of the Whitworth and the Armstrong guns. If this cost were essential to great and undoubted superiority, the English nation ought to rejoice at it, as giving at once the advantage to their long purse. But if an equal effect, or a greater effect, could be produced by simpler means, then it would be a sorrowful disadvantage to the English nation to make expensive guns, while their foes were making cheaper ones.

Upon the whole, the improvement in artillery was favourable to the cause of humanity, for fortresses, the strongholds of irresponsible power, would be no longer defensible. Long-range guns of enormous power of propulsion would explode them, and their only defences would be mounds of earth, easy for numerous assailants to swarm over. The cost of armies must be enormously augmented, by increased numbers, when they could no longer be sheltered by fortresses, and their expense would, ultimately, be destructive to the nation depending on them.

Meanwhile, the threshold of improvements in artillery was only now reached, and the subject had yet to be exhausted, before it would be safe to rest upon the present amount of knowledge.

Mr. LONGRIDGE said, before replying to the remarks which had been made, in the course of the discussion, he might be allowed to refer, for a moment, to a matter personal to himself. He was sorry that anything in his Paper should have been construed into an attack, either upon military authorities, or upon the Select Committee on Ordnance, at Woolwich. From some of the observations, it appeared as though he had drawn up the Paper with the express purpose of decrying military men and exalting his own profession, or of venting on the Select Committee the spleen of a disappointed inventor. If that had been his intention, he would have been unworthy to be a Member of this Institution; and he was sure that had the Paper been justly liable to this reproof, the Council would never have allowed it to be read. But in point of fact, not a word in his Paper could be fairly construed in that sense. What he said was this; that the Civil Engineer,

from his daily practice, was more conversant with the properties of the useful metals, than his military brethren ; and might, therefore, be allowed, without presumption, to discuss the question how to make a strong gun. He still adhered to that opinion. With regard to the Select Committee, he had the highest respect for certain members of that body ; and amongst them, for the gentlemen who had taken part in this discussion. It had been remarked, that it was not right to attack an absent body. Even if that had been done, the Committee had ample opportunities of defending itself, through such of its members as were invited to be present. It had, moreover, been said, that whether the matter, brought by Mr. Longridge before the Committee, had been fairly dealt with, or had received due consideration, was open to fair argument. He thought he could show, by a few observations, that it had neither been fairly dealt with, nor fully considered. When he said this, he did not intend to convey any charge of personal unfairness ; but he contended, that the project did not meet with that consideration, to which it was entitled from the experiments and results, laid before the Committee. He thought too that, personally, he had not been quite fairly dealt with, by members of that Committee who had taken part in the discussion. It had been represented, as though he had sent a gun to be reported upon by the Committee. He did no such thing. All that he placed before them was a simple brass cylinder bound round with wire. In introducing it to their notice, he took care to state, that it was not a gun, but merely an illustration of what he conceived to be a means of obtaining great circumferential strength in a gun. He also handed in a written description of how guns might be made upon that principle, and also how the trunnions were to be attached. That written description was accompanied by two drawings,—one of a 32-pounder and the other of a 10-inch howitzer,—showing how the trunnions were to be attached. He thought, then, that after explaining precisely what the construction of the cylinder was, (Fig. 12, page 308,) it could not be said, that his plan had been fairly dealt with, when this wire-bound brass tube was virtually hung by the muzzle and fired with 2 lbs. of powder ; the usual proof charge for a 3-pounder gun being $1\frac{1}{4}$ lb. Under such circumstances, it could not be a matter of surprise to any one, that the breech should have been blown off. He believed, that a trial with the heaviest gun in the service, under similar conditions, would have been attended with the same results. Therefore, he submitted, he had made out a case, that the invention in question had neither been fairly tried, nor fully considered. In further proof of this, he might state, that he held in his hand a letter from a very influential member of the Select Committee, and one who had taken part in this discussion, written in September, 1855. He said :—"I believe it is generally thought, that the experiments

made, do not go towards any disapproval of the leading idea of your proposition."

Before entering upon the subject of the Paper, he begged to be allowed to make a few remarks, upon one, or two matters which, incidentally, had come before them. He had been greatly disappointed that, in the course of the discussion, particularly, considering the presence of so many gentlemen of eminence, in this department of science, they had heard so little upon the subject of the effect of twist in rifled guns. Several descriptions of guns had been brought forward, varying greatly in the character of the twist, and for each class, superiority was claimed. But no clear explanation had been given of the effect of the twist, or indeed, what it was sought to effect. A note on this subject was appended to the Paper,¹ and he would now only briefly refer to such of the causes of deviation, as were affected by the rifling of the gun and shot. He believed a misconception existed upon that point, and therefore, he was glad of an opportunity of saying a few words upon it. The first object, in rifling a gun, was to correct the deviation, which was due to the want of symmetry in a projectile. When a projectile was thrown from a gun, if the resultant of the resistance of the air acted through the centre of gravity, there would be no such deviation; but inasmuch as an exactly symmetrical shot could not be practically obtained, when the shot met the resistance of the air, it was deflected to one side, or the other. To correct that deviation was the primary object of rifling; and it was supposed, that by causing the rotation of the shot, it would describe a series of spirals round the prolonged axis of the gun. That he believed was incorrect. The projectile did not describe spirals, but a series of curves, the general direction of which was a straight line, deviating uniformly from the prolonged axis of the gun, and the projections of which, on a plane at right angles to the axis, were of a cycloidal form. It was a mistake to suppose, that the shot described spirals round the prolonged axis of the gun. It made a deviation to one side, or the other, dependent upon the first impulse of the air, as the shot passed from the gun. A small amount of twist was sufficient to counteract that deviation. Taking, for instance, the three guns which had been more prominently brought before their notice: viz., Mr. Haddan's gun, with a twist of one turn in 40 feet; Sir William Armstrong's gun, with a twist of one turn in 9 or 10 feet; and Mr. Whitworth's gun, with a twist of one turn in 5 feet. In a distance of 1,000 yards, Mr. Haddan's shot would make sixty turns, Sir William Armstrong's, three hundred turns, and Mr. Whitworth's, six hundred turns

¹ *Vide Appendix, page 325, ante.*

The cycloidal curves, he spoke of, were very minute indeed, varying from $\frac{1}{30}$ th to $\frac{1}{3000}$ th part of an inch. The shot made a deviation equal to the length of the base of one of these curves, in each revolution. In a range of 1,000 yards, the deviation, in the case of Mr. Haddan's gun, would, therefore, be 2 inches; in Sir W. Armstrong's, $\frac{4}{10}$ ths of an inch; and in Mr. Whitworth's, $\frac{2}{10}$ ths of an inch. Consequently, a very small amount of twist was sufficient to correct the deviation, arising from the cause now under consideration. The next cause of deviation was due to the position of the centre of gravity of the projectile, and was well exemplified in the gyroscope. If the centre of gravity was behind the centre of resistance, the shot would deviate in a long spiral to the right hand, in a right-handed twist, and to the left, in a left-handed twist. If the centre of gravity was in front, the reverse would be the case; and the greater the velocity of rotation, the less would be the amount of deviation. To get rid of this deviation, great velocity of twist was required. This deviation was always due to the position of the centre of gravity. By arranging the centre of gravity as nearly as possible in the centre of the figure, this deviation would be, to a great extent, avoided.

There was another cause which, no doubt, operated in a considerable degree; the rolling motion of the shot upon the air. If a cylinder was rolled between two surfaces with different pressures, it would move itself in the direction of rotation, along the plane where the pressure was the greatest. The motion of a shot revolving and falling in the air, was of that character. The resistance on the top was less than on the bottom, and the rolling motion tended to move the shot to the right hand, if the gun had a right-hand twist. This deviation was greater, as the twist was greater; consequently, if he was correct in this position, it would appear, that the great amount of twist given to some of these guns, if really necessary, was only rendered so by the position of the centre of gravity in the shot. He could see nothing wrong in the reasoning which had led him to the principles he had laid down. This reasoning was fully given in the note before referred to.

There was another deviation, which was not confined to rifled guns, but was common to all ordnance; that was, the want of some means of adjusting the trunnions perfectly horizontal. In a long range, with high elevation, it was a very material point. If the trunnions deviated only 1° from the horizontal line, the elevation being 35° , a range of 8,800 yards gave a lateral deviation of 110 yards, and with an elevation of 10° and a range of 4,000 yards, the deviation would be 12 yards. Consequently, an error of 1° , which was so slight as not to be detected by the eye, gave

rise to important deviations in firing. He thought, that in any comparative experiments with guns, that point should be carefully attended to.

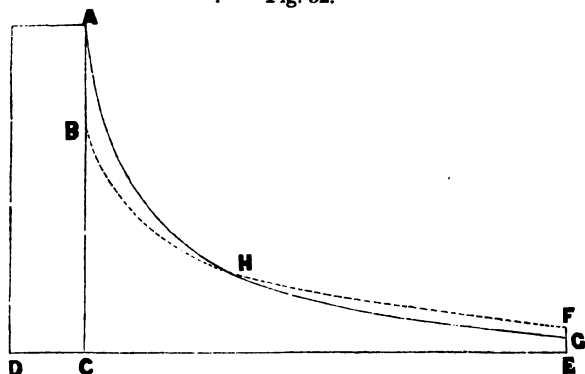
It was not difficult to calculate the amount of force required to give the twist. It was inconsiderable, even in the case of the Whitworth gun. He could not, however, agree with the statement made by Mr. Whitworth, that the amount of rotation helped the shot forward. He thought it had a contrary effect, and that whatever force was employed in giving rotation, diminished, to that extent, the longitudinal force; but the amount was inconsiderable. In the Whitworth 80-pounder gun, supposing the velocity of the shot to be 1,300 feet per second, the mean force, or pressure for that velocity, would be 306,900 lbs. throughout the length of the gun; whilst the force to give rotation, including friction, would be only 7,000 lbs.

The next point, upon which he would offer some remarks, was as to the force of gunpowder. He had estimated it at from 17 tons to 20 tons to the square inch. That result was arrived at from his own experiments. The plan adopted was, in his opinion, a correct mode of ascertaining the strength of gunpowder. The more ordinary way of estimating it was by two methods; first, by ascertaining the actual volume of the gases, and their expansion by the heat generated; and secondly, by experiments with the ballistic pendulum. As to the first of these methods, the absolute knowledge was very imperfect. The volume of gases generated was known with tolerable accuracy; but not the actual temperature. The amount, however, of the heat given out by the carbon and sulphur of the powder was known, and the specific heat of the gases at a constant pressure; but what was required, was the specific heat at a constant volume, which though known for air, nitrogen, and sulphuret of potassium, was, he believed, not known accurately for carbonic acid. From these data, an estimate might be formed, which would bring the temperature to about $9,440^{\circ}$; and this would result in a pressure of about 42 tons per square inch, if the pressure was measured by Mariotte's law. As far as the experiments with the ballistic pendulum went, Hutton had arrived at the conclusion, that the force of gunpowder was from 12 tons to 16 tons to the square inch. Captain Boxer, in his "Treatise on Artillery,"¹ calculating by the volume and assumed temperature of the gases, gave the force as 2,154 atmospheres, or $14\frac{1}{2}$ tons to the inch. This, however, was on the supposition of the truth of Mariotte's law, as applied to the circumstances of the gases from gunpowder, a hypothesis which Captain Boxer correctly stated, the present state of knowledge would lead to doubt. Applying Hutton's formula to the Whitworth 80-pounder gun, and taking

¹ Vide "Treatise on Artillery, &c.," by Capt. F. M. Boxer, R.A. Sect. 1, Part I. 8vo. London, 1854.

the velocity at 1,300 feet per second, Mr. Longridge found the initial force to be about $17\frac{1}{2}$ tons per square inch, if Mariotte's law was used. But if, instead of this, the true thermo-dynamic law was made use of, the initial force would be found to be $24\frac{1}{2}$ tons per square inch. In both cases, it was supposed, that the whole of the powder was burnt instantaneously. If, for example, D E, (Fig. 32), represented the chase of a gun, and D C the space occu-

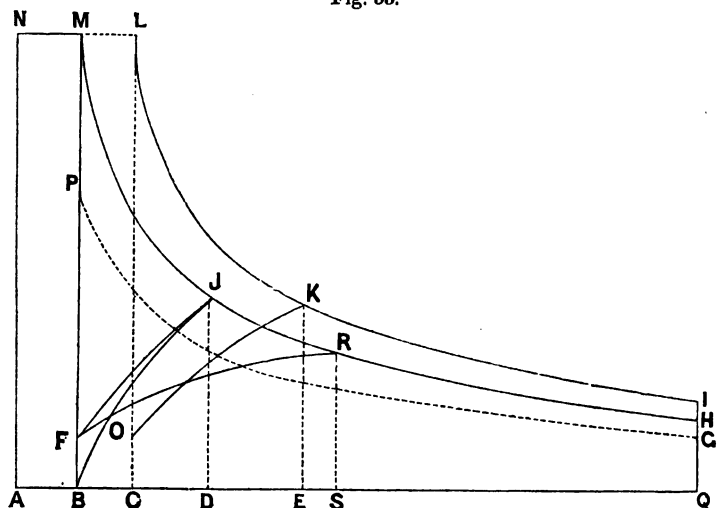
Fig. 32.



pied by the powder, then, if the explosion took place instantaneously, the pressures at any point would be represented by the ordinates to the curve B H F, if Mariotte's law was used; but by those of the curve A H G, if the true thermo-dynamic law was employed in the calculation. The curve A H G was what was called an adiabatic curve, or curve of no transmission; the amount of heat contained in the gases, during the whole process of expansion, being considered constant, whereas, according to Mariotte's law, the temperature was considered constant. It was, however, well known, that powder did not burn instantaneously; and it was important to know what effect the rate of burning had upon the initial force. With the view of illustrating this, he had prepared a diagram, (Fig. 33, page 439,) which he would explain. Taking A Q to represent the chase of the gun, and A B the space occupied by the charge, and applying Hutton's formula, modified by the adoption of the thermo-dynamic law, a curve P G would be found, whose ordinates would represent the pressures, and whose area B P G Q B, the work done. This was on the hypothesis of instantaneous burning. If, however, the powder did not burn instantaneously, the case would be different. As soon as the pressure behind the ball exceeded the friction, the ball would begin to move, and the space for the gases would increase. If F B represented the pressure needful to overcome friction, the curve of pressure would be somewhat like F J; the ordinates increasing up to a certain point, probably till all the powder was burnt. At any rate, let the ball be at D, when all

the powder was burnt; then D J would represent the pressure. From this point, the curve of pressure would be an adiabatic curve J H. Now the work done on the ball must be the same as in the

Fig. 33.



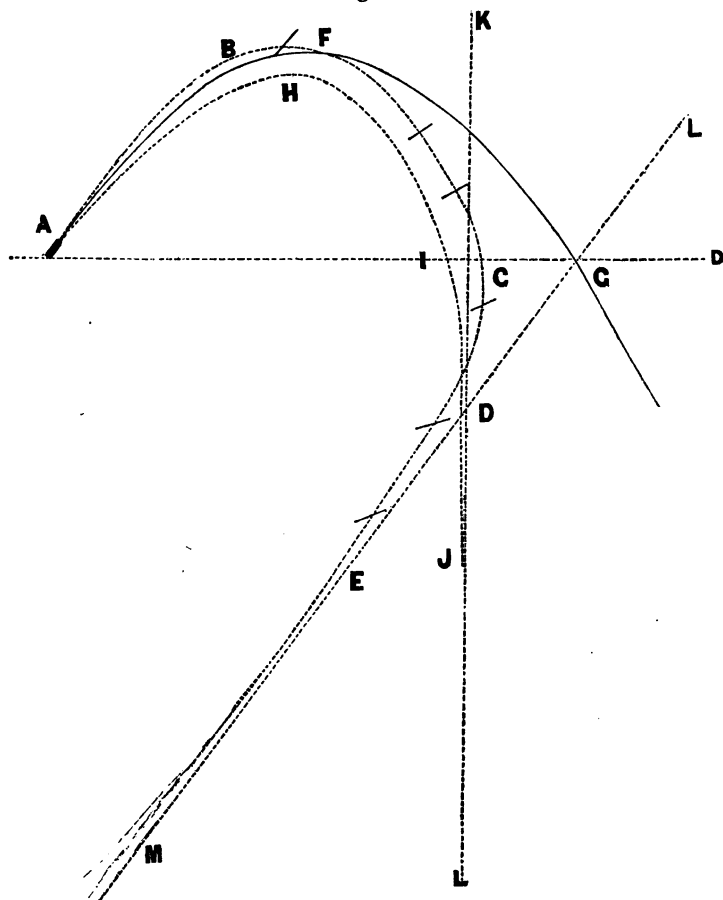
former case; consequently, the area $B F J H Q B$ must be equal to the area $B P G Q B$. It was, therefore, evident, that the point J must lie outside the curve $P G$. This being so, it followed, that if the curve $H J$ was prolonged backwards, the ordinate $B M$ would be greater than $B P$; that was to say, the initial force of the slow burning powder, if not allowed to expand, would be greater than the initial force of instantaneous burning powder, doing the same work and calculated by the law above named. Hence it followed that, taking the same case as before, viz., the Whitworth large gun, the initial force of the powder, if it had not been allowed to expand, would have been greater than $P B$ as determined for that case, viz., $24\frac{1}{2}$ tons per square inch. How much greater, there were no means at present of knowing, because the position of the point D , when the powder was all burnt, could not be determined. The next remark was, that the slower the burning of the powder, the less was the effect produced with a given quantity. Taking the curve $M J R H$ for instance; if the powder burned instantaneously, the work done would be the area $B M J H Q B$. If the powder was not all burnt until the ball arrived at D , the work done would be the area $B F J R G H B$. If a still slower powder was used, which did not all burn until the ball arrived at S , the work done would be only $B F R H Q B$. From this, the great loss of effect, caused by the use of slow-burning powder, was very obvious, and

if the same effect was to be obtained, it could only be accomplished by using larger charges. For instance, let there be two powders, one of which burned slower than the other. Let AB be the length of charge of the quick burning powder, and let it all be burnt when the ball had reached D ; then the work done was $BFJHQB$. If now the same work had to be done with a slower powder, a larger charge AC must be used, so as to obtain an area $COKIQC$ equal to the area $BFJHQB$; and this increased length of charge, required for slow powder, would be peculiarly inconvenient in the new system of guns, where the calibre was so much less, than for an equal weight of projectile, under the old system. That being so, it was with great surprise that he read, in the speech of the Secretary at War upon the Army estimates, on the 17th of March, 1860, a statement to this effect:—"That the last gun sent by Sir William Armstrong to be tried, was a 12-pounder, upon which the experiment was made, of firing forty rounds, with a charge of $1\frac{1}{2}$ lb. of slow powder. These experiments showed, that they had been wrong in using powder of so quick a detonating nature for artillery practice, and especially for rifled cannon, which required a weaker and slower powder than in other cases." Mr. Longridge presumed the Right Honourable Gentleman would not have made that statement, unless upon authority. He thought, that if such were the case, although improvements might have been made in the construction of the guns, a retrograde step had been taken in the use of them. If the statement were true, it would be advisable to go to the Chinese, and to learn how to make good rifled cannon gunpowder. For his part, he believed the statement to be erroneous. It could not be otherwise, than that the more rapidly the powder was burnt, the greater must be the effect. He could only account for the desire for slow powder, by supposing that there was some fear lest the guns would not stand the great increase of work, which would have to be put upon them, if the rifled principle was carried out, to its fullest extent, with quick powder. It had been stated, that the Astronomer Royal entertained the opinion, that the strength of gunpowder was equal to 80 tons to the square inch. Upon hearing that, Mr. Longridge wrote to Professor Airy, and had received from him a reply to the effect, that it was the result merely of a rough mental calculation, and was never intended to be made public. He had since ascertained, that the divisor 2 had been omitted in the computation, which would bring the figure down to 40 tons instead of 80 tons; and even this, the learned Professor admitted to be based only on a very rough approximation.

He could not agree in the opinion, expressed by Sir William Armstrong, that the effect of a resisting medium was to prolong the range of the shot. No doubt the tendency of the resisting medium

was, at some portion of the flight, to raise the trajectory of an elongated shot, which preserved the direction of its axis, above what it would be, if the shot moved tangential to the curve. He had prepared a diagram in illustration of this, (Fig. 34).

Fig. 34.



Let A F G be the range of a projectile, in vacuo, from the gun A. Then the range, in air, of a shot moving with its axis tangential to its path, would be somewhat like A H I J; and would ultimately be asymptotic to a vertical line K L. But, if the projectile had such a spin, as to preserve the original direction of its axis of rotation, the action of the air on the under-side would be, to raise the trajectory above A H I, to some other curve A B F C, part of which might possibly be above the parabolic track. If so, it would

soon cross it, and as it lost its horizontal velocity, it would be more and more deflected back, till finally, the horizontal element of the resistance of the air would cause it to cross the trajectory of the tangentially-moving shot; and eventually, if no obstacle interfered, it would pass beneath the gun, in a curve asymptotic to a line LDEM. This result was easily proved, by a consideration of the forces. It was, therefore, seen, that the range of a spinning shot did not, in every case, exceed that of a tangentially-moving shot, this depending upon the position of the point of intersection of the two trajectories.

He did not believe that, in any case, the actual range could exceed the parabolic range, or the range in vacuo. He had calculated the parabolic ranges for some of the experiments made with the Armstrong and the Whitworth guns, and the results were:—

—	Elevation of Gun.	Actual Range.	Parabolic Range.	Difference.	Initial Velocity in Feet per Second
Armstrong 12-pounder Gun.	7°	Yards. 2,480	Yards. 2,940	Yards. 460	1,080
	8°	2,797	3,349	552	1,080
	9°	3,000	3,755	755	1,080
Whitworth Guns. 12-pr. 80-pr.	5°	2,342	3,057	715	1,300
	10°	4,120	6,020	1,900	1,300
	5°	2,604	3,057	453	1,300
	10°	4,730	6,020	1,290	1,300

In all these cases, the actual range was much below the range in vacuo, or the parabolic range.¹

¹ In his remarks on closing the discussion, the President gave the following experiments, as corroborative of Sir W. Armstrong's assertion:—

Description of Gun.	Elevation.	Actual Range.	Initial Velocity in Feet per Second.
Whitworth 12-pounder ..	2° 0'	Yards. 1,252	1,300
Armstrong 12-pounder ..	1° 15'	840	1,080

Now the parabolic ranges in this case, taking into account the difference of level, would be:—

Whitworth gun		1,260 yards.
Armstrong gun		717 ..

[The

To come now to the subject of the Paper, it would seem, from what had been said by some of the speakers, that what had been sought was altogether unnecessary, that cast iron was strong enough for the purposes of ordnance, and that no better material was required. There were two classes of opinions on the subject. With one it was the attachment to an old and faithful friend; with the other it seemed a more romantic feeling, delighting in the anticipated development of future excellence. One gallant officer had spoken, with great warmth, of his own experience of the good service done by the old cast-iron guns, and all must sympathise with him, to some extent, in his feelings of attachment to his old and trusty friend. Such feelings were very nigh akin to those which had so long enabled 'Brown Bess' to hold her ground, in spite of the refined graces of her more youthful rival. But he thought this class of admirers might be left with a passing acknowledgment to their constancy, and that attention might be turned to those who anticipated a great future for cast-iron guns. It had been said, that he had understated the strength of cast iron in putting it at 8 tons to the square inch. He had taken that as the average strength of cast iron in general. He believed, that in the inside of guns, it was not nearly so much. He had been rather astonished by the statement, that some experimental cast iron, which had been tested, was equal to a tensile force of 44,000 lbs. On inquiry, he found, that in that instance Acadian charcoal iron was used, and it seemed, that one specimen had borne nearly that force. But in the same page of the pamphlet, from which this high result was quoted, there were instances in which the tensile strength of the same iron was not quite 8 tons.¹ Mr. Longridge had referred

The last of these results was not at all conclusive; first, because a slight error in estimating the velocity would greatly affect the parabolic range, and secondly, because a very small error in elevation, at so low an angle, would also greatly vary the range. For instance, an error of only 15 minutes would extend the range from 717 yards to 972 yards. It was, therefore, quite possible that the anomalous result of the last experiment might be attributed to one, or both of the causes just pointed out—J. A. L.

¹ Since making the above remarks, Mr. Longridge's attention had been called to the fact, that the specimen of iron, which bore under 8 tons per square inch, was of the first melting. The results of the different specimens tried are, therefore, given:—

FIRST MELTING.

Tensile Strength.

1st.	One bar	.	.	16,973 lbs. per square inch.
2nd.	"	.	.	19,271 " "
3rd.	"	.	.	24,227 " "

HALF PIG AND HALF SCRAP FROM PREVIOUS EXPERIMENTS.

1st.	Mean of two bars	.	.	34·926 lbs. per square inch.
2nd.	"	"	.	21·062 " "
3rd.	"	"	.	41·519 " "

There was thus, evidently, a great variation in the quality of the iron.—J.A.L.

to one, or two documents upon this subject; and he found in the last Blue Book, (1858,) containing the Woolwich experiments, that the maximum strength of cast iron there tried, was 15 tons, and the minimum strength, $4\frac{1}{2}$ tons, the average being 10 tons. Those experiments were made upon irons prepared, and sent specially by the makers, and doubtless, considered by them as the best for the purpose. The result of Mr. Hodgkinson's experiments, recorded in his edition of "Tredgold," showed an average tensile strength of 7 tons to $7\frac{1}{2}$ tons per square inch; Low Moor iron being $6\frac{1}{2}$ tons, and Carron iron, $6\frac{1}{2}$ to 7 tons. From the Report of the "Commissioners on the use of Iron in Railway Structures," (1849,) it appeared, that the tensile strength of Bowling iron was 6 tons to $6\frac{3}{4}$ tons; and that of Low Moor iron 7 tons per square inch. He, therefore, thought, that he had not understated the strength, in taking it at 8 tons. It should also be borne in mind, that those irons which had the highest tensile strength, were less suited for ordnance, than those which, like the Low Moor and Bowling irons, had less power of ultimate resistance, but possessed more elasticity.

A great deal had been said, about the mixture of certain alloys; but he thought the present state of knowledge, upon that subject, was not such as to warrant them in looking for any great improvement in that respect. The increased density of iron had been spoken of as a means of improvement. All that Mr. Longridge would say upon that point was, that iron of a density of 7.3 was considered at Woolwich, unsuitable for gun purposes. He had been surprised by the observations which had fallen from the Chemist to the War Department, for whose chemical knowledge he had the highest respect. But, at the same time, he thought an undue importance had been attached to identity of chemical composition, as forming a guide to the mechanical properties of iron.

Many striking instances might be given to show, that identity of chemical composition might co-exist with great variation of physical properties. For example, phosphorus was a deadly poison, and ignited with the least friction in its ordinary state; yet in another state, without any change chemically, it might be swallowed without causing any injury, and did not ignite by friction. He believed there were certain compounds, such as one of chlorine and naphthaline, which existed in the gaseous, the liquid, and the solid form, and yet no chemical difference could be detected. Therefore, he did not think that chemical identity had much to do with the mechanical properties of iron. He was supported in that opinion, by the Report of a Committee of Chemists appointed in the United States, in 1849, to investigate this question. In 1851, their first Report was made, which was of a hopeful character. In 1852, it was reported, that a

decided relation, it was believed, had been observed between the amount of uncombined carbon and the tensile strength of the metal. But in the final Report, in 1855, all the former reports were withdrawn, and it was stated, that "though at first largely appreciating the extent of our labours, the completion of them sensibly diminished that estimate of their usefulness." Therefore, he thought, however desirable it might be to ascertain the chemical qualities of iron, practical men were yet very far from being in a position to accept them as indices of its tensile strength.

The next point was with respect to wrought iron. Mr. Clay intended, apparently, to object to the statement, made in the Paper, with respect to the deterioration of the iron on the inside of large forged-iron guns; but he merely took objection to the statement, and made no comment upon it. Now that statement having been taken from the writings of Mr. Clay himself, it could only be presumed, that when he recorded the facts, he had been satisfied as to the accuracy of what he had written. It had also been mentioned, that a new process of making guns had been discovered; and although the method was not communicated, it was stated, as a proof of its value, that a tube $2\frac{1}{2}$ inches in calibre, and 2 inches thick, was fired with a charge of $1\frac{1}{2}$ lb. of powder, twenty-two round balls, and a cylinder projecting 12 inches from the muzzle, without injury to the tube. In answer to this, Mr. Longridge would point to the tube represented in Figs. 14 and 15, (pages 312 and 313), which was 3 inches in diameter and only $\frac{3}{8}$ ths of an inch in thickness; and which was fired with 2 lbs. of powder, under circumstances, described in the Paper, infinitely more trying than those now referred to. This cylinder, $\frac{3}{8}$ ths of an inch thick, had been tested far more severely than the iron cylinder, 2 inches in thickness.

He had very little to say upon the subject of steel guns. Krüpp's steel had been alluded to, as possessing extraordinary tensile strength; and it was stated, that the accident to that gun was caused entirely by the nature of the projectile used in the trial. The form of shot shown in Fig. 19, (page 319,) had been ridiculed; but he begged to say, that it never had been his intention to use that shot, except for a particular experiment, and with a very small charge of powder. In his absence, it had been put into the gun with 2 lbs. of powder behind it, and the result had been the accident he had described. With regard to the form of shot used in Mr. Krüpp's gun, there was a wrought-iron hoop round the breech, and that hoop seemed to have shifted, sheared away the cast iron, and been jammed in the gun. He thought that such a shot ought not to have been fired with a heavy charge of powder.

Whatever material was used, it ought to be applied in the best possible manner, to enable it to do its full duty; and this was cer-

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tainly not the case in a homogeneous mass. The attempt to cast guns hollow, and to cool them from the inside, had, he believed, been long since abandoned in the American arsenals. Several instances had been given of cast-iron guns which had shown great powers of endurance; but he would observe, that it was no proof of the sufficiency of cast iron, that a gun had been fired a certain number of rounds, with given charges. A cast-iron gun, if well made, and of uniform texture, did not fly to pieces all at once. It burst gradually, beginning at the inside by very minute cracks. It was quite possible that, for a certain time, these minute cracks might increase the strength of the gun. He would not detain the Meeting by going into a proof of this, but it might be received as a fact. These cracks gradually extended, and finally, the gun gave way. In fact, theory had distinctly shown the limit to which any homogeneous cylinder, or gun, could be strained, and he, for one, believed, that the indications of theory were the only sure guides to success.

He would now briefly allude to the principle, which it was the more immediate object of the Paper to bring forward. That principle, which had been adopted both by Captain Blakely and by Mr. Longridge, was, building up guns with concentric rings, or hoops, or binding them with wire. He was glad that the principle was no longer in the region of theory; but that facts might be referred to, as establishing the correctness of the means which Captain Blakely and he had, for so many years, entertained. Sir W. Armstrong and Mr. Whitworth had both adopted that principle in their large guns; but he was bound to say, he did not think it was being carried out by either of them, in the best way. In order to derive the full benefit from this mode of construction, it was necessary, that the rings should be put on at certain definite degrees of tension. The diagrams he had exhibited would illustrate the effect of not putting them on with the proper tensions. The theory of this principle had been worked out by a gentleman not unknown in this Institution,—Mr. C. H. Brooks,—to whom he was greatly indebted, as was also the Institution, and who had drawn up a very elaborate Memoir, containing the mathematical part of the question, which was given in the Appendix to the Paper.¹ The result of these calculations satisfied Mr. Longridge, that if the object was to obtain the utmost amount of strength from hoops, they must be put on with a certain definite tension. If that was not done, there would be risk of injury to some of the hoops, or rings of the gun. Mr. Whitworth had stated, that his method of putting on the hoops was by hydraulic pressure. This was one plan proposed by Captain Blakely, and was preferable to shrinking them on. Mr. Whitworth put them

¹ *Vide* Appendix, page 329, *ante*.

on with a great initial tension; in fact, with the utmost the material would bear without a permanent set; and consequently, the least increase of strain caused by the explosion would give them a permanent set. Thus, Mr. Longridge was convinced, that Mr. Whitworth not only failed to get the maximum amount of strength, but introduced an element of destruction into his gun. Sir W. Armstrong had said, that the hoops were put on hot in his gun. Mr. Longridge's experience in contracting on hoops showed, that it was impossible to ascertain the exact amount of tension, as no two pieces of iron would contract precisely alike; and it was only by accident, that the amount of tension, which theory indicated as necessary, could be applied. Therefore, though Sir W. Armstrong and Mr. Whitworth were both travelling in the right direction, he felt bound to say they were travelling without a chart, and were not following the sure indications of theory. In the Armstrong gun, the chase of the gun was made spirally, and welded up, and then upon that, at the breech part, there were other hoops made of iron, laid with the fibre longitudinally. Upon these the trunnion piece was shrunk, and over that again iron hoops, made spirally. Mr. Longridge thought, that the iron to resist the longitudinal strain should be placed outside, and he preferred to have it altogether independent of the gun; he believed it would stand the work better in that way. In the Armstrong gun, the whole of the recoil was dependent upon the adhesion of the longitudinally-laid tubes upon the other tubes. These might easily be affected by the concussion of repeated firing, more particularly, as they were not put on with any definite initial strain, and the result might be fatal to the gun. The Whitworth gun also depended on the adhesion of friction for the trunnions, and for longitudinal strength it was dependent on the inner tube. He must express his decided opinion, that large guns, of 8-inches, 10-inches, or 12-inches calibre, could not be safely made, by either of the methods adopted by Sir W. Armstrong, or by Mr. Whitworth.

It had been said, that the system he advocated would never be adopted by Artillery Officers. One of the objections to the system was, that there was no sufficient end-hold. He did not exactly understand what was meant by that expression, but he fancied it was as much as to say, that there were no means of fixing the ends of the wire. Now, nothing could be more simple. In the gun, (Fig. 15, page 313,) a small hole was drilled through the cast iron in a slanting direction; the end of the wire was put into it, and a wedge was driven in, so that there was no possibility of its uncoiling. In finishing off the end of the wire, the same plan was adopted; and a layer of solder was run along these coils, so that if two, or three, or even more, wires were cut, by any means,

the rest would not uncoil. It was further intended to protect these wires by a coating of sheet iron. The fixing of the breech, about which a great deal had been said, presented no difficulty. The material which transmitted the recoil was placed outside the gun. There was no difficulty in carrying out that system, and he felt satisfied, that ultimately, all large guns would be so constructed. With regard to the effect of temperature on wire-bound guns, he had calculated, that at 200° , the result would not diminish the tension of these coils more than $1\frac{1}{2}$ lb. or 2 lbs. There was, therefore, nothing to fear as to temperature. He was sorry to find, that Mr. Anderson did not like wire; and he would remind that gentleman, that only a few years ago he had an equal distaste for hoops. Having now so far changed his opinion as to build guns with hoops, Mr. Longridge was satisfied, that in a few years, Mr. Anderson would appreciate and acknowledge the superior merits of wire.

Mr. BIDDER,—President,—said, that the Institution might take credit for the very interesting character of this discussion, which was one of national importance; in fact, it had fully justified the observation, which he had made in his opening Address on taking the Chair;—that there was not any arena in this, or in any other country, so well calculated for the discussion of this class of subjects, as within the walls of the Institution of Civil Engineers.

The more immediate subject of the discussion, was the capability of vessels to resist great internal pressure, as proposed in Mr. Longridge's Paper. This had induced the question of the construction of guns, and the proposition to extend the range of the subject, had led to the interesting discussion which had taken place.

It must be admitted, that the theory propounded by Mr. Longridge, with regard to the tensile force of metal, had been satisfactorily demonstrated; and theoretically, the plan he suggested would appear to answer the intended purpose. Practical objections had, however, been fairly and legitimately urged against the proposed plan. These related chiefly to the difficulty of securing the ends of the wire, of strengthening the breech, and of forming the muzzle. How far the Author had succeeded in meeting those difficulties, the Members must judge. It was, however, evident, that he had established such a *primâ facie* case as should have entitled him to have received the attention of the Government, in his attempts to produce a light and efficient gun; especially when it was considered, that the subject was only now, for the first time, undergoing that careful scientific investigation to which its great importance entitled it.

There had been exhibited to the meeting two guns of the most scientific design and beautiful workmanship; the respective inventions of Sir W. Armstrong and of Mr. Whitworth. With regard

to the manufacture of the former, the President could not coincide with the opinions expressed, as to the difficulty of forging such guns. He had witnessed the operations, and as far as his experience went, he must say, that the mode of construction appeared to be most satisfactory, and he had no doubt, that the guns so constructed, in the new gun factory at Woolwich, possessed all the characteristics claimed for them. At the same time, from the general tenor of the discussion, it was evident, that there was a strong feeling in favour of the homogeneous metal used in the manufacture of the Whitworth gun, and in that opinion the President participated. The metal certainly possessed the characteristics claimed for it; it was nearly as hard as steel, and as tough as copper, and as a material for artillery, what more could be desired? Cast-iron guns had, no doubt, occasionally, exhibited wonderful results. They had withstood an immense amount of firing and strain; but there was not any certainty of uniform results being obtained. In one case, a cast-iron gun had sustained fifteen hundred, or two thousand rounds, whilst another gun, stated to have been cast from the same metal, and under precisely the same conditions, had not resisted for a single day. This was one grave objection; whilst the great weight of cast-iron guns must also, in the present day, be a formidable objection to the use of that material, either for field guns, or for naval armament. In the production of artillery, the first cost ought not to be weighed against attaining the greatest efficiency, combined with the utmost saving of weight; the latter he considered a point of primary importance. In the field it was imperative, both on account of the rapidity of manœuvring now required, and to facilitate the transport, at considerable speed, over a rough country; and on board vessels of war, carrying an armament of one hundred and forty guns, the saving of weight must, evidently, be of the most material consequence. The rifling of cast-iron guns might have been a very proper subject for consideration, some twenty, or thirty years ago; and no doubt, blame must attach somewhere, that it was only within the last few years, that the rifling of ordnance had been seriously thought of, or had been practically tried.

In making a few remarks upon the practical results of trials, and on the present practice of gunnery, the President begged the Members to receive the calculations which he should present, and the results which he should submit to them, as mere approximations. There had been communicated to him, the results of an immense number of experiments in gunnery, and upon testing them he had found, that, to a great extent, the results were not such as could entitle them to implicit reliance. He did not mean by this, to say that apparent facts had been garbled, or misrepresented; but circumstances which had attended the experiments,

but which had been omitted, had influenced the results, and thus rendered them useless for the purposes of investigation. Circumstances had occurred, which convinced the President, that he had made very little progress in the science of gunnery; a few weeks ago, he thought that he was on the eve of ascertaining some definite results, but he now felt, after more careful investigation, that, far from being at all advanced in the science, the threshold was only just reached, and that scarcely anything was really known of that important science. The very first thing in gunnery, but upon which the authorities were not agreed, was the initial pressure of gunpowder, and its 'modus operandi' upon the shot. Upon that subject the most discordant opinions prevailed. What he meant by the initial pressure of gunpowder gas was, the pressure of the gases evolved on the explosion of gunpowder, when confined within the actual space allotted for the gunpowder itself. That was what must be arrived at, in order to make the calculations as to the effect produced, considering also the circumstances under which the powder was exploded. In the course of the discussion, Captain Boxer, who occupied a high position in the Royal Arsenal, at Woolwich, and who claimed to have considerable information relative to the explosion of gunpowder, and the requisite strength of metals to resist its force, stated, that the forces of gunpowder gas were of two distinct kinds,—statical and percussive. Now from this proposition, the President must distinctly beg to differ. Statically, a pound of gunpowder gas, would not have more effect, in generating velocity, than a pound of butter; and if it acted percussively, it must destroy both the projectile and the gun.

In the work of Mr. Lynall Thomas¹ it was stated, that gunpowder had some mysterious action; that before the gases acted upon the shot, there was some kind of undulation, or oscillation, which, by some means, communicated an initial velocity to the shot, irrespective of their expansive action. Mr. Treadwell,² again, stated, that the whole force of gunpowder was due to the inertia of the shot, or of any mass with which it was placed in contact, at the moment of explosion; and he, moreover, said that a large charge of gunpowder, if exploded in vacuo, would not burst a sheet of tissue paper. From that position, it was scarcely necessary to record an unconditional dissent. All the phenomena attending the explosion of gunpowder, could be explained by simple mechanical effects. If it was assumed, that in exploding gunpowder

¹ *Vide* "Rifled Ordnance; a Practical Treatise on the Application of the Rifle to Guns and Mortars," etc. By Lynall Thomas. 8vo. London, 1859.

² *Vide* "On the Practicability of Constructing Cannon of Great Calibre," etc. By D. Treadwell. Tract, 8vo. Cambridge, 1856. (From the Memoirs of the American Academy.)

in a vessel, the resulting gases derived their force from the pressure of a column, 100,000 feet in height, of a uniform density, it would be found, that all the subsequent effects observed could be satisfactorily accounted for. In general, the element of time was completely neglected. Now in explosion, gunpowder produced initially the same pressure, whether in a gun, or in vacuo, or in the atmosphere; but in a gun, the inertia of the shot compelled a longer time to be occupied in exploding the powder. In the open air, the same quantity of powder exploded with immense rapidity, and the pressure was no sooner formed, than it was lost by expansion. For instance, in the case of an 80-lbs. rifled shot, the velocity of 800 feet per second was communicated in the three-hundredth part of a second; and in the case of a rifle bullet, the same velocity was acquired in the three-thousandth part of a second. From this statement some conception might be formed, of the very minute elements which were imported into the consideration of the question, where time alone gave the necessary explanation of all those phenomena, which were exhibited by the explosion of gunpowder. The President had tested the initial power of the gases by the mechanical effect, *i. e.* by the velocity with which the shot quitted the gun. It was assumed, which was not, however, correct, that all the powder exploded instantaneously. Under those circumstances, it required a pressure of nearly two thousand atmospheres, to generate the initial velocity. He then supposed it to explode, so that a constant pressure was maintained throughout the length of the gun. This would indicate, that the initial pressure could not be less than five thousand atmospheres. Therefore, he was unable to arrive at the conviction, that the initial pressure of the gunpowder was so low as three thousand atmospheres, or 20 tons to the inch. There was also another element to be considered; the enormous loss of the explosive power in following the shot, and in the loss of the caloric by the expansion of the gases. Assuming the tension of the gas to represent a column 100,000 feet in height, the velocity at which gas of that height would flow into vacuum, would be 2,500 feet per second. When the velocity of an 80-lbs. shot was 800 feet per second, the effect was, that the power of the gases was then reduced to a column which represented the difference of the velocity; a column which produced the velocity of 1,700 feet per second, (*i. e.* 2,500 - 800,) was two-fifths of the whole column; so that before the shot travelled 16 inches, the velocity of the gas, besides being reduced, by the space it occupied, to one-half, was further reduced three-fifths, or 60 per cent. Therefore, as to the mechanical effect of gunpowder, the result could not be produced, unless the initial force of gunpowder was much higher than three thousand atmospheres.

It was curious, although, perhaps, not germane to the subject, to compare the mechanical effect of 1 lb. of gunpowder with that

of 1 lb. of coal. In the Whitworth 3-pounder gun, the initial velocity was 1,300 feet per second, and that was generated in the two-hundredth part of a second. The mechanical effect was measured by the shot being elevated 26,000 feet, which was represented by 1 lb. raised 78,000 feet, and that was done in the two-hundredth part of a second. A nominal horse-power was represented by 33,000 lbs. raised 1 foot high per minute, or $2\frac{3}{4}$ lbs. in the two-hundredth part of a second. Therefore, the work performed by 8 oz. of gunpowder was represented by 28,000 H.P. The 8 oz. of powder, in the two-hundredth part of a second, represented 4,000 tons a-day. An engine of 28,000 H.P., working twenty-four hours would consume 1,200 tons of coal, so that 1 lb. of coal achieved the result obtained by 3 lbs. of powder.

The next element for consideration was that of atmospheric resistance. The only well-recorded experiments were those of Dr. Hutton;¹ they were made with immense care and research, and if the mechanical appliances of the present day had been at the command of that distinguished man, the theory of gunnery would have been in a very different state from what it now really was. But the best appliance for his experiments was a 1-pounder gun, made expressly for his use. The experiments were tried in Galleons' Reach, a part of the River Thames, about 800 yards wide. Cadets were posted along the banks, on each side of the river, to take notes of the flight of the shot, along the water; it was however found, that the shot did not travel in the presumed line of fire, frequently passing over the heads of the cadets, first on one side of the river and then on the other, and that the only chance of hitting an object, 1 mile distant, was by aiming some 400 yards either to the right, or to the left of the target. This was owing to the windage of the shot in the gun, causing it to travel from one side to the other, according to chance. Still, all these difficulties were struggled against, by Dr. Hutton, and all who wished to have their minds imbued with the difficulties of the question, should examine the details, and go through his calculations. The velocity of the shot was determined by the ballistic pendulum, whilst the atmospheric resistance was ascertained by revolving machinery, and the result established the enormous resistance of the atmosphere, of 102 lbs. upon a 1-lb. shot, 2 inches in diameter, travelling at a velocity of 2,000 feet per second; at 1,300 feet per second, the resistance was $42\frac{1}{2}$ lbs.; and curiously enough, it would appear, that the actual resistance of the atmosphere was nearly that which would be due to the impression of wind, upon a disc of the same diameter and at the same velocity.

The resistance of the atmosphere was due to three causes; viz., the displacement of the atmosphere, the friction of the atmosphere

¹ Vide "Tracts on Mathematical and Philosophical Subjects," &c. By C. Hutton. 8vo. Vol. III. Tract 37. London, 1812.

upon the periphery, and the back pressure, which was the vacuum formed in the rear of the shot, and which it was necessary for the atmosphere to fill up again. The latter did not follow the ordinary law of the squares, and it was believed, that in all cases of spherical shot, it was increased beyond what theory gave; for it was assumed, that the air adjacent to the path of the shot was in a quiescent state, and was ready to fill up the vacuum. But when the shot expelled the atmosphere, it was driven off in lines tangential to the periphery, so that the force must be more intense, as it had, not only to change the direction of the particles, but to retard their motion, before they flowed into the wake left by the projectile. The result had been to establish, that the experimental resistances were nearly double the amount derived from theory.

It was owing to the enormous resistances occasioned by the atmosphere, that Dr. Hutton, following in the track of Robins, was induced to recommend, as the only remedy, the use of elongated projectiles. This, of course, could only be effected by rifling, and thus insuring the correct flight of the shot.

With regard to rifling, the remarks might be confined to the two systems respectively adopted by Sir W. Armstrong and by Mr. Whitworth; the former by impressing the rifle grooves on the soft-metal covering of the projectile; and the latter, by fitting the hard-metal shot to a polygonal-shaped bore.

The elongated projectile reduced the enormous resistance of the atmosphere, in several ways. In the first place, there was more momentum according to the area. Comparing the 1-pounder gun, 2 inches in diameter, with the Whitworth 3-pounder gun, $1\frac{1}{2}$ inch in diameter, the surface exposed to resistance was nearly double in the former, to that of the latter, besides which there was a momentum of nearly 3 to 1 in respect of their relative weights; reducing resistance in the proportion of nearly 6 to 1. By careful experimental trials, and by giving to the front of the shot a sharper form than the hemisphere, a still greater reduction in the total amount of resistance had been attained. In order, however, to produce the greatest effect, *i.e.* to elongate the projectile to the utmost extent, it was necessary to determine the proper limits of rifling. Upon this point the President did not agree, either with Sir William Armstrong, or with Mr. Longridge, in thinking that the amount of twist was immaterial. On the contrary, he thought it most material; because upon the extent of the twist depended the power of using long, or short projectiles; especially considering, also, how small an amount of power was absorbed in giving the necessary twist. Mr. Longridge had stated it to be about one-fortieth part of the power necessary for generating the velocity of the shot, inclusive of the power absorbed by the friction; but the result of the President's own investigation of the 3-pounder gun was, that the twist given, required only the one hundred and seventieth part

of the force which was necessary to give the requisite initial velocity to the shot.

In testing the experiments, the most convenient system was, to ascertain the ratio of the velocity of the retardation of the shot, per element of distance, rather than per element of time. Theoretically and practically, it appeared, that the retardation, in passing through equal spaces, was a fixed proportion of the velocity. For example, if a shot going 1,000 yards per second, lost 10 feet of velocity in a certain space; then with a velocity of 500 yards per second, it would lose 5 feet of velocity in the same space. These statements must, however, only be received as approximations; for the reliable experiments had, hitherto, been so few, that no one could be justified in making an exact statement upon the subject. They must be further carried out, with the assistance of the electric telegraph, both for time and range, so as to enable the initial velocity to be determined beyond all doubt.

Having made these remarks, the President proceeded to give a few details of the results of some recent experiments, in order to show what had been accomplished by rifling.

The first examples were with

SPHERICAL SHOT.

Weight.	Diameter.	Charge of Powder.	Elevation.	Initial Velocity.	Actual Range.	Parabolic Range.
lb.	Inch.	oz.	Degrees.	Ft. per sec.	Feet.	Feet.
1	1.96	4	15	1,234	4,660	25,000
		8	15	1,644	6,066	44,000
3	2.78	..	20	1,200	4,800	31,200
		..	30	1,200	7,680	45,000

It would be seen, that the parabolic exceeded the actual range, in a proportion varying from five-fold to seven-fold.

The next results were with

THE WHITWORTH RIFLED CANNON.

Weight of Projectile.	Diameter across the Flats.	Length of Barrel.	Twist, one turn in Inches.	Charge of Powder.	Initial Velocity.	Number of revolutions per Second.	Elevation.	Actual Range.	Parabolic Range.
lbs.	Inches.	Inches.			Ft. per sec.		Degrees.	Feet.	Feet.
3	1½	72	40	8 ozs.	1,300	400	3 10 20 35	4,707 12,567 20,970 28,740	5,550 18,300 34,500 49,200
12	3¼	93	60	28 ozs.	1,300	260	2 5 10	3,756 6,960 11,739	3,780 9,210 18,300
80	5		100	12 lbs.	1,300	156	5 7 10	7,722 10,476 13,665	9,200 12,900 18,300

Comparing the experiments with the Whitworth 3-pounder gun, with those of the 1-pounder gun, used by Hutton, the following would be the results:—

The charge of powder was one-sixth in place of one-half the weight of the projectiles, and the elevations were 10° and 15° , respectively. The initial velocity of the rifled shot was one-fourth less than that of the round ball; and yet, with a lower elevation, the range of the Whitworth projectile was more than double that of the spherical shot; whilst the parabolic range exceeded the actual range by only 50 per cent. in the former, as compared with 600 per cent. in the latter case.

Now the difference of these results must be attributed to the fact, of the spherical 1-lb. shot losing 15 per cent. of velocity in 100 yards of flight; whereas the 3-lb. shot, from the Whitworth gun, lost only two-thirds per cent., or less than one-twentieth of the former. In other words, the resistance of the 3-lb. shot was only one-twentieth of that of the 1-lb. spherical shot, which was partly due to the less surface presented by that form of shot, to the resistance of the atmosphere. It had been stated by Sir John Burgoyne, that for breaching a fortification, the old service guns could not be surpassed, because the projectiles possessed so high an initial velocity. That was quite true, as far as it went; but it must be borne in mind, that after the first 500 yards had been traversed, there was so great a reduction in the velocity of the spherical shot, as to equalise its velocity with that of the rifled shot, which latter very nearly maintained its initial velocity; whilst from its form, its power of penetration was, *ceteris paribus*, in excess of that of the spherical shot. It was, moreover, a fact, that a rifled shot, at the distance of a mile, was as effective against a wall, or any other object, as a 68-lbs. spherical shot, at a distance of 500 yards; therefore, even for breaching purposes, the great advantage of rifling must be apparent.

The next examples were in connexion with

THE ARMSTRONG GUN.

Weight of Projectile.	Diameter.	Charge of Powder.	Initial Velocity.	Elevation.	Actual Range.	Parabolic Range.
lbs.	Inches.	oz.	Ft. per sec.		Feet.	Feet.
5	..	..	1,000	$4^{\circ} 26'$ 6° 11°	4,500 5,892 9,000	4,860 6,600 12,000
12	3	24	1,100	7° 8° 9°	7,440 8,400 9,000	9,300 10,660 11,880

The above experiments showed, with all deference to Mr. Long-

ridge, that there were proofs of the actual range being all but identical with the parabolic range, at low elevations. Sir William Armstrong had alluded to a new element, and it was by that new element, that those effects were produced; viz., the angle between the axis and the path of the projectile, increasing, therefore, with the angle of elevation. The diagram, (Fig. 34, page 441), exhibited by Mr. Longridge, illustrated this hypothesis, that a shot fired at an elevation of 2° , would, towards the end of the range, meet the atmosphere at an angle of 4° ; and resolving the resistance of the atmosphere at that angle, into a vertical elevation, it would be found to be $\frac{1}{2}$ lb. on the 3-pounder projectile. The result, therefore, would be, that from the letter F upon the diagram (Fig. 34), the line of the curve would be straightened off, and the ultimate course of the shot would be nearly straight, at the end of its flight.

In comparing the present results of the Whitworth and the Armstrong guns, as far as experiments had hitherto shown, there was a difference in the initial velocity of between 200 feet and 300 feet per second, in favour of the former. As regarded elevation, that was important, particularly with respect to accuracy of fire. When Sir William Armstrong made his observations with regard to the long range, the President could hardly agree with him. But that was not the main object. The practical object of attaining exceedingly long ranges, must be for attacking any fortified place, or for bombarding a naval arsenal, so as to be able to fire all day and night, still keeping out of the reach of the enemy, and to drop shots and shells, with impunity, into apparently inaccessible places, so as to cause, if not absolute ruin, at least very considerable annoyance in any naval arsenal, or maritime establishment. It was a very material element, to be able to lower the elevation, as by that means, the accuracy, of the firing was increased, or a longer range attained with the same elevation. Thus, for instance, with 2° of elevation, the range, with a velocity of 1,000 feet per second, would be 730 yards; with 1,300 feet per second it would be 1,230 yards; with 1,500 feet per second it would be 1,620 yards; the latter velocity giving the same accuracy, at double the range, which the initial velocity of 1,000 feet could command.

Objections had been raised to Mr. Whitworth's mode of breech-loading, as being exposed to injury from shot. But did not the trunnions of every gun offer quite as great projection, on both sides of the gun, as the hinge did on one side? On the other hand, it had been objected, that Sir William Armstrong's gun did not admit of a choice in the variety of the projectiles. It had also been objected, that the Armstrong gun was not adapted for firing red-hot shot, or shot filled with molten metal. Also, that the Armstrong projectile could not be fired from the Whitworth gun.

There could not, however, be any doubt, that the ingenuity which had enabled Sir William Armstrong to design that very beautiful gun, would also enable him to overcome such difficulties with regard to the projectiles. These objections only induced the conviction of the paramount importance of a combination of the two systems, propounded by Sir William Armstrong and by Mr. Whitworth, which would, undoubtedly, produce a most perfect weapon. The exertions of both those gentlemen had considerably added to their reputation, and their sacrifices and devotion of their time in the service of the public, deserved the highest encomiums.

Mr. Longridge had made a remark with regard to the statement by the Minister of War, a short time since, upon the subject of gunpowder. Upon the authority of Sir William Armstrong, the President might state, that what was intended to be said was, not that Sir William Armstrong could not use more powerful powder, as in fact, he could advantageously use powder of any strength, but that in experimental firing, it had been found, that more regular practice was obtained by using slow-burning powder.

Very little had been said upon the subject of muskets. Now it was of equal importance, that all the branches of the service of this country, should be provided with the most efficient weapons. The great contest had, hitherto, been between the Enfield and the Whitworth rifles. With regard to the former, it was impossible to say how the adopted twist had been arrived at, or how the diameter of the bore had been fixed. The President had carefully examined the experiments with that rifle. The shots fired from the rest, which ought to have been the most perfect, were entirely devoid of accuracy; but when the rifles were fired from the shoulder, and individual skill was imported into the trials, the practice was very much improved. The Enfield rifle might be the proper weapon, but certainly very little, if any, scientific acumen was exhibited in the selection. It must, however, be generally admitted, that the Whitworth rifle, both for length of range and accuracy of firing, was vastly superior to the Enfield; and if there was any small difference in the cost of the two weapons, it ought not to weigh one moment in comparison with efficiency. Every soldier in the service cost the country from £50 to £100 for his education, and £50 a-year for his maintenance, and to hesitate upon a question of five, or ten shillings, or even ten pounds in the cost of the weapon, to arm the soldier, appeared to be carrying economy in the wrong direction. Within the last few weeks, the President had made some experiments with Mr. Whitworth's rifled musket. Those experiments were made with paper screens placed at distances of 50 yards from each other, throughout the length of Mr. Whitworth's gallery. The height was taken from a fixed level, to where the shot passed through each screen successively, and, the fall of the shot being

thus ascertained, the velocity at each interval was deduced. The experiments were satisfactory ; but phenomena were observed, which required to be elucidated by further experiments. Judging from the fall of the shot, from the line of elevation, in the first 50 yards its velocity was only one-half of its subsequent velocity, and instead of falling only $2\frac{1}{2}$ inches below the line of fire, in 50 yards, it fell $7\frac{1}{2}$ inches in that space. The following results were obtained in a range of 500 yards:—With a rifle of 0·442 inch calibre, and 39 inches long in the barrel, the charge of powder being 70 grains, and the weight of the projectile 530 grains, the initial velocity was 1,300 feet per second, at an elevation of $1^{\circ} 26'$. With the same trajectory and a similar projectile, but with a charge of powder of 100 grains, the initial velocity was 1,400 feet per second, at an elevation of $1^{\circ} 8'$. With a rifle of the same diameter, but 33 inches long in the barrel, the charge of powder being 50 grains and the weight of the projectile 530 grains, the initial velocity was 1,200 feet per second, at an elevation of $1^{\circ} 40'$.

These results confirmed what had been stated, with respect to cannon, that as the elevation was increased, the greater was the amount of retardation during the passage through the air, which was owing to the greater angle at which the axis of the shot met the atmosphere in its path. As regarded rifling, there was no evidence in what way the twist could generate error, whilst it was admitted, that at the same time, it gave immense range to the projectile ; therefore, it appeared, that short of what might be found inconvenient in the manufacture, there was not any limit to the twist of the rifling of a musket barrel.

In conclusion, the President would make a few general observations which must have occurred to all who had followed the discussion. Seeing for how long a time gunnery had been known, and what enormous interests were at stake, it did appear extraordinary, that in the year 1860, there were only, as it were, indications of beginning fundamentally the proper study of the subject, although owing to the ingenuity of Sir W. Armstrong and of Mr. Whitworth, the mechanical appliances appeared to be approaching near to perfection. There must be some error in the systems of all Governments, because neither in this, nor any other country, had any marked improvement, or decided progress taken place. The reason for this might be chiefly attributed to the unjust manner in which that distinguished branch of the service, the Royal Artillery, had been treated, by the governing powers. The officers were required, or expected, to be perfect in the science and practice of gunnery, so as to be able to conduct their operations, under the most terrible circumstances and under conditions of the most disturbing character. Besides which, they were expected to be acquainted with the mechanical branch of the science and even

to construct guns and projectiles, although they could not have had any opportunities of acquiring mechanical knowledge. The education of the artillery officer for conducting his routine duties, and in acquiring the power of enforcing military discipline, was the least adapted for qualifying him for the performance of mechanical duties; therefore, his education ought, hereafter, to be conducted on different principles, more especially as the powers of the weapons, which he would have in charge, would be so much enlarged as to demand more careful cultivation, than had hitherto been necessary. This duty ought to be confined to obtaining complete mastery over the weapon that was placed in his hands; and his experience would then enable him to judge of the merits and demerits of the mechanism of the guns, and thus to indicate to the mechanician, that which should be preserved and that which it was desirable to correct. If, however, in addition to these duties, the artillery officer was called upon to organise factories, and to superintend the construction of guns and projectiles, the demand was such as no ordinary human being could adequately respond to. It was owing to this system, no doubt, that such slow progress had been made in gunnery. It was owing to this, also, that until very recently, guns were bored by means of horse machinery at the Royal Arsenal, at Woolwich, whilst in almost every petty workshop, steam machinery was concurrently employed. It was owing to this, that a highly talented mechanician had been wasting his time in merely managing a gas work, until he had been, literally, 'dug out,' to be placed at the head of a great public establishment, where there was scope for the exhibition of his powers. It was owing to this, that the year 1860 had overtaken the Government, before anything had been realised in the true science and practice of gunnery, although, thanks to Whitworth and Armstrong, the mechanical department was fast approaching perfection.

The Emperor Napoleon III., in the preface to his admirable treatise on the "Past and Future of Artillery,"¹ had offered an explanation on this subject. He said, that the progress of artillery had to contend against two "*ennemis redoutables, les innovations imprudentes et la routine.*" He remarked: "Inventions that are before their age, remain useless, until the stock of general knowledge comes up to their level." Again:—"Whatever is complicated, fails in producing good results in warfare; the promoters of systems forget, always, that the object of progress ought to be, to obtain the greatest possible effect, with the least possible effort and expense." He, however, laid the greatest blame on the opposition of routine,

¹ *Vide* "Œuvres de Napoléon III." Paris, 1856. Tome IVme. "Du Passé et de l'Avenir de l'Artillerie: avant-propos," pp. 15, 17. The Preface is dated, Fortress of Ham, May 24, 1846.

“which, being enamoured with old ways, has preserved for ages, practices that are most stupid.” And “not only does routine scrupulously preserve, like some sacred deposit, the errors of antiquity, but it actually opposes, might and main, the most legitimate and the most evident improvements.”

Although the Government was really beginning, in 1860, to stir in the matter of artillery, yet in point of ship-building the state of the public establishments was scarcely advanced beyond that of the year 1760. There were still in construction, ships to carry, in some cases, one hundred and forty guns, with a full conviction that half a dozen of the projectiles, such as were now made, would destroy any one of those vessels.¹ They could never be brought into a position to use that vast armament effectually, against a fast steamer powerfully armed, or to bear upon a fortress. As to line-of-battle ships, their time had evidently gone by; because the conditions which enabled them to range alongside and to bombard each other, were entirely changed. Fighting at sea would become something like American duelling,—dodging after each other, and seeing who could get the first, or best shot:—in such manœuvring, the advantage would, evidently, be in favour of the fastest vessel.

In America, they were building corvettes to carry only ten, or twelve very heavy guns, with enormous engine power, to attain a higher speed than anything the Royal Navy of this country possessed. With such advantages of speed, having guns only on the upper deck, and having means and appliances for working them, which could not be obtained with the present crowded armaments, the conviction must be induced, that the Royal Navy of England would yet have to be reconstructed.

In conclusion, the President would express a hope, that either during the present Session, or early in the succeeding year, this question of the science of gunnery would be re-opened, with more assured data than had been brought forward on the present occasion.

¹ The recently published statement of the increasing liability of large ships, to strain and to become leaky, clearly proves the necessity of some change, either by the reduction of the present armaments, or by introducing guns of lighter construction.—G. P. B.

McKENZIE AND WENTWORTH'S BREECH-LOADING RIFLES.

After the Meeting, Mr. W. Strode, (Assoc. Inst. C.E.,) exhibited specimens, and explained the action, of Messrs. McKenzie and Wentworth's breech-loading rifles.

The conditions sought to be fulfilled in these rifles were:—

- 1st. That they should be able to take the present ammunition.
- 2nd. That they should have a sound joint at the junction of the breech and the barrel.
- 3rd. That there should be nothing liable to be discharged against the soldier himself, by his own act, in case of the bursting of the rifle; and, therefore, no plug, or stopper to be put in from the back, or breech end of the barrel.
- 4th. That the strength should not be less, and the weight scarcely more, than in the present guns.
- 5th. That the moveable breech should be capable of adaptation to any barrel, as well as to any form of ammunition.

He stated, that these rifles only differed from those of the Enfield pattern, in the breech and in the stock; so that, if the changes were made which these alterations involved, they could be readily manufactured at the Government establishment at that place. It was thought, that such a weapon would be especially useful for Volunteer Rifle Corps, as muzzle-loading would not be necessary.

February 21 and 28,
and March 6, 13, and 20, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

[- The discussion upon the Paper, No. 1,014, "On the Construction of Artillery, &c.," by Mr. J. A. Longridge, occupied the whole of these five evenings, to the exclusion of any other subject.

At the Meeting of March 6, the following Candidates were balloted for, and duly elected:—ALEXANDER BASSETT, as a Member; WILLIAM BEARDMORE, THOMAS BROWN, R.N., JOHN WILTSHIRE CLARKE, HENRY LEE CORLETT, NATHANIEL GREW, JAMES LOVEGROVE, JOSEPH PHILLIPS, EMANUEL ROBERTS, CHARLES RITCHIE WALKER, and CHARLTON JAMES WOLLASTON, as Associates.

March 27, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

No. 1,010.—“On Combined Steam.”¹ By the Honourable
JOHN WETHERED, United States.

IN this age of progress and improvement, there is, perhaps, no branch of science which has more occupied the attention of scientific men, than the application of steam, an agent which has tended so much to ameliorate the condition of mankind, by decreasing the cost of production, by economising time, and by extending commercial relations throughout the world. In looking at the many improvements for generating steam in boilers, by which the heat from the furnaces is, apparently, brought, as closely as possible, into contact with nearly every particle of the water, it might be supposed, that as much heat is utilised as is practicable. And in examining the mechanism of the engine, by which the required motions are so much simplified, it might also be supposed ‘that no great improvement could be made in that direction. By these improvements, steamers have acquired a rate of speed which, a few years ago, would have been deemed fabulous. They have also performed voyages of such extent, as to falsify the predictions of the philosophers, who, a few years since, asserted, that it was impossible for a vessel to carry sufficient coal to steam across the Atlantic.

Notwithstanding the existing rapid steam communication between the several commercial nations, the present progressive race of men, particularly the Anglo-Saxon portion of them, is restless and desirous of travelling at a still greater speed. Governments have caught the infection, for, in renewing ocean mail contracts, greater speed is demanded, even at largely increased subsidies. Few persons not belonging to this and similar associations, are aware of the great additional cost of obtaining an increase of speed. For example, a steamer of 3,000 tons burthen, assuming the consumption of fuel to be only $4\frac{1}{2}$ lbs. of coal per

¹ The discussion upon this Paper occupied portions of two evenings, but an abstract of the whole is given consecutively.

indicated H.P. per hour, will consume, at the several rates of speed, the following number of tons of coal, per day :—

Speed, per hour . Miles	10	11	12	13	14	15	16	17	18	19	20
Coal, per day . . Tons	46	62	80	102	128	157	191	228	271	319	372

To obtain an increase of speed of only 10 per cent., requires an increased consumption of fuel of nearly 50 per cent. ; to double the speed would require eight times the quantity of fuel. The great desideratum appears to be, to obtain increased power, or economy in the consumption of fuel, without the great commercial disadvantage of occupying increased space, by enlarging the dimensions of the boilers and machinery.

The object of this Paper is to call attention to a system of applying steam, by which this desideratum is accomplished. It consists in the application of ordinary and superheated steam, mixed. The mode adopted to carry out the system, is, in addition to the ordinary pipe by which the steam is conveyed to the cylinder, to attach another pipe to the boiler, for conveying the steam to be superheated to pipes, or other contrivances, placed in any convenient form near the fire, or in the up-take, or chimney of the boiler, or in a separate furnace. The superheated steam is thence conveyed by a pipe, which is joined to the ordinary steam pipe at, or before its entrance, into the cylinder. In its passage through the superheating apparatus, this portion of the steam is heated to 500°, or 600° Fahrenheit, by the waste heat. The heat, thus arrested, is conveyed to, and utilised, in the cylinder, by its action on the other portion of steam from the boiler, which is more, or less saturated, according to circumstances. The combined steam is used in the cylinder, at from 300° to 450° Fahrenheit, instead of at the low temperature, at which steam is generally employed. The effect of the two kinds of steam is, that the superheated steam yields a portion of its excess of temperature to the ordinary steam, converting the vesicular water which it always contains, into steam, expanding it several hundred-fold ; whilst, at the same time, the ordinary steam yields a portion of its excess of moisture, and converts the steam gas into a highly rarified elastic vapour, in other words, into pure steam at a high temperature. Experience has proved, that the employment of the combined steam produces a mechanical effect superior to that from steam as ordinarily used, or steam gas, as superheated steam has been properly termed by Professor Rankine.

Since the introduction of the Author's system into England, and its successful application, four years ago, to the Admiralty

yacht, the 'Black Eagle,' and subsequently, to several other steamers, endeavours have been made to employ steam simply dried, or superheated. But this is only an attempt to galvanise the corpse of an obsolete system. It has been repeatedly tried in England, France, and America, and it has been, in all cases, abandoned. To superheat steam partially, will result in a certain degree of economy; but to obtain the full power and economy from its use, it is absolutely necessary, that it should be heated to a high degree of temperature, when it assumes the state of a dry gas, which absorbs the lubricating materials, and, consequently, destroys the packing and the rubbing parts of the machinery. This is the inevitable result, which has rendered its use of no permanent commercial value. It follows, that when all the steam from the boiler is superheated, its temperature must vary, in proportion as the heat of the furnaces varies; the temperature of the steam in the cylinder is, therefore, beyond the control of the engineer. It is well known that, at times, it is extremely difficult to generate a sufficient supply of steam for the engines. When this is the case, 'hard firing' is resorted to, which suddenly raises the temperature of the steam to such a dangerous degree, that it damages the machinery. There are two steamers now in the Victoria Docks, whose cylinders and slides have been so far destroyed, by the use of superheated steam, as to require to be re-bored and replanned. It was the consideration of this difficulty which led to the discovery of the system of employing mixed steam. Independent of the fact, that the mere mixing of the two kinds of steam produces a new motive power, superior to, and more economical, than the use of either, separately, it is a pure vapour of highly expansive force at a high temperature, which is entirely under control, by merely turning a valve, and can be so regulated, as to produce the greatest mechanical effect, with the most perfect lubrication to the slides and cylinders. This is the main advantage which it possesses over all other applications of steam. Another is, that, as the supply of steam to the cylinder is conveyed through two channels, if any accident should happen to the superheating apparatus, the connection with the boiler is cut off, by merely closing the supply valve to it, and the cylinders can be worked with plain steam alone. As a practical illustration, it may be stated, that in working mixed steam, on board the Royal Mail Steam Packet Company's steamer 'Avon,' Mr. Drummond, the engineer, reported, that on one occasion, in the tropics, the temperature of the steam rose higher than usual, when he discovered, that the slides were being affected. Having the remedy at hand, he immediately turned on more saturated steam, since which he has experienced no further difficulty in running 150,000 miles.

For the purpose of testing the comparative efficiency of the three kinds of steam, the 'Avon' was worked; first, with ordinary steam; secondly, with the steam from three boilers superheated, the fourth boiler supplying plain steam; thirdly, with the steam mixed in proper proportions; the pressure on the boiler being, in each case, the same. The result was, that the plain steam gave 1,070 I.H.P.; that when three-fourths were superheated and one-quarter plain, it gave 1,076 I.H.P.; and that when the area superheated, was 61, and that of the plain steam, 69, the result was 1,200 I.H.P.

The experiments which have been conducted by order of the Lords of the Admiralty, extending over twenty voyages, have shown, by the reports of the engineers who were appointed to conduct them, an average economy of 32 per cent. With a proper application of this system, an economy of from 30 to 50 per cent. can be realised, or a steamer will perform a voyage one-third further, at the same rate of speed, with the same weight of fuel. The Lords of the Admiralty are so well satisfied of the value of the system, that its application is being extended, and H.M.S. 'Rhadamanthus,' is to be immediately fitted with it. A plan has been submitted and approved, by which either kind of steam can be employed.

The mixed steam has been employed by Mr. Hobbs, (Assoc. Inst. C.E.,) in the high-pressure boiler and engine propelling his machinery. The application, in this case, simply consists of a pipe, 12 inches in diameter, convoluted in the flues of a Cornish boiler. Mr. Hobbs reports an economy of 33 per cent. in fuel, and in water. It has also been applied, by Mr. Dorman, to an engine which did not produce the required power. As there was no available space for the superheating apparatus, a separate furnace was placed near the engine. This resulted in an increase of power of 30 per cent., as was shown by indicator cards taken before and after the application. Combined steam has been applied to all the steamers of the Collins line. As there was not sufficient space in the up-take for the superheating apparatus, separate furnaces were employed. The effective economy, taking into consideration the increased speed of two revolutions of the wheels per minute, amounted to upwards of 30 per cent. Mr. Turner, Consulting Engineer to the Spanish and Portuguese line of steamers, made a series of experiments, on board the steamer 'Gibraltar,' with plain, superheated, and combined steam. The steam simply superheated, at a pressure on the boiler of 10 lbs., gave 222 I.H.P.; ordinary steam, at a pressure of 14 lbs., gave 307 I.H.P.; while combined steam, at a pressure of 15 lbs., produced 376 I.H.P. The combined-steam card was taken fifteen minutes after the ordinary-steam card, without

any alteration of the fires. The discrepancy between the pressures on the boiler, and those shown by the indicator cards, is readily accounted for. In employing ordinary steam, the pressure in the boiler can never be maintained in the cylinder; whilst, with the use of mixed steam, the pressure in the boiler is not only fully maintained, but it is frequently exceeded. It may be remarked, that the steam in the 'Gibraltar' was partially superheated, before the application of the mixed steam.

If it is asked, why superheated steam at the same temperature, will not produce the same increase of power, or exhibit the same economy as mixed steam, it may be replied, that when steam is merely superheated, or dried, it is converted into steam gas. It partakes, consequently, of the nature of gas; it is a bad conductor of heat, and it gives out, with difficulty, the heat necessary to transform it into mechanical power. On the other hand, mixed steam participates, at the same time, in the qualities of steam proper and of superheated steam, being a pure, highly-rarefied vapour, which readily parts with its heat, and thus produces greater mechanical power. In the experiments which have been made with the mixed steam, the difference of temperature between entering and leaving the cylinder was always greater than when ordinary, or superheated, steam alone was used. As more heat was utilised, greater mechanical power was the result.

Experience has proved, that the application of combined steam presents the following advantages:—An economy of fuel of from 30 to 50 per cent.;—a diminution of about one-third in the quantity of water necessary for feeding the boilers, and consequently, of the deposits which corrode them;—the employment of smaller boilers to obtain the same power;—the facility of maintaining the desired pressure, or of increasing, at will, the motive power, in cases of emergency;—a steamer will make a voyage one-third further with the same weight of coal, or one-third of the space now occupied by coal may be used for freight;—there is less risk of explosion, as there is no necessity for a high pressure in the boiler;—boilers will last one-third longer;—a better vacuum is obtained;—and finally, one-third less injection water is required.

It may be proper to state, that this Paper presents no crude theory of an untested principle; but the faithful description of a plain and simple application of the combination of vapour and gas, by which, not only the long-known increased power of the latter, at high temperatures, is effectually controlled, but the fact, highly interesting to science, is established, that the power is greatly increased. At the temperature which is necessary to develop the full power, steam gas becomes so entirely dry, that for want of lubrication, it is destructive to the rubbing parts

of the machinery ; whilst combined steam develops increased power, which can be utilised to its maximum extent, with entire safety, as has been most satisfactorily demonstrated, by the test of long experience.

The Paper is illustrated by diagrams, showing the results of the different experiments.

The Honourable JOHN WETHERED said, that since the Paper was written, he had learned, that in one instance, the coil of pipes in the separate furnace built on the plan of Mr. Perkins, had become oxidised and destroyed, where there was actual contact with the fire, after the apparatus had been in operation only a few months. It had been said, that iron pipes could be thus employed without injury, but he had never met with any such instance in his own experience; when thus exposed, they answered very well for hot water, but not for steam.

He had requested Mr. Dinnen, who was appointed by the Admiralty to superintend the experiments in H.M.S. 'Dee,' to take indicator cards, with superheated and combined steam at the same pressure, taking care that the supply valves should be each of equal area. The cards showed, that a better vacuum was obtained, and that the expansion was much greater, when using combined steam. During the voyage of the 'Dee' to Plymouth, when those cards were taken, the consumption of fuel, by using combined steam, was 2·57 lbs. per I.H.P. per hour, whilst the average consumption in thirteen trials with plain steam, was 5·53 lbs. per I.H.P. per hour. The engines, it should be stated, were built, twenty-seven years ago, by Messrs. Maudslay and Co.

In reply to a remark by the President,—that the boilers of the 'Dee' might have been new, and that the superheated steam appeared to have been used at a pressure of 8 lbs., whilst the pressure of the combined steam was higher,—Mr. Wethered said, the object was to take the cards at exactly the same pressure, but as soon as the superheated steam was turned off, the pressure invariably rose half a lb.

In reply to other questions from Members, he stated, that he proposed to mix the steam as near to the cylinder as possible, and in the case mentioned, this was done close to the steam chest; and that 2 superficial feet of heating surface for each nominal H.P., would give about 500° of temperature.

Mr. LONGRIDGE had not lately paid much attention to superheated steam, but having used it, about fifteen years ago, he found the economy to be much nearer 10 than 30 per cent. In one, or two instances, he was obliged to take the steam through a long range of pipes, and in spite of all the protection he could give them, there was a considerable amount of condensation, although the vertical pipe of cast iron was surrounded by flame, and a special furnace was built for the purpose. If the engine was a moderately good one, the advantage of heating the steam in the cylinder, would not amount to 30 per cent. By passing steam into the cylinder and using it expansively, a portion of the steam was, no doubt, condensed; and if a sufficient charge of heat could be previously given to the steam, to prevent that condensation,

a nearer approach might be made to the true theoretical result of expansion, and a better vacuum obtained. But even if the best system of superheating was applied to the best expansive engine, and condensation was altogether prevented, he doubted whether the benefit arising from superheating, would amount to more than about 15 per cent. Great importance had been attached to the effect of ordinary superheated steam upon the valves and the packing ; now if this extra-heated steam was introduced, along with the other steam, into the steam chest, so as to give it that charge of heat necessary to prevent condensation, he could not see what difference there was between that system and heating the whole of the steam exactly to that point. He had also directed his attention to the amount of heat that could be imparted, by superheating the up-take. He did not know what proportion of the steam it was intended to pass through the apparatus employed for this purpose, but in any case, the amount of heat imparted would be small. Some experiments, in which he had formerly been engaged, relative to the consumption of smoke, bore upon this point, as they, eventually, took the form of ascertaining the evaporative power of different fuels. For this purpose he employed a good marine boiler, of the ordinary type, and he found, that in the up-take before the chimney, the temperature of the gases was, generally, when burning a moderate quantity of fuel, about 600° ; and that when burning a large quantity of fuel, say 30 lbs. per square foot of fire grate per hour, with the ordinary draught, the temperature rose to 700° and 750° . The general temperature of the gases, however, was 600° . He then took off the chimney and put up a vertical tubular boiler, through which all the gases passed, the tubes being 3 inches in diameter. These tubes contained, in the aggregate, 320 square feet of exposed surface, or nearly equal to half the whole surface of the boiler, which was 740 square feet. The water entered the heater at a temperature of about 60° , and as was expected, reduced the temperature of the chimney, but only to a small extent, for the gases which entered the heater at 600° , came out at 550° ; so that only 50° of heat were taken off by the 320 feet of surface. He had ascertained the temperature of the gases before they passed into the tubes, by Daniell's pyrometer, and he found, that with brisk combustion, it was about $3,000^{\circ}$. The tube surface reduced this temperature from $3,000^{\circ}$ to 600° , and thus four-fifths of the heat of the gases were abstracted, whilst by the 320 square feet of the heater, only one-twelfth of the remaining fifth was abstracted, or one-sixtieth of the whole, showing the small effect of the heating surface in the up-take.

Mr. A. C. HOBBS stated, that he had a small engine, with a Cornish boiler, which formerly required 36 cwt. of coal, per week. By the introduction of a pipe 40 feet in length, into the side flue,

the temperature in the cylinder was raised from 270° to 330° , or 340° , and the consumption of coal was reduced from 36 cwt. to 28 cwt. per week. With another set of pipes in the other side of the boiler, the temperature rose to 420° , but at that temperature, a peculiar action took place in the cylinder which induced its reduction to 360° , by the introduction of plain steam; and at that point, the consumption of fuel was as low as 24 cwt. per week. Formerly, the tank containing the feed water, ran short; but now, one-third of the water was unconsumed daily. The same man had attended to the firing of the furnace, before and after, the change. The economy thus effected, without any alteration of the engine, or of the boiler, was one-third, and it had continued in the same proportion, for the last eight, or nine months. He had since tried the experiment of discontinuing the superheated steam for some days, when the consumption of coal and water invariably rose to the original amount.

Mr. HUMPHRYS said, that to the Author of the Paper was due all the credit of having taught the use of superheated steam. His own attention was first drawn to the subject, by the experiments of the Admiralty; and from what he then observed, and from his subsequent experiments, he did not think, that the mixing of steam could afford any advantage superior to that of superheating the whole. He was employed by the Peninsular and Oriental Company, to apply superheated steam to those of their vessels of which he had supplied the engines. The 'Ceylon,' which ran between Southampton and Alexandria, was the first; its engines had cylinders 72 inches in diameter, with a stroke of 3 feet, and making from fifty to sixty strokes per minute. At that time, she had run a distance of about 42,000 miles, having performed seven passages with ordinary steam; and the consumption of fuel, out and home, was 1,500 tons per voyage. The superheating apparatus was applied, and it increased the temperature of the steam, rather more than 100° . The surface in that apparatus was about 1 foot per I.H.P., the engines working to three, or four times their nominal power. She had now completed five voyages, or a distance of 30,000 miles, and the average consumption of coals had been, 1,100 tons per voyage. The cylinders, slide valves, and all the parts of the engines, were in better condition now, than when the apparatus was first applied. In the 'Alhambra,' the consumption of fuel, previous to superheating, was 19 cwt. per hour; it was now reduced to 13 cwt. per hour, with the same, or rather an increase of speed. The economy effected in the 'Nepaul,' was most remarkable; she formerly consumed 36 tons per day, whereas since superheating had been used, the consumption had declined to only about 17 tons per day. The whole of the steam was superheated, and the temperature was in-

creased about 100° over that due to the pressure. In all cases, he allowed one superficial foot of surface in the superheater for every indicated H.P.

In reply to a question from Mr. Wethered, Mr. Humphrys said, it was difficult to ascertain the consumption per I.H.P. The engines of these vessels were, generally, worked at about three times the nominal power; the 'Nepaul,' of 200 H.P., was working up to 700 H.P.

Mr. PATRIDGE acknowledged, that although experiments in superheating steam had been previously tried, great praise was due to the Author, for having recalled public attention to the advantages to be derived from its employment. He observed, that the indicator card representing the superheated steam alone, was produced by steam throttled by the small size of the steam pipes; whereas that representing mixed steam, was produced without any such obstruction. The engines from which those cards were taken were two in number, each of 100 H.P. The two superheated steam pipes conveying the steam to the cylinder, were not in connection. When the arrangement of the steam pipes was first made, it was not intended to work both engines with superheated steam, at the same time, but each engine with plain and with superheated steam, alternately, and the minimum diameter of the pipes which conveyed the superheated steam, was only 9 inches. It was afterwards decided to make trials with superheated steam alone, in both engines at the same time, and then the two superheated steam pipes were connected, and larger stopcocks were fitted. The cards taken since that alteration, had been quite equal to that representing the mixed steam, and in his opinion, the comparative fulness of the steam line of that card was due to the fact, that the large ordinary steam pipe was in connection with the engines, in addition to the superheated pipe, and not from the fact of the steam being mixed. Taking a cubic foot of steam at a pressure of 10 lbs., raised to a temperature of 300° by a direct process of superheating, then taking superheated steam at 500° mixed with ordinary steam so as to bring the mixture to 300° , his impression was, that the water in a cubic foot of either steam, at the same pressure, would be equal; and that, therefore, their lubricating qualities would be similar. Great economy, no doubt, resulted from the use of superheated steam; he believed that, in the best boilers, if the steam was raised 100° beyond the temperature due to its pressure, the saving would be equal to 15 per cent. But in one vessel, with an indifferent construction of boiler, when comparative trials were made with plain and superheated steam, the result was an economy of 34 per cent., in favour of the employment of the latter. This was partly due, in his opinion, to the construction of the boiler, and to the application of the apparatus having

increased the amount of steam room. He did not believe, that the use of superheated steam was detrimental to the engine; the 'Dee' was a proof of this. In that vessel, the temperature of the superheated steam at the boiler was about 380° , the length of the main steam-pipe being between 30 feet and 40 feet. The difference of temperature between the steam at the boiler, and at the end of the steam pipe next the cylinder, was about 20° . There was a considerable reduction in the temperature of the steam in the slide jacket, before it entered the cylinder, so that, although the steam at the boiler was 380° , it was reduced about 70° in the slide jacket, prior to its entering the cylinder, bringing it down to 310° , and sometimes, to 300° . The 'Dee,' he might add, had been running with superheated steam for more than two years, yet there was not the slightest appearance of any injury to the machinery; nor had there been any deterioration, or difficulty in the packing.

In reply to a question from the President, Mr. Patridge said, that in all cases, with ordinary steam, the temperature in the cylinder was much lower than that due to its initial pressure, but he had found it very difficult to ascertain the temperature of steam in the cylinder, owing to its rapid decrease throughout the stroke.

Mr. JOHN TOPHAM, having had opportunities of fitting superheating apparatus to vessels, would state the results. In one of the vessels belonging to the Intercolonial Royal Mail Company, the consumption of fuel was, formerly, 2,986 lbs. of coal per hour. The apparatus, consisting of a number of boxes of short tubes, fitted in front of the ordinary boiler tubes, and connected, by a wrought-iron cylinder placed in the funnel, with the boiler, through which the steam passed freely, was applied, and had the effect of immediately reducing the consumption to 1,900 lbs. per hour, with scarcely any variation during the passage of four, or five days, from London to Milford Haven. The temperature of the superheated steam was from 310° , to 340° and 350° . In the steam pipe, half way between the boilers and the cylinder, the average temperature was 330° . No injurious effects resulted from the use of superheated steam, nor was there, in the voyage of the same vessel from Milford Haven to the Cape of Good Hope, any trouble experienced with regard to the packing, which was of the ordinary kind, canvas and india rubber. The 'Airedale' was also supplied with the same apparatus, and the report of her performances was so favourable, that the Company determined to have their other vessels fitted in a similar manner. He would particularly call attention to the fact, that, within certain limits, the area of the heating surface in the superheating apparatus, should be as large as possible. In a new vessel recently fitted on this system, the area was only 45 superficial feet, scarcely more than half a square foot per nominal H.P.; that proportion of

heating surface was far too small, and would be of little use. In another case which had come under his notice, a wrought-iron cylinder was fitted into the lower part of the chimney and filled with seventy-eight, or eighty tubes, $4\frac{1}{4}$ inches in diameter and 7 feet in length. The area of heating surface was 800 feet, or 4 superficial feet per nominal H.P. He expected, that the consumption of fuel in that vessel would be much reduced. The Author's system of combined steam was applied to the 'Gibraltar.' When the boilers were first fitted, the superheating system was partially adopted. Between the tops of the ordinary boilers was placed an ordinary cylinder boiler, with the addition of a tube to carry the heated air from the up-take through this boiler, which was connected with the others by steam pipes, into the chimney, so that before entering the funnel, the smoke and flame passed through the tube in the cylinder boiler, for a space of 14 feet, or 15 feet.

Mr. PARSON said, that the advantages of superheating had been sufficiently established, in an economical point of view, by the results already given; therefore, not being himself a professional Engineer, he would not have offered any remarks upon this subject, but for the observations which had been made upon the system of superheating by means of pipes placed directly over the fire,—a system in which he was much interested. That arrangement had been adopted in the 'Osprey,' one of the boats of the Watermen's Company, which had been running uninterruptedly for ten months. In no case had the pipes lasted less than five months, and whenever they had given way, it was owing to their not having been properly welded; no injury had ever occurred, when the weld was double lapped. The saving by this arrangement was greater than that effected in the 'Dee.' The certificate of the manager of the Waterman's Company stated, that it amounted to between 30 and 40 per cent., and it had, therefore, been resolved, that the whole of that Company's vessels, thirteen in number, should be fitted with the apparatus, for the approaching summer. The engine had not sustained the least injury, for according to the statements of the Company's engineer, the cylinders in the 'Osprey' were as perfect as when common steam was used. The Author of the Paper had visited the 'Osprey' on several occasions, and had expressed his admiration of the apparatus employed.

Mr. HUMPHRYS said, that a good example of what might be accomplished by superheated steam, was afforded by an engine in use at the works with which he was connected. The piston travelled at the rate of 530 feet per minute, and the consumption of coal did not amount to 2 lbs. per I.H.P. per hour. At present, 15 lbs. of water per H.P. per hour, were converted into steam; whereas before superheating was applied, the quantity was 17 lbs.

per H.P. per hour. In this instance, all the steam passages were surrounded with steam cases.

In answer to a question by Mr. Greaves, Mr. Humphrys said, that in the case of the Peninsular and Oriental Company's vessels, previously referred to, the cylinders were not jacketed.

Mr. W. BEARDMORE said, that by the use of superheating apparatus, he had effected a saving of from 15 to 25 per cent. in the consumption of fuel. In some instances, where the boilers did not freely generate steam, the economy was apparently slight, but in boilers affording a good supply of steam, it was as much as 23 per cent. In one of the boats belonging to the General Steam Navigation Company, travelling between the Thames and Scotland, an average of twelve voyages, previous to superheating, showed a consumption of 126 tons of fuel per voyage; it was now reduced to about 90 tons per voyage. That was the largest amount of economy he had observed. The temperature of the up-take was from 650° to 680° . After superheating, it was not reduced more than 50° , but the temperature of the steam was increased 100° .

Mr. HOWARD had expected, that greater prominence would have been given in the discussion, to the rationale of superheating steam. He had practically experimented upon it on a large scale, about thirty years since, having, long ago, perceived the scientific grounds for its adoption, and wherein its chief advantages consisted. Although the Author was entitled to all the credit which had been given him, for having resuscitated the question, and having brought it prominently before the Engineers of this country, Mr. Howard doubted, whether he had rightly understood the theory of its use. There was no advantage in mixing steam. Two quantities of water at different temperatures, might be mixed together and be called combined water, with as much propriety as to give the name of combined steam to two quantities of steam, at various degrees of heat. It was not strictly a combination, but a mixture. Whether steam were superheated by external appliances, or by adding surcharged steam, it made no difference. There could be no doubt of the advantages of the use of superheated steam, but the cause must be sought for in another direction. He had made experiments on the four low-pressure engines at his own works, the King and Queen Ironworks, Rotherhithe, further confirmatory of his views, that the gain did not arise from any physical law, but from the prevention of loss in the use of dense steam. When the steam entered the cylinder, if there was but one degree of heat less in the cylinder, water must be formed, the resultant effect of which had not been appreciated; not even, he believed, by Watt himself, for the pressure in the cylinder being maintained from the boiler, his pressure gauge, or indicator did not show the loss.

When the vacuum stroke was made, the deposited water being relieved of the pressure due to its temperature, rapidly passed off as rarefied vapour, cooling the interior of the cylinder. On the steam entering to make the return stroke, it brought the cylinder up to a temperature due to its pressure, for otherwise, it could not act to its full pressure; and the stroke was made at that loss, by the deposit of water over the whole interior surface of the cylinder and its adjuncts. When the condenser again came into action, the same thing recurred, and so on continually. He had applied the pressure indicator simultaneously with thermometers, and he found, that the loss of temperature in the cylinder, without working expansively, with an average pressure of 8 lbs. in the boiler, approached 20° , which indicated a loss of steam of between one-third and one-fourth. This injurious effect must always occur by using dense steam, whereas, by using superheated steam, there was no deposit of water, and the action was analogous to that of a permanent gas, but with the advantage of easy and complete condensation. This, he believed, to be the rationale and the cause of its superiority. It was also stated, that by mixing superheated steam with dense steam, the power was increased, by vaporising the vesicular water which came from the boiler. Now supposing, for example, (for the same proportion would hold good in all cases,) that in a cubic foot of steam, at the atmospheric pressure, being 1 cubic inch of water with $1,000^{\circ}$ of latent heat, there was half a cubic inch of free, or vesicular water, in suspension, and that all the latter was to be vaporised by the application, or admixture of superheated steam; then there must be added the necessary proportion of 500° for the latent heat of conversion, giving an increase in volume of half a cubic foot. A cubic foot of superheated steam, surcharged by 500° , or more strictly, 460° , would become double its original volume. Now the 500° had been taken from the superheated steam, and thus its own volume was reduced one-half. With all deference to the Author, he believed his theory of the combination of the two kinds of steam to be a fallacy, and he could not accept it as explanatory of the advantages unquestionably attendant upon the use of superheated steam; nor was it necessary to raise the temperature very high, about 100° , or indeed less, above the normal temperature, being ample. There was another gain in superheating. When the vacuum stroke was made, since the cylinder was perfectly dry, the exhaustion of the whole steam was performed in a moment; but with dense steam, the cylinder being wet, the deposited water had to be vaporised and condensed, and thus damaging the vacuum. As this action did not occur with superheated steam, some increase of mechanical effect was produced. The condensing appliances were also relieved, by so much as was gained in the cylinder.

He had used such steam expansively, for which it was well adapted.

Mr. PATRIDGE said, his experience was confirmatory of the fact, that by the use of superheated steam to obtain the same amount of power as by ordinary steam, the feed water was reduced about one-third, and about one-fourth less water was required for condensation.

Mr. CHARLES MAY observed, that in the explanation which had been given of superheating, there was one element which had been omitted. When steam was expanded, a large quantity of the heat became latent; the full effect, consequently, was not obtained from expanding ordinary steam, because, as it expanded in the cylinder, it cooled, and there was not sufficient caloric to keep up the specific heat during the stroke. He thought the practical limit of the use of superheated steam would be found, in giving it such an additional amount of caloric, as would permit of its remaining dry steam, to the end of its required expansion.

Mr. HOWARD observed, that while by superheating steam, the loss arising from the use of dense steam was prevented, another advantage was derived by the utilisation of the waste heat in the up-take. The bulk of the steam being doubled for every 460° of temperature, about one-fifth, consequently, was added to it by the 100° gained in the up-take, and that was sufficient to support the elasticity of the steam, from one end of the stroke to the other. In his own experience he had found little advantage to have resulted from the use of steam jackets, and experiments showed, that the heat was not transmitted with sufficient rapidity through the jackets, to have any material influence, at least, when the piston was reciprocating very rapidly.

Mr. DINNEN,—through the Secretary,—would call the attention of the Meeting to the views entertained by many professional men, respecting the condition of common steam, before superheating. The Paper spoke of ‘vesicular water’ being converted into steam; others had spoken of ‘wet steam.’ Now this was altogether opposed to his own experience and observation; he had never found wet steam, unless the water of generation was more, or less charged with adventitious matters and caused the boiler to prime; then the water was manifestly vesicular, for the vesicles of steam were seen in active motion in the glasses of the water gauges. It was true, that water, not vesicular, would pass over with the steam, if too little steam room was afforded; but that was not priming, properly so called. The best practical proof of steam from a boiler being plain, or common steam, was, that he had travelled over thousands of miles at sea, without drinking any other water than that produced in the jacket of the steam engine; had it been vesicular it must, he thought, have been salt. In fact, the law, that

pressures and densities were invariable, and the formulæ founded on experiments thereon, must fall to the ground, if the Author's views were correct. Steam, in contact with its water, must, therefore, be taken as saturated, in considering the question of superheating alone, or of mixing according to the principle of the Author. He had made several voyages in H.M.S. 'Dee,' of 200 H.P., with the view of testing the comparative advantages of both systems. He would first give the results of two trials with common steam, against steam simply superheated. Equal pressures of $3\frac{1}{2}$ lbs. in the boilers, gave equal H.P. and equal amounts of injection, but with an economy of 23·8 per cent. in favour of superheating; each boiler and engine being alternately used on the respective systems, while still working on the same shaft, so that external influences operated equally on both. It was worthy of remark, that the plain steam exhibited, within 1° , the same temperature at the boiler, as it did after passing through the jacket to the slide casing; while the superheated steam fell 20° on its passage from the boiler to the cylinder, a distance of $37\frac{1}{2}$ feet. It fell 82° after entering the jacket, and it lost 26° more, after entering the cylinder; while the plain steam, after making its entire circuit from the boiler into the cylinder, lost only 23° . The superheated steam left the boiler at 382° , and it retained 251° in the cylinder; while the plain steam was 220° at the boiler, and 197° in the cylinder. The most curious and instructive feature of this experiment was, that there was no water formed in the jacket, when superheated steam was used; but a considerable quantity was deposited, as usual, from the plain steam. Another experiment with superheated and plain steam, when each system was allowed full play, gave the following results. Steam, when superheated, was easily kept to a pressure of 7 lbs. in the boiler, but a pressure of only 4 lbs. was maintained, when common steam was used; with superheated steam, 245 H.P. was obtained, and with plain steam, only 213 H.P.; and there was, moreover, an economy of coal equal to 20 per cent. in favour of superheating.

The Author's system, when manipulated by himself in the 'Dee,' so as to obtain, as nearly as possible, equal temperatures in the cylinders, was about as economical as the superheated steam, when the whole was passed through the superheater. The H.P., on both systems, was about the same, when each engine was worked on a separate principle: but when the engines were worked in pairs, on the respective systems, Mr. Wethered was enabled to develop more power, on account of the superheating pipes being too small to pass all the steam that the boilers were capable of developing. The fact of the superheated steam having lost 20° only, by radiation from the long steam pipe, while at the jacket it lost 82° , pointed directly to the seat of loss of both power and economy. The necessarily low mean temperature of the masses of

the cylinders, pistons, &c., and the large amount of jacket water yielded, when plain steam was used, without any tangible difference in temperature between the boiler and the jacket, must be conclusive, that much power was sacrificed, before the steam entered the cylinder. It should be remarked, that a certain quantity of jacket water was always obtained, when combined steam was used, but none was ever obtained from the superheated steam, until it was cut off by the expansion gear before half-stroke, when jacket water manifested itself, and the economy of fuel began to fall off. This, as he had remarked on a former occasion,¹ might be considered the dew point; and superheating, to be fully effectual, should, in his opinion, be carried at least as far as in the 'Dee,' where the best result at full power, was 2.6 lbs. of Welsh coal per indicated H.P., when both engines were working on the same system, at the same time. If superheating was carried farther than was necessary to prevent condensation in the jacket, and therefore, in the cylinder, the effect was likely to be injurious to the working parts, and to carbonise the packings and lubricating substances. Neither of these results had occurred, during his voyages in the 'Dee.' When an excess of heat, beyond the point alluded to, was experienced in superheating, the remedy must be looked for in the boiler, unless there was a power of reducing the temperature.

The amount of heat suddenly lost at the jacket,—from 80° to 90° in the 'Dee,'—pointed to that portion of the apparatus as the one through which means should be found, for maintaining a suitable temperature in the cylinder. He thought, therefore, that the whole of the steam passing into the cylinders, should be made to pass through the jacket, if the construction would admit of it, rather than that the jacket should be fed by small pipes.

Subsequent voyages in H.M.S. 'Rhadamanthus,' fitted with engines of about the same power and construction as those in the 'Dee,' furnished results entirely confirmatory of those obtained in the latter vessel.

Mr. RENNIE had witnessed the experiments of the Author at Paris, and he fully believed, that the saving effected by the Author's system was from 24 to 30 per cent., but he had not actually verified the fact. The experiments he had himself conducted, merely carried out the ideas of others. The firm with which he was connected, had been early in the field, and had fitted eighteen sets of apparatus, some upon the plan of Mr. Patridge with tubes, and some on Mr. Lamb's principle with flues, from both of which good results were obtained. With the first, he considered the saving to be about 25 per cent., and with the other about 30 per cent. He had supplied the superheating

¹ *Vide* Minutes of Proceedings Inst. C.E. vol. xviii. p. 277.

apparatus for the 'Pera,' which now consumed 380 tons less than formerly, or about 1,200 tons of coal per voyage. He was desirous of knowing why the Author preferred combined steam, and what advantage he found in using several small boilers, instead of one large boiler.

Mr. C. W. SIEMENS had, for nearly the last fifteen years, occupied himself with this question. When he first turned his attention to the subject, he endeavoured to ascertain the advantage of superheating steam, and he made some rather elaborate experiments to determine the rate of expansion. He found, that Gay-Lussac's law of an uniform rate of expansion of elastic fluids by heat, was not applicable, but that steam, near its point of saturation, expanded in a greater ratio than afterwards. A curve representing the rate of expansion was rounded at the beginning, but gradually approached a straight line; it was, in fact, a hyperbola, the asymptote of which ran parallel to a line representing the uniform progression of expansion of air. So that at a temperature considerably removed from the boiling point, air and steam expanded, practically, at the same rate; whereas at first, steam expanded four times, or five times more than air, for the same increase of temperature. He also found, that steam near its point of saturation, possessed great capacity for heat; and therefore, by simply superheating steam, no great economy could be produced. The results of recent practice showed, however, in many instances, a very large saving, which might be ascribed to a secondary cause. Steam, in expanding behind a working piston, lost a portion of its heat, which was converted into mechanical effect. This heat was entirely lost, and a portion of the steam must condense, or form water. This water re-evaporated when the pressure had become reduced by expansion, and cooled the sides of the cylinder, thereby producing condensation in the fresh steam from the boiler; and thus an action was established on the sides of the cylinder, (resembling that of a sponge, which, under the influence of an alternating pressure, absorbed and emitted water,) by which a certain amount of steam passed, at each stroke, through the cylinder, without exerting its elastic force upon the piston. To prevent this action, it was necessary to sufficiently increase the temperature of the steam, that it might be expanded through the stroke, without condensation. According to the present well-established law, expressing mechanical force by the quantity of heat, he found, that an increase of temperature of 100° Fahrenheit, would suffice to prevent condensation in an ordinary expansive engine. Increasing the heat beyond that point would not be accompanied with any marked economy, and would engender many difficulties; lubrication would become difficult, and friction would be produced. Great stress had been laid upon the

peculiar properties of mixed steam, and the advantages which it possessed over superheated steam, but he had never seen any remarkable results from its use. In an engine which he designed, he virtually used the two steams together, inasmuch as, at each stroke, he added to a volume of superheated steam, a certain proportion of saturated steam; but he never observed any spontaneous increase in the bulk. There was, however, one advantage in the Author's system,—by mixing ordinary saturated steam with superheated steam, he had the means of regulating the temperature. If, then, a compensating rod was introduced into the steam pipe, so as to limit the admission of steam in proportion to the temperature, beneficial results might be obtained. He was convinced, however, that in simply superheating steam, the ultimate degree of economy would not be reached. The chief advantage of superheating was to prevent condensation in the working cylinder, whereas Mr. Siemens's object had been, and still was, to prevent the loss of the latent heat of the steam, by means of the regenerative system. Although he had advanced but slowly, owing to the difficulties and expense attending experiments of this description, he had two engines at work, which gave him every confidence of ultimate and complete success.

Mr. LAVINGTON E. FLETCHER remarked, that the idea of superheating steam was not new. Fifteen years ago, there were two locomotive engines on the Great Western Railway, which were each fitted with a superheating apparatus, consisting of a tubular steam chest placed in the smoke box, the hot air passing through the tubes; but the system was not favourably received. Messrs. Hawthorn, of Newcastle, made several locomotive engines, with return tubes for superheating the steam, but they did not come into general use. Others tried superheated steam, and condemned it. In America, Mr. Frost brought out a system of superheating to a high temperature, but the engine was so much injured by friction, that the experiment was abandoned. These repeated failures had caused the use of superheated steam to be much neglected, until it was successfully applied by the Author, at moderate temperatures, to marine engines. Mr. Fletcher differed from the Author as to the merits of combined steam. He had been shown the results of most elaborate experiments made by competent Engineers in France and America, who reported, that they had certainly met with anomalous effects for which they were not prepared, and that combined steam appeared to present superior advantages to ordinary superheated steam. Until he had had the opportunity of verifying these results, he could not, however, pronounce in favour of combined steam, since its stated action was altogether of so novel and anomalous a character. He was about to apply it in one, or two cases, and if he found any

advantage from its use, he would communicate the result to the Institution.

As to the rationale of the economy resulting from the use of superheated steam, some views had been advanced with which he could not agree. It was said, that the economy was produced by the evaporation of the vesicular water which passed over with the steam, when the steam was not superheated. But the gain arising from this could not be large, for even if one-fourth part,—and that would be an extreme proportion,—of the water supposed to be evaporated in the boiler, passed over unevaporated, in a vesicular state, the heat carried over by that amount of water, and consequently wasted, would be only one twenty-fourth part of the whole heat of evaporation; thus the gain by the prevention of this waste would not be more than about 4 per cent., and was inadequate to explain the full amount actually experienced, which was 25 per cent. By others, the economy was attributed to the utilisation of waste heat in the funnel. But since the steam was only raised about 120°, by superheating, it followed, taking the specific heat of steam into account, that the heat absorbed in superheating to that point, would not amount to more than about 3 per cent. of the whole quantity; this cause, therefore, was also insufficient to account for the result produced. The principal source of gain in the use of superheated steam consisted, in his opinion, in the prevention of condensation in the cylinders, which gave rise, as he had already observed at the discussion, in 1859, on the combined-vapour engine,¹ to an alternate action of re-evaporation and condensation, destructive of all economy. He so entirely agreed with the remarks of Mr. Howard upon this subject, that he thought it unnecessary to offer any further explanation. In addition to the direct economy produced in the expenditure of steam in the cylinder, there would also result an indirect advantage from the relief afforded to the boiler, which would enable it to perform its duty more economically. He concluded, therefore, that whilst the economy produced by superheating steam mainly arose from working with dry steam, and thus preventing condensation in the cylinders, all the other causes, to which should be added the increased volume of the steam due to superheating, contributed to the advantageous result. He thought, that if the use of Watt's steam jacket had been continued to the present time, it would have saved the steam marine a large sum. The action of the steam jacket had, evidently, been misunderstood, and its advantages unappreciated. Mr. John Bourne, in his "Catechism of the Steam Engine,"² said:—"The cause of this effect is not of very

¹ *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., p. 282.

² *Vide* "A Catechism of the Steam Engine, &c." By John Bourne. 8vo. 4th edition. London, 1856. Page 288.

easy perception, for the jacket exposes a larger radiating surface for the escape of the heat, than the cylinders; nevertheless, the fact has been established beyond doubt, by repeated trials, that engines provided with a jacket are more economical than engines without one." Tredgold described the steam jacket as a useless incumbrance to the engine, which had better be dispensed with.

Mr. J. JAMES mentioned, that many years ago, Captain Douglas had used superheated steam in India. He was about to return to England, and would willingly furnish all the information he possessed. Mr. James had been engaged in experiments for him on the subject, but was not, at present, in a position to give an account of them.

Mr. GREAVES observed, that there was a general law which supposed the existence of a relation between temperature and pressure, in all conditions of steam. But in pursuing the variations of this relation, it became necessary to separate fluidity from elasticity, and to distinguish between the mere enlargement of the cylinder space, filled with steam of initial density, or a similar enlargement accompanied by a density diminished simply by spontaneous expansion, and carrying a diminished load; and an enlargement arising from a strong initial impulse, as in the Cornish engine, and from a continued impulse, as in rotative engines, whether for locomotive, or fly-wheel motion. To counteract the loss of force, or power due to condensation, or external radiation of heat, superheating, clothing and jacketing were, undoubtedly, most useful. Any process by which steam could be generated and maintained at a temperature sufficiently high, that it should never condense in the cylinder, would be attended with economy; but beyond this, he was not of opinion, that there would be any advantage from superheating, when engines were not worked expansively. When an engine was worked on the Cornish system, and a load, say of 12 lbs. per inch, was started into motion, by a pressure of 33 lbs., diminishing, after the steam was cut off, to 6 lbs., or 7 lbs., the motion being altogether irregular,—or in a rotative engine, or in an engine with a fly-wheel, where the same irregularity of pressure took place, yet with a regular motion,—whenever, in short, the motion was not produced by a force always maintained equal to the load, then the necessity for superheating, or clothing and jacketing became urgent, and to neglect such provisions was most prejudicial. In these cases, the expansion was not continually spontaneous, and therefore, to the loss of heat by radiation, was to be added a further loss due to the expansion produced by the result of a past effort. This, for the sake of economy, should be obviated, by maintaining from another source, the highest possible temperature the steam itself could carry. Al-

though fully impressed with the advantage and economy of steam jackets, where they could be conveniently fitted, to engines working expansively, yet he had no doubt, that equal benefit would be obtained by superheating, where the jackets could not be applied. But in engines not worked expansively, the same advantages might be obtained by a proper system of clothing.

Mr. HOBBS observed, that his engine was not worked expansively, yet the economy was about 33 per cent., simply from superheating.

Mr. E. A. COWPER differed from the opinion, that if an engine was worked with full steam throughout the stroke, a steam jacket would not be of any advantage. If, indeed, the cylinder was never opened to the condenser, or to the atmosphere, he agreed, that the water which was condensed would then remain in the cylinder; but whenever the eduction valve was opened and the steam was let out of the cylinder, evaporation took place, and the effect on the cylinder was the same as in an expansive engine; the only difference being, that in one case, the expansion took place quickly and after the piston had made its stroke, whilst in the other case, the expansion took place more slowly, and during the time that the piston was making its stroke. He granted, that there would be as much radiation from the steam jacket as from the cylinder, but the advantage of steam jackets was far greater than the mere question of radiation indicated. If the cylinder was kept hot by a steam jacket, no steam would condense in the cylinder when the steam entered it, and, therefore, no water would have to be evaporated when the steam passed out; thus all the steam passing through the cylinder would be utilised, instead of a portion only, as was the case in all engines without steam jackets. He had taken numbers of diagrams, showing the extent of the loss which occurred with different amounts of expansion, when no steam jacket was used, and he had mentioned the results last Session.¹

In one case, when the steam was cut off at one-eleventh of the stroke, the loss was 44 per cent. Immediately after the steam was cut off, the fall in the curve was very rapid, owing to condensation; instead of falling below the true expansion curve at the end of the stroke, it fell much slower, and towards the end of the stroke, it had almost a tendency to become horizontal, from the re-evaporation of the water. He thought, therefore, that steam jackets were very valuable in any engine, whether working with full steam, or expansively, and he rejoiced to hear, that Government had given instructions, that several marine engines, which they had lately ordered, should be supplied with them.

So far back as 1834, or 1835, when he was a pupil, he wit-

¹ *Vide Minutes of Proceedings Inst. C.E., vol. xviii., p. 284.*

nessed some experiments with Ericsson's engine, in which superheating was carried to a higher degree than in any of the cases now alluded to. The results showed, that an engine of 24 H.P. could be worked with a very small fire, but there were, at that time, some practical difficulties which prevented the general adoption of the system. In 1843, an engine was worked at Coventry, with mixed air and steam; the air was pumped at a high temperature through pipes and mixed with common steam, and considerable effect was produced, though, probably, without any economy.

The first advantage of superheating, was to evaporate all the water contained in the steam; the next was to prevent the loss by condensation in the steam pipe, and the cylinder, thus saving the cost of a steam jacket; the third was to increase the bulk of the steam; the fourth was, that more of the steam was condensed into water, from a portion of the heat being converted into power, by expansion; and lastly, superheated steam condensed more readily, from its having no water in it, requiring to be cooled down, before a good vacuum could be obtained. He had applied superheating to a pair of engines of 25 H.P., but he was unable to state the exact saving in fuel. He had no doubt, however, that in engines with bad boilers, it would be, in many cases, as much as 30 per cent.; but in engines, where the boiler was really good and generated dry steam, and the cylinder was protected by a steam jacket, so large a saving must not be expected. He hoped, that superheating would, eventually, be carried much further, and in such a manner, that the work would mainly be done by the expansion of steam, without the loss of so much latent heat as at present. The consumption of fuel had been less in Mr. Siemens's engine, in which common and superheated steam were combined, or mixed, than in any other, though not owing to such mixture; and it was satisfactory to find, that the result of the Author's experiments agreed so nearly with those made by others, as to the temperature at which ordinary cylinders might be worked, without injury. He did not believe, however, in the difference attempted to be established in the Paper, between combined steam, and superheated steam; nor did he understand the allusions made in it, to a mixture of steam and gas, as there was no gas of any kind in steam.

Mr. DORMAN had tried the Author's system of superheating steam, and a considerable gain in pressure was the result. The apparatus, which was similar to that applied by Mr. Perkins to his hot-water ovens and stoves, had, through some neglect, not worked well, the lower tubes having been nearly destroyed; but the use of it would be resumed, as soon as the necessary repairs had been effected.

The Hon. JOHN WETHERED said, in reply, that at first, he had followed in the footsteps of his predecessors, in the use of superheated steam, and like them he had found, that when it was heated sufficiently high to develop the full power, it destroyed the cylinders and slides. On considering the subject, he came to the conclusion, that the best remedy for this evil would be, to combine ordinary steam with the superheated steam, and he found, that it not only had the desired effect of regulating the heat of the latter, but that it also gave increased power. This assertion appeared to be doubted, even by some who had given him credit for re-introducing the system, and for utilising it. He would be content to dispense with half the credit, for half the emolument which others had derived from it. In his Paper, he had criticised the plan of superheating by pipes placed over the fire, but it had been said by the promoter of that system, that he had seen it in operation and had admired it; his admiration, however, had been called forth by the simplicity of the idea, that iron pipes could be made to sustain a heat of $3,000^{\circ}$. In his own experience, he had never found any iron capable of sustaining one-third of that heat, without oxidation, and ultimate destruction; the only metals that could resist it, were gold and platinum.

A superheating apparatus had been applied, about a year ago, to the 'Valeтта,' and an account of it was published, as though it had only just been discovered, that by the employment of superheated steam, considerably less feed water was required. He had understood, that the apparatus was soon destroyed, and had never been restored. It had been remarked, with reference to the indicator cards of the 'Dee,' that the superheated steam was throttled; and that if the steam had been admitted differently, the card would have been equal to the one made by the combined steam. Now the area of the opening of the valves was the same in both cases, when those indicator cards were taken. After his plan of superheating had been in operation, for some months, in H.M.S. 'Dee,' objections were made by the Admiralty, whilst admitting that the economy was entirely satisfactory, that the pipes in the chimney were liable to be struck by the shot of an enemy, and, therefore, that the pipes could not be placed there in war steamers. Experiments were then made with the apparatus of Mr. Patridge, which failed to produce superheated steam of sufficiently high temperature to develop the full economy, and the increased power due to the system; in proof of which, he would refer to the Report of the Board of Admiralty, of the average results of twenty voyages of the 'Dee.' The indicated H.P. obtained was, with combined steam, 500 H.P.; with superheated steam, 409 H.P.; and with plain steam, 404 H.P. The communication from Mr. Dinnen referred only to experiments

made with Mr. Patridge's superheating apparatus, which, as before stated, did not obtain sufficiently high temperatures by 150° , and consequently, was but a very imperfect trial of the system of combining steam.

By the use of mixed steam, it was found, that more heat was lost in the cylinder, or was transferred into mechanical force, than with ordinary, or with superheated steam; and it was a well-established fact, which went far towards proving the correctness of the Author's theory, that a cistern of water could be boiled with combined steam, in one-third less time than with superheated steam at the same temperature. Experiments were made by a committee appointed by the Maryland Institute, U.S., from which it resulted, that with ordinary steam, and with the same pressure, in each case, upon the boiler, a cistern of water could be boiled in seventy-eight minutes; with superheated steam, in eighty minutes; and with combined steam, in forty-four minutes. It was unnecessary to reply to the doubts entertained, whether sufficient heat could be obtained in the up-take to maintain the desired temperature; experience had sufficiently proved the fact, that steam absorbed heat with greater avidity than water. He thought all would agree, that the greater the quantity of heat lost in the cylinder of an engine, the greater would be the power exerted; and in all the experiments that had been made with mixed steam, that fact was distinctly evident. Allusion had been made to the saturation of steam with water. He had thoroughly examined that subject, and had measured the water with the greatest accuracy; the result was, that combined steam from six parts of water, would effect as much work as ordinary steam from ten parts of water. This entirely overthrew the theory of those who asserted, that they could measure the work done, by the quantity of water which passed through the cylinder. No doubt, a vast deal of water was mechanically mixed with steam, in its passage from the boiler to the cylinder, besides what was condensed in the cylinder itself. Most of those who doubted the superiority of combined over superheated steam, had never tried it; for experience had proved, that if the steam was sufficiently superheated, and was then mixed down to a proper temperature, a greater power was exerted by the mixed, than by the superheated steam.

He had not desired to advance any crude theory, but had simply brought forward facts. The rationale of combined steam, given in the Paper, was exactly that of Professor Regnault, President of the Institute of France, after investigating the experiments in Paris. The difference between superheated steam and combined steam consisted in this; that the former being of a gaseous nature, was a bad conductor of heat, and parted with it with difficulty; whereas combined steam, being pure vapour, and a

better conductor of heat, parted with it more readily, and left more in the cylinder of the engine, to be converted into mechanical power.

On first making this discovery, he applied to the Government of the United States. The Secretary to the Navy took so much interest in the subject, that he directed the Engineer-in-Chief of the Navy, to proceed to New York to investigate it. The result of those investigations was to prove an economy of combined steam over ordinary steam, of 52·5 per cent., and over superheated steam, of 25 per cent. He then submitted his plan to the Board of Admiralty in London, by whom every facility was given for carrying out his experiments. He availed himself of this opportunity to express his acknowledgments for the kindness and courtesy extended to him by the officers of the Admiralty. He next introduced his system in France, and a committee was appointed by the Minister of Marine to carry out experiments with a small engine. The results of those experiments showed a superiority of combined steam over ordinary steam, of 52·7 per cent., and over superheated steam, of 24 per cent. It was very remarkable, that the experiments ordered by the French and American Governments, should have given results within about 1 per cent. of each other. The British Admiralty tried the system upon a larger scale, in their own yacht, the 'Black Eagle,' and also in the 'Dee,' of 200 H.P., and the average results of twenty voyages were those already given. These were facts well authenticated by the Reports of the French Government, by the Reports of the British Admiralty, and by the Reports of the trials made by the United States Government, given in the "Franklin Journal."

He was fully aware of the difficulties which attended the introduction of any new system of this kind; he knew the strong influence of old theories gained from limited experience; he knew also, that the facts he had established by years of experience, conflicted with those theories. The results he had obtained were said to be anomalous; but in any important discovery, they were seldom, at first, capable of explanation. It seemed highly improbable, that a boiler could be made to feed itself by its own steam pressure, yet Mr. Giffard had lately given ocular demonstration of the fact. When theories and facts apparently came into conflict, theories must yield, and be made to conform with the results of experience.

Mr. BIDDER,—President,—said, the subject was one of great importance, and the discussion upon it had enabled the Meeting fairly to appreciate the rationale of the advantages which had been derived from superheating steam. It had been the unanimous opinion of those who had taken part in it, that for the practical introduction of this principle to the steam engine, Engineers were

indebted to the Author of the Paper. He was also possessed of that energy and perseverance for which his countrymen were so remarkable, and which had enabled him to make an impression, even upon the Board of Admiralty. The President was afraid, that any English Engineer could hardly have hoped to achieve the same success. There appeared to be, in the public departments of this country, a great leaning towards inventions introduced from abroad; this was advantageous in one respect, for it attracted, at all times, the best talent from other countries. He thought, however, that the Author had not clearly established his case in favour of combined steam. It rested upon the facts he had quoted, and not upon any explanation of the rationale which ought to produce such success as he claimed for it. The President was, generally, inclined to look with suspicion, upon all improvements connected with steam, or applied to other machinery in daily use; for he felt, that the fact of special attention being directed to any part of it, would induce more care, which alone would be attended with beneficial results, and they were supposed to arise from the particular modification then in course of being tested. Such was invariably the case, with regard to the permanent way of railways. Where any supposed improvement was tried upon a length of half a mile, or a mile of road, and the attention of the platelayers was directed to it, the almost certain result was, that that particular piece of road stood remarkably well. It was on that account, that he thought the case in favour of combined steam was scarcely satisfactory, unless it were accompanied by a scientific explanation of the results claimed for it. He had no doubt, however, that in the arrangement made for the passage of the steam through the pipes, some advantage was obtained. But the results of the experiments in the 'Dee' were most anomalous. It seemed to have been universally admitted, that the use of superheated steam gave an advantage of from 15 to 30 per cent. over that of ordinary steam, yet in the 'Dee,' 409 H.P. were obtained from the former, and 404 H.P. from the latter, showing a superiority of only 1 per cent. Although the Meeting had been, generally, of opinion, that the benefit to be derived from the use of combined steam was less than had been claimed, it was greatly indebted to the Author for the Paper he had communicated on the subject, and the practical discussion which it had elicited.

April 3, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

The following Candidates were balloted for, and duly elected :—
JOHN AYRIS, JAMES AUSTEN DICKINSON, JOHN DINNEN, R.N.,
DANIEL MAKINSON FOX, SILVANUS WILLIAM JENKIN, EDWARD
FRANCIS MURRAY, WALTER MONTGOMERIE NEILSON, and HENRY
JAMES ROUSE, as Members; and WILLIAM BRANCH POLLARD,
Junior, WILLIAM RIGBY, and ROBERT SHARPE, as Associates.

The discussion upon the Paper, No. 1,010, "On Combined
Steam," by the Honourable John Wethered, was continued
throughout the evening, to the exclusion of any other subject.

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

"That in order to insure a fuller attendance of Members than
could be obtained on Easter Tuesday, the Meeting be adjourned
until Tuesday evening, the 17th of April."

April 17, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

THE President announced to the Members, that the late Mr. Joseph Miller, for many years a Member of Council, had kindly bequeathed to the Institution the sum of Five Thousand Pounds, of which Three Thousand Pounds would be receivable immediately, and Two Thousand Pounds on the demise of a gentleman resident in the West Indies. The funds of the Institution would thus be materially augmented, as there would also be soon receivable, the bequest of Two Thousand Pounds from the late Mr. Robert Stephenson. To these amounts must be added the sum of nearly Five Thousand Pounds, bequeathed by the first President, Mr. Telford; of Two Hundred Pounds, presented by Mr. Charles Manby; and of One Thousand Pounds, which had recently been invested out of income. The total invested property would thus, ultimately, become :—

	£.	s.	d.
Bequest from Thomas Telford . . .	4,894	12	4
Donation from Charles Manby . . .	200	0	0
Bequest from Robert Stephenson . . .	2,000	0	0
Bequest from Joseph Miller . . .	5,000	0	0
Invested by the Institution . . .	1,000	0	0
	<hr/>		
	£13,094	12	4
	<hr/>		

No. 1,018.—“On the Efficiency of various kinds of Railway Breaks; with Experimental Researches on their Retarding Powers.” By WILLIAM FAIRBAIRN, F.R.S., M. Inst. C.E.

AMONG the various improvements which are in course of introduction into the working of railways, in order to diminish the dangers of travelling, attention is now specially directed to increasing the power of retarding trains, by means of various kinds of breaks. Few objects can be of more importance to safety in travelling, than the ability to destroy the momentum of trains with ease and rapidity, not only in ordinary cases where they have to be brought to a stand, as at stations, &c., but under special circumstances, such as obstructions perceived by the engine driver,

when the power of immediately retarding the train being within reach, might be the means of altogether preventing collision, or at least, of diminishing the violence of contact. It is already allowed by many of those connected with railways, and it has been expressly stated by the Lords Commissioners of the Railway Department of the Board of Trade, that the amount of break power supplied to trains is, in most cases, insufficient. In the Report of the Railway Department for 1857,¹ ten accidents are enumerated which might have been materially modified, if indeed not altogether prevented, by an increased retarding power under easy command.

• Upon this subject the most important communication hitherto published, is the Report prepared by Colonel Yolland, for the Railway Department of the Board of Trade. It gives the results of a large number of experiments with heavy trains, at high velocities. The breaks experimented upon were those most generally employed, and which appeared to have been most successfully used. They were the steam break of Mr. McConnell, (M. Inst. C.E.,) the continuous break of Mr. Fay, the continuous and self-acting break of Mr. Newall, and the self-acting break of M. Guérin. The special results arrived at were, the recommendation of the break of Mr. Newall, and the provisional recommendation of that of M. Guérin, for some descriptions of heavy traffic.

The present Paper is based upon an inquiry arising out of this Report. Mr. Fay felt aggrieved, that Colonel Yolland should have given the preference to the break of Mr. Newall, whilst at the same time, it was admitted that, in respect to retarding force, his own breaks manifested some superiority. This decision was, doubtless, not given without reason. Retarding force is but one of the elements of comparison; and Colonel Yolland states it as his opinion, that Mr. Newall's break "is the more comprehensive machine," and "accomplishes more objects than are aimed at by the other." Nevertheless, a question of efficiency arose, which could only be settled by additional trials. Hence, upon application being made to the Directors of the Lancashire and Yorkshire Railway, Mr. Fay obtained permission for a further investigation of the subject. The Author was invited to arrange the conditions of the trial, and to superintend the experiments, in order that they might be impartially carried out. The original instructions were conveyed in the following extract from the "Minutes of Proceedings of the Rolling-Stock Committee of the Directors, dated Manchester, September, 1858 :"—

¹ *Vide*, "Report upon the Accidents which have occurred on Railways, during the year 1857." Folio. London, 1858. Page 11.

"Letter from Mr. Fay read, applying for an investigation as to the statements in the recent Report of Colonel Yolland, of the Board of Trade, respecting his break.

"After a long conversation it was arranged, that the two breaks should be tried, side by side, on two lines of rails, under exactly similar conditions, (say on the Oldham Incline, or some other suitable line); and that a second experiment should be tried, reversing the lines of rails on which the breaks are respectively run. Also, that Mr. Fairbairn should be invited to superintend the experiments, and to fix the conditions thereof, and afterwards, to give his opinion as to the respective merits of the two breaks.

"In this arrangement, Messrs. Newall and Fay both concurred."

On the receipt of these instructions, the Author agreed to enter on the investigation. The objects he had in view were; first, to ascertain the respective retarding power of each of the competitive breaks; and, secondly, to obtain some data in regard to the rapidity with which a train, with an engine and tender attached, could be brought to a stand, when travelling at a high rate of speed, so as to determine the value of the continuous and self-acting breaks, as compared with those ordinarily in use, and with others recently introduced. The Report founded upon these experiments was sent to the Board of Directors, some time since.

In the following Paper a more comprehensive purpose is aimed at. It is wished not only to place the experiments permanently on record, for the guidance of the profession, and as an addition to existing knowledge on the subject, but also to incorporate with them the experiments previously referred to, in order that more general results, as to the comparative value and power of such breaks, may be arrived at. The Author is the more induced to attempt this, because such experiments appear, hitherto, to have remained as isolated results, without being reduced to any common standard of comparison. To effect this reduction, a few dynamical laws will suffice. The case of trains on a dead level may first be considered. Suppose a train impelled by a locomotive engine, until it attains a uniform measured velocity, v , in feet per second; that then the breaks are applied, and the train brought to a stand, after passing a distance, s , in feet; it is required to find a measure of the force, by which the momentum has been destroyed. Inasmuch as the breaks act by friction, which may be considered, with sufficient accuracy, to be uniform throughout the operation of breaking, the train may be assumed to be stopped by a uniformly retarding force, acting through the space s , and subject to the dynamical laws for such forces. If the retarding force in this case is called f , consisting, mainly, of the friction of the broken wheels, and, for simplicity,

including also the friction of the axles, resistance of the air, &c. :—

$$\text{then,} \quad f = \frac{v^2}{2s} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

But supposing, as is generally the case, that the retarding force of the breaks is proportional to a part of the weight of the train only, that is, that the retarding force generated varies as the weight on the rubbing surfaces, and supposing the breaks to be applied to a few carriages only; putting w for the weight of the break carriages in tons, and W for the weight of the train :—

$$\text{then,} \quad f_1 = \frac{W v^2}{2 w s} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which gives the retarding force per unit of mass of the break carriages; or, in other words, f_1 is the diminution of the velocity of the train, in feet, in one second of time.

It will be convenient to reduce this force to terms of weight, instead of mass. Call f_2 the retarding force in lbs. per ton weight of the break carriages :—

$$\text{then,} \quad f_2 = f_1 \times \frac{2,240}{32.19} = 69.587 f_1 \quad . \quad (3)$$

Again, supposing that, instead of being on a level line, the breaks are applied on an incline. Then the action of gravity will cause the train to go further, if it is descending the incline, or to stop sooner, if ascending, than if the line was level; and gravity is a uniformly accelerating, or retarding force, as the friction of the carriages. Hence, the net result, in distance and velocity, of a train stopped on an incline, may be supposed to arise from two forces: f , a retarding force dependent on the friction of the broken wheels; and ϕ , a retarding, or accelerating force dependent on gravity, and assisting, or opposing the action of f , according as the incline rises, or falls :—

$$\text{thence,} \quad f \pm \phi = \frac{v^2}{2s} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Now, the value of ϕ in terms of the inclination θ of the plane to the horizon is known, for if g be the velocity generated by gravity in one second :—

$$\phi = g \sin \theta,$$

or, putting z for the vertical height, fallen through by the train, between the time of applying the breaks and stopping the train :—

$$\phi = g z$$

$$\therefore f = \frac{v^2}{2s} \pm g \sin \theta;$$

$$\text{or, } f = \frac{\frac{1}{2} v^2 \pm 9 z}{s} \quad (5)$$

$$f_1 = \frac{W}{w} \times \frac{\frac{1}{2} v^2 \pm 9 z}{s} \quad (6)$$

$$f_2 = \frac{2,240}{g} \times \frac{\frac{1}{2} v^2 \pm 9 z}{s} = 69.587 f_1 \quad . . . (7)$$

where the + or - sign is to be adopted, according as the gradient falls, or rises.

In the increase of the break power of trains, the principles hitherto most successfully employed have been; first, the use of steam acting direct on the breaks; secondly, the connection of several of the ordinary form of breaks, so as to unite them under the control of a single breaksmen; and thirdly, the introduction of break apparatus connected with the buffers, so as to make the momentum of the train itself, available in generating a retarding force.

Mr. Mc'Connell's break, which is applicable only to the engine, consists of two wrought-iron sledges, each 48 inches in length and 4 inches in breadth, and turned up at the ends. These sledges are suspended from the lower side of the fire box, between the driving and the trailing wheels of the engine. The pressure is placed on them, by admitting steam from the boiler into two cylinders, each 9 inches in diameter, placed horizontally, one on each side of the fire box, above the sledges, and forcing these latter down upon the rails by means of an elbow joint. The pressure can be applied to either side of the pistons in the cylinders, according as the breaks have to be raised, or depressed. The pressure of the sledges upon the rails, calculated from the pressure of the steam in Colonel Yolland's trials, would amount to about 6 tons. On the weighing machine, however, the actual pressure was found to vary from 4 tons to 9 tons, or a mean of about 7 tons; and these anomalies he confesses himself unable to solve. The principal advantage of this break appears to be, that it is immediately applied, without exertion, and is under the control of the engine driver, who, in most cases, is the first to perceive any obstruction on the line. It will be seen, however, that although efficiently generated, the amount of retardation caused by this break is comparatively small, and, as Colonel Yolland concludes, insufficient to prevent collision in those cases in which its use would be specially desirable.

Mr. Newall's and Mr. Fay's breaks, which, in their present condition, are identical in principle, are distinguished from other breaks by this; that two, or more carriages, or, if necessary, the whole train, are fitted with break blocks, all of which are brought

under the direction of one guard, by means of a longitudinal shaft, which transfers the motion of the guard's wheel to the breaks, throughout the whole length of the train. In this way, an enormous increase of retarding power may be obtained, proportional to the weight of the carriages to which breaks are applied, and with this further and important advantage, that the retarding force is distributed equally throughout the train, instead of being accumulated at either end, and thus the shock upon the wheels and axles is much diminished. Mr. Newall and Mr. Fay have also adopted a partially self-acting apparatus of springs, by means of which the breaks are applied throughout the length of the train, on the simple release of a catch, by the guard, or the engine driver, as the case may be.

In Mr. Newall's break, the motion of the guard's, or the engine-driver's wheel, since either, or both of these may have the control of the breaks, is transferred through the medium of a short vertical shaft in the van, or tender, to the longitudinal shaft placed beneath the carriages of the train, by a pair of bevil wheels, or by a spur wheel and pinion. The longitudinal shaft passes either beneath the centre, or at one side of the carriage, under the framework; and it is connected by simple, but very effective, jointed couplings between each pair of carriages, so as to permit the free action of the buffers, and the rise and fall of the carriages with the inequalities of the line. Near to the middle of the carriage, a bevil wheel is fixed on the longitudinal shaft, which is toothed into a similar one on a short cross shaft, carrying also a spur pinion geared into a horizontal rack. Thus, on revolving the guard's wheel, this rack is drawn back, withdrawing, at the same time, the principal arm of the rocking shaft, at the centre of the carriage, and compressing a spring placed on the other side, to both of which the rack is attached by a simple connecting rod. In this position, the break blocks are off the wheels, and the break is ready for use. If the bevil wheel, or pinion in the guard's van, or tender, is now released, or lifted out of gear by a lever, or treadle, the springs throughout the train will force back the arm of the rocking shaft, which carries the levers that press the break blocks on the wheels. If it is required to put on the breaks harder, and to skid the wheels, the treadle is again released by the guard, and the pressure increased by revolving the wheel in the ordinary way. In other words, Mr. Newall and Mr. Fay provide a number of springs, or, in some cases, weights, under each carriage, in which is stored up, ready for instantaneous use, a stock of break power, derived from the one guard acting through a longitudinal shaft, communicating with every break by means of an arrangement of spur wheels and pinions. From this it will be seen, that on any emergency, the retarding force may be in-

stantly employed, by simply releasing a catch, which permits the break blocks to be forced upon the wheels, by the springs, throughout the train. In justice to Mr. Newall, who claims the earlier application of this combination of breaks, it must be admitted, that Mr. Fay has adopted identically the same principle, with very slight modification in form, or in construction.

In the class of breaks in which greater retarding power is obtained, by increasing the number of breaked carriages and combining their action, the systems just described appear the best and most comprehensive, hitherto adopted. But the Author has received from Mr. E. W. Watkin, the details of some experiments on an auxiliary break carriage designed upon a different plan. In this case, an ordinary break arrangement is employed, with a double elbow joint, to which a long vertical lever is attached, moving in an arc against the side of the van. A rope from the end of this lever is conveyed to the tender, or the guard's van, and is attached to a drum, on the axis of the ordinary guard's wheel. Hence, when the guard, or the fireman revolves his wheel to put on his own break, the rope coils upon the drum, drawing back the lever, and thus putting on the auxiliary breaks at the same time.

Belonging to the third class of breaks before enumerated, in which the momentum of the train itself is employed in generating the retarding force, there is only the break of M. Guérin, which is entirely self-acting, and is brought into use by the recoil of the buffer rods, when, by the application of the tender break, a retardation has been caused in front.¹ Colonel Yolland thus describes this break :—The buffer rods at the after end of the carriage, abut against a spring that extends across its width ; one buffer rod acting against each end of the spring. This spring, instead of being fixed against the under-framing of the carriage, or the break van, in the usual manner, is moveable in a groove. On one side, the centre of the spring is secured to the draw bar, and on the other, it is attached to the arm of a short lever, fixed almost vertically over the rocking shaft, so that when the buffers are pressed in by the sudden check to the velocity, caused by shutting off the steam, by the application of the tender break in front, and by the momentum of the train in the rear, the buffer spring is carried forcibly against the lever, the rocking shaft is turned, and the break blocks are forced against the peripheries of the wheels. The breaks are prevented from being put on, when a train is required to be shunted, by a crosshead, or stop. But this provision interferes with the application of the breaks, when the train is in motion at a low velocity. For such a speed, it is assumed the tender break will be sufficient.

¹ *Vide* Minutes of Proceedings Inst. C.E., vol. xvii., p. 153.

These, therefore, are the breaks upon which the experiments have been made, and the results of which have been tabulated and classified.

EXPERIMENTS WITH MR. NEWALL'S AND MR. FAY'S CONTINUOUS BREAKS.—In carrying out the views of the Directors of the Lancashire and Yorkshire Railway, the Author arranged, in the first place, for a series of experiments on the Oldham Incline, where two similar trains of carriages, one fitted throughout with Mr. Newall's, and the other with Mr. Fay's breaks, were started alternately. After passing over a measured distance, by the action of gravity, the breaks were applied, and the distance within which the trains were respectively brought up, was carefully ascertained, as giving the measure of the break power of the trains. Each train consisted of three weighted carriages, and they were started by simply releasing a stop. Having descended, by gravity, a previously measured distance, with a uniformly accelerated velocity, they passed over a fog signal, which gave notice to the guard to put on the breaks. Then the train having been brought to a stand, the distance from the point at which the train stopped, to the fog signal, was measured back, and the train was dragged up the incline for another trial. Unfortunately, the day on which these experiments were made, proved misty and foggy, with rain at intervals, so that the rails were in the worst condition for facilitating the stopping of the trains. The significance of this fact will be seen, on comparing these results with later ones obtained in dry weather.

TABLE I.—EXPERIMENTS with Mr. NEWALL'S and Mr. FAY'S RAILWAY BREAKS, on the Oldham Incline, February 6, 1859.

Weather foggy and wet; gradient falling 1 in 27; weight of trains, 26 tons 10 cwt. each; no engine attached.

Mr. Fay's Flap Breaks.					Mr. Newall's Slide Breaks.				
No.	Time of Running.		Distance Run.		No.	Time of Running.		Distance Run.	
	Before Breaking.	After Breaking.	Before Breaking.	After Breaking.		Before Breaking.	After Breaking.	Before Breaking.	After Breaking.
	Seconds.	Seconds.	Yards.	Yards.		Seconds.	Seconds.	Yards.	Yards.
1	35	..	150	153	1	35	14	150	281
2	40	13	200	250	2	40	16	200	336
3	48	14	300	360	3	48	17	300	459
4	58	15	400	499	4	56	25	400	608
5	59	12	400	326	5	56	14	400	371
6	62	25	500	739	6	62	19	500	663
7	72	17	600	575	7	68	17	600	545
..	..	..	..	..	8*	63	32	500	798

* In this experiment, the self-acting part of the break only was employed.

In these experiments, the whole of the wheels were sledged, or skidded, before the train was stopped. The self-acting arrangement of springs was fitted to Mr. Newall's carriage alone. In the later experiments, it was adopted also by Mr. Fay.

Taking the mean of the number of seconds required in breaking each train, in experiments Nos. 2, 3, 4, 5, 6, and 7, which were made under precisely corresponding circumstances in the case of each break, and at similar velocities, it is found, that the train was brought to a stand :—

By Mr. Newall's break . . . in 21·6 seconds,
 „ Mr. Fay's „ . . . „ 19·2 seconds;

or about two and a half seconds of time in favour of Mr. Fay's.

It will, however, be advisable to ascertain the precise value of the retarding force in each case, by the formulæ already given. To effect this, the initial velocity of the train at the instant of applying the breaks, must first be ascertained. For this purpose the least objectionable formula, and at the same time the most simple, is :—

$$v = \frac{2s}{t} \dots \dots \dots (8)$$

where v is the velocity in feet per second ; s is the distance run, in feet ; and t is the time of running, in seconds. From this formula the following initial velocities of the train, in feet per second, in the preceding experiments, are obtained :—

No.	Mr. Fay's.	Mr. Newall's.
1.	25·71	25·71
2.	30·00	30·00
3.	37·50	37·50
4.	41·37	42·85
5.	40·66	42·85
6.	48·38	48·38
7.	50·00	52·94
8.	—	47·61

Hence, by the equation (6), since, in this case :—

$$\frac{W}{w} = \frac{26\cdot5}{26\cdot5} = 1,$$

and therefore,

$$f = f_1,$$

and

$$f_1 = \frac{\frac{1}{2} v^2 + g z}{s};$$

the relative values of each description of break, and their comparative efficiency in each trial, may be derived. The retarding force of each break is found to be as follows :—

No.	Mr. Fay's.	Mr. Newall's.
1.	1·9115	1·3246
2.	1·7922	1·6388
3.	1·8432	1·7030
4.	1·7645	1·6946
5.	2·0280	2·0152
6.	1·7205	1·7811
7.	1·9167	2·0480
	Mean 1·8538	Mean 1·7436

Giving, as in the previous comparison, the advantage to Mr. Fay, in the proportion of 1·8538 to 1·7436, or as 1,051 to 1,000. The entire agreement of the results among themselves, and with the preceding comparison derived from the time, is sufficient evidence of the practical accuracy of the formula of reduction employed. The breaks stand, in these experiments, very nearly on an equality of merit.

The experiments in the next Table were made at Southport, upon a line more nearly level. The speed was obtained by means of a locomotive engine, which was detached, at the instant of applying the breaks, by a slip coupling. The velocity, which was maintained uniform over the last half-mile, was measured by noting the time required to pass two fog signals, placed half a mile apart. The breaks, in every trial, were applied almost instantaneously, after the report of the second fog signal, and the distance was measured after the train had come to a state of rest.

TABLE II.—EXPERIMENTS with MR. NEWALL'S and MR. FAY'S RAILWAY BREAKS, on the line between Liverpool and Southport; January 7, 1859.

Weather fine and frosty; gradient rising 1 in 485; weight of trains, 26 tons 10 cwt. each.

Mr. Fay's.					Mr. Newall's.				
No.	Time of Running ½ Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.	No.	Time of Running ½ Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.
1	42	42·85	62·86	184	1	31	58·06	85·16	240
2	40	45·	66·	206	2	30	60·	88·	Lost,
3	34	52·94	77·65	272	as also another experiment.				
4	31½	37·14	83·81	313	Engine alone }	30	60·	88·	960
5	30	60·	88·	329					

The second experiment with Mr. Newall's breaks failed, in consequence of the guard applying the breaks too soon, and a third was lost from the fracture of the slip coupling.

When these results are reduced by the same formula, the following values of f_1 , representing the efficiency, or retarding force of each break, are obtained :—

No.	Mr. Fay's.	Mr. Newall's.
1.	3·5125	4·97
2.	3·4579	
3.	3·6274	Mean 3·6256
4.	3·6738	
5.	3·8566	

These experiments give a superiority in favour of Mr. Newall's breaks, in the proportion of 4·97 to 3·6256, or as 1,378 to 1,000.

The value of the retarding force, f , for the tender break, derived from the last experiment, is 1·2781, and reducing this, in proportion of the weight of the tender to the weight of the engine and tender, it becomes :—

$$f_1 = 4·3455.$$

At this period, Mr. Fay requested permission to attach a self-acting apparatus to his breaks, as he considered they were not fairly matched against those of Mr. Newall, when applied by hand. The experiments were, therefore, postponed for two months, to enable Mr. Fay to effect this alteration. They were again resumed on the 14th of April, 1859.

TABLE III.—EXPERIMENTS with Mr. NEWALL'S AND Mr. FAY'S RAILWAY BREAKS, on the line between Liverpool and Southport; April 14, 1859.

Weather, dry during the first, with a slight shower during the remaining experiments; gradient falling 1 in 3,453; weight of trains, 27 tons 6 cwt.

Mr. Fay's.					Mr. Newall's.				
No.	Time of Running + Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.	No.	Time of Running + Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.
1	56½*	31·8	46·7	121½	1	58	31·	45·5	101
2	53½	33·4	49·1	124	..	..	..	..	..

* Engine attached, and tender break applied.

Reducing the results, as before :—

$$\begin{aligned} f_1 &= 3·2329 \text{ with Mr. Fay's break;} \\ &= 3·4161 \text{ with Mr. Newall's break;} \\ &= 2·9956 \text{ with Mr. Fay's break and tender break.} \end{aligned}$$

Here the superiority lies with Mr. Newall, in the ratio of 1,056 to 1,000. The experiment with the engine attached, when reduced in the ratio of the weight of the train to the weight on the wheels broken, gives :—

$$f_1 = 4·84.$$

It will be observed, that as the value of the retarding force of the breaks is here obtained in terms of the coefficient of friction,

for the rubbing surfaces, the efficiency of the break varies with the condition of the weather. Thus the mean of the Oldham experiments gave a retarding force of 1·7987 feet per second; the mean of the first experiments at Southport gave 4·2978, and the mean of the second, 3·3245. On each day the experiments were consistent with one another, but they differed widely, on different days, from the change in the condition of the rubbing surfaces. At Oldham, the experiments were made with the rails in a greasy condition, from fog; at Southport, in the later of the two trials, with the rails slightly wet, and in the earlier, with the rails dry, and in the best condition for breaking. This is in accordance with the experiments of M. Morin on the friction of iron on iron; in which it was found, that the coefficient of friction varied from 0·05 to 0·3, according as the surfaces were greasy, wet, or dry. This consideration must be borne in mind in estimating the results, and together with some improvements in the adjustment of the breaks, and the introduction of increased power, from time to time, will explain the discrepancies which may be found, on comparing the results obtained at different periods of the trials.

The remaining experiments upon the self-acting breaks were all made under uniform and favourable conditions; the weather was fine, and the wind blew each day from the west, or the north-west. The results, also, are uniform for these days, and there can, therefore, be no error in placing these experiments in the same Tables, and averaging them together. This classification will, therefore, be adopted as more convenient, and they will be arranged under the following heads:—

- 1°.—Experiments on the friction of the carriages.
- 2°.—Experiments with slide breaks, with the engine detached.
- 3°.—Experiments with flap breaks, with the engine detached.
- 4°.—Experiments with the engine attached to the train.

EXPERIMENTS ON THE FRICTION OF THE CARRIAGES.—These experiments were made by running the train, as before, past two fog signals, half a mile apart, to obtain the velocity, detaching the engine at the second fog signal, and allowing the train gradually to come to rest.

TABLE IV.—EXPERIMENTS with Mr. NEWALL'S and Mr. FAY'S RAILWAY BREAKS, on the line between Liverpool and Southport; June 2, 1859.

	Time of Running ½ Mile, in Seconds.	Speed, in Miles per Hour.	Distance run, after applying Breaks, in Yards.	Time of Running, after apply- ing Breaks, in Seconds.	Height fallen through by Train from inclination of Line, in Feet.
Mr. Fay's . .	40·	45·	4,840	430	7·06
Mr. Newall's .	44·5	40·45	6,380	780	13·91

Reducing, as before, the normal friction of the carriages, $f = f_1$, is found to be :—

Mr. Fay's break	0.16565
Mr. Newall's break	0.10961

and therefore, f_2 , or the friction per ton weight of the carriages, is :—

Mr. Fay's break	11.527 lbs. }	Mean
Mr. Newall's break	7.627 „ }	9.577 lbs.

This shows that there is a considerable difference between the friction of the two sets of carriages ; and a small correction should, therefore, be made, in the reductions of the experiments on breaks, in favour of Mr. Newall, if perfect accuracy were required. The correction, however, does not exceed one-sixtieth of the retarding force of the breaks, and may be neglected without appreciable error. These experiments were made with carriages fitted with slide breaks, and the friction of those with flap breaks was not determined.

In an experiment recorded in Colonel Yolland's Report, the friction, derived in the same way, for a train of carriages fitted with Mr. Newall's breaks, and attached to an engine and tender, amounted to 11.4 lbs. per ton. This, when allowance is made for the greater friction of the engine, nearly agrees with the Author's results.

EXPERIMENTS WITH SLIDE BREAKS, WITH THE ENGINE DETACHED FROM THE TRAIN.—The following experiments were made between the Birkdale and Amsdale Stations, on the line between Liverpool and Southport. As before, the engine was attached by a slip coupling to the train. At the quarter and the three-quarter mile posts from Amsdale, fog signals were placed, and the time of passing between these, being accurately observed by stop watches, gave the average speed of the train. At the second fog signal, the slip coupling was unfastened, and the breaks applied, instantly on hearing the report. The Author's assistant, Mr. Unwin, and other disinterested persons, were placed in the guard's van, to prevent the premature application of the breaks ; and others on the engine with the driver, to see that there was no change of velocity, in passing over the half-mile in which the speed was observed. The line where these experiments took place rose, for 500 feet from the first fog signal, with a gradient of 1 in 1,087, and then fell for upwards of a mile, with a gradient of 1 in 3,543, a fall so slight, in the short space in which the trains were brought to rest, that it cannot appreciably affect the results ; and in the reductions, the line has been considered as level.

TABLE V.—EXPERIMENTS with Mr. NEWALL'S and Mr. FAY'S SLIDE BREAKS, at Southport; May, 1859.

Mr. Newall's.					Mr. Fay's.				
No.	Time of Running ‡ Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.	No.	Time of Running ‡ Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.
2	55	32·72	48·	56‡	2	51	35·29	51·76	56
4	49	36·73	53·87	77	4	41	43·9	64·39	98
6	41‡	Failed	..	..	5	36	50·	73·33	129
7	41	43·9	64·39	136	6	33	54·54	80·	144
8	39	46·15	67·69	140‡	7†	33	54·54	80·	161‡
9	34	52·94	77·64	205‡	9‡	47‡	37·89	55·58	97
10*	33	54·54	80·	192	10‡	30	60·	88·	204‡
11†	38	47·37	69·47	260‡	11‡	30	60·	88·	214
12‡	33‡	53·73	78·8	222	..	..	..	..	..
13‡	28‡	63·16	92·63	273	..	..	..	..	..

* Self-action only.

† Breaks not applied at the proper time.

‡ Train consisting of two carriages, and weighing 18 tons 4 cwt. In the other experiments, there were three carriages weighing 27 tons 6 cwt.

The trains with which these experiments were made, consisted of three heavily-weighted carriages, each with breaks to every carriage, except in the two last experiments, when, in consequence of an accident to one of Mr. Newall's carriages, the trains were reduced to two. The carriages were loaded with iron rail-chairs, so as to weigh 9 tons 2 cwt., each.

The power of these continuous breaks was well exemplified upon the 18th of May, when Mr. Fay's guard inadvertently applied the breaks, whilst the train was running at a comparatively slow velocity; the strong coupling hook which united the tender to the guard's van, was instantly snapped, and the train brought to a stand.

Making a reduction of the preceding results, the following values of $f = f_1$, representing the comparative retarding powers of the breaks in each case, are arrived at:—

Mr. Fay's.		Mr. Newall's.	
7·9749	Mean 6·7030	6·7765	Mean 5·4984
7·0512		6·2813	
6·9480		5·0810	
7·4074		5·4292	
6·5979		4·8929	
5·3076		4·6625	
6·3062		5·2385	
6·0311		5·5555	

In this case, the breaks of Mr. Fay exhibit a superiority in the ratio of 6·7030 to 5·4984, or as 1,215 to 1,000; or making a correction, as above stated, for the friction of the carriages, the

relative efficiency of Mr. Fay's and Mr. Newall's breaks, would stand in the ratio of 6.553 to 5.4084, or as 1,210 to 1,000.

EXPERIMENTS WITH FLAP BREAKS, WITH THE ENGINE DETACHED.—These experiments were made in precisely the same manner as the last, the trains consisting of three carriages, with breaks to each, loaded to 9 tons 2 cwt.

Mr. Fay's.					Mr. Newall's.				
No.	Time of Running ½ Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.	No.	Time of Running ½ Mile, in Seconds.	Speed, in Miles per Hour.	Velocity in Feet per Second.	Distance of Pulling up, in Yards.
1	35	51.43	75.43	158½	1	36	50.	73.33	132½
2	35	51.43	75.43	162½	2	36	50.	73.33	123
3*	33	54.54	80.	184	3*	35	51.43	75.43	192

* Self-action only.

Reducing the results, the comparative efficiency is:—

Mr. Fay's.		Mr. Newall's.	
5.9889	} Mean 5.8718	6.7560	} Mean 6.3272
5.8294		7.2870	
5.7971		4.9387	

In this case, the superiority lies with Mr. Newall, in the ratio of 6.3272 to 5.8718, or as 1,000 to 928.

EXPERIMENTS WITH THE ENGINE ATTACHED TO THE TRAIN.—These experiments were made with slide breaks, upon the same ground, and in the same manner as the last experiments. The rails also were in the same dry condition. The only difference was, that the engine and tender remained attached to the train, instead of being uncoupled, and the tender break was applied, as rapidly as possible, along with the other breaks.

TABLE VII.—EXPERIMENTS with Mr. NEWALL'S and Mr. FAY'S RAILWAY BREAKS, with engine not disconnected from train.

Weight of engine, 24 tons; weight of tender, average 10 tons; weight of train, 27½ tons; weight of tank engine, 30 tons.

Mr. Fay's.					Mr. Newall's.				
No.	Time of Running ½ Mile, in seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.	No.	Time of Running ½ Mile, in Seconds.	Speed, in Miles per Hour.	Velocity, in Feet per Second.	Distance of Pulling up, in Yards.
1	56½	31.8	46.7	121½	1	53	33.96	49.81	124½
2	53	33.96	49.81	137	2	48½	37.11	54.43	169½
3	43	41.86	61.39	192½	3	43	41.86	61.39	221
4*	35	51.43	75.43	274	..	..	..	..	..

* Tank engine.

Reducing these results, the comparative retarding force is found to be :—

Mr. Fay's,		Mr. Newall's.	
2·9956	} Mean 3·0934	3·3169	} Mean 3·0250
3·0184		2·9160	
3·2663		2·8422	

where the efficiency of the breaks is almost identical, Mr. Fay having an advantage in the ratio of 1,022 to 1,000.

From the above extended and somewhat laborious experiments, the following summary of results is derived :—

TABLE VIII.—GENERAL SUMMARY OF RESULTS OF EXPERIMENTS WITH MR. NEWALL'S AND MR. FAY'S BREAKS.

	Average Number of Experiments.		Average efficiency of Breaks.	
	Mr. Fay's.	Mr. Newall's.	Mr. Fay's.	Mr. Newall's.
Oldham Incline, Table 1 .	7	7	1·8538	1·7436
Southport, Table 2 . .	5	1	3·6256	4·9700
Southport, Table 3 . .	1	1	3·2329	3·4161
Southport, Table 5 . .	8	8	6·7030	5·4984
Southport, Table 6 . .	3	3	5·8718	6·3272
Southport, Table 7 . .	3	3	3·0934	3·0250

The general average from this Table gives, for the efficiency of Mr. Fay's breaks, 4·0634, and for that of Mr. Newall's 4·1650, showing a slight superiority in favour of the latter.

The following conclusions seem borne out by these experiments :—

1st. That with slide breaks, the greater number of experiments gave a manifest superiority to Mr. Fay's.

2nd. That with flap breaks, there was a decided advantage on the side of Mr. Newall.

3rd. That when the train was breaked, with the engine attached, the results were uniform ; neither Mr. Fay's breaks, nor Mr. Newall's, gaining any decided superiority.

During the whole of these trials, there was a strong feeling of rivalry, which rendered necessary the greatest caution, in order to prevent any interference, which might modify and vitiate the results. To reconcile these differences, and to obtain correct returns, the Author's assistant, Mr. Unwin, was employed to take charge of the train, and to see that the breaks were applied at the right time ; also to register the velocity of the train, and the distance of pulling up, during each experiment. There is, therefore, every reason to believe, that the results recorded are a strict expression of the efficiency of the breaks, at their respective times of trial.

COLONEL YOLLAND'S EXPERIMENTS ON MESSRS. NEWALL AND FAY'S RAILWAY BREAKS.—It may be interesting to compare with these results, the earlier experiments obtained by Colonel Yolland, on the same class of breaks, and under somewhat similar conditions of trial, as detailed in his Report to the Board of Trade, dated the 12th of June, 1858. These results do not appear to have been reduced, hitherto, to any common standard of comparison. But as they embrace a wider range of circumstances of gradient, weather, weight, &c., than in the Author's experiments, they will instructively test the method of reduction employed.

In the experiments on the Accrington Incline, the trains weighed 72 tons each, and consisted of six weighted carriages, used in all the experiments, and of three carriages fitted with Mr. Newall's, and three with Mr. Fay's breaks, respectively, and employed alternately. The required velocity was obtained, by permitting the carriages to descend a distance of from three-quarters of a mile to a mile, along the incline, which falls at the rate of 1 in 38 to 1 in 40. The initial velocity at the instant of applying the breaks was ascertained, by observing the time required to traverse the quarter of a mile immediately preceding; and the mean velocity over this distance is used in the reductions, as the initial velocity of the train, although it is slightly below the real speed, at the instant of breaking.

TABLE IX.—EXPERIMENTS ON THE ACCRINGTON INCLINE, with Mr. NEWALL'S and Mr. FAY'S BREAKS; February 27, 1858.
Weather fine.

No.		Velocity, in Feet per Second.	Gradient, falling —.	Distance of Break- ing, in Yards.	Remarks.
2	Mr. Newall's	69·47	— 1 in 39	1,587	Heavier Train. Three continuous Breaks.
3		52·8	— 1 in 39	777	
4		48·9	— 1 in 39	822	
5		28·7	— 1 in 40	414	
6		69·47	— 1 in 39	1,114	
7		62·86	— 1 in 40	1,208	
9		30·7	— 1 in 39	430	
10	Mr. Fay's . .	60·	— 1 in 39	923	

Reducing the results in Table IX., the retarding force is:—

Mr. Newall's . . .	1·0275	}	Mean 1·3231
	1·4233		
	1·3102		
	1·1363		
	1·5285		
	1·3705		
	1·1700		
Mr. Fay's	1·4754		

It will be remarked, that in these experiments, the breaks were applied to a part of the train only. Hence, for comparison with the Southport experiments, a reduction must be made, to the condition of trains with breaks throughout, by the formula (6) already explained in that way:—

$$f_1 = \frac{W}{w} f.$$

The retarding force in terms of the mass of the carriages actually broken is:—

Mr. Newall's	3·7955	Mean 3·5516
	3·4940	
	3·0301	
	4·0759	
	3·6547	
	3·1200	
Mr. Fay's	3·9345	

showing an advantage to Mr. Fay in the ratio of 1,107 to 1,000. But a comparison is here scarcely fair, seeing the disproportion in the number of experiments with each break.

TABLE X.—EXPERIMENTS ON the ACCRINGTON INCLINE, with MR. NEWALL'S and MR. FAY'S RAILWAY BREAKS; February 28, 1858.

Weather Misty.

No.		Velocity, in Feet per Second.	Gradient, falling —.	Distance of Break- ing, in Yards.	Remarks.
11	Mr. Fay's	66·	— 1 in 39	2,060	Three continuous Breaks.
13		62·86	"	1,660	
16		50·77	"	1,070	
17		35·67	— 1 in 40	492	
12	Mr. Newall's	60·	— 1 in 39	2,142	
14		60·	"	1,754	
15		50·77	"	1,450	
18		69·47	— 1 in 132	880	Tender & ditto.

The results in Table X. show, that the mean of the retarding force is:—

Mr. Fay's.		Mr. Newall's.	
1·178	Mean	1·105	Mean
1·221		1·167	
1·227		1·122	
1·257			
	1·220		1·131

or with breaks throughout:—

Mr. Fay's.		Mr. Newall's.	
3·141	} Mean	2·946	} Mean
3·256		3·112	
3·272		2·992	
3·352			
	3·255		3·017

Giving a slight advantage to Mr. Fay, in the ratio of 1,070 to 1,000.

TABLE XI.—EXPERIMENTS ON MR. NEWALL'S BREAKS, between Liverpool and Preston; February 22, 1858.

Train of carriages weighing 83 tons 18 cwt.; engine, 29 tons 2 cwt.; tender, 13 tons 4 cwt.

No.		Velocity, in Feet per Second.	Gradient, rising +, falling —.	Distance of Break- ing, in Yards.	Remarks.
1	} Mr. Newall's	57·39	— 1 in 135	285	} Tender and six Breaks.
2		48·88	— 1 in 180	208	
3		66·00	+ 1 in 135	206	
4		45·52	+ 1 in 168	246	} Tender and self-action of six Breaks.
6		57·39	— 1 in 150	276	
7		48·88	— 1 in 130	204	} Tender and six Breaks.

Reducing as before, these data give:—

$$f = \left. \begin{array}{l} 2 \cdot 165 \\ 2 \cdot 095 \\ 3 \cdot 285 \\ 2 \cdot 204 \\ 2 \cdot 201 \end{array} \right\} \text{Mean } 2 \cdot 390$$

or, reduced in the ratio of the weight on the wheels broken to the weight of the train:—

$$f_1 = \left. \begin{array}{l} 4 \cdot 304 \\ 4 \cdot 164 \\ 6 \cdot 530 \\ 4 \cdot 381 \\ 4 \cdot 375 \end{array} \right\} \text{Mean } 4 \cdot 671$$

This agrees with the value $f_1 = 3 \cdot 5516$, obtained in the first experiments at Accrington, with the rails dry, and is in excess of the value $f_1 = 3 \cdot 017$, obtained with the rails wetted by the mist.

For experiment No. 4, with the self-action of the breaks alone, $f = 1 \cdot 211$, or about one-half the full break power.

TABLE XII.—EXPERIMENTS between Preston and Liverpool, with Mr. NEWALL'S RAILWAY BREAKS; February 22, 1858.

Weight of train, 101 tons, $1\frac{1}{2}$ cwt.; weight on breaks, 71 tons, $19\frac{1}{2}$ cwt.

No.		Velocity, in Feet per Second.	Gradient, rising +, falling -.	Distance of Pulling up, in Yards.	Remarks.
8	} Mr. Newall's	73·33	0	196	} Tender and seven con- tinuous Breaks.
9		57·39	+ 1 in 402	130	
10		44·	0	107	
11		60·	- 1 in 120	167	

Reducing these results:—

$$\left. \begin{array}{l} f = 4 \cdot 567 \\ 2 \cdot 975 \\ 3 \cdot 015 \\ 4 \cdot 118 \end{array} \right\} \text{Mean } 3 \cdot 668$$

or, when the train is broken throughout:—

$$\left. \begin{array}{l} f_1 = 6 \cdot 406 \\ 4 \cdot 173 \\ 4 \cdot 229 \\ 5 \cdot 776 \end{array} \right\} \text{Mean } 5 \cdot 146$$

This would seem to indicate, that the breaks act more efficiently, the more nearly they are applied throughout the whole train.

TABLE XIII.—EXPERIMENTS between Liverpool and Preston, with Mr. NEWALL'S BREAKS; February 23, 1858.

No.		Velocity, in Feet per Second.	Gradient, rising +, falling -.	Distance of Pulling up, in Yards.	Remarks
1	} Mr. Newall's	77·65	- 1 in 204	314	} All the Breaks.
2		73·33	0	179	
3		52·8	0	183	
4		78·33	- 1 in 132	227	} All but the Tender.
5		66·	+ 1 in 135	189	
6		73·33	0	249	} All the Breaks.
7		62·86	- 1 in 150	208	
8		44·	- 1 in 700	138	
9		77·65	- 1 in 120	235	

Reducing:—

$$\left. \begin{array}{l} f = 3 \cdot 3582 \\ 5 \cdot 0068 \\ 3 \cdot 7041 \\ 4 \cdot 0797 \\ 3 \cdot 5992 \\ 2 \cdot 9516 \\ 2 \cdot 2922 \\ 4 \cdot 0081 \end{array} \right\} \text{Mean } 3 \cdot 6249$$

Or, when further reduced in the ratio of the weight of the whole train to the weight on the wheels broken :—

$$\begin{array}{r}
 f_1 = 3 \cdot 9660 \\
 5 \cdot 9130 \\
 4 \cdot 3745 \\
 4 \cdot 8181 \\
 4 \cdot 2506 \\
 3 \cdot 4858 \\
 4 \cdot 7335
 \end{array}
 \left. \vphantom{\begin{array}{r} f_1 = 3 \cdot 9660 \\ 5 \cdot 9130 \\ 4 \cdot 3745 \\ 4 \cdot 8181 \\ 4 \cdot 2506 \\ 3 \cdot 4858 \\ 4 \cdot 7335 \end{array}} \right\} \text{Mean } 4 \cdot 5057$$

The preceding results obtained by Colonel Yolland are as follows :—

	Mr. Fay's.	Mr. Newall's.	
Engine detached	$\left\{ \begin{array}{l} 3 \cdot 9345 \\ 3 \cdot 255 \end{array} \right.$	$\left\{ \begin{array}{l} 3 \cdot 5516 \\ 3 \cdot 017 \end{array} \right.$	$\left\{ \begin{array}{l} \text{Dry.} \\ \text{Wet.} \end{array} \right.$
Engine attached		$\left\{ \begin{array}{l} 4 \cdot 671 \\ 5 \cdot 146 \\ 4 \cdot 505 \end{array} \right.$	Mean 4 \cdot 774

TABLE XIV.—EXPERIMENTS ON MR. M'CONNELL'S STEAM SLEDGE BREAK, between Bletchley and Oxford; January 19, 1858.

The engine weighed 29 tons; the tender 14 tons 10 cwt.; the carriages 102 tons 9 cwt.; the guards' vans 10 tons $4\frac{1}{2}$ cwt.; steam sledges assumed at 7 tons 2 cwt.

No.		Velocity, in Feet per Second.	Gradient, rising +, falling -.	Distance of Pulling up, in Yards.	Remarks.
1	Mr. M'Connell's Steam Break.	27·5	+ 1 in 150	430	None.
2		32·19	+ 1 in 142	285	Steam and Guard's.
3		19·35	+ 1 in 142	163	Guard's.
4		52·8	- 1 in 214	590	All.
5		60·	- 1 in 149	870	Steam and Tender.
6		48·88	- 1 in 209	673	All.
7		36·67	- 1 in 1,430	623	Steam.
8		55·	- 1 in 163	880	All.
9		55·	0	763	Tender and Guard's.
10		57·39	0	1,320	Steam.
11		52·8	- 1 in 2,211	496	All.
12		60·	0	540	All.
13		55·	+ 1 in 2,211	345	All.
14		47·14	0	538	Tender and Guard's.
15		47·14	- 1 in 452	388	All.
16		62·86	+ 1 in 163	669	All.
17		57·39	0	880	Tender and Guard's.
18		55·	+ 1 in 209	1,194	Steam.

Reducing these results:—

$f = 2$	0·03793	11	0·93677
3	0·01562	12	1·11111
4	0·93791	13	1·42800
5	0·90567	14	0·64824
6	0·74567	15	1·02560
7	0·38224	16	0·78690
8	0·77041	17	0·62379
9	0·66076	18	0·42225
10	0·41586		

Separating those experiments which were made on different breaks, and taking the mean of those made on the same, the value of each break respectively, is as follows:—

For the guard's break	$f = 0·1562$
„ tender break	0·4989
„ steam break	0·4373
„ steam and tender breaks	0·9057
„ steam and guard's breaks	0·3793
„ guard's and tender breaks	0·6443
„ guard's, steam, and tender breaks	0·9678

These numbers represent the actual proportion of the break power supplied in the experiments by each break, or set of breaks. The efficiency of the breaks, in relation to the weight upon them, is:—

For the guard's break	$f_1 = 2·2268$
„ tender break	5·0216
„ steam break	9·1176
„ steam and tender breaks	6·1197
„ steam and guard's breaks	3·1930
„ guard's and tender breaks	3·8014
„ guard's, steam, and tender breaks	4·4365

This shows, that in proportion to the weight upon it, a sledge break, which can be applied instantly, acts most efficiently.

COLONEL YOLLAND'S EXPERIMENTS ON M. GUÉRIN'S SELF-ACTING BREAK.—The train, in the first four experiments, consisted of an engine, tender, and nineteen carriages, two being fitted with M. Guérin's breaks. The total weight of the train was 151·88 tons. In the remaining experiments, two ordinary break vans were substituted for M. Guérin's, the total weight being then 152·8 tons.

2 M 2

TABLE XV.—EXPERIMENTS between Erith and Woolwich ; August 27, 1858.

No.		Velocity, in Feet per Second.	Gradient, rising +, falling -.	Distance of Pulling up, in Yards.	Remarks.
1	M. Guérin's Breaks	50·78	- 1 in 912	597	Tender and two Guérin Breaks.
2		62·86	0	738	
3		57·39	0	552	
4		45·52	+ 1 in 912	395	
5	Ordinary Break Vans.	48·57	- 1 in 912	359	Tender and two ordinary Breaks.
6		66·	0	593	
7		50·77	0	521	
8		52·8	+ 1 in 912	510	

Reducing these results :—

M. Guérin's Breaks.		Ordinary Breaks.	
0·075488	Mean	0·11300	Mean
0·089238		0·12243	
0·099445		0·08245	
0·083898		0·08757	
	0·0870		0·1013

From these experiments, M. Guérin's breaks appear to be less efficient than two equally heavy break vans, of the ordinary description, when used in conjunction with a tender break, in the ratio of 1,165 to 1,000.

The value of f_1 for the mean of these experiments, gives :—

For M. Guérin's break	0·5169
„ the guard's ordinary break	0·5874

EXPERIMENTS WITH MR. INGRAM'S AUXILIARY BREAK, ON THE MANCHESTER, SHEFFIELD, AND LINCOLNSHIRE RAILWAY.—When these experiments were made, the weather was, unfortunately, bad, rain falling the whole time. The train weighed 124 tons $\frac{1}{4}$ cwt. ; the tender, 17 tons ; the carriages to which Mr. Ingram's break was applied, 25 tons $17\frac{1}{2}$ cwt.

TABLE XVI.—EXPERIMENTS with Mr. INGRAM'S Breaks ; October 18, 1858.

No.		Velocity, in Feet per Second.	Gradient.	Distance of Pulling up, in Yards.	Time in Stopping, in Seconds.
3	Mr. Ingram's Break	46·9	1 in 124	870	90
3		44·0	1 in 124	795	77
4		51·3	1 in 130	768	65
7	Tender only	54·2	1 in 120	1,320	134
9		46·9	1 in 120	1,152	114

Reducing these results, on the assumption that the gradients were all falling ones :—

The mean of the Author's experiments from Tables II., IV. and V., gives the retarding force at 389·6 lbs. per ton. The mean of Colonel Yolland's, from Tables IX., XI., XII., XIII., is 311·2 lbs.; or omitting Table IX., 332·6 lbs. Bearing in mind, that the Author's results were obtained under circumstances of competition, and that there was, in consequence, the greatest care in the adjustment of the breaks, the above results agree sufficiently well. Hence also, may be observed the very large diminution of the retardation of the breaks in wet and foggy weather. In wet weather, the retardation does not appear to exceed 200 lbs. per ton, and in foggy weather, with the rails greasy, it appears that it may be reduced to 121 lbs., or nearly one-fourth of the maximum in dry warm weather.

MR. FAY'S BREAKS.

Table		Mean Retarding Force, in lbs. per ton weight of Break Carriages.	
1.	Flap breaks (wet and foggy)	129·0	Fairbairn
3.	" " (slightly wet)	224·9	"
2.	" " (dry)	252·2	"
4.	" " (dry)	408·6	"
5.	Slide breaks (dry)	466·4	"
9.	" " (dry)	273·7	Yolland
10.	" " (misty)	226·5	"

There the maximum retardation amounts to 466·4 lbs. per ton, or nearly one-fifth of the weight of the broken carriages. The mean of Tables 2, 4, 5, in dry weather, gives 375·7 lbs. per ton.

MR. M'CONNELL'S STEAM BREAK.

Table XIV.		Mean Retardation, in lbs. per ton weight, on Wheels and Sledges Broken.	
	Guard's van	154·9	Yolland
	Tender break	349·4	"
	Steam break	634·4	"
	Steam and tender breaks	425·8	"
	Steam and guard's breaks	222·1	"
	Guard's and tender breaks	264·5	"
	Guard's, steam, and tender breaks	308·7	"

These reductions seem to show, that in proportion to the weight upon it, the sledge break is more efficient than any break applied to the wheels. Of course, it is not asserted, that the sledge break could be employed in lieu of the present arrangement; but that it renders useful, in retarding the train, a larger proportion of the weight on it.

M. GUÉRIN'S BREAK.

Table XV.	Mean Retarding Force, in lbs. per ton, on Break Carriages.
M. Guérin's and tender break . . .	35·9 . Yolland.
Ordinary van and tender break . . .	40·8 . "
Both these breaks were, for some reason, acting inefficiently.	

MR. INGRAM'S BREAK.

Table XVI.	Mean Retarding Force, in lbs. per ton weight of the Carriages Broken.
Mr. Ingram's and tender break . . .	240·9 . M. S. & L. Ry.

This break appears to act with something less than the average efficiency.

GENERAL SUMMARY.

		Ratio of weight on Breaks, to retarding force generated by them, or mean co- efficient of friction for each Break.
Mr. Newall's, (dry)	Fairbairn	from 0·1544 to 0·1965
" (wet)		0·0542
Mr. Fay's (dry)	Yolland	from 0·1126 to 0·2082
" (wet)		0·0576
Mr. Newall's (dry)	Yolland	0·1116
Mr. Fay's (dry)		0·1020
Mr. Ingram's (wet)		0·1075
Mr. Guérin's (dry)		0·01048
Mr. M'Connell's steam break . . .		0·28325

That is, the retarding force generated by these breaks varies from $\frac{1}{100}$ th to $\frac{5}{18}$ ths of the weight of the carriages to which breaks are applied, and is, ordinarily, from $\frac{1}{10}$ th to $\frac{1}{8}$ th.

This agrees very well with the deductions from experiments on the friction of metal on metal, which give, for smooth surfaces, a coefficient varying from 0·15 to 0·2, or nearly identical with the best experiments above reported.

PRACTICAL FORMULÆ. — To find the distance on a level line, required to bring a train to a stand by breaking:—

- Let s be the distance of pulling up, in yards.
 v the velocity of the train, in feet per second.
 w the weight on the broken wheels, in tons.
 W the total weight of the train, in tons.
 θ the inclination of the incline to the horizon, if the train is on a gradient; so that if the incline rises

$$1 \text{ foot in } x \text{ feet, then } \sin \theta = \frac{1}{x}.$$

$$g \quad \text{the action of gravity} = 32 \text{ 19.}$$

Then, if the train is broken throughout, and on a level line :—

$$s = \frac{v^2}{6 f_1};$$

and, if breaks are applied to a part of the train only :—

$$s = \frac{v^2}{6 f_1} \times \frac{W}{w};$$

or, if the train is on an incline :—

$$s = \frac{v^2}{6 f_1 \pm 6 g \sin \theta} \times \frac{W}{w},$$

where the + or - sign is to be taken, according as the incline falls, or rises.

The value of the coefficient f_1 , must be selected from the 'Tables of experiments already given, that coefficient being selected, which was obtained under circumstances most nearly approaching those of the case to be determined.

Thus, if the trains are stopped by the friction of their bearings, &c., without the application of breaks, $f_1 = 0.13$ (mean), and $6 f_1 = 0.78$.

If the breaks are ordinary guard and tender breaks, applied together, $f_1 = 4$, and $6 f_1 = 24$, approximately.

For breaks, such as Mr. Newall's and Mr. Fay's, acting with maximum efficiency, $f_1 = 5.5$ to 6.5 , and $6 f_1 = 33.0$ to 39.0 .

Thus, supposing it is required to ascertain the distance in which a train weighing 60 tons, with breaks to 20 tons weight of the carriages, would be brought to rest, in ascending an incline of 1 in 27, at a velocity of 60 feet per second; then taking $f_1 = 4$:—

$$\begin{aligned} s &= \frac{v^2}{6 f_1 + 6 g \sin \theta} \times \frac{W}{w} \\ &= \frac{(60 \times 60)}{6 \times 4 + 6 \times 32.19 \times \frac{1}{27}} \times \frac{60}{20} = 116 \text{ yards.} \end{aligned}$$

If the rails are wetted by rain, the value of the coefficients given above must be taken at one-third less, and if greasy, their value may be reduced by as much as one-half, or three-fourths.

It is convenient, in some cases, to estimate the breaking power in time, rather than in distance. Now, theoretically, putting t = the time of breaking in seconds :—

$$\text{then,} \quad t = \frac{v}{f}.$$

In practice this does not hold strictly true :—

$$\text{hence,} \quad t = \frac{v}{f} - x,$$

where x is a constant to be derived from the experiments :—

$$\therefore x = t + \frac{v}{f}.$$

Now, from the Oldham experiments :—

For Mr. Fay's breaks, $x = 3, 6, 8, 8, 3, 8.$

For Mr. Newall's breaks, $x = 4, 2, 5, 0, 7, 8, 9.$

hence, mean, $x = 5.4.$

$$\therefore t = \frac{v}{f} - 5.4 = \left(\frac{v}{f} \times \frac{W}{w} \right) - 5.4.$$

The above formulæ will be found sufficient for the purpose of ascertaining the amount of break power required to arrest the motion of any train, within such a distance as may be considered safe by the Railway Company, or by the Engineer; or in any given case, to determine the distance within which a train of any required weight may be stopped, when travelling at any velocity.

[Mr. SINCLAIR

Mr. SINCLAIR said, that some years ago, he had seen the apparatus of Mr. Newall, and from the experiments with it which he then witnessed, he thought it an improvement. But he was not able to enter into a comparison of the respective merits of that, and the system introduced by Mr. Fay, as he had never seen the latter; but he understood from the Paper, that the principle of the two was so nearly alike, that the results might, naturally, be supposed to be similar. It should, however, be observed, that both systems were tried under conditions peculiarly favourable, and were not put to the test of the ordinary working of a train. It appeared to him, that to obtain their full effect, they required to be very nicely adjusted, so that the breaks might bear equally. From the little he had seen of Mr. Newall's break, he thought that when it was applied under the ordinary conditions of railway service, particularly when it was used on lines where the train was habitually composed of various descriptions of carriages, it was not calculated to supersede the ordinary break. He would qualify that statement, however, by adding, that it was a long time since he had seen it in operation, and he had not had proper means of making accurate observations. If these breaks, after having been tried under the ordinary conditions in which they must be worked in the service of a railway, exhibited advantages over the ordinary break, he had no doubt, that a fair trial would be given them by the various railway companies.

Mr. HEMANS remarked, that both these breaks must be good and very efficient, to be able to stop a train, running with velocity down an incline of 1 in 27, which was, probably, the steepest gradient that could be found on any railway. It was stated in the Paper, that the train was stopped in twenty seconds, but the distance within which the stoppage was effected was not given in that particular instance. Those distances appeared to have varied from 153 to 575 yards.

Mr. BAMBOROUGH believed the distance, in that case, was 300 yards, and that the velocity of the train was 58 miles an hour, the rails being dry at the time. The velocity of the train on which the experiments were made, was, generally, between 36 miles and 40 miles per hour.

Mr. VIGNOLES said, the remarks he should offer would be of a general character. From the practical results recorded in the experiments made by the Author, it appeared, that Mr. Newall's and Mr. Fay's breaks were upon a par with each other; with the priority of invention, this Institution had not to deal. Although he had been present at a great number of experiments made with various kinds of breaks, he had never yet seen one which would, under all circumstances, efficiently replace the ordinary break now in use. One of the most interesting points in these experi-

ments, was the astonishing difference which the varying state of the weather, and of the rails, produced on the action of the breaks. It was a question in which Engineers were particularly interested at the present moment. The extension of the railway system to almost all parts of the world, had necessitated the occasional introduction of exceedingly steep inclines, often passing over mountain ranges, subject to fogs and rain : thus causing forced retardations of every kind, and severely testing the efficiency of the breaks. The conclusion at which he had arrived from careful observation was, that although many ingenious improvements had been suggested, yet the old break, combined perhaps with the steam break on the engine, was still the most successful.

Mr. BRAMWELL had long been of opinion, that the break upon the guard's van, now generally in use, was the worst that could be designed, and he thought the great care taken in fitting it, caused the effects of the error to be fully felt. His opinion was based on the following consideration. The ordinary break was supposed to apply the break block to each of the wheels of the carriage, whether four, or six in number. Each break block was worked by a lever, keyed fast on a shaft, fitting and working in bearings, and it was only when all the wheels were in the same exact condition, when all the blocks were of equal size, and when all were equally worn, that it was possible to get the action of the break to bear upon them all, at the same time. But taking into consideration the various qualities of wood used for the blocks and their unequal wear, it could readily be understood why it so commonly happened, in practice, that only two of the wheels were acted on by the break, and that even those two were retarded, not by the blocks acting efficiently on both, but by one block acting on one wheel, and transmitting the strain through the axle to the other. With the present construction of break, accurate fitting involved bad results in the working ; but if the shaft, instead of fitting in a bearing, were laid in a slot hole, so that there could not be a pressure upon any one wheel until it was equal upon all, he thought the break would be more efficient. The old waggon break, where the shaft hung in a sling, was even better than that which was now fixed, at great expense, in the guard's van. In America, where there were generally eight wheels to each car, the breaks were made so as to be capable of being put into action from either end of the car, and were so arranged, that all the wheels had an equal amount of pressure ; chains were used as a means of connection. In his opinion, if there was less good workmanship, but more common sense employed in the construction of breaks, and more attention was given by the guards, the apparatus would be far better than at present.

Mr. WIGHTMAN remarked, that it was possible to have too much

break power as well as too little. The system of using four, or more break carriages in a train, caused, in his opinion, too much reliance to be placed upon them, and encouraged the engine driver to maintain the speed up to the last moment, depending entirely upon the guard for stopping the train. The ordinary break of the guard's van, and that on the tender of the engine, constituted as good a system as any hitherto suggested. He did not think, that long continuous breaks were likely to serve the desired purpose, so well as the inventors imagined. Those fitted with bars were, probably, safer than those fitted with chains. He had not seen the application of Mr. Newall's break upon a train, but a somewhat similar plan was in operation upon the North London Railway, where the break was applied, simultaneously, to four carriages, and the train was brought up within a very short distance. But he doubted, whether the train could be stopped sufficiently quick to prevent a collision, supposing some impediment to be suddenly seen on rounding a sharp curve. The President could best judge of the effect which the sudden stoppage of a train would produce upon the passengers. A six-wheeled tender, and a guard's break with all the wheels skidded, would be found, in his opinion, quite as usefully powerful as the breaks described in the Paper.

In answer to a question from the President, Mr. Wightman said, that on the North London Railway, the engine drivers sometimes entered the stations at a greater velocity than formerly, depending upon the power of the break for arresting the train.

Mr. HAWKSLEY said, this was a question upon which, though not practising as a Railway Engineer, he had some experience. He confessed, that he had not been able to understand the object of the Paper. It seemed, from the opening remarks, that it was intended to give information with respect to the mechanism, power, and operation of breaks for stopping the wheels of railway trains, but the instances which were given, related to the stopping of the trains themselves, by the friction between the wheels and the rails. Now if in the two cases cited, the trains were of equal weight, and the wheels to which the breaks were applied had equal weights upon them, then if the wheels were stopped in the same time by the application of either apparatus, the results must, of course, be precisely the same, although the mechanism of the one break might not only be more manageable, but vastly superior, in construction, durability, and economy, to that of the other. The results given in the Tables were, therefore, those which might have been arrived at, without any experiments whatever on the breaks in question; inasmuch as any apparatus used for stopping the wheels, would have produced exactly the same amount of friction, and would, therefore, have brought the train to rest, in exactly the same time and after running exactly the same distance. The

trains to which Mr. Fay's apparatus was applied, afforded a coefficient of 4.06, and those to which Mr. Newall's apparatus was applied, a coefficient of 4.10. Practically, there was no difference between the two sets of experiments, nor ought there to be; and the real question at issue was not, and could not be determined by experiments of this nature. The experiments simply showed, that the two descriptions of breaks were each capable of being applied to the wheels, with equal rapidity, and with sufficient force to arrest their revolving motion, but they left untouched the determination of their relative value, in an instrumental point of view.

With regard to the break applied as a dynamometer, it appeared, that under ordinary circumstances, the friction, or adhesion on a railway, deducible from these experiments, was about the same as had been generally received, since the time at which the Comte de Pambour wrote upon the subject; being about, on an average, one-seventh of the weight. In certain states of the weather, when the rails were wet and greasy, it was found to be as little as one-eighteenth to one-twentieth of the weight on the impeded wheels; and when the wheels were perfectly dry, as much as one-fourth, or one-fifth of that weight. All, therefore, that could be said as to the effect of breaks upon railway trains, was, that it must depend upon the velocity of the train, and upon the weight on the wheels to which the break was applied, as compared with the total weight of the train. Bearing in mind these two conditions; first, that if a train was suddenly stopped, it would greatly damage the carriages and, probably, injure the passengers; and secondly, that if the engine drivers had the means of employing a break which would stop a train within a very short distance, they would depend upon the use of that power, under all circumstances; it was probable, that instead of the number of accidents being diminished by a more powerful breaking apparatus, they would be greatly increased.

The formula to be used for the determination of the time and distance in which a heavy body could be stopped by friction, had been known and applied, ever since the law of gravitation had been discovered, and might be found in almost all books on mechanics. It was only necessary to know the pre-acquired velocity of the train, the total weight of the train, the weight insistent upon the sliding wheels, the gradient, and the materials of the sliding wheels and rails, to determine, within the limits of error due to the accidental condition of the rails, the distance and time at which the train would be brought to rest. From these considerations it appeared, that the Paper still left, in an undecided state, the relative merits, as mechanical appliances, of the two inventions.

Mr. VIGNOLES said, he could not concur in the opinion, that the

friction, or adhesion on a railway, averaged one-seventh of the weight of the train. For all practical purposes, he thought it was not to be taken at more than one-twelfth, in all states of the weather. That was a material point, because the friction, or adhesion of the engine upon the rails, was never, under the most favourable circumstances, one-fifth of its weight.

Captain HUISE had seen Mr. Newall's break in operation some years ago, but he did not recollect ever having seen Mr. Fay's apparatus; the principle of both appeared, however, to be the same, and any future invention would, he supposed, be directed towards devising some method of locking all the wheels. There were two points for consideration; first, whether it was desirable to produce the effect proposed; and secondly, if desirable, whether in the conduct of a railway it could be carried into effect. He doubted whether this system of breaking could be efficiently maintained, even in a selected train, continuously used; and in any case, he did not believe, that railway companies would ever arrive at such an unanimity of opinion on the subject, as to cause it to be generally adopted. In the Report of Captain Galton,¹ it was stated, that "the efficient action of such a system of breaks might be, to some extent, diminished by the mere adoption of the system, by all the companies whose carriages are used on the lines; but they would point out, that the cases are few in which, if the larger companies would adopt it for their own stock, careful management in working of the trains could not enable the system to be effectually applied." Now they might be few; but if throughout the kingdom, a single trunk line objected, there would be no possibility of effectively carrying out the plan, for the trains of the London and North Western Company came in, with the carriages of many other companies intermixed. Although the London and Brighton, and perhaps, the Eastern Counties Railways, with their tributary branches might, as isolated systems, carry into effect an arrangement of this kind, it was certain, that the general assent of all the companies would never be obtained, and unless all the carriages of every company were built for the purpose, the whole system was rendered ineffective. The strongest evidence against it was to be found in the fact, that although it had been constantly brought under notice by paragraphs in the public newspapers, yet the system did not extend. Even upon the Lancashire and Yorkshire Railway, where it was first introduced, he doubted whether it was considered in any other light than an experiment. He entirely concurred in the opinion, that by giving the guards such efficient machinery and perpetually calling it into action upon

¹ *Vide* "A Letter from the Railway Department, Board of Trade," September 27th, 1858.

all occasions, the vigilance of the persons in charge of the trains would be materially relaxed. The best break he had hitherto seen, for universal application, was Mr. M'Connell's steam break. A railway train consisted, upon an average, of about seven carriages, the average weight of which would be from 28 tons to 30 tons; including the passengers, it would not exceed 30 tons. The weight of the engines averaged about 30 tons; thus the momentum produced was equal to the entire weight of the train. The introduction of a steam break would place at the disposal of the engine driver, a power which could be brought into operation, whenever there was any imminent danger. The action of that break was very powerful; it certainly did injury to the rails, but it was only intended to be used in exceptional cases.

Mr. HAWKSHAW, V.P., could not subscribe to the doctrine, that the more perfect the machinery, the more mischief was likely to arise in the application of it. This system of breaks had been tried, for some years, on the Lancashire and Yorkshire Railway, and it was admitted, that it could only be successfully applied to carriages which always ran together; it became useless when carriages belonging to other companies formed part of the train. These improvements, therefore, although ingenious, had not been generally applied on the Lancashire and Yorkshire Railway. He did not think there would be so much danger as was apprehended, in locking all the wheels of a train, for the stoppage would not be so sudden as to injure the passengers. The breaks now in general use, were good; the break vans were constructed so as to command a large amount of break power, and these vans, having an especial object, were likely to be properly attended to, and could be relied upon when wanted. The application of breaks to the locomotive engines, which had been recommended, was a very old expedient; the system was applied, twenty years ago, on lines near Bolton, but it was found to act so injuriously upon the engines, that the plan was abandoned. The value of a break was to be measured by its capability of locking the wheels of a train; and upon the quickness and facility with which this could be accomplished, depended the whole value of every system of breaks. The comparative merits of the two breaks mentioned in the Paper, consisted simply in the greater, or less facility of their application.

Mr. HEMANS said, as there appeared to be some difficulty in discovering the object of the present Paper, he would, in the absence of the Author, venture an opinion. He thought it was intended to establish a general formula which would apply to the time and distance in which a break ought to stop a train, at all velocities, and on all inclines, up, or down; and that the break which produced an effect most nearly approaching that standard of comparison, was considered to be the most perfect. It was obvious,

that from the moment the wheels of a train became completely locked, the effect was precisely the same, whatever break had produced the result. The value of the break, therefore, consisted in the rapidity with which this could be accomplished. The formula gave the results that ought to be obtained ; and that break which approached the nearest to it, from the instant of its being applied to the wheels, was to be considered the best.

Mr. HAWKSLEY said, there never had been any doubt as to the formulæ ; they had been laid down in all the books upon the subject, long before railways were invented, and the conditions of the action of gravity and of resistance by friction, must remain the same for ever.

Mr. POLE inquired the results of the application of the continuous break, in use upon the North London Railway. He had understood, that one of the great advantages of the system, was, that it reduced the time required for frequent stoppages ; and he presumed, that the expense of it on that line would supply evidence on the point.

Mr. WIGHTMAN replied, that he had only seen the break in operation, when the North London train ran into the station of which he had charge. He could not, therefore, give any exact details, but it appeared to be a continuous break, worked by a chain communicating with four carriages. He had observed, that the engine drivers of that company depended too much on the great power of the break, and although little danger was to be apprehended at intermediate stations, he feared some accident might, eventually, occur at the terminal stations.

Mr. CHAMBERS said, he had introduced the apparatus referred to, which had been in operation for two years on the North London Railway, and had never yet failed. The manager had declared, that without this break, the present accuracy of running could not be maintained on the line.

Mr. BATEMAN considered, that the chief object of the Paper was to institute a comparison between the break of Mr. Newall and that of Mr. Fay, rather than to treat, generally, of the principle of breaking. Having been a frequent passenger upon the East Lancashire Railway, where, for some years, Mr. Newall's break had been applied to some carriages, he had noticed, that the train was stopped in a very short time, and that the plan appeared to answer exceedingly well.

Mr. BROUGHTON was in the service of the East Lancashire Railway, some years ago, when Mr. Newall's break was first introduced. Since then, he had tried it upon the Ulster Line, where it was still in daily use. One advantage was, that it stopped a train much sooner than the break on the guard's van ; and another advantage was, that the wheels were not skidded, and

there was a considerable diminution in the wear and tear of the rails, at the stations. He had also been informed by the superintendent of the locomotive department, that the tyres of the tender and carriage wheels now lasted longer than before this break was used. The experience of four, or five years had convinced him, that the advantages of the system were considerable.

Mr. BIDDER,—President,—said, that if the object of the Paper was, as had been stated, to define the difference between Mr. Newall's break and that of Mr. Fay, it certainly had not pronounced in favour of the one, or the other. A decision of that kind was very difficult to arrive at, as there were so many elements to be taken into consideration; the state of the weather, the condition of the rails, and the condition of the breaks themselves. If, on the other hand, the object of the Paper was to establish a formula of the adhesion of metal to metal, that could be easily obtained from the simple rules already laid down, and must be well known to every one in charge of a railway. The adhesion varied, according to the state of the atmosphere, from 1 in 4 to 1 in 20; and the only question for those who had the control of the machinery, was to determine the amount of adhesion, under the circumstances most commonly met with in their experience. Whether the velocity was 30 feet, or 100 feet per second, if the resistance was 1 in 6, or 1 in 20, it would take six times, or twenty times the height required for a body to fall, to estimate the distance to bring the train into a state of rest; and the time required would be in proportion. But to propose a formula, was to go back to the most elementary proposition that could be submitted to a scientific institution. Experiments upon breaks, to be of any practical value, and to test their capabilities, ought to be made under circumstances of difficulty and intricacy. But after all, the practical solution of the question was in the commercial view of it; and if the Inspectors were paid by a per centage on the net earnings, with a corresponding deduction for the damages occasioned by accidents, the most efficient breaks would soon be provided.

One great object of breaks was the saving of time in stopping a train, particularly on short lines. Now the safety of the passengers being mainly dependent upon the engine driver, the greater control he had over the train the better, for he was, generally, in a position to see the first indication of danger. If he had to rely upon a self-acting apparatus, or upon apparatus under the control of others, he would not, as observation had proved, shut off the steam in approaching a station, whether intermediate, or terminal, so soon as he otherwise would do; and the safety of the public was thereby endangered. The break could not be applied to the engine, without injuring that most expensive element of railway machinery; and in a commercial country it could not be expected, that a rail-

[1859-60. N.S.]

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way company would provide for the safety of the public, in a manner so greatly disproportionate to the commercial results. He thought, therefore, that all things considered, the most efficient break was that ordinarily in use.

This discussion recalled to his recollection, an anecdote which was somewhat *à propos* of the subject. A gentleman connected with the office of the late Mr. Robert Stephenson, but who had no scientific knowledge, had ascertained a formula for deciding upon the merits of breaks, which was more efficient than that given in the Paper. When inventors, each in their turn, insisted on laying their plans before him, he would inquire of them, the peculiar advantage of their particular breaks. "It will stop the train dead," said the inventor. "That will be the state of the passengers," retorted the other. "But it will also stop a train gradually." "So will the break of anybody else," was the conclusion at which the unscientific gentleman had arrived.

April 24, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

No. 1,019.—“Account of the Works recently constructed upon the River Severn, at the Upper Lode, near Tewkesbury.” By EDWARD LEADER WILLIAMS, M. Inst. C.E.

THE whole of the works constructed, during the past fifteen years, for the improvement of the navigation of the River Severn, between Stourport and Gloucester, were projected in 1841, by Sir William Cubitt, (Past-President Inst. C.E.). The impediments at that time existing to the navigation of the river, in the district in question, were of two kinds. Those in the upper portion, between Stourport and Tewkesbury, consisted of beds of marl rock and compact gravel, which intersected the channel at various points, causing shallow rapids at low water; but acting also as natural dams, they penned up the water in the intermediate deep reaches. The other class of impediments consisted of shoals of quicksand and mud, deposited by the winter floods and the spring tides, in the lower district, between Tewkesbury and Gloucester, wherever a sudden increase of width in the river channel gave an enlarged sectional area, and caused a corresponding decrease in the velocity of the current. The distance between Stourport and Diglis, near Worcester, is fourteen miles, and the fall per mile, at low water, in 1841, was 19·75 inches. From Diglis to Tewkesbury, sixteen miles, the fall, at low water, was 4·70 inches per mile, whilst the fall in the ten miles between Tewkesbury and the Upper Parting, near Gloucester, is only 2 inches per mile.

The scheme of improvement laid down by Sir William Cubitt, in 1841, was that indicated by the natural features of each district. In the upper portion of the river, the natural dams were to be removed by dredging, artificial weirs being substituted, with corresponding locks, in side cuttings, for the passage of the traffic. In the lower district, the tendency to deposit was to be counteracted, by the construction of fascine embankments running parallel to the course of the current, and equalising the sectional area of the channel; the accumulations of sand being, in the meantime, taken up by steam dredgers, and deposited behind the fascine embankments. The proposed sites for the locks and weirs

were Lincomb near Stourport, Holt, Bevere, Diglis, near Worcester, and Ryall, between Upton and Tewkesbury.

The intended application of artificial works to so important a river as the Severn, whose floods have been known to rise 18 feet in five hours, and, not unfrequently, to attain a height of 25 feet above the level of low water, naturally excited much discussion, especially amongst the owners of land upon the river banks, and these discussions led to a very searching and protracted Parliamentary investigation into the merits of the scheme of improvement. The result was the appointment of a body of Commissioners, to whom power was delegated to execute the whole of the projected works, with the exception of the lock and weir at Ryall, which was to be abandoned, and in its stead, an attempt was to be made to improve the navigation of that portion of the river, between Diglis and Ryall, by dredging through the extensive beds of gravel and marl, which intersected the channel at several points in that district. This decision was arrived at, in opposition to the evidence given upon the part of the promoters, to the effect, that the removal of the natural dams, without the substitution of an artificial weir, would lower the surface, instead of increasing the navigable depth.

Papers giving a full description of the works erected between Stourport and Diglis, together with the blasting of the red sandstone, and marl rocks, under water, and the dredging operations, have been already presented to the Institution;¹ these portions of the work, therefore, require no further notice. It must, however, be stated, that the result of the experiment, forced by Parliament upon the promoters of the improvements, was, that after having kept four steam dredgers constantly at work for upwards of two years, and having removed more than 1,000,000 tons of marl, gravel, &c., it was found, that the level of low water upon the lower cill of Diglis Lock was lowered 3 feet below its original mark, and that the required navigable depth could not be maintained. This state of things obliged the Severn Commissioners to renew their application to Parliament, for powers to carry out Sir William Cubitt's original plan in its integrity, with a slight alteration in the position of the proposed lower lock. This alteration was suggested, to meet the local requirements of the town of Tewkesbury, the site of the projected works being removed from Ryall, four miles above that town, to the Upper Lode, half a mile below it. A fierce, and protracted, Parliamentary struggle, extending over five years, resulted, in 1852, in the ultimate triumph of the projectors; but the unfavourable state of the money market delayed the execution of the works, and it was not until the autumn of 1856, that they were commenced.

¹ *Vide Minutes of Proceedings Inst. C.E., vol. iv., p. 361; and vol. v., p. 340.*

The channel of the Severn at the Upper Lode, formed, at that time, an acute angle, changing from N.W. to S.S.E. This sudden bend interfered prejudicially with the navigation, as well as with the discharge of the flood waters; it was, therefore, determined to construct the proposed works in new channels, with easy curves, and to cut off the objectionable angle. The course of the river, both in its unimproved, and its improved state, is shown in Plate 5, Fig. 1. Twelve years' practical working of the locks and weirs between Diglis and Stourport, had demonstrated the correctness of the principles upon which they had been constructed, and little in the shape of improvement remained to be effected, in the details of the works at the Upper Lode. The introduction, upon the river, of steam power for towing, had, however, proved, that much time could be saved by an arrangement that would admit of the passage, at one locking, of a steam tug, with an accompanying fleet of vessels. It was, therefore, determined to effect this object by adding a basin, or second chamber, to the lock proper, which basin should be entered by a pair of gates, similar in every respect, to those of the lock, the line of the bottom of both lock and basin being uniform throughout, and therefore, by working the upper gates of the lock with the lower gates of the basin, the whole intervening space would form a large lock chamber. The lock proper is 120 feet in length and 34 feet in width, and the basin is 156 feet in length by 80 feet in width; the total length between the upper gates of the lock, and the lower gates of the basin, being 276 feet, and the enclosed space, sufficient to lock up a steam tug, together with six Severn trows, each of 100 tons burthen. A plan and longitudinal section of the lock and basin are given in Figs. 5 and 6; and transverse sections, in Figs. 2, 3, 7, and 8.

The depth of excavation required for the lock pit, was 35 feet, and here some difficulty was anticipated, as the trial pits gave the following section. There were 2 feet of loam beneath the grass surface, 16 feet of sound red brick clay, 14 feet of imperfect blue lias clay intermixed with black mud and decayed vegetable matter, in which strong springs of water were met with, and 4 feet of compact blue lias clay, overlying a thick bed of water-bearing gravel. The sides of the first 18 feet of excavation stood at a batter of 1 to 1, and no difficulty was experienced, until the 14-feet stratum of imperfect blue clay and black mud was reached, but no sooner was the sound upper crust removed, than the semi-fluid mass beneath it began to rise in the pit, letting down the clay on each side, and breaking up the whole into a confused mass. The remedy first suggested, was to drain off the water by powerful pumps, but the proximity of the river, which surrounded the work on three of its sides, gave little prospect of success from such an attempt. Relief was, therefore, sought in another, and as experience

proved, the right direction. Finding that it would be useless to attempt to encounter the whole mass at once, it was determined to attack it in detail, and a pit, 20 feet long and 12 feet wide, was commenced in the line of the proposed east wing wall of the lock forebay. As the excavation of this pit proceeded, its sides and ends were planked with half timbers, well strutted; but notwithstanding every precaution, much anxiety was felt, as the pressure of the surrounding mass was so great, as frequently to cause whole timber struts to give way. As soon as the sound clay was reached, the pit was filled with concrete, the timbers being removed as the filling proceeded. A second pit, of similar dimensions to the first, was then commenced at a distance of 20 feet from it, in the intended line of the lock wall, the intermediate space of soil being left in, until the concrete in the first pit had become consolidated. This process was continued throughout the whole length of the excavation, on each side, and by the time it was completed, the concrete in the pits first filled was sufficiently firm, to admit of the excavation of the remaining spaces of 20 feet between them.

The whole of the wall foundations, composed of concrete 12 feet in width, and 14 feet in depth, were thus got in. It was expected, that this large mass would, by its gravity, sufficiently resist the pressure of the soft soil by which it was backed, especially as the blue clay, in the centre between the two lines of concrete, was left in; but this expectation was not realised, as the pressure proved to be sufficient to force the two concrete walls bodily inwards, to the extent of 18 inches on each side, and to press up the intervening blue clay, until its surface assumed the appearance of a troubled sea. Strutting timbers of large scantling were then fixed, to prevent the further inward movement of the side foundations, whilst transverse pits were sunk between them, and were filled in with concrete, forming an inverted arch, 6 feet thick at the crown. These pits were gradually carried through the whole length of the work, upon the same plan of operation as had been adopted in the side wall foundations. The sinking of the first of these transverse pits, however, demonstrated, that something more efficient than struts of whole timber was required, to prevent the inward movement of the sides. The balks gradually sprung, until it was evident, that they were unequal to their work, and that before the bottom could be reached, they would give way. To meet this new difficulty, pits were sunk at the back of the longitudinal walls of concrete, at right angles to them, running 10 feet into the slipping mass of clay at the back. These pits were 10 feet wide, and were sunk to a depth of 2 feet below the bottom of the concrete wall foundations, in order that the bottom line of the whole mass should not be uniform. The concrete filled into these pits, formed counterforts to the lock wall foundations, and they were carried 12 feet

apart, through the whole length of the work, at the back of each side wall. This arrangement very much reduced the movement, but it was not quite overcome, until the inverted arch of concrete, which formed the centre, was completed. The pressure in some of the transverse pits was so great, as to render it unsafe to remove the whole timber struts, and in these cases, they were left in the pit, and buried in the concrete. The concrete foundations thus completed, contain upwards of 7,500 cubic yards, and upon them the lock walls are built.

The lock chamber, invert, and wing walls, together with the walls of the lower entrance to the basin, are faced with blue Staffordshire bricks, and are backed with bricks made from the upper stratum of red clay. The walls of the basin are built of rubble stone from the neighbourhood of Chepstow, set at a batter of $\frac{1}{2}$ to 1 upon the face. The quoins and copings are of grey sandstone, from the Forest of Dean. The cills, into which shutting cills of elm timber are firmly bolted, are of red sandstone, from Areley, in Shropshire. The lock gate-frames are of oak, and they are planked diagonally with pitch-pine planks; they are worked by balance beams of cast iron. The upper gates of the lock are 25 feet high, and 20 feet 6 inches wide; the two pairs of lower gates are 33 feet high, with the same width as the upper gates, and being carried upon an iron pivot, with a hemispherical steel top, are easily worked by one man. The lock and basin are filled and emptied, by means of culverts, 6 feet high and 4 feet wide, in each of the side walls, and the whole area contained between the upper gates of the lock and the lower gates of the basin, is filled through these culverts, in five minutes. The paddles, with their lifting gear, are similar in construction to those of the Upper Severn locks, which have been described in the Paper already alluded to; they will, therefore, require no further description.

The weir, of which transverse sections are given in Figs. 4 and 9, is 500 feet long, 40 feet wide, and 6 feet high. A row of close piling at the foot of the inclined apron forms the lower cill, the upper cap cill being laid upon walings of half timbers, which are bolted to gauge piles, driven 12 feet apart through the whole length of the weir. The space between the two cills is filled with rubble limestone from Chepstow, and the face of the apron and back of the upper row of piling is pitched with the same description of stone, set in courses, and rough-pitched upon the face. The section of the back pitching is a parabolic curve, which is continued over the cap cill; experiments tried upon the Upper Severn weirs having proved, that this form facilitates the discharge of the water, instead of causing that upheaving of its surface at the line of contact, which is common to weir cills of other forms. The apron inclines at an angle of 1 to 5 down to the

lower cill, beyond which, a horizontal stone sheeting, similar in construction to the apron, extends to the width of 10 feet, and protects the lower row of sheet piles from the scour to which they would otherwise be exposed. The east end of the apron is considerably curved, in order to admit of its extension at that point, and to ease its inclination of 5 to 1, to 10 to 1; the object being to facilitate the passage of the salmon up the river, which is further promoted by a groove 4 feet wide, and 12 inches deep, formed in the face of the inclined plane, in the centre of the curve before alluded to. The effect of this arrangement is to secure a sufficient depth of water always passing down the groove, to cover the back fin of the fish, and thus they are enabled to pass up the river, without hindrance, even in seasons of drought. The heavy freshes to which the Severn is subject, are discharged by the weir with such rapidity, as to fill the channel of the river below it, five times as fast as that above it is filled, until the surface forms one unbroken plane, by the weir being thrown out of operation. Thus a rise of 1 foot 6 inches in the upper level, causes a rise of 7 feet 6 inches in the lower level, at which height the weir is out of action.

The lock and weir being completed, the old channel of the Severn was dammed up, at a point indicated upon the plan, (Fig. 1,) where the width of the channel was 60 yards. The dam was carried across the river, and the water raised to the height at which it would flow over the upper cill of the weir, by a simple process. A jetty, formed of thorn kids, or fascines,—each kid, or bundle, being about 6 feet long and 15 inches in diameter,—was commenced on each side of the river, and after the abutment of each jetty was well punned, with stiff clay, into the opposite banks, the work was advanced from each side, at an angle of $22\frac{1}{2}^{\circ}$ pointing up the stream, the kids being laid in courses three deep, and the brush end of each course being advanced a head, 12 inches beyond that upon which it was laid. After the three courses were got in, they were covered, and weighted, with stiff clay, which was well trod into the thorns, and upon this bed, three other courses of kids were laid as before, the work advancing, both a head, and in depth, as the process was continued. As the heads of the advancing jetties approached each other, reducing the open space between them, the rush of water through it was like a mill race; but notwithstanding the continued increase of the velocity, due to the increasing head of the upper level, the brush ends of the kids were advanced without the slightest interruption, or difficulty, until both jetties met and formed the base of the permanent dam. This was, ultimately, increased to a width of 130 feet, and the towing path was carried over it, banked up, above the flood level.

The estimated cost of the whole of the works which have been described, was £35,000, and although the contingencies were unusually heavy, the expenditure has been within the estimate. They have stood the test of the floods of last winter, without damage, and have demonstrated the correctness of the principles originally laid down by Sir William Cubitt, for the improvement of the navigation of the River Severn.

The tugs used upon the river are paddle-wheel boats, 93 feet long by 19 feet 6 inches over all. They are worked by side-lever condensing engines, with cylinders 30 inches in diameter, and a stroke of 4 feet. The boilers have three furnaces with ninety return tubes, each being 5 feet 9 inches in length, and $2\frac{1}{2}$ inches in diameter; the steam is worked to 20 lbs. per square inch. One of these boats sometimes brings up, from Gloucester to Worcester, a string of twelve loaded Severn vessels behind it; the dead weight, hauled at the rate of three miles per hour, against the stream, being upwards of 700 tons.

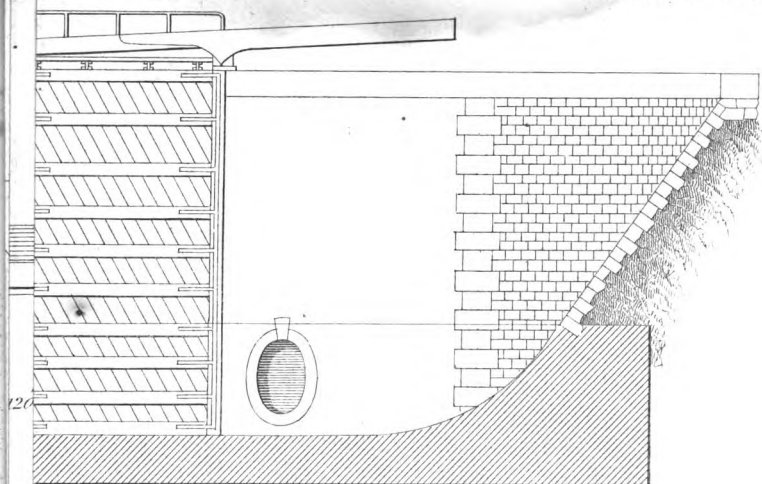
The Paper is illustrated by plans and diagrams, from which Plate 5, (Figs. 1 to 9,) has been compiled.

[Mr. W. A. BROOKS

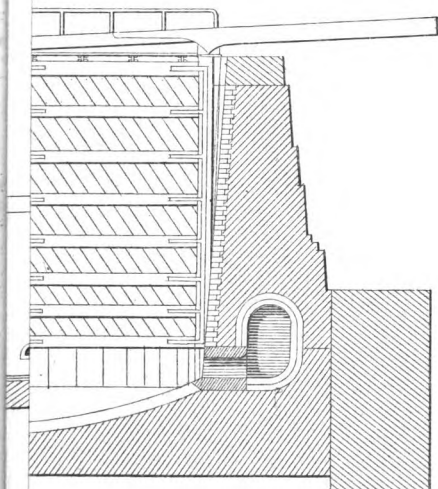
Mr. W. A. BROOKS had great respect for any work designed by Sir William Cubitt, but not being acquainted with the locality, he could not pronounce an opinion, without further examination of the plans, and a knowledge of the state of the river. He was opposed, as a general principle, to placing weirs across navigable rivers, and he was, therefore, not convinced, that the present experiment would be successful. He wished to know the state of the navigation below this weir, which had occasioned, it appeared, a loss of tidal range of about 6 feet. He thought the navigation below must suffer, although it would, no doubt, be alleged, that the upper navigation of the Severn was mainly due to the power of the land floods; and, therefore, that the tidal scour was not so essential as in other cases. In the Severn, the floods rose to an enormous height; and this, probably, contributed to maintain the channels. But a better effect would have been produced upon the navigation below, if the full tidal flow had been preserved. If any recent soundings had been taken of that part of the navigation, it would be desirable, that they should be laid before the Institution.

Mr. JOHN MURRAY had been much pleased with the Paper, which gave a very detailed account of a difficult work,—getting in foundations, so near to, and nearly surrounded by, a large body of water. The Author had treated the case very effectually, by executing the work in patches, and leaving the natural surface of the ground untouched, until he had secured the portion upon which he was immediately engaged. By raising this dam, the upward flow of the tide would, no doubt, be obstructed, and the scour be thereby lost; but on the other hand, the weir improved the navigation in the upper part of the river, for without it there would have been very little depth of water. To keep up the navigation, the weir must be maintained and the river improved below, to compensate for the exclusion of the tidal water. The river should not be allowed to remain in its present state, but improvements should be made in the bed below, to keep pace with the other works, carried on in the upper part of the stream.

Mr. BEARDMORE was not well acquainted with this part of the Severn, but he admired the third pair of gates and the large basin, below the lock, because it was the only method he saw of accomplishing the object of transporting trains of barges, towed by steam power, by which means alone competition could be maintained with railways, especially in the carriage, for long distances, of great quantities of merchandise, at comparatively slow speeds. He had recommended a similar plan, for a river in which vessels of large tonnage were employed, where the third pair of gates and head of the basin had to be constructed, without stopping the traffic through the lock above. The Author of the Paper had



TION OF BASIN



TION OF LOCK

KELL BROS LITHRS CASTLE ST HOLBORN.

demonstrated a plain fact, worth more than whole days of evidence before a Parliamentary Committee, that although the water was penned-up 5 feet at the weir itself, yet the effect of giving a large mean depth, and bringing the water over the weir by a good section and ample weir pool, resulted in the floods being carried over the weir with less heading above, in the proportion of 1 to 3, compared with the 'tailing-up' of the natural river below. This was to be attributed to enlarging the body of water, to increasing the hydraulic mean depth, to removing the shoals, and to regulating the section of the river.

When a river, from its natural position and character, required canalisation, the most convenient method to pursue, was to make a weir and to combine it with the requisite increase of depth; but, of course, at the lower, or tidal, end of a river, there must always be a point, at which dredging and a freer admission of tide, became more desirable and was also less costly. Increase of the hydraulic mean depth, however, was not less remarkably advantageous in the latter case; being the most perfect way of conveying a volume of water, and of maintaining a deep channel where there was a deficiency of fall, either in the continuous downward flow of an upland river, or the oscillating flow of a tidal estuary. He considered this to be one of the golden rules in works for navigation, for drainage and other hydraulic operations, as well as in the treatment of tidal channels. He did not now propose to enter into the question, whether the weir and lock, under consideration, were placed at the proper point, with regard to the tidal action of the Lower Severn; that was a purely local and practical question, and one on which the Author of the Paper must be the most competent authority. That the increase of sectional area and an efficient hydraulic mean depth, combined with a judicious application of weirs, would benefit the flood-discharging power of the River Severn, was the great matter of debate in the early history of these improvements; he would, therefore, ask whether the lands were now better drained, and whether the water stood higher, or lower, at flood time, than formerly.

Mr. J. COCKBURN CURTIS, during the Parliamentary contest alluded to by the Author, was called upon, in 1848, by the Admiralty, to examine and report upon the question of erecting the weir and locks which had been described in the Paper.

There was one point regarding the treatment of rivers, which had been touched upon, to which he would call attention. It had been correctly stated, that whenever it was wished to increase the facilities of navigation in a river subject to droughts, the great point was to increase its hydraulic mean depth. He concurred in this general principle, but he contended, that with the same

amount of water passing down a channel, it was possible to increase the hydraulic mean depth, by three different processes; first, by the erection of a weir and penning back the water, as described in the Paper; secondly, by excavating the head of the channel to a sufficient depth so as to reduce the slope; and thirdly, by embanking the channel at the sides so as to reduce its width. The determination of which of these modes of treatment should be adopted, must be dependent upon the character of the supply of water from the catchwater basin, and the configuration of the valley and channel of the river, in the district under consideration.

He could not concur in the sweeping denunciations which were sometimes made against the use of weirs in the improvement of rivers, for his experience and examination of river works in India and Spain had convinced him, that in many cases, the physical disadvantages of rivers might be overcome, and both the navigation and the drainage might be materially benefited by their erection. In districts where the inclination of the river bed and the slopes of the adjacent valley were considerable, and when the natural channel above the weir was much encumbered by shoals, if the weir was constructed of sufficient length, it would, generally, be found, that the ordinary summer supply, and the first freshes and floods would pass down the portion of the river in which the weir was placed, with considerably more velocity than they did before it was erected. This result was to be attributed to the shoals which formerly obstructed the stream at its lower levels, being covered by the water penned back by the weir, so that the ordinary drainage and the first freshes and floods, coming in the earlier stages of their progress, into a channel pre-occupied by water, experienced less resistance to their passage than they did previously, when they had to contend with an almost empty channel impeded by shoals. He confined, however, these observations, as to the effect of a weir, to the lower levels of the river, and to the early stages of the freshes and floods; when the water accumulated below the weir, and the weir became 'drowned,' or thrown out of action, it then acted simply as a steep shoal, or ford, in the bed of the river, and, as such, no doubt, created an obstruction to the final passing off of the flood waters.

During his examination in 1848, he had a series of observations made at two stations, one, half a mile above, and the other, half a mile below the weir, at Diglis; and he found, that at the lower levels, the discharging power of the channel above the weir, even exceeded that stated by the Author. These observations showed, that when there were only 3 inches, or 4 inches passing over the weir, the channel above the weir discharged the water eight times faster, than the channel below it could carry it off. But this pro-

portionate rapidity of discharge decreased very rapidly as the water rose in the lower, or natural channel, and eventually, as the slope of its surface and the hydraulic mean depth increased, the natural channel had the advantage as regarded rapidity of discharge. He considered these results attributable to the relative existence, or absence of friction; in short, to the fact, that water could pass over water much more easily than it could over stones, or gravel.

The fall of the River Severn between Stourport and Diglis, before the weirs were erected, was about 21 inches per mile, while the natural fall between Diglis and the site of the Tewkesbury weir was about $4\frac{1}{2}$ inches per mile, or about one-fifth of the natural fall of the upper district; the valley of the lower district was also considerably broader. Before the Tewkesbury weir was erected, spring tides, occasionally, rose as much as 18 inches at Diglis, and 4 feet at the site of the Tewkesbury weir. Looking at the marked difference in the character and configuration of the upper and lower districts, he had come to the conclusion, that the system of improvement by weirs, would be attended with great disadvantages, if extended below Diglis; but at the same time, he freely admitted, that much might be said on the other side of the question. It appeared to him, that from the much greater extent of district to which the level of the penned water would extend, at the lower levels of the river, and the circumstance of its being so much sooner thrown out of action, by the banking up of the tail water, or the rise of spring tides, it could not but have an injurious effect on the drainage of the valley lands; that if the weir was made sufficiently long for the discharge of the flood waters, it would have a tendency to drain off the summer water too rapidly, and thus to diminish the summer supply for navigation in the channel below it; and that cutting off 16 miles of the tidal flow of the river during spring tides, could not but have a prejudicial effect upon it and its estuary. He thought, therefore, that all these objectionable results might have been avoided and the opposite advantages been attained, if the head of the channel between Diglis and Clevelode, had been originally deepened to such an extent as to lessen the slope of the channel, and thus to retain the water in it during the times of drought; while the deeper channel which the river would have thus afforded for the passage of the heavy floods, would have allowed their full and continuous energy to have been subservient to the maintenance and deepening of the bed of the river.

In the absence of plans and sections, and of hydraulic observations of the present condition of the river and its estuary, it would be impossible for him to state, how far the conclusion at which he

had then arrived, was borne out by the facts of the case. A careful survey of the physical condition of the river had been made by the late Admiral Beechey, prior to the erection of the weir, and if similar information existed of the present state of the river, it would have been instructive to have compared the results which had actually ensued, with the opinions expressed by the different Engineers who had previously discussed the question.

Mr. PARKES had paid some attention to this subject, at the time this weir and lock were in contemplation, and he confessed he had arrived at the conclusion previously expressed in the discussion, that the portion of the river between Worcester and Tewkesbury ought to have been differently treated. He thought, however, it was not very important, whether the weir was placed there, or not; but the assertion, that the river could not have been improved by the more natural system of regulating its width and depth, should not be allowed to pass without protest. The Severn, between Worcester and Tewkesbury, consisted of a series of deep reaches, which lay diagonally along the valley of the Severn. The river at the ends of these deep reaches struck upon the sides of the hills on either side, and there, coming upon hard ground, the channel of the river was shallow, and the stream ran with considerable velocity. He believed, that almost the whole of the fall between Tewkesbury and Worcester occurred in these shallow places, against the sides of the valley; and at the time that Parliament decided against a weir below Diglis, near Worcester, an attempt was made to deepen these places, so as to give the depth of 6 feet, which was assumed to be required. He thought that operation had not been performed in such a manner as was likely to be successful; for it appeared, that although the channel of the river at the summer level was increased, yet nothing had been done to diminish it in those parts of the section which were only covered during floods, when the channel was subject to the greatest scour. The consequence was, that the places which had been excavated, were filled up when the floods came down, and when the sectional area was larger than before the improvements were effected. But there was another operation which ought to have been carried on, simultaneously with enlarging the channel in the shoal parts. The deep-water reaches between these natural weirs had no fall whatever; the channel, therefore, ought to have been contracted so as to increase the inclination and to raise the surface of the water, at the lower end of the shoal next above. There might then have been a less inclination over the shoals, than previously existed, without the level of the water at the head of the shoals being lowered. The same process might have been carried on in all the reaches, so as to raise the water

1 foot, or 2 feet at the bottom of the next shoal, which might then have been deepened, without lowering the water at the head. By these means, the level of the surface of the water would have been maintained at Worcester. The depression of the water at this place, was thus owing to the deepenings having been executed without a corresponding raising of the parts which were unnecessarily deep: had the river been regulated instead of merely deepened, the failure would not have occurred. With regard to the works at, and above Worcester,—he had not seen those at Tewkesbury,—he could bear his testimony to their good effects, in improving the navigation and passing off the floods very rapidly. Complaints had even been made, that the floods were carried off too quickly, so that there was not sufficient water for the navigation, in the parts of the river above the influence of the weirs. That, however, was not a fault, as the object of the works was to carry off the floods quickly, and that result had, certainly, been attained.

Mr. M. SCOTT was anxious to hear from the Author, the principle on which he was led to adopt that peculiar curve for the profile of the weir.

Mr. HAWKSLEY was acquainted with the River Severn, and having lived for many years upon the banks of the Trent, where these weirs were first introduced by Mr. Jessopp, so far back as the year 1780, he had ample opportunity of observing their effects. Oblique weirs were not, usually, of any great benefit, but under ordinary conditions, they delivered the water with a lower depth over the top than straight weirs, and so far, they might be said to be serviceable. That was an effect, however, which it was not always desirable to obtain, and in these cases, an oblique weir was less beneficial than a straight one. The change effected in the tidal action by placing a weir across a river, was, in general, detrimental; not seriously so, perhaps, in the lower part of the river, but only in the part near the weir itself. In the case of the Severn, he did not think any great evil had resulted, or was likely to result, from the weir, because the floods were so great, that they had a scouring effect quite equal to the daily tidal action, which the weir would interrupt. There was a point, however, in every river, and there was one in the Severn, where the effect of a flood became exactly equal to that of the ordinary tidal action. It would occur at a certain distance below the weir, and at that particular point a certain amount of deposit would take place, which would require to be removed by dredging. That point, in the case of the Severn, would not be within 10 miles, or 12 miles of the weir. But there was another circumstance to which attention must be drawn; the liability of the channel above the weir to become gradually filled up, in rivers with small slope, such as the Severn.

Whether that effect would result in the upper part of the Severn, must depend upon the floods producing sufficient scour to cleanse out the basins formed by the weir; if they did not, the channel would, infallibly, be gradually filled up. Another fact had escaped observation, the effect of a weir upon the adjacent lands. It would not be contested, that any obstruction placed in a river must have the effect of elevating the surface, in a greater, or less degree, according to circumstances. In the case of a weir like the one under consideration, the increased elevation above the top of the weir during floods, would, perhaps, not be considerable; but nevertheless, low-lying lands to which the water did not, formerly, reach in a flood of given magnitude, would be liable to be submerged, after the weir was established. The Severn was, fortunately, a river in which a weir of that kind could be placed without much injury, for the banks were, generally, high and precipitous. Although some portion of the valley was flooded during heavy rains, yet, considering the extent of country drained by the river, it was not a large proportion of the whole. In the Trent, in consequence of the elevation of the weirs, the water had overflowed the land, covering it for many miles, and to the extent of many thousand acres at a time. Whether in this case, the advantages accruing to the navigation did not counterbalance the damage to which the country had been liable, he did not pretend to say; that was a question which Parliament must determine. But as he had heard it asserted, that works of this kind did not have the effect of elevating the surface of the water, against this doctrine, he protested, both in a practical and in a scientific point of view.

Mr. HEMANS remarked, that a number of weirs erected on the Shannon, had had the effect of raising the general level of the water in floods.

Mr. J. COCKBURN CURTIS stated, that the oblique weirs had been known in India and Spain for a long period; it was probable there were some in the former country, which were as much as two thousand years old. Under certain conditions, weirs of this kind possessed great advantages over those placed at right angles to the direction of the stream. He did not think it a necessary consequence, that an oblique weir would have the effect of raising the bed of the river on which it was placed; on the contrary, there was every reason to suppose, that some of the rivers on which they had been used, had not materially altered the level of their beds, for centuries.

Mr. E. L. WILLIAMS observed, that great importance had been attached to the construction of weirs in a tidal river, in such a position as not to interfere with the tidal flow; that the lowest weir should, therefore, be above the point reached by the tide.

But the Severn was peculiar in many of its features, and one of those peculiarities was its tidal flow. High water of spring tides in the Severn, extended forty-five miles beyond high water of neap tides. It became, therefore, a question of considerable importance to decide upon the position of the lowest weir, and how far it was desirable to carry out the canalising principle which had been adopted; and that question was discussed and tenaciously fought, in the Parliamentary Committees. The argument against the weirs was, that they would abstract from the flow of spring tides, a certain volume of water necessary for the scouring of the lower channel. This was met by the consideration, that at Tewkesbury, there was no tide at neaps, but only at springs, and as there was a great depth of water in the river above Worcester, by canalising at Tewkesbury, the flow of the floods would be facilitated, and by their more rapid discharge, a greater scouring of the lower channel would be produced. The character of the river altered at Tewkesbury so completely, that nature pointed out that spot, as the proper position for the lowest weir; for above Tewkesbury almost all the shoals were hard marl, or gravel which resisted the scouring of the floods, whereas the whole of the shoals below that place, with one, or two exceptions, were shoals of deposit, caused by the too great sectional area of the channel, in times of flood. Nothing, therefore, had been abstracted from the tidal flow of the river, whilst the passage of the flood water had been facilitated, through the district canalised. The water was discharged with so much rapidity, that the energy of the scour in the lower district, was more efficient than it was before the construction of these weirs. He was once asked by a Past-President of the Institution, who was standing near one of the Severn weirs, how the passage of the floods could be facilitated by construction, which had required the sinking of 7,000 tons of stone in the channel of the river. He admitted the result, but he could not account for it. The theory which Mr. Williams proposed was, that previous to the erection of the weirs, when the first fresh came down the channel of the river, it met with a series of impediments in the shape of fords, extending from 100 yards to 1,000 yards in length, which, of themselves, acted as natural dams in the river, and offered sufficient resistance to materially reduce the velocity; whereas a fresh coming into the district which had been brought under the regulation of these works, found, instead of a river comparatively empty of water, a series of deep quiescent pools, having, as had been correctly stated, greater hydraulic mean depth. The result of every year's experience had established the fact, that the discharge of the floods was due to the increased hydraulic mean depth; they ran faster through water,

[1859-60. N.S.]

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than over gravel. The flood, instead of having to drag its way over gravel shoals, now came into deep water; the whole mass was put in motion, and the discharge was so rapid, at the large weirs, that no obstruction was offered to the current above. An idea, held by many opponents to the original plan of improvement was, that the construction of the weirs would facilitate the deposit of silt above them, and that the weirs themselves would silt up to the level of the upper cills, thus forming an inclined plane above, and gradually causing the river to fill up. But experience had proved the contrary. On comparing the original section of the river, made in 1842, before the weirs were erected, with a series of transverse soundings taken at 16 feet from the head of the weir, it was found, that the depth of the channel had been actually increased 3 feet to 5 feet. On taking longitudinal soundings, one mile and a half up the river, it was again found, that the depth had been increased the whole distance, 3 feet to 5 feet. The same effect had been produced in the whole of the pools between the weirs; the channel, indeed, was deepened to such an extent, that the banks which had stood for many years previously, had, in some places, fallen in, from the increased energy of the current having scoured out the bottom. Except where there was a sudden bend, there was seldom a deposit above the weirs. Below the lowest weir, the mode of improvement adopted, was to equalise the sectional area; and the consequence had been, that the increased velocity of the floods had tended to maintain the navigable depth more effectively, than before the construction of the weirs. The probable flooding of the low lands was a point insisted upon, before the Committee of 1842, and it was also the reason why the lowest weir was, at that time, abandoned. The Committee, having given power to construct four of the weirs, resolved to see the effect produced by them, before they would consent to place a long flat valley, 16 miles in length, under the same régime. But the effect above was so satisfactory to the landowners, that they willingly gave evidence in favour of the prosecution of the plan; for the floods were not only discharged with greater velocity, but they did not rise so high upon the land. It had been objected, that a weir could not be placed in the river without raising the surface of the water. Now one of the objects aimed at, was precisely to raise the low-water surface for the purposes of the navigation; another was, to construct the weir in such a manner as to facilitate the passage of the floods, so that whilst it improved the navigation, it should not prejudice the drainage of the low lands. To effect this, it was necessary, in the first place, to ascertain, by cross sections of the valley in all directions, the relative level of the low lands, as compared with the height of the water when penned-up, as proposed. When it

was found, that the height would not prejudice the drainage, the landowners were satisfied. It was now no longer matter of theory, but of fact, that the lands had been improved, and there was not a proprietor in Worcester, or Tewkesbury, who would not acknowledge it. It had also been objected, that the proper mode of improvement had not been adopted below Diglis, that instead of establishing a weir, the sectional area in the long reaches should have been equalised, the impediments being confined to some obstructions of marl, or gravel, in the bed of the river. Now, he thought Nature was the best instructor, and that those deep reaches taught Engineers the direction in which they should proceed. For instance, there was one reach of the Severn above Tewkesbury, 4 miles in length, with a section as defined as that of any canal, not varying for each half-mile, more than a few feet. Three of the cross sections, indeed, were so close, that they would scarcely be imagined to be those of a river which carried such floods as the Severn; they did not vary more than 13 feet, or 14 feet in 1,300 feet, or 1,400 feet of section. Nature, then, having pointed out the width at which a sufficient navigable depth could be maintained, instead of interfering with that section, it was taken as the best guide for the efficient drainage of the flood water, and the maintenance of the navigation. The sectional area of the channel was increased, thus creating an hydraulic mean depth equivalent to that of the reach alluded to, and a weir was constructed to discharge the water as fast as it came down. He was satisfied that was the right course, and the landowners and others who were originally opposed to placing a weir near Tewkesbury, now thought it the best part of the whole work. The Earl of Coventry and Sir Edmund Lechmere, both being landowners in that district, had confessed, that their land was considerably improved by these works. He had been asked why a parabolic curve was adopted at the back of the weir, and why it was supposed to discharge the water more rapidly than any other form. He was led to adopt it, in consequence of experiments made in a trough of water, with floats placed at different distances from the bottom. Observing that with an oblique weir, the floats, as they simultaneously approached it, all described a parabola, that curve was given to one of the upper weirs, which was then found to discharge, at a head of 9 inches, as much water as a weir four miles above, of the same length, but with an inclined backing, at a head of 12 inches. In the one case, there was an upheaving of the water as it approached the edge of the cill; in the other case, the flow was uniform. He would give a striking illustration, which admitted of no dispute, that a weir could be placed in a river, without interfering in any way with the action of the under-current below the cill. Nearly

twenty years previous to the construction of the Severn works, a vessel had sunk, about four miles above Worcester, in a deep part of the navigable channel; but as it offered no obstruction to the navigation, it had been allowed to remain, and had become half filled with sand. The first fresh which came down the river, after the erection of the Severn weirs, removed the old vessel and deposited it upon an island, a little above the next weir. During a trip up the river, he related the circumstance to Sir William Cubitt, to the late Mr. Brunel, and to Mr. George Edwards, who had superintended the dredging operations for Messrs. Grissell and Peto. Mr. Edwards then mentioned, that he had deposited, whilst the works were in progress, about 100 tons, or 150 tons of gravel boulders in the channel, at Holt, between the entrance of the navigable cut approaching the lock, and the weir. The man who deposited these boulders happened to be employed, when this conversation occurred, as assistant lock-keeper at Holt, and on the party arriving there, he was instructed to sound the channel at the place of deposit, when it was found to be perfectly clear, all the boulders having been carried away. That was within 100 yards of the head of the weir, and similar results had taken place throughout the whole of the district.

He did not pretend, that there was any great novelty in the works, but they had been very successful for the purposes for which they had been designed.

Mr. BIDDER,—President,—said, the Paper had excited hopes, which had not been allayed by the remarks addressed to the Meeting by the Author. He could not believe, that all the facts connected with the works of the Severn had been detailed, because, so far as he understood the results, they had been produced without adequate causes. It, certainly, was difficult to perceive how the placing of a weir in a river, could have the effect, without other auxiliary works, of relieving the adjoining lands from the flood water. The reasoning by which that view was attempted to be supported was, he thought, not quite correct. It had been said, that by introducing a weir, the water above it was deepened, and that when the floods came down, they met with deep, instead of shallow water, and were carried off, consequently, with greater velocity. But if the floods came down a river denuded of water, they would first have to raise the water to the height which the weir had created, before it could discharge itself down the river. Neither could he understand, that the erection of a weir should increase the scouring power of a river, nor that a particular parabolic curve at the back should augment the flow over it, 9 inches to 12 inches, which was equal to 50, or 60 per cent. The River Severn was one with which he was not intimately acquainted, having only once passed up it in a row-boat; but

it was a river which, undoubtedly, presented natural phenomena of an interesting character, which deserved to be carefully recorded. He regretted, therefore, that a Paper of so much interest, did not convey sufficient information to be able to fully appreciate the value of the works which had been so ably carried out. He hoped the Author would, on some future occasion, present that information to the Institution, in a more ample and detailed form.

May 1, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

The following Candidates were balloted for and duly elected :—
WILLIAM CUDWORTH, FRANCIS FOX, and LEWIS WAINWRIGHT
SAMUEL, as Members; HENRY RUSSELL SHAW and GEORGE BARNARD
TOWNSEND, as Associates.

No. 1,024.—“ On Coal-Burning and Feedwater Heating in Locomotive Engines.” By DANIEL KINNEAR CLARK, Assoc. Inst. C.E.

THE general abandonment of coke as fuel in railway engines, and the use of the combustible in its normal condition as coal, will, probably, in the course of a few years, become an accomplished fact, as the change is now rapidly taking place. The immediate motive for it, is the necessity for reducing the expenditure on railways, in order to increase the dividends. As the cost of coke is, necessarily, greater than that of coal, and inasmuch as, with proper management, a pound of coal does as much duty as a pound of coke,—occasionally, rather more,—the whole difference in cost of the two descriptions of fuel is economised. But there is the nuisance of smoke to contend with, on the railways using coal, when the coal is not completely burned and the smoke is not prevented, particularly in running without using the blast, and in waiting at stations. The term ‘smoke nuisance’ has only a relative signification dependent on the views and tastes of railway travellers, and of the public generally; but the term ‘smoke prevention’ has an absolute meaning, and those systems by which smoke is entirely prevented, must be considered the best.

The chemistry of the combustion of coal is now so generally understood, that it will be unnecessary to repeat it in this Paper, of which the chief object is to discuss the existing practice of coal-burning on railways; the Author, therefore, will proceed to a consideration of the physical conditions of the complete combustion of coal. These are; first, that the introduction of air should be in sufficient quantity, and suitably distributed amongst the solid and gaseous portions of the fuel in the furnace, altogether, or partially through the grate, and partially above the fuel directly to the

¹ The discussion upon this Paper extended over portions of two evenings, but an abstract of the whole is given consecutively.

gases; secondly, that the temperature should be maintained sufficiently high within the furnace, so that it should not be lowered by external causes, during the combustion of the hydro-carbon gases, in order to effect the union of the element of carbon with its full proportion of oxygen; and thirdly, that the combustible gases should be thoroughly mixed with their supply of air.

QUALIFICATIONS OF THE ORDINARY LOCOMOTIVE BOILER FOR COMBUSTION OF COAL.—The thorough mixture of the gaseous elements with air, within the fire box, is the condition, the fulfilment of which constitutes the main difficulty in dealing with the locomotive boiler, because in burning coal, the space, or capacity must be open, in order that the gases in progress of combustion may freely intermingle; whereas, the necessary extension of surface for the absorption of heat, by means of the ordinary flue tubes, cuts up a large portion of the flue capacity of the boiler, divides the burning gases into isolated streamlets, and thus effectually suspends the continuous intermixture, instrumental towards the completion of combustion. The gases in front of the tube plate enter the tubes, just as they are located; where the mixture is rightly adjusted, combustion may proceed within the tubes, and be there completed; where there is too much, or too little air, combustion does not proceed. In practice, the latter is usually found to be the case; and, accordingly, when the columns of gas and air, released from confinement within the tubes, pass into the smoke box, and again commingle, combustion is frequently resumed there, and it may continue till the gases are disposed of at the top of the chimney. This is the cause of the blistering of smoke boxes and chimneys of ordinary engines, when coal is exclusively burnt; and they become, occasionally, red hot. Such results are not now, however, so common as they have been; for whilst the earlier engines passed off the gases at a high spontaneously-igniting temperature, through their short boilers, the more recently-built engines abstract a greater quantity of heat from the gases, on their passage through the boiler, and the gases are, of course, discharged at lower temperatures.

The unfavourable action of the flue tubes in suspending combustion, is commonly ascribed to their cooling the gases below the minimum temperature requisite for the maintenance of combustion, and thereby, cutting short the process altogether. It is not so; because either the rate of conduction of heat through the substance of the tube, or the rate of absorption of the heat by the water, would be totally insufficient to account for the presumed fall of temperature. Carburetted hydrogen, the combustion of which is the desideratum, burns at 800° Fahrenheit; and it is known, that in engines doing heavy duty, the temperature of the gases, after

having traversed the entire length of the tubes, is frequently above 800° in the smoke box. It is, in short, an ascertained fact, that the gases spontaneously re-ignite in the smoke box, where they are free to continue mixing with the air drawn either into that compartment with them, or through leaking joints. The cooling within the tubes is far from being so rapid as has been assumed. The extended length of flue, or 'run' available for the mixing and combustion of gases, pertaining to ordinary stationary boilers, contrasts forcibly with the limited fire box of the ordinary locomotive boiler.

The utility of the long run in stationary boilers is clearly established, by the observation of the lengthened flame, indicating lengthened, or deferred combustion in the flues. Mr. C. W. Williams, (Assoc. Inst. C.E.,) found, in one instance, that the flame extended to a length of nearly 30 feet in the flues, before complete combustion was effected. The locomotive boiler, then, as it stood, though excellently suited for the combustion of coke, was, according to ordinary practice, inferiorly constituted for effectually consuming coal, having neither time, space, nor run to facilitate the process. The defects of the multitubular-flue system had, in this respect, been experienced in the practice of marine boilers, in which the prevention of smoke appears to have been as nearly impracticable, as it has been in locomotives. There is, however, one condition favourable for smoke prevention in the locomotive; the peculiar intense-ness of the combustion. Cornish stationary boilers, with flues indefinitely drawn out, burn coal at the low rate of 4 lbs. per square foot of grate per hour; common stationary boilers burn from 12 lbs. to 20 lbs. per foot per hour; and locomotive boilers consume 50 lbs. to 100 lbs. per hour. The more intense the combustion, the less is the required run, notwithstanding the increased speed of the draught, as the process of combustion is accelerated in a still greater ratio.

The comparatively superior strength of the draught in locomotive boilers is, however, directly beneficial in its power of drawing more, or less air through the grate, available for the combustion of the gases. A large quantity of free air is thus drawn in, mixed with the gases, and is quickly united with them; and there is no doubt, that in this way, by the introduction of air entirely through the grate, the combustion of coal is sometimes effected in the locomotive boiler, with very little smoke. But the result is variable, because the means are variable; and though an engine, whilst working with full steam, a powerful blast and a thin fire, may thus consummate the process of combustion, those conditions are wanting when the fire is thick, or the duty is light, as the draught is powerless to draw through the mass of the fuel, a sufficient quantity of free air. Still less favourable are the conditions, when

the steam is shut off after heavy duty, the natural draught of the boiler being totally insufficient for the purpose. Thus it is, that the discharge of smoke is greatest, when the engine is doing the least duty, and when the blast is off; and that, whereas in other classes of boilers, there may be a long run for the gases undergoing combustion, with a comparatively uniform duty and a uniform draught, in the locomotive boiler, the run is reduced to a minimum, and the duty and the draught are extremely variable, being both greater and less than those of any other class of boiler. It has been found, accordingly, that an extension of the grate surface is attended with a diminution of smoke in the combustion of coal, as the same mass of fuel spread over a larger area, is less in depth, and a larger quantity of free air may be drawn through it. The most efficient coal-burning locomotives, under circumstances otherwise the same, are those which have the largest grates; and these are found, practically, to require a very small proportion of air to be introduced above the fuel, whilst working with the blast.

Still further to assist in the prevention of smoke, in common locomotive boilers, an auxiliary jet of steam has been thrown into the chimney, in order to maintain the draught through the fire, when the steam is shut off from the engine. The steam jet is a very serviceable adjunct; and no doubt, it materially aids in the prevention of smoke. It is, indeed, an indispensable element in all existing plans for effectually burning coal in locomotives; and much of their superiority in smoke prevention, though it may be ascribed to other and more apparent characteristics, is based upon the action of the auxiliary jet.

With the limited fire box of a common locomotive boiler to deal with, enginemen have had recourse to various ways of treating the fire, in order to diminish the nuisance of smoke. They have relied, chiefly, on the instrumentality of the ash pan, the dampers, and the fire door, for carrying out a system of careful firing. They have endeavoured to prevent the formation of smoke, by controlling the admission of air through the grate, and adjusting it precisely to the requirements of the fuel; by similarly manœuvring the fire door, for the admission of air above the fuel; by stoking with large pieces of coal and deep fires, for heavy duty, and smaller coal with shallow fires, for lighter duty; by firing more frequently to lighten the duty and at all times, by keeping the bars covered with fuel, to prevent excessive local draughts through the grate. That much may be effected by these means, there is no doubt. It is well understood, that the nuisance of smoke on entering, or waiting at stations, may be very much, or in some cases, even altogether, subdued, by thoroughly closing the ash pan, and by opening wide the fire door.

Low-pitched doorways at the level of the fuel, introduce fresh

air to greater advantage than those which are set high, as the air through the low door mixes more quickly and more freely with the gases, as they rise. It was, accordingly, found advantageous to adopt the practice of charging the fresh coal, chiefly under the fire door upon the hind part of the grate, and subsequently, when relieved of its gaseous element, pushing it forward towards the tubes, thus making way for a succeeding charge behind. Such have been the usual modes of treating coal, as fuel, in locomotive boilers of the normal type, in their normal condition. But the elaborations of practice in ordinary boilers have been to a considerable extent, superseded, within the last year, or two years, by the methods recently introduced of admitting air above the fuel, near its surface and amongst the combustible gases. These methods may be classed as acting; first, by currents of air introduced through tubular, or other openings in the sides of the fire box, uniformly distributed over the surface of the fuel; and secondly, by the deflection of a body of air introduced through the doorway, upon and over the surface of the fuel. A different principle of operation has also been introduced, by constructing large and spacious fire boxes, with large grates and long runs.

The three leading classes of coal-burning boilers, thus indicated, are variously represented, in chronological order, by the diagrams; (Plate 6). First, the adaptation of large extended fire boxes and combustion chambers, by Mr. M'Connell, (M. Inst. C.E.,) in 1853; (Fig. 1); by Mr. Beattie, in 1855; (Fig. 2); and by Mr. Cudworth, in 1857; (Fig. 3).

In Mr. M'Connell's plan, (Fig. 1,) there is a large grate, a long run, and air tubes through the front and sides; and the system, though heavy on fuel, provides the greatest practicable area of fire grate, in order that the coal may burn with moderate intensity and free from clinker, and that plenty of air may pass into the fire box, through the grate, to consume the gases. The combustion is assisted by the introduction of air through the front and sides of the fire box; and a large capacity and length of run is provided, by adding a combustion chamber, which is, simply, an extension of the fire box into the barrel of the boiler. To provide for alternate firing, the fire box is divided longitudinally into two compartments. This plan is, with good management, tolerably effective in preventing smoke.

In Mr. Beattie's boiler, (Fig. 2,) as in Mr. M'Connell's, the grate is large, and the run is divided and extended, by the addition of a combustion chamber projected into the barrel, with brick arches and tiles. The fire box is divided transversely into two compartments, by an inclined water-space diaphragm. Both compartments are arched over with fire tiles, at narrow intervals; the combustion chamber also is stocked with perforated bricks,

or curved tiles, placed at some distance clear of the tubes. The fire tiles receive and retain a portion of the heat from the passing gases, when the fuel is incandescent and smokeless, and restore it to the smoke passing from fresh coal; thus acting as equalisers of temperature. The tiles, also, break up and mix the gases and air; and partly by mixing, and, it is supposed, partly by heating, they promote combustion. The first furnace is the most actively worked; the second is chiefly designed to carry incandescent fuel; and by means of independent ash pans and dampers, the admission of air through the grate may be adjusted for each furnace. Air is also admitted through the doors by small apertures, and similarly, through the back of the fire box, by slides. The gases from the first and principal furnace, pass through the tile bridge, are deflected by the hanging transverse diaphragm towards the hot fire in the second furnace, and then pass through the second tile bridge and the mass of tiles in the combustion chambers, before they enter the tubes. The object of this invention, which has already been submitted to the Institution,¹ is to equalise the temperature of the gases, to prepare the colder gases for ignition, under all fluctuations of temperature, and also to perfect the mixture of the gases and air, in order to effect their entire combustion. An auxiliary steam jet is opened into the chimney, when the blast is off, to stimulate the draught, and to prevent smoke. This plan works very well.

Mr. Cudworth's plan, (Fig. 3,) combines an inclined grate with a long run. The incandescent fuel slides forward, and the fresh coal is charged near the doorway. The fire box is divided, by a longitudinal diaphragm, into two compartments, making two furnaces, which unite in front of the tube plate. With the aid of an auxiliary jet in the chimney, Mr. Cudworth's plan may be made tolerably effectual in preventing smoke.

But it was necessary to adapt the existing engines, as they were, for burning coal without smoke, by simple means, and independently of extensive structural alterations. This had been attempted by Messrs. Gray and Chanter, in 1837, and again in 1839. They divided the fire box into two compartments, one for coal, and the other for coke; they also admitted air in streams, through tubes in the walls of the fire box, and they were the first who applied a steam jet in the chimney, when the blast was off, to maintain a draught. Mr. Dewrance, in 1845, divided the fire box into two parts, the furnace and combustion chamber. Messrs. Dubs and Douglas, in 1856, constructed a deflecting mid-feather, proceeding from the back of the fire box, towards the tube plate, to throw down the smoke over the incan-

¹ *Vide Minutes of Proceedings Inst. C.E., vol. xvi., p. 41.*

descent fuel ; and Messrs. Evans and Dubs, in 1857, added to this, a moveable inclined grate, capable of being raised, or lowered, for the management of the fire. After various attempts, Mr. Yarrow, in 1857, projected an arch of brick from the tube plate towards the fire door, and admitted air through tubes in the tube plate under the arch, and through the fire door, to mix with the smoke. He has, subsequently, superseded the air tubes, by upright air bars near the tube plate ; (Fig. 4.) In the same year Mr. Jenkins, admitting air through numerous air tubes in the tube plate, applied a curved partition across the fire box, to throw forward the air, distributing it through numerous small holes at the upper part ; (Fig. 5.) He regulates the supply of air by a damper, and he has, more recently, added a row of air tubes under the fire door.

Towards the end of 1857, the Author, believing that a simple means of increasing the supply of air, and forcibly distributing and mixing it with the smoke, in a common open fire box, independently of internal appliances, was a desideratum, devised his system of steam-inducted air currents, in which, in its simplest form, air currents are admitted just above the fuel, by tubes, or otherwise, through the sides of the fire box, and are forcibly accelerated, by means of jets of steam, directed from the outside, through the openings, into and across the fire box ; the steam nozzles operating through the air tubes, as the blast does in the chimney. In December, 1857, the Author applied his system to one side of a small stationary locomotive boiler, at the railway foundry, New Cross. There were two air tubes, and a jet of steam to each tube, from small nozzles on the outside, $\frac{1}{8}$ th of an inch in diameter, to increase the quantity and velocity of the air introduced. This simple apparatus was for several months in operation, and the dark smoke which was discharged when it was not in action, was entirely prevented by its use. In January, 1858, tank engine No. 12, on the North London Railway was fitted with four air tubes, $1\frac{1}{2}$ inch in diameter, through one side only, the other side being inaccessible. With the assistance of jets of steam, of $\frac{1}{8}$ th of an inch, smoke was completely prevented, when the surface of the fuel was below the level of the air tubes. In April, 1858, passenger engine No. 64, on the Eastern Counties Railway, was fitted with a row of air tubes on each side, four on one side, and three on the other, with steam nozzles, one to each air tube. With this application, the engine worked the main line trains for eight, or nine months, and the smoke was, in general, effectually prevented. In January, 1859, passenger-engine No. 9, on the South Eastern Railway, was fitted and put to work, with two rows of air tubes through the front and back walls of the fire box, seven in each row, or fourteen in all, with induction nozzles to every tube ; (Fig. 6.) The per-

formance of this engine has been quite satisfactory, and it has continued at work, thus fitted, up to the present time. On the Great North of Scotland Railway, engine No. 5, was fitted on the Author's system, with front and back air tubes, under the direction of Mr. Cowan, the locomotive superintendent, and was started in March, 1859. The results have been, in all respects, satisfactory, and the same system has been applied, by the superintendent, to the entire locomotive stock of that railway. Fig. 7 shows its application to the goods engines, now in course of construction by Messrs. Robert Stephenson and Co., for the same line. The Author's system has also been adopted on the Londonderry and Enniskillen Railway. In 1859, it was applied to some of the tank engines on the North London Railway, the back air tubes being placed high, to clear the water tank, and the grate being inclined to correspond. It was, occasionally, found, that the back jets, placed so high above the front ones, drew the smoke into the flue tubes; and that the front ones, impinging directly upon the fuel, did not clear the smoke so effectually as when the back jets were afterwards removed, and the grate levelled, so that inducted currents passed above the fuel. In 1859, one of the old passenger engines, No. 89, of the London and Brighton Railway, was fitted with the Author's apparatus, with a row of air tubes, and induction jets in the front only. Accidental circumstances rendered impracticable, the insertion of air tubes in the back. Smoke was effectually prevented, except when heavy charges of coal were delivered at once.

On the Great Western Railway air is admitted into the fire box, over the fuel, by numerous air tubes, regulated by slides, through the front, sides, and back. On the Oxford, Worcester, and Wolverhampton Railway, a system was introduced, by Mr. Edward Wilson, in 1858, in which air is conducted from the front of the smoke box, through several of the lower flue tubes, into the fire box; (Fig. 8). On the northern division of the London and North Western Railway, Mr. Ramsbottom introduced air under a brick arch, by two openings 7 inches square, through the front of the fire box; (Fig. 9).

With respect to the class of contrivances, acting by deflection, the initiative appears to have been taken, on the Birkenhead Railway, by Mr. Douglas, who, in January, 1858, applied a deflecting plate, fixed to the inner side of the door, through openings in which air was admitted, whence the air passed, and was deflected towards the fuel. In June, 1858, on the same line, a moveable deflecting plate, and an under-hung door, with a sector and notches to regulate the opening for air, was applied, and, more recently, a plain inverted scoop, or shovel; (Fig. 10). In July, 1858, on the East Lancashire Railway, Messrs. Lees and Jacques fixed the de-

flector to the door which was under-hung, and was provided with a valve, the opening of which was regulated by a sector. They built, in addition, a narrow brick arch against the tube plate, so that the smoke and air might, by their combined action, be better intermixed; (Fig. 11). In December, 1858, Mr. Sinclair, (M. Inst. C.E.,) on the Eastern Counties Railway, applied a baffle plate inside the fire box, hung over the doorway, and an under-hung door regulated by notches, with two steam roses, one on each side of the fire box, to project steam downwards upon the fuel, when the blast was off; (Fig. 12). This system was introduced by Mr. Frodsham. In 1858-9, an inverted shovel, or scoop, similar to Mr. Douglas's, but longer, was rather extensively introduced, being placed in the doorway, and inclined towards the centre of the fire, in order to deliver the air directly on the top of the fuel.

It has not been attempted, in the foregoing notices, to describe all the plans that have been designed for the purpose of consuming coal without smoke in locomotives; the intention has been to show generally, the directions in which Engineers have looked for the means of insuring complete combustion. There is one feature, of considerable importance, which is common to all; the steam blow-pipe, or auxiliary jet, in the chimney. This is made use of, for the purpose of continuing the draught of the furnace, when the powerful artificial stimulus of the blast is, at intervals, suspended. The action of this smoke annihilator is well worthy of study; and it is a question of direct practical interest, to ascertain the reason of the blow pipe operating thus serviceably. Looking into an ordinary fire box, at rest, under the action of the blow pipe, smoke may be perceived in the fire box, wending its way into the tubes, which becomes totally invisible at the top of the chimney. This visible smoke is rendered invisible, either by absorption, or precipitation, or a little of each, and by the steam from the blow pipe; the jet of steam 'paints the smoke.' Imperfect though such a mode of banishing smoke may appear, it is, nevertheless, true, that every one of the coal-burning contrivances falls back to a greater, or less extent, upon the blower, for the means of consummating the extinction of smoke. The action of the steam blast in destroying visible smoke, while the engine is at work, is apparent to the most ordinary observer, inasmuch as the discharge from the chimney, which may be clear and colourless when the blast is on, may become densely brown, or black when the blast is off, and may only be mitigated by a discharge of steam from the blow pipe. That steam possesses a considerable power of precipitating the particles of smoke, may readily enough be observed over an ordinary domestic fire, by directing into the ascending smoke, the steam spouting from a kettle; it is found to banish the smoke in a greater,

or less degree, certainly not by any act of combustion, or other chemical process, but by simple physical action. And, it is in virtue of the same faculty of absorbing, or precipitating the particles of smoke, that certain processes for preventing it operate, by passing the smoke into intimate contact with water. It is found, similarly, that smoke from stationary chimneys is subdued, or mitigated by discharging the exhaust steam into the chimney. Of course, the action of exhaust steam, so directed, in accelerating the draught, and thus in another way diminishing smoke, is distinguishable from its action as a precipitant, or absorbent; but, in many cases, doubtless, the prevention, or reduction of smoke by the agency of steam in the chimney, is mainly due to its operation in the latter capacity.

It cannot properly, therefore, be assumed, that combustion is, in every case, complete, when smoke is prevented by the agency of steam in the chimney: whether the engine is running under the blast, or standing under the blow pipe. This is a fair subject for practical investigation: in the meantime, one conclusion of practical value may be drawn from the general experience of the smoke-preventing agency of steam, that observers should be on their guard not to be misled by appearances, and should not be diverted from the accomplishment of the main object, the thorough combustion of coal by the ample admission and the intimate mixture of air above the fuel, and amongst the smoke. It may be added, that where pure steam is discharged upon, or over the surface of the fuel, whilst it operates favourably in preventing, or reducing smoke, it also damps the fire, and retards combustion; such at least, is the tendency of this mode of using steam, so far as it has come within the Author's experience, in whatever way it is introduced. Consequently, modes of smoke prevention by the sole agency of pure steam, over, or upon the fuel, are usually attended by a difficulty in making steam, and a great expenditure of fuel.

The necessity for admitting a supply of air above the fuel, is understood and acknowledged by all, whether it is introduced through the grate, or otherwise. When the grate is very large, and a thin fire is maintained, the supply of air may be taken almost entirely through the grate. It is, however, indispensable to this mode of managing the fire, that the fuel should be carefully supplied, at short intervals, and in small quantities, uniformly distributed; in recognition of the principle, that the more nearly uniform the supply of fuel, the more so is the generation of combustible gases, and the more probable, accordingly, is the combustion of these gases, and the prevention of smoke. With inclined grates, of course, the fuel is only deposited near the entrance, and it finds its way down, by gravitation; and their greater ease of management, in this respect, notwithstanding the frequent stoking they require,

compared with ordinary level grates, mainly constitutes their superiority. With small, or moderate-sized level grates, a coal fire demands still greater care than with larger grates, where smoke is to be prevented by air from the grate. The firing must be still more frequent; and as the door, of course, is more frequently opened, another evil is incurred, the admission of a large proportion of cold air into the fire box, most of which passes off unburnt through the tubes, chilling the boiler, and checking the production of steam. Such evils are much reduced in magnitude, by the simple process of admitting fresh air for the combustion of the gases, otherwise than through the fuel, in greater, or less proportions, according as it may be utilised; and as this independent admission of air is a very simple process, there can be no reason for endeavouring to dispense with it.

It may be adopted as an axiom in locomotive practice, that an independent supply of air should be admitted above the fuel, for the purpose of burning the combustible gases; and that the air so admitted, must be thoroughly mixed with the gases. Length of run promotes the mixture, and in Mr. Beattie's boiler, the mixture is further advanced by the interposition of fire tiles. The brick arch in the common fire box operates beneficially in the same way, doubling the run, and promoting the mixture. The baffle plate, the hood, and the scoop operate reversely, and with considerable success, by inverting the draught of the air admitted through the doorway, and directing it over the surface of the fuel. The baffle plate is objectionable in inducing by its action, the suction of particles of coal through the flue tubes, and the burning of the smoke box, unless counteracted by an internal brick arch; and the system requires powerful blow pipes in the chimney. To insure the prompt and effective mixture of the air and gases, the air should be admitted and distributed at the surface of the fuel. But the ample admission and intimate intermixture of air with the gases above the fuel, must be effected in conjunction with a sufficiently high temperature; and this condition has led to the adoption of various modes of heating the air, or the gases, or both, prior to, or at the time of mixture. Mr. M'Connell suggested the use of an air chamber within the smoke box, in which air was designed to be heated, and led into the fire box through tubes. Mr. Wilson conducts air from the front, through the smoke box and the boiler, by a number of the flue tubes, and these also partially heat the air on its way to the fire box, letting down the steam in a proportionate degree. Mr. John Gray established a coke fire on a supplementary grate, as a reservoir of heat within the fire chambers; and Mr. Beattie embeds his reservoir in masses of firebrick. Others attempt to heat the air by conducting it partially over the surface of the fire-box shell. Expedients of this

class operate, in some degree, advantageously; but in the adoption of such supplementary contrivances, it appears to have been overlooked, that there is abundance of heat within the incandescent fuel itself, to supply the demand for temperature. Mr. C. W. Williams states, that the combustible gases themselves, taken at their origin, are, in general, sufficiently high in temperature to sustain combustion with cold air; and it is demonstrable in practice, that this may be simply and naturally effected, by the preliminary process of heating the air at, or near the surface of the fuel, whence the gases emerge at their highest temperature, and where the radiant heat from the fuel is at its maximum intensity. In boilers with internal furnaces, more than in others which have their furnaces surrounded with brick, this immediate process of mixture and combustion is needful, inasmuch as the absorption of heat by the surrounding surfaces, and the decrease of temperature in the atmosphere of the furnace, take place more promptly in the former, than in the latter.

The problem of the direct and efficient combustion of coal, in the ordinary locomotive boiler, appears to resolve itself into the immediate and thorough intermixture of a plentiful but regulated supply of air, with the ascending smoke, or combustible gases at, or near to the surface of the fuel. Practically, it is found necessary, for this object, to operate from both the front and the back of the fire box; admitting air, or using arches, deflectors, or bafflers, or doing both, as has already been described. But these contrivances, which deal with air in bulk, though generally effective in preventing smoke, are usually attended by the escape of a considerable quantity of unconsumed air through the flue tubes, and a difficulty, in many cases, in keeping up the steam at high speeds. The various forms of door-way deflectors are also objectionable, as previously stated, in facilitating, by their mode of action, the suction of particles of coal through the tubes, and the burning of the smoke box, unless counteracted by an internal arch. The Author has found, by experience, that to burn smoke when the engine is running, it is sufficient, that the air should be admitted at, or near the surface of the fuel, by air tubes distributed over the width of the fire box in the front and the back, without the aid of internal arches, or deflectors. The draughts of air, through the front tubes particularly, are very strong when the engine runs ahead, and they carry the currents into the middle of the fire box, where they meet the counter-currents from the back, effect the mixture of the air and smoke, and prevent the suction of small coal through the tubes. But in all systems applied to ordinary fire boxes, operating by means of the draught available in a locomotive engine, aided, when the blast is off, by the steam jet in the chimney, range of power is wanting to overtake the extremes

of intense ignition and rapid generation of smoke-making gases, immediately after the steam is shut off, or when fresh fuel is added; and also to suit itself to the quiet state of the fire when the glow and excitement subside, as well as to all the varying conditions of a locomotive furnace. The means of extending the range, volume, and power of the air currents, and of adjusting them to the wants of the furnace, are supplied by the instrumentality of the jets of steam employed by the Author, as means of inducting and accelerating currents of air. The steam nozzles, with the air tubes towards which they are pointed, are like so many miniature blast pipes and chimneys, turned into the fire box; and they possess, relatively, the same power of urging and creating draught. By this method of steam induction, the air currents are delivered with such precision and velocity, as to sweep the whole surface of the fuel, and forcibly, to distribute the air amongst the gases. Of the virtue of forcible impingement in promoting and accelerating combustion, numerous examples occur in ordinary practice. It is in virtue of this principle, that the ordinary bellows accelerate ignition; and if the upper front of a fire place is closed with a sheet of paper, to direct the draught through the bars, upon the fuel and combustible gases, the air currents so deflected and forcibly directed upon them, consume the gases and prevent smoke. The contracted neck of the glass tube of a moderator lamp, adjusted so as to direct the ascending air within the tube, upon the upper part of the flame, increases its volume and brightness. In Mr. Beattie's boiler, the smoke-consuming process is, to a great extent, based on the same principle of forcible mingling of the air and gases amongst each other, as they roll against the fire tiles and diaphragms. By the Author's process, the entire operation is consummated within the four walls of an ordinary fire box, the inducted currents sweeping up the gases and forcibly intermixing with them.

In practice, it is only, occasionally, necessary to put the steam jets in action when the engine is at work, if the air openings are sufficiently numerous, as the action of the blast alone draws a large supply of air through them into the fire box. The time at which the full inducting power of the jets of steam is in demand, is immediately on the steam being shut off, when the engine is drawing up to a station. Then the heat in the fire box is fierce, and there is an extensive distillation of combustible gases, which are discharged as smoke from the chimney, unless met and consumed by the inducted currents above the fuel. The intenseness of the heat, of course, subsides rapidly, and the jets may be moderated as desired, and be continued in action till the engine is again in motion.

TAES.

Class.	Place and Route.	Observer, or Authority.	Date.
Passenger		Woods and Marshall	June and July, 1854
"	Le wind	Clark	March and April, 1856
"	calm. Heated per 191°.	"	" "
Special	and. Heated	"	" "

would be greater than that of coke.

again in motion.

The in-draught of air into the fire box may be regulated by the use of slides, or dampers over the air openings. But, by so limiting the number of air openings, and consequently, the supply of air, as to prevent any material excess of supply when the fire is in its ordinary condition, the dampers may, in practice, be dispensed with, without prejudice to the economy of fuel. And, inasmuch as, in ordinary locomotive working, the fire is unavoidably subject to extreme changes,—dull, for example, and evolving gas when the blast is on, or bright and intense, and profusely distilling combustible gases when the blast is off,—it is evident, that the air channels do not, and cannot, at all times, supply the requisite quantity of air for perfecting the combustion, with the requisite velocity of impinging action for accelerating it. The desideratum suggested by this deficiency is supplied, in a direct and effective manner, by the method of steam-inducted air currents.

In comparing the merits and performances of various methods of coal-burning without smoke, there is the difficulty of diversity of circumstances to contend with, as to weather, fuel, engine, and duty. The data contained in Table I. of performances, (facing page 558,) will, nevertheless, be useful for general comparison. The Table comprises the principal results obtained by the Author, of the performances under different systems of coal-burning, with comparative statements of the performances of the same, or similar engines burning coke. Of the three systems of extended fire boxes, by Mr. M'Connell, Mr. Beattie, and Mr. Cudworth, it appears, that with nearly equal gross weights, 102 tons to 116 tons of engine, tender, and train, and at nearly equal speeds, Mr. M'Connell's system consumes $35\frac{1}{2}$ lbs. of coal per mile, Mr. Beattie's 24 lbs., (the feedwater heater being shut off,) and Mr. Cudworth's 26 lbs.; or, per ton gross,—Mr. M'Connell's system consumes 0·31 lb., Mr. Beattie's 0·235 lb., and Mr. Cudworth's 0·225 lb. The evaporative powers rank in the same order; i. e., Mr. M'Connell's evaporates 5·9 lbs. of water per lb. of coal, Mr. Beattie's 8·31 lbs., and Mr. Cudworth's 8·6 lbs. The excellence of Mr. Beattie's and Mr. Cudworth's systems is to be ascribed, to the proximity of the radiant heating surface to the fuel and the flame, and in both systems, the steam is well kept up.

Comparing coke with coal on similar duty, whilst Mr. M'Connell uses one-half more coal than coke, Mr. Cudworth employs, upon the whole, rather less coal than coke, the general average being 27·3 lbs. of coke, against 25·8 lbs. of coking coal per mile, or $5\frac{1}{2}$ per cent. less coal than coke. But he has found, that coking coal ranks higher in evaporating power than other coals he has tried,—Lord Ward's coal and Ruabon coal,—of which the consumption would be greater than that of coke.

Of the systems of coal-burning engrafted on the common fire box, Mr. Yarrow's consumes 26·8 lbs. of Scotch coal against 22·1 lbs. of coke per mile, on the same duty, showing an excess of 4·7 lbs., or 21 per cent. more coal than coke. Mr. Jenkins uses 30·35 lbs. of coal against 32·43 lbs. of coke, or 6 per cent. less coal, on the Lancashire and Yorkshire Railway, under his own direction. When tried on the Brighton Railway, his system used 6·4 lbs., or about 23 per cent. more coal than coke. Mr. Lees's system, on the same line, used 5 lbs., or about 20 per cent. more coal; and the Author's system, on the same line, and on the same engine as that upon which Mr. Jenkins's plan was tried, used only 2·1 lbs., or about $7\frac{1}{2}$ per cent. more coal than coke.

On the Eastern Counties Line, the Author's system, working against a strong wind, used 11 per cent. more coal than coke, working in a calm; and on the North London Railway, the same weight of coal as of coke, was used, viz., 26 lbs. per mile.

The Author's system has been best applied on the South Eastern, the Great North of Scotland, and the Londonderry and Enniskillen Railways, for elsewhere, the air admitted was deficient in quantity; and it is to its performance on these lines, that he wishes to direct the attention of the Members. On the former line, 14·4 lbs. of coking coal was used against 16·28 lbs. of coke per mile in a similar engine, on the same duty, for twelve months, showing a consumption of $11\frac{1}{2}$ per cent. less coal than coke. On the Great North of Scotland Line, there has not been an opportunity of trying coke against coal for long periods, as coal has for several years been largely consumed, along with coke, on that line. But engine No. 5 was put upon trial for several weeks, burning English coke alone, against different kinds of Scotch coal. The trains, as well as the weather, were variable; but under similar circumstances, 9 per cent. less coal than coke was consumed, per ton of the gross moved weight of the train. At the same time, whilst $8\frac{3}{4}$ lbs. of water appeared to be evaporated per lb. of coke, the water raised per lb. of coal, averaged only 8 lbs. Thus with a lower evaporative duty, there was a higher working duty with coal than with coke. This may be ascribed, probably, to the great command of steam afforded by the use of coal, in these engines.

In comparing the coal-burning practice on different lines, the inferior strength of Scotch coal to that of English coke and coal is to be borne in mind. The frequency of the stoppages is another element, as every stoppage of a passenger train, under ordinary circumstances, occasions a delay of from three minutes to five minutes, as compared with a through-running train. Table II., (facing page 560,) contains a selection of cases for comparison from Table I., for passenger, and for goods traffic. It gives the average

TABLE II.—PER

Designers.	Railway.				Calculated Equivalent Speed, without stopping.
					Miles per Hour.
McConnell	London and North V	..	..	..	42.
Beattie .	London and South V	..	..	..	34.
Cudworth	South Eastern .	..	..	..	32.
Douglas.	Birkenhead . . .	..	..	..	29.
Yarrow .	Scottish North East	..	..	..	36.2
Connor .	Caledonian . . .	..	..	..	33.3
Frodsham	Eastern Counties.	..	..	..	29.
Clark .	Great North of Scotl	..	..	..	32.
Douglas.	Birkenhead . . .	..	..	..	..
Connor .	Caledonian . . .	..	..	..	..
Frodsham	Eastern Counties.	..	..	..	..
Clark .	Great North of Scotl	Passenger and Goods .			..
		Ditto ditto, six months,			..
		January, 1860.			..
		Goods only			..
AVERAGE					
Douglas.	Birkenhead . . .	..	..	..	..
Connor .	Caledonian . . .	..	..	..	..
Frodsham	Eastern Counties.	..	..	..	..
Clark .	Great North of Scotl	..	..	..	..

civil Engineers, Vol. xix., Session 1859-60.

number of vehicles drawn, and the gross weight of the engine, tender, and train; the average distance run between the stopping stations; the average speed including stoppages; the gross consumption of coal per train mile, and per ton gross per mile; also, in the last column, the equivalent speeds without stoppages, or such as would have been averaged had there been no stoppages, estimated in terms of an allowance, or deduction in time, of four minutes for each stoppage actually made by the passenger trains, except for the Birkenhead trains, with which, owing to the frequency of the stoppages, the actual delay is only three minutes per stoppage.

Of the systems based upon the ordinary fire box, those of Mr. Yarrow and of Mr. Douglas show the greatest consumption of fuel, and the Author's the lowest. Thus, the former, with 86 tons and 90 tons gross weight of passenger train, consumes 27 lbs. to 28½ lbs. per mile; whilst the latter with about 110 tons gross weight, consumes only 21 lbs. of coal, or one-fourth less. The systems of Mr. Frodsham and Mr. Connor occupy intermediate positions in the scale of consumption, which are more exactly defined by the rates of consumption per ton gross, as follows:—

By Mr. Douglas's system. 0·32 lb. of coal, per ton gross per mile.

„ Yarrow's	„	0·32 lb.	„	„	„
„ Connor's	„	0·26 lb.	„	„	„
„ Frodsham's	„	0·25 lb.	„	„	„
„ Clark's	„	0·19 lb.	„	„	„

The calculated equivalent speeds, are, no doubt, various, but the differences will not account for the variety in the consumption. The Author's system is shown to consume but five-eighths of the fuel used by Mr. Douglas and Mr. Yarrow, and only three-fourths of that used by Mr. Connor and Mr. Frodsham. It is also shown to be more economical in fuel per ton gross, than either of the systems with elongated fire boxes and combustion chambers.

In comparing the Author's system with the others, in taking goods trains, it is also found to consume less fuel per ton gross, where the trains are of equal weight. As, on the Great North of Scotland Railway, the goods engines, until lately, worked both goods and passenger traffic daily, the passenger and goods trains are averaged in two entries for that line in the second part of Table II.; the third entry contains the working of goods traffic only, in February and March, 1860, in stormy weather. The Great North of Scotland Railway has long gradients, many of them 1 in 100 to 1 in 150, with quick curves, and the goods engines ascend these inclines, at the rate of ten miles per hour, with thirty-five fully-loaded waggons, each weighing 12½ tons to 14 tons gross, or a gross total train weight of 460 tons.

FEEDWATER HEATERS. — In their application to locomotive engines, Mr. Beattie has laboured with considerable success, at feedwater heaters, using the exhaust steam for the purpose. The results of the working of Mr. Beattie's heater in 1856, have already been communicated to the Institution,¹ when a saving was found of at least 15 per cent. of fuel by the use of the heater, as compared with cold water; a conclusion which has been confirmed by the experience of others, in locomotive practice. In Mr. Beattie's earlier form of heater, the feedwater was discharged through perforations in the end of a pipe, into a chamber, placed in front of the chimney, where the exhaust steam was freely admitted. By this plan, the water was raised to a temperature varying from 180° to 190° with an ordinary blast, or 212° with a heavy blast. Mr. Beattie has, more recently, employed a surface heater,—consisting of a steam pipe from the exhaust within a water chamber, the feedwater circulating in the annular space,—in combination with a cistern below the footplate, into which the feedwater is delivered from the tender, through a ball cock, to regulate the supply, and is dropped through a perforated plate. Into this chamber the exhaust steam also is discharged, after having passed through the annular heating chamber. In this apparatus, though it is complicated and expensive, the water is well heated, but occasionally, the influx of the water to the cistern is prevented, by the counter pressure of the exhaust steam. Mr. P. Stirling, of the Glasgow and South Western Railway, states, that a saving of 11½ per cent. of fuel has resulted from the use of this apparatus;² but it has been since discontinued, as it was found difficult to be kept in order.

The Author has recently introduced a simple and compact feed-water heater, (Fig. 13,) in which the steam from the blast pipe is projected into a short tube, in conjunction with the feedwater, which is delivered in a thin annular sheet around the steam nozzle. The steam forcibly impinges upon and breaks up the water into spray, and is instantly absorbed by the water, which may be heated to any degree of temperature. The steam operates partly by suction, in inducing the influx of the water, and partly by driving it before it, and so aiding the hot water pump, by the acquired momentum of the water. This apparatus is in process of application to locomotives, and has been employed, for some months, on a non-condensing stationary engine, of 20 nominal H.P., working at a pressure of steam of 60 lbs. per square inch. In its first form, the temperature of the feedwater was raised 120°, and the

¹ *Vide Minutes of Proceedings Inst. C.E., vol. xvi., p. 41.*

² *Vide "Transactions of the Institution of Engineers in Scotland." Vol. ii. "On Coal-burning Locomotive Engines."*

saving of fuel by its use was 10 per cent. In the second form, the water was heated from 39° to upwards of 191° , the engine making forty revolutions per minute, when the saving was greater, though it was not measured. There is no doubt, that the heater may be made to heat the water to the boiling point, and it is well adapted for agricultural and other engines on wheels, where lightness and compactness are important considerations.

The Paper is illustrated by a series of diagrams, from which Plate 5, (Figs. 1 to 13,) has been compiled.

Mr. D. K. CLARK wished to direct special attention to certain important points in the working of coal-burning locomotives. In the first place, the steam should always be kept up, while the engine was running. In most of the systems in use, a considerable quantity of cold air was drawn through the flue tubes, the strength of the draught towards the flue tubes not allowing it to approach sufficiently near the surface of the fire, to mix with the smoke. In Mr. Connor's system, although the inclined plate, or deflector, at the doorway, gave the air a downward direction, yet, when it was under the action of a strong blast, the current undulated over the arch. In Mr. Douglas's and Mr. Frodsham's systems, the diverting action was still more injurious, because there was no assistance from a brick arch in bringing together the smoke and the air; the air did not strike upon the fire, but went at once towards the tubes. On that account, he thought the brick arch a very useful adjunct, in connection with the inclined entrance of the air into the box. In the plan of Mr. Ramsbottom, the air was introduced in too large a body, through only two places, and, therefore, a great quantity of it must pass through unmixed. He had come to the conclusion, that the air, where it entered the box, should be introduced at the proper level for immediate mixture with the smoke.

But if it was necessary to keep up the steam when the engine was working, it was also important to keep down the steam whilst the engine was standing at the stations, or running without steam on. The chimney blower was required, in order to create a sufficient current of air in the fire box to consume the smoke. Where tubes were placed in the front and back of the box, and induction jets were used, instead of a jet in the chimney, as the nozzles operated above the level of the fuel, the tendency was to prevent the draught through the grate, and thus to suspend the generation of smoke, as well as the consumption of fuel. This was the reason that the steam was not so liable, in this system, to rise to a dangerous pressure.

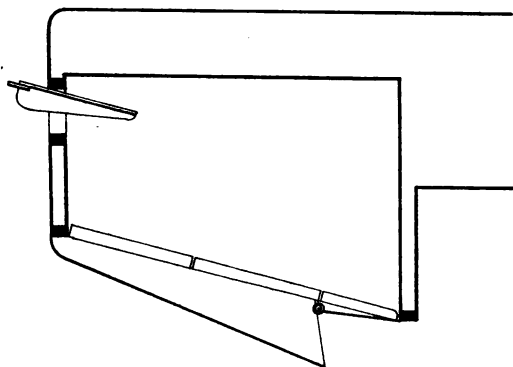
Another important point was the question of management. It was desirable, that any system employed should be capable of being easily managed; and as the difficulty principally occurred at high speeds with express trains, he considered that which required the least fuel, and the least labour from the stoker, was the best.

He thought, that with deflectors, there was a large quantity of cold air introduced into the fire box. He had taken the sectional areas of the air entrances in some of the systems he had alluded to. In Mr. Connor's plan, for example, the sectional area of the entrance was $67\frac{1}{2}$ square inches, or 5 square inches per square foot of grate. In Mr. Ramsbottom's system, it was 7 square inches per square foot of grate. In Mr. Sinclair's arrangement of Mr. Frodsham's system, upon the Eastern Counties Railway, the area of entrance

IN LOC

PLATE 6, VOL. XIX

Fig. 10.



DOUGLAS' SYSTEM, BIRKENHEAD RAILWAY.

From the *Journal of the American Medical Association*, Vol. 10, No. 1, 1916

was $5\frac{1}{2}$ inches per square foot of grate. In his own apparatus, the area had been as low as 3 inches; and the maximum allowed was 4 inches per square foot of grate. A plan had been applied to one, or two engines on the Caledonian Railway, for supplying the air direct upon the top of the fire, through a wide spout from the doorway; and that plan was more effective than any other upon the deflecting principle, and was worked with a small sectional area of air entrance. The exit of the air was about 3 inches above the usual level of the fuel, and the air was discharged upon the middle of the fire. A similar plan was employed upon the North London Railway, but it was even simpler; it consisted in using a long shovel instead of a short one, reaching to the middle of the fire box, with an arch. The brick arch appeared to be considered an essential adjunct, in the present practice of coal-burning on railways.

The feedwater-heating apparatus was simply applied to locomotive engines, in the form of a pipe on one side, and a return pipe to the other side. It did not alter the arrangements of any existing engines.

Mr. HAWKSHAW, V.P., had noticed, that in the comparisons made by the Author between his own system and those of others, he had only alluded succinctly to that of Mr. Jenkins. Mr. Hawkshaw had long been acquainted with that system, and it was now extensively applied, at a very small cost, on the Lancashire and Yorkshire Railway. He thought it one of the best, because it was the simplest. One great advantage it possessed, was that it did not interfere with the future use of coke, in the locomotives to which it was applied. It was also free from most of the objections urged against many of the systems of smoke-burning. It mainly consisted in making a certain number of the stays hollow, instead of solid, with as many rows of apertures in the fire box as might be desired. Atmospheric air introduced in this manner, mixed with the products of combustion, and effectually destroyed the smoke. Moveable slides were fixed outside the fire box, by means of which the apertures could be opened and closed, at the pleasure of the engineman. When the apertures were closed, the locomotives were fitted for burning coke as before. He had sent to India, several engines fitted with this apparatus; on account of the equal facility which it afforded for burning either coke, or coal, as circumstances might render necessary. As far as he understood the Author's system, it seemed to be nearly the same as that of Mr. Jenkins, except that the former introduced jets of steam through the apertures. As the latter system had been some time in use upon the Lancashire and Yorkshire Railway, and had effected, as he believed, a large saving in

fuel, he thought it was entitled to more notice than had been bestowed upon it in the Paper. The Author had referred to the difference in the consumption of coal by Mr. Jenkins's system, when applied on the Lancashire and Yorkshire Railways and on the Brighton Railway; but was the same description of coal used in both cases?

Mr. WRIGHT observed, that the experiments on the railway were entirely conducted by his Brother, and he was not in a position to state the results. He only knew, generally, that a considerable saving was considered to have been effected, by the adoption of the system.

Mr. FLETCHER inquired, when the jets were most in use; whether with the engine running, or standing still.

Mr. D. K. CLARK replied, that the steam jet was used, occasionally, whilst running, but it was mostly used when the engine was standing still. Staveley coal was employed in the comparative trials on the Brighton Railway, but he took no part, personally, in the experiments.

Mr. G. BERKLEY remarked, that the plan with hollow stays, which had been described, was tried, about nineteen years ago, on the Midland Railway, and was the invention of Mr. Hall, (of Basford). He had also made experiments himself, and with results similar to those at present given. The evaporation of water was from 5 to 15 per cent. less per lb. of coal, than per lb. of coke. An objection was made at the time, that the tubes for the admission of air were liable to be burnt, and he had yet to learn, that such was not still the case. Economy, however, was the order of the day, and where coal could be obtained at much less cost than coke, a want of simplicity, or slight additional repairs would be overlooked. He found, generally speaking, that in the north, coal was not used for the engines; on the North Eastern Railway, it had not been introduced, or only to a slight extent. This was owing, probably, to coke being almost as cheap as coal, in the north of England; so also it was at Bombay. In coal districts, or where there was a large charge for freight to be added to the first cost of the fuel, there was little, or no advantage in using coal. Practically, there was only a saving where the price of coal was considerably lower than coke, but it was obtained at a sacrifice of cleanliness, and, to a certain extent, of simplicity. Mr. Hall's plan and that in use on the Great Western Railway, which was the same as Mr. Hall's, had not been mentioned in the Paper; this was a serious omission. If the complexity was not found objectionable, the introduction by the Author, of the nozzles for blowing steam through the hollow stays, to mix the air with the incandescent gases, commended itself to notice.

Mr. W. H. BARLOW remarked, that upon the Midland Railway there was in operation, a very simple, but efficacious, contrivance, which cost only a few shillings. It consisted of a broad, inclined shovel, placed on the top of the fire-box door; there were no hollow stays and no brick arches. It was assisted by a small jet of steam in the chimney, during the stoppages at the stations; but when the engine was running, the jet was not used. This simple and cheap arrangement was employed, with complete success, upon all classes of engines.

Mr. LONGRIDGE said, that the plan introduced upon the Midland Railway, had particularly come under his notice. He regarded the Author's plan as essentially the same as that introduced, many years ago, by Mr. Hall; a system of hollow stays, introduced into the fire box, by which the air was brought in, to mix with the hydro-carbons. The chief fault found with the system, at the time it was brought forward by Mr. Hall, was, that when an engine was standing at a station, it emitted black smoke. Mr. Hall remedied this, by a small steam jet in the chimney, to create a draught, whilst the engine was standing still, and thus the smoke was effectually prevented. Objections were then raised to the whistling sound of the steam jet, and eventually, the plan was abandoned. He thought, if more willingness had been exhibited by the officers of the company to assist in bringing the plan into working operation, Mr. Hall might have carried it out quite as well as others who had followed him.

Addressing himself to the general question, he said that, after a great many experiments upon the consumption of smoke, he had arrived at this conclusion, which he was satisfied was correct, that deflecting plates and brick arches were of little importance. All that was wanted, was sufficient air to mix with the hydro-carbons, and if it was moderately diffused over the surface of the fuel, there would be little, or no smoke. He had burnt at the rate of 66 lbs. per square foot of fire grate per hour, of bituminous coal, without emitting smoke from the chimney. With the apparatus proposed, many years ago, by Mr. Williams, he was able to introduce a sufficient amount of air, without deflecting plates, or brick arches. With respect to the jets of steam, it was curious, that in 1856, at the time he was much engaged upon this subject, among the plans which came under his notice, was one from Mr. Robert Longridge, of Manchester, who proposed to use hollow stays, with small jets of steam to promote the passage of a sufficient quantity of air. The reason of proposing jets of steam in that case, was, that in the boilers of marine engines, great difficulty was experienced in getting the requisite quantity of air, through the limited number of tubes which could be conveniently

used ; but by employing jets of steam, the quantity of air passing through these hollow stays, might be greatly increased. With a jet of steam, as much air passed through a tube 2 inches in diameter, as through one 6 inches, or 8 inches in diameter, without the jet. In a locomotive engine with all the sides open, he did not see any difficulty in putting in a sufficient number of hollow stays, to admit the proper quantity of air for consuming the hydrocarbons. In the furnaces of marine engines for burning north country steam coal, there should be an air entrance of about 4 square inches per square foot of grate. With that amount of area, any gas coal could be burnt without smoke ; with less, a sufficient quantity of air would not enter, unless aided by an artificial draught.

The only other point to which he would allude, was the comparative heating powers of coal and coke. This subject was discussed in a Paper, "On the Evaporation of Water from Steam Boilers," which was read at the Institution many years ago.¹ The theory then propounded was, that the same evaporative effect was obtained from coke, as from the quantity of coal from which it had been made. Having had considerable experience with coke ovens, he could not avoid the conclusion, that the flame which issued from them during the process of the manufacture, would boil water, and, therefore, he entirely dissented from that theory.

In a series of experiments with Garesfield coal and coke, which he made in conjunction with Sir William Armstrong and Dr. Richardson, at Elswick, in 1857-58, it was determined to settle this question. The best mode of treating coal, in order to produce the greatest amount of evaporative power, was first ascertained, and then the coke made from the same coal was similarly treated. The final result was, that the evaporative power of the coal comparatively to that of the coke was, weight for weight, as 100 to 89 ; the actual numerical quantities being :—

Water evaporated from 32° by 1 lb. of coal . . lbs. 12·54

Do. by 1 lb. of coke made from the same coal . . lbs. 11·16

But this was scarcely the correct way of stating the question. To produce that coke, the coal would be reduced at least one-third in amount, 100 tons of coals producing less than 66 tons of coke ; consequently, taking the above figures, it would be found, that whilst 1 lb. of coal would evaporate 12·54 lbs. of water, the same coal converted into coke, would only evaporate $11\cdot16 \times 0\cdot66 = 7\cdot44$ lbs. of water ; so that the true proportion was as 100 to 58. He

¹ *Vide* Transactions Inst. C.E., vol. ii., p. 16., *et seq.* and Minutes of Proceedings Inst. C.E., vol. i., (1838), p. 17, *et seq.*

could vouch for the accuracy of those results, and he thought they set at rest the theory to which he had alluded.

Mr. HAWKSLEY said, that having been engaged, incidentally, upon this subject, he was gratified at the account of the experiments made twenty years ago, upon the Midland Railway. Mr. Samuel Hall, (of Basford,) the pioneer of surface condensation, was also the originator, almost in its present form, of the system now in use for burning coal in locomotive engines. He was personally acquainted with Mr. Hall, and had taken some share in the experiments from which Mr. Hall obtained his economical results. The apparatus used was extremely simple, and it did not materially differ from some of the diagrams exhibited by the Author of the Paper. It consisted of small tubes, not originally intended for hollow stays, but afterwards frequently adopted for them, which were inserted in the sides, and sometimes, also in the front of the fire box of the engine. By this plan the smoke was almost, but not entirely consumed. The railway companies being then more particular with regard to the consumption of smoke, than they appeared to be at present, refused to adopt the plan which was, therefore, supposed to have failed; but so far as he knew, its success was fully equal to that now obtained with other apparatus. The smoke was not perfectly consumed under all circumstances, nor was it now, for the amount of dust and smoke, issuing from the coal-burning engines of the fast trains of the present day, was so considerable, that the windows of the carriages could not be opened, without covering the passengers, and filling their eyes with small particles of dirt and cinder.

Mr. Hawksley had also occasion, at that time, to make some elaborate experiments upon the relative values of coal and coke. As the Engineer of the Nottingham Gas Company, and of the Nottingham Waterworks Company, he had an opportunity of exactly determining the quantity of coke which could be manufactured, in closed vessels, from a given weight of coal. He had also the opportunity of trying the evaporative power of the coke, as contrasted with that of the coal from which it was made, under the furnace of a pumping engine, raising a known quantity of water to a known height. He found, upon the average of a year's working, that from 20 cwt. of coal of the midland counties, which was very different from the north country coal, $11\frac{1}{2}$ cwt. of coke were obtained, besides other products. He then ascertained, that 6 cwt. of coke would evaporate exactly the same quantity of water as 7 cwt. of coal. It followed, therefore, that it required almost precisely 3 tons of coal reduced to coke, to produce the effect of 2 tons of the same coal, and consequently, there was a loss in converting the coal into coke for the purpose of producing steam; but considered only in reference to weight, coke had an advantage over

coal, in the proportion of 7 to 6. Such would be the result with most of the midland coals. The Derbyshire coal contained in every ton, no less than 300 lbs., or 350 lbs. of oxygen and hydrogen in such proportions as to combine, during distillation, and to form water: this quantity was, of course, entirely lost. Tar was another product of distillation, which was burnt in the ordinary process of forming coke, but which, in the process used for manufacturing gas, was caught in tanks; that amounted to another 100 lbs. per ton. So that, in every ton of this Derbyshire coal, there were from 400 lbs. to 450 lbs. of matter which could not be utilised in the shape of coke, in addition to more than 350 lbs. of inflammable gases which passed off in the conversion, besides carbonic acid and other non-inflammable gases, amounting altogether to more than 900 lbs. in every ton of coal; the residue alone was coke.

Reference had been made to the possibility of consuming smoke economically. He believed it to be physically impossible to consume smoke once formed, without loss, and the reasons were sufficiently obvious. If all the carbonaceous matter of the smoke was consumed, additional heat was, certainly, obtained, but by means which entailed more than a corresponding loss. This was proved both by experiment and by calculation; because if a certain quantity of oxygen was introduced into a furnace, a much greater quantity of nitrogen was introduced at the same time; 7 lbs. of nitrogen, in fact, for every 2 lbs. of oxygen. The nitrogen did not contribute to the combustion, but required to be itself heated, and therefore, it carried up the chimney a large quantity of the heat evolved from the combustion of the particles of smoke. But independently of that, there were no means of getting every particle of smoke which escaped unconsumed from the fire, to meet with the necessary particles of oxygen, unless a much larger quantity of air was introduced above the fire, than was theoretically necessary for the purpose. But in so doing, a correspondingly larger quantity of nitrogen was also introduced at the same moment, and thus the loss occasioned by heating the excess of oxygen and nitrogen, was much greater than the gain from utilising the heat of the carbonaceous particles of which the smoke consisted.

In making his experiments on the combustion of smoke, he had used a very perfect, self-acting apparatus applied to the boilers of a pumping engine, with the following results:—1st. When the engine was well fired, without the admission of air above the fire, the consumption of fuel was 100; but in this case, smoke was evolved in considerable quantity. 2nd. When the air was admitted in sufficient quantity to consume the smoke, without being shut off during any portion of the interval between the firings, the consumption of fuel became 120. 3rd. When the self-acting apparatus was brought into operation, to exclude the air so soon as smoke had

ceased to be emitted, then the consumption of fuel was reduced to 110.

The smallest additional consumption consequent on the consumption of the smoke, was about 10 per cent. The trial was frequently continued for several days together, without putting the air holes into operation, and then the consumption uniformly went down; when they were again called into action, the consumption as uniformly increased. He believed it was impossible to conduct experiments with greater care, or in a better manner, for determining the point in dispute between those who alleged, that the combustion of smoke was not, in a pecuniary sense, economical, and those who asserted the contrary. He thought the Paper had sufficiently shown, by the average of all the experiments related, that weight for weight, the consumption of coal in locomotives was greater than that of coke; and this result was confirmed by calculation, which proved, that such must, necessarily, be the case. It might, nevertheless, happen, that when the coke was consumed in an ill-adapted furnace and the coal was consumed in a well-adapted furnace, the contrary would result, and the experimenter would be led to an erroneous inference. Unless, however, 6 cwt. of coke could be delivered into a tender as cheaply as 7 cwt. of coal, when the smoke was not consumed, and about 8 cwt. when the smoke was consumed, the mere pecuniary economy must be on the side of the coal.

Mr. YARROW,—through the Secretary,—observed, that it would appear from the Paper, that the system used upon the Great North of Scotland Railway was superior to the others, as regarded the amount of fuel consumed per mile, relatively to the gross load carried. To enable a proper comparison to be made, the Paper should give, in addition to the average speed per mile for the whole distance run, and the number of pounds of water evaporated by 1 lb. of coal, the greatest altitude surmounted in each case,—the average gradient,—and also its length. The experiments should be made on the same line of rails, with a train running at the same speed, and with the same stoppages. On the Scottish North Eastern Railway, for example, the speed of the passenger trains was about 35 miles per hour, whilst on the Great North of Scotland Railway, it did not exceed 25 miles per hour. The two cases did not, therefore, admit of comparison, as there was a greater amount of resistance, and consequently, more power was expended, on one of the lines, than on the other. Again, the speed of the goods trains, upon the Great North of Scotland Railway, was about 13 miles per hour, whereas upon the Caledonian Railway, it was about 20 miles per hour. On the Scottish North Eastern Railway, there was a rise of $340\frac{1}{2}$ feet above the level of the terminal station, of which $234\frac{1}{4}$ feet had to be surmounted within a distance of

7 miles, the heaviest gradient on that part of the line being 1 in 91. There were various other steep gradients on the line; for instance, on another portion, 171½ feet had to be surmounted within a distance of 3½ miles.

Mr. ZERAH COLBURN,—through the Secretary,—presented the following observations on coal-burning locomotives in the United States:—

The circumstances affecting the substitution of coal for other fuel, were dissimilar in England and in the United States. So far as the question concerned English railway companies, it appeared to have already received a satisfactory solution; but it was still the subject of much discussion in America.

The cost of fuel on the lines in the Northern and Eastern States, averaged as much as 9*d.* per train mile. This fuel consisted, generally, of wood, which was sold by the 'cord' of 128 cubic feet, weighing about 1½ ton. One cord and a half, or rather more than 2 tons of good wood, would evaporate as much water as 1 ton of coal; and this quantity of wood sometimes cost as much as £2. 3*s.* The Directors of the Baltimore and Ohio Railroad Company, although their line intersected rich beds of semi-bituminous coal, stipulated, so long ago as 1831, that the engines to be constructed for their line should burn anthracite, so as not to emit any smoke. In 1837, the Beaver Meadow Railroad Company adopted the use of anthracite coal in their engines, which were of ordinary construction, and they still continued to employ it. In attempting, however, the general introduction of the same coal for long and heavy traffic, it was found, that although there was not any smoke, the steam was kept up with difficulty, and that the fire box and fire bars were quickly burnt out. Notwithstanding these objections to its use, anthracite was, in some cases, a cheaper fuel than wood. The Reading Railroad, the principal channel of the anthracite coal trade, was worked with this fuel, which was burned, however, with considerable waste. The fire box was very large, and was sometimes placed entirely behind the wheels of the engine, so as to obtain sufficient room for a grate, 5 feet in width. For the heavy goods engines, the grates were, generally, 7 feet in length and 3 feet 6 inches in width. The fire box was made, in most cases, of iron plates, $\frac{5}{8}$ ths of an inch thick, but copper was sometimes used about the bottom. The tubes were of iron, 2¼ inches in diameter and 14 feet in length. The top of the fire box sloped off at an angle, and the fire was partially fed through a door in the roof. The hinder end of the fire box had no water space, being covered with a cast-iron plate, having two doors across its entire width, for feeding the fire, and two others below them, for raking out the fire bars. A few large openings were made in these doors, for the admission of air. The fire bars were always of cast iron, and were,

in the engines under notice, very heavy; the width between the bars was as much as $1\frac{1}{2}$ inch. They were cast in pairs, and each pair of bars had a shank, or stem, projecting behind the fire box: through a round hole in this shank, a bar was, occasionally, inserted, to shake the grate and to break up and dislodge the accumulated clinker. The ash pan was made with a water-tight bottom, and water was, occasionally, turned into it from the tender, to quench the hot coals and cinders dropping through, which would, otherwise, burn out the fire bars. A variable blast pipe was always applied, with a nozzle capable of being contracted from 18 to 6 square inches. With these arrangements, the reports of the company showed, that anthracite coal was burned at the average rate of 117 lbs. per mile, in engines weighing 28 tons, and drawing a gross load of 750 tons, at a speed of from 10 miles to 12 miles per hour, on a level. The evaporation of water was from $5\frac{1}{2}$ lbs. to 6 lbs. only, per lb. of coal. Mr. Colburn had conducted trials over the line in winter, when the consumption was 13,500 lbs. of coal, for 95 miles, or 142 lbs. per mile. The engines on this line had eight coupled wheels, 3 feet 7 inches in diameter; the diameter of the cylinders was 19 inches, and the stroke was 22 inches.

When the question of a general substitution of coal for wood was agitated, ten, or twelve years ago, among railway authorities in the United States, it was assumed, that only anthracite coal could be adopted, as smoke was thought to be inseparable from the use of bituminous coal. The locomotive superintendents of the various lines took out no patents, however, and the question was left open to inventors generally, most of whom appeared disposed to make the greatest possible changes in the ordinary form of boiler. Their plans were carried out by builders of locomotives, as well as by the different railway companies, and were worked with more, or less success. As some of these plans had, in this way, acquired considerable prominence in America, it might be desirable to give a brief description of them.

Phleger's boiler, as constructed in the engines made by Messrs. Richard Norris and Son, of Philadelphia, had a combustion chamber, formed by a deflecting water bridge in front of the tubes which, with the barrel of the boiler, were placed rather lower than usual. At first, the fire boxes of these boilers had close water bottoms, the air for combustion being forced in by a blower. The water bottom was still retained, although the air was allowed to enter in the usual way. A variable blast pipe, and a steam jet, or blower in the chimney, were provided, as in all American coal-burning locomotives.

In Dimpfel's boiler, the water was contained in the tubes, and also in a long annular water space, answering to the ordinary

[1859-60. N.S.]

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cylindrical barrel of the boiler. The tubes were of iron, and of small diameter. They descended from the roof of the fire box, bending and extending thence to a water space at the forward end of the boiler. This plan had been extensively introduced by the Taunton Locomotive Manufacturing Company.

Boardman's boiler, as constructed in the engines made by Messrs. William Mason and Co., of Taunton, had upright tubes, through which the heat descended from a long combustion chamber, or flue, in the upper part of the boiler. This flue extended to and opened into the smoke box, but a plate was interposed, a few feet from the smoke box, the heat descending through the tubes next the fire box, and traversing an ash pan, before ascending the front tube.

Phleger's, Dimpfel's, and Boardman's boilers had been used, more, or less, for the last ten, or twelve years; but experimentally rather than practically. They had been made to burn bituminous as well as anthracite coal, and they were used on passenger as well as on goods trains. Each of these plans furnished considerable space for combustion, and in each, air was admitted in numerous fine streams over the fire. Each had obvious disadvantages, and neither was applicable to the existing stock. All were heavy and complicated, Boardman's boiler especially, and much of its weight was thrown upon the front wheels of the engine, where it was least desirable. The boiler occupied the whole space between the framing of the engine. This was not, however, an important objection, as, since 1854, all American locomotives had been made with outside cylinders, not only for the gauge of 4 feet 8½ inches, but also for those of 4 feet 10 inches, of 5 feet, of 5 feet 6 inches, and of 6 feet; the latter being the widest railway gauge in the United States. The staying of the flat sides of the Boardman boiler required great care, and with the stays fastened, as they had been, on the inside of the boiler, it was very difficult to replace them.

Within the last three, or four years, renewed attempts had been made, to adapt the existing stock of engines to the use of bituminous coal. The difficulties to be encountered, consisted in the large quantity of coal required to be burned in a given time, under an intense draught, and in the character of the coal itself, which produced a great quantity of clinker. American railways, generally, required more power to work them than was necessary on English lines. This arose from the steeper gradients and the sharper curves in the former, and from the inferior condition of the permanent way. The trains, although less frequent, and although running at a speed slower by one-third to one-fourth than the trains on English lines, were from one-third to one-fourth heavier. The engines, on the other hand, were not heavier, on

the average, than in England, nor had they more heating surface. They burned more fuel, however, and evaporated fully one-third more water, in a given time; a result depending on the strength of the draught. The average evaporation in American locomotives, might be taken at 120 cubic feet of water per hour. The 8 cwt., or 9 cwt. of coal necessary for this evaporation, had to be burned on a grate of which the area, generally, was not more than 14 square feet, giving a combustion of from 60 lbs. to 70 lbs. per square foot of grate per hour. The coal, besides being smoky and often sulphurous, was of that kind which quickly crusted over the top of the fire, and it, generally, made a large quantity of clinker: this was sometimes carried, by the strength of the draught, against the ends of the tubes, which thus became, in some cases, completely closed within half an hour after starting.

In December, 1856, Mr. George S. Griggs, of the Boston and Providence Railroad, introduced a fire-brick arch below the tubes, in the fire boxes of some of the engines of that line. With a few holes for the admission of air above the fire, this arch was found to improve the working of the fire box, when burning coal. A good deal of fine coal was carried through the tubes, and the ordinary spark-arrester of the wood-burning engines was, therefore, retained. By carefully selecting the best coal in the market, Mr. Griggs was enabled to use the ordinary fire bars; but on other lines, with coal producing more clinker, bars had been adopted which could be moved while the engine was running. In some cases, the whole fire grate had been formed of stout chains, suspended across the fire box; the flexibility of this arrangement had an important influence in keeping the fire clear of clinker. It had been also found, that with coal producing much clinker, the air opening of the grate required to be materially diminished, so as to localise the draught over a comparatively small area, and thus, by intensifying the combustion, to thoroughly fuse the clinker, which then ran off like glass. On the Iowa Central Railroad, the grates of the coal-burning engines were, at one time, covered with fire brick, so as to leave exposed only about 2 square feet of the original surface of the bars, and of this area less than 1 square foot was used as an air opening. This arrangement was found to work well, with coal producing much clinker; and, notwithstanding the diminution in the area of the air opening, as much coal was burned, and as much steam was made as before. In other coal-burning locomotives, it was now usual to close up from 12 inches to 30 inches of the front ends of the fire bars, and in many cases, the 'dead plate,' as it was called, extended along the sides of the fire box, reducing the area of the grated portion of the fire box bottom to 4, or 5 square feet, of which less than one-half was air opening. Upon the dead plate, the coal

became partially coked before entering into active combustion. The fire was fed, generally, with small quantities of coal, frequently and equally applied. More recently, perforated door plates and air distributors, the latter set up within the fire box, had been extensively adopted. It was only rarely, that the spark arrester of the wood-burning engines was dispensed with, in burning coal. This apparatus intercepted considerable quantities of fine coal, which, under a strong draught, would otherwise have been thrown out at the top of the chimney.

The circumstances affecting American railway traffic were so different from those which existed in England, that it would hardly convey an accurate idea of the success of these plans, to give the consumption of coal per train mile. It might be said, however, that they worked with tolerable freedom from smoke, that they generally afforded a good command of steam, and that they gave an evaporation of from $6\frac{3}{4}$ lbs. to $8\frac{1}{2}$ lbs. of water per lb. of coal.

Mr. LONGRIDGE said, he had already given the results of some carefully-conducted experiments, showing a decided advantage in the evaporative powers of Garesfield coal, over the coke made from that coal. He should not have again referred to those results, had it not been contended, that theoretically, as well as practically, coke was superior in heating power to the coal from which it was made. It was not possible to determine, theoretically, the precise temperature arising from the combustion of fuel, owing to ignorance of the specific heats of the products of combustion at high temperatures, and also of the latent heat of some of the generated gases which were, probably, emitted in a solid form in a body, like coal. So far, however, as theory would go, calculation¹ showed, and his experience confirmed the fact, that the

¹ HEAT GENERATED BY 1 lb. OF COAL.

	lbs.	Units of Heat.
Carbon	0·8152	$\times 14,500 = 11,820$
Hydrogen	0·0522	
Less the amount to combine with the oxygen in the coal	0·0077	$= 0·0445 \times 62,032 = 2,760$
Sulphur	0·0115	$\times 4,152 = 48$
Total heat generated		<u>14,628</u>

PRODUCTS OF COMBUSTION, MULTIPLIED BY THE SPECIFIC HEAT.

	lbs.	Specific heat.
Carbonic acid	2·989	$\times \cdot 217 = 0·6486$
Sulphurous acid gas	0·023	$\times \cdot 240 = 0·0055$
Nitrogen	8·912	$\times \cdot 245 = 2·1834$
Steam	0·470	$\times \cdot 475 = 0·0223$
Ash	0·046	$\times \cdot 200 = 0·0092 = 2·860$

Consequently, the resulting temperature = $\frac{14,628}{2·860} = 5,098^{\circ}$.

[Proceeding

temperature resulting from the combustion of 1 lb. of Hartley coal, was greater than that resulting from 1 lb. of the coke made therefrom, in the proportion of 5,098° to 4,695°, or of 100 to 92; and the proportion would be still higher in a coal like the Garesfield, containing more hydrogen.

It had been urged, against the economical use of bituminous coal, that to prevent the smoke arising from its use, it was necessary to introduce into the furnace, an extra quantity of air, which carried off a large amount of heat. Now the extra quantity of air required for perfect combustion, was not nearly so large as was generally thought. He had made direct experiments to ascertain the fact, with an apparatus so arranged, that the quantity of air actually passing through the furnace could be accurately measured, separating, moreover, what passed through the bars from what was admitted above the fire surface. The result was, that to burn 1 lb. of Hartley coal perfectly, it required 149·4 cubic feet of air at 60° and 30 inches of mercury.¹ Now it was found, that

Proceeding in the same manner with the coke, consisting of:—

Carbon	. . .	94 per cent.
Ash	. . .	6 "

HEAT GENERATED BY 1 lb. OF COKE.

Carbon	. . .	$0\cdot94 \times 14,500 = 13,630$
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PRODUCTS OF COMBUSTION, MULTIPLIED BY THE SPECIFIC HEAT.

	lbs.	Specific heat.	
Carbonic acid	. . .	$3\cdot415 \times \cdot217 = 0\cdot7411$	
Nitrogen	. . .	$8\cdot774 \times \cdot245 = 2\cdot1496$	
Ash	. . .	$0\cdot060 \times \cdot200 = 0\cdot0120 = 2\cdot9027$	

Consequently, the resulting temperature = $\frac{13,630}{2\cdot9027} = 4,695^\circ$.

This, in the case of coal, was found = 5,098°;

Therefore, 1 lb. of coal : 1 lb. of coke :: 5,098° : 4,695°
:: 100 : 92 nearly.

¹ QUANTITY OF AIR REQUIRED FOR THE PERFECT COMBUSTION OF 1 lb. OF HARTLEY COAL.

Composition of 1 lb. of Coal.

Oxygen	. . .	0·0615
Hydrogen	. . .	0·0522
Nitrogen	. . .	0·0150
Sulphur	. . .	0·0115
Carbon	. . .	0·8152
Ash	. . .	0·0446

Oxygen required for the Perfect Combustion of the above.

Carbon	. . .	$0\cdot8152 \times \frac{8}{3} = 2\cdot1738$ lbs.
Hydrogen	. . .	$0\cdot0522 \times 8 = 0\cdot4178$
Sulphur	. . .	$0\cdot0115 \times \frac{16}{3} = 0\cdot0615$

$\frac{2\cdot1738 + 0\cdot4178 + 0\cdot0615}{\quad} = 2\cdot6031$

Deduct oxygen in coal . . . 0·0615

Oxygen required from the air . . . 2·5416 lbs.

Hence, the weight of air will be $2\cdot5416 \times \frac{14}{3} = 11\cdot4372$ lbs.; from which the volume of 60° and 30 inches of mercury is found = 149·4 cubic feet.

with the ordinary system of stoking and making a dense smoke, the quantity of air which passed through the furnace, was only 100 cubic feet per lb. of coal burnt, showing conclusively, that a great deal of carbonic oxide was passing off with the products of combustion. When smoke was entirely prevented, by air admitted above the fire, the quantity was found to be 158 cubic feet, being only 9 cubic feet above what was necessary for the perfect combustion of the coal. The temperature of the up-take was also ascertained under the two circumstances, with and without the additional quantity of air. When 100 cubic feet of air per lb. of coal passed through, the temperature was 448° ; with 158 cubic feet of air passing through, the temperature was 480° .¹ To the boiler in which these experiments were tried, there were adapted means of regulating the quantity of air admitted above the grate. In another case, with no air except that which passed through the bars, and with much smoke, the temperature in the chimney was 600° . With one aperture open, the temperature was 625° ; with two apertures, it was 633° ; with three, it was 638° ; and with five, it fell to 620° ; showing again, that up to a certain point, the admission of air did not decrease, but rather increased, the temperature. He thought these results were conclusive proofs; first, that it was not necessary to introduce an excessive quantity of air, above what was chemically required for combustion; and secondly that the result of the admission of air, in such quantity as was required to prevent smoke, was to increase the temperature of the chimney, and to decrease the consumption of fuel.

This had, however, been fully confirmed by experiments made by order of the Admiralty, in March and April, 1859, on board the 'Bustler,' to ascertain the advantages, or otherwise, of the use of ventilating doors for the prevention of smoke. Those made in March, showed an advantage of $16\frac{1}{2}$ per cent., and in April,

¹ RESULTS OF TEN EXPERIMENTS ON HARTLEY COAL, made at ELSWICK, in July, 1857, by J. A. LONGRIDGE, SIR WILLIAM ARMSTRONG, and DR. RICHARDSON.

Weight of Coal burnt, per Square Foot of Fire grate per Hour, in lbs.	Water Evaporated from 212° by each lb. of Coal, in lbs.	Total quantity of Water Evaporated, per Hour, in Cubic Feet.	Quantity of Air used per lb. of Coal burnt, in Cubic Feet.	Temperature of Smoke Box.	Remarks.
17.34 21.00	12.27 10.06	56.71 56.01	158* 100	480° 448	No Smoke. Much Smoke.

* Of this, about 88 cubic feet were admitted through the doors, and 70 cubic feet through the fire bars.

The quantity of air required for the perfect combustion of 1 lb. of this coal, is 149.4 cubic feet, at 60° Fahrenheit, and 30 inches of mercury, as shown in the preceding note; (pages 576-7).

of not less than 21 per cent. of coal per indicated H.P., in favour of the use of the ventilating doors; thus fully bearing out the results arrived at by Mr. Longridge, Sir William Armstrong, and Dr. Richardson, in the Elswick experiments. The following Table was extracted from a return to the House of Commons, "of the Results of Experiments which have been made by the Admiralty on board any of Her Majesty's ships, with a view to the consumption of smoke, combined with the economy of fuel, within the last eighteen months," and which was ordered by the House to be printed on the 18th of April, 1860:—

Date of Trial.	Draught of Water.		No. of Experiments.	No. of Runs in each.	Duration of Experiment.	Total Coal burnt.	Mean Indicated H.P.	Coal per H.P. per Hour.	Mean Revolutions.	Mean Pressure.	Mean Speed.
	Forward.	Aft.									
1859.	Ft. in.	Ft. in.			Hours.	lbs.		lbs.	Per Min.	lbs. sq. in.	Knots per Hour.
10 Mar.	7 1	6 8	5	4	4	6,326	244·9	6·45	23·10	16·92	8·879
18 Mar.	6 11	6 9	5	4	4	7,410	241·4	7·67	23·20	16·60	8·964
15 Apr.	6 11	7 2	5	4	4	5,600	245·4	5·70	22·34	17·53	8·467
19 Apr.	6 11	7 2	5	4	4	7,100	246·6	7·20	22·80	17·26	8·793

The experiments on the 10th of March and on the 15th of April, were made with the ventilating doors open, and firing at the sides of each furnace alternately; on the 18th of March and the 19th of April, these doors were closed. Bituminous, north country coal was used in the first pair of experiments. On the 10th of March, smoke was only visible when the fires were urged and stirred; it was then of a light brown colour, and in moderate quantity. On the 18th of March, the smoke was dense and black during the trial. In the second pair of experiments, the coal was of good quality, and in a fair condition as to size. On the 15th of April, only a light brown smoke issued from the chimney, and for a considerable portion of the time, there was no smoke. The mercury in the barometer, on this occasion, stood at 29·1 inches, and there was a strong gale of wind on the beam, so that it was with difficulty the vessel was kept in a straight course, consequently showing a reduced speed. On the 19th of April, the weather was calm, the mercury in the barometer stood at 29·6 inches, and much black smoke was emitted.

Mr. HAWKSLEY did not doubt, that the experiments made at Elswick, had been as carefully carried out, as experiments of that nature could be, and he concurred in the results, but he could satisfactorily explain them. If there was an insufficient quantity of air below the fuel, carbonic acid was generated, and as it progressed, it decomposed into carbonic oxide, an unflammable gas, which required fresh air to be introduced, in order to re-convert it

into carbonic acid gas. That action always took place when the fire was deep. It was evident, that in the cases mentioned, carbonic oxide was formed, because, when the air was admitted over the fire, the temperature was increased. Those experiments, therefore, although perfectly well made, were not experiments which settled the matter in dispute. The furnace was not in that particular condition which was necessary for burning the particular fuel placed upon it, and his observations had reference to coal and coke, burnt in furnaces constructed for the purpose. In his own experiments, perfect combustion was obtained in both cases, and as he had already stated, the result was, without entering into the commercial part of the question, that, weight for weight, coke was superior to the coal from which it was made. He believed the reason of it to be, that the coal contained moisture and other matters which did not contribute, during its combustion, to the production of heat.

Mr. E. A. COWPER,—through the Secretary,—remarked, that when burning coke, carbonic oxide was always formed in greater, or less quantities, even with a moderately thin fire, and it was interesting to know the area of opening for air which was considered sufficient, per square foot of grate, to properly consume it. The coke, as well as the coal, ought to be burnt in the best manner, in order to institute a fair comparison. He was much gratified, that the decisive experiments upon the quantity of air actually used for the consumption of fuel, had proved it to be nearly the amount theoretically required, as it confirmed him in a conviction he had long entertained, that Count Rumford and others were wrong in stating, that twice the theoretical quantity of air had always to be passed through the fire, to effect proper combustion. Now it was quite certain, that with any ordinary boiler fire, some air must be passed over the fuel, in order to prevent smoke; for it was, practically, impossible to keep a fire sufficiently thin to pass enough air through it, for burning all the gases and carbon, as then the grate would frequently be partially uncovered. There was a decided economy in burning the carbonic oxide and hydrogen, by the introduction of air in proper quantities, so as to prevent smoke, though the advantage was not very great. In numerous experiments which he had made with different sorts of coal and coke, he had found, that good oven coke was more valuable, weight for weight, than good coal, when each was burnt in the best manner, and that good gas coke was, generally, equal to coal. He believed, that the steam coals for the navy ranked, to a great extent, in proportion to the per-centage of carbon in them.

With regard to the water heater, he mentioned, that about the year 1848, he had seen a similar contrivance at Renfrew, in Scotland, and that it answered very well, giving the feedwater a tem-

perature of nearly 212° . It consisted of a box, into one side of which the steam entered, passing out at the top, whilst the water to be heated came in at the top, and fell through the steam, being entirely encompassed and in direct contact with it; he believed the same system had been employed at Mr. Hague's, and at many other places.

The plan of admitting air through hollow stays into the fire boxes of locomotives, as proposed by Mr. Hall, of Basford, had been revived by Mr. Mc'Connell, with the addition of jets of steam supplied from the boiler, to drive in the air. The advantage of being able to forcibly drive powerful jets of air into the gases was, no doubt, very great, as the jets would penetrate the gases in a similar manner to the blast in a blast furnace; and the opportunity it gave of preventing all vacuum, or draught in the fire box, when standing at a station, was also of great utility. He did not think, that it was clearly stated in the Paper, that all the plans had the means of regulating the quantity of air admitted, so that in none of them was there, necessarily, a large quantity of air entering at all times. In the most recent plan, that of the long spout, or shovel, there were on many lines, proper sliding doors in front, so that the driver could easily regulate the quantity of air, by touching the doors with his foot, or hand. It was an exceedingly simple and excellent contrivance, but did not, like the tubular stays and steam jets, give the power of introducing air when the engine was standing.

Mr. C. GREAVES,—through the Secretary,—thinking it desirable, that the progress in the economy of fuel, so far as those occupied in the locomotive branch of engineering had succeeded in carrying it, should be made known, would bring forward some results derived from the experience of stationary engines, to assist the discussion, so far as there might be any analogy between the different circumstances.

The philosophy of the production of smoke appeared to have been succinctly stated in the Paper. The hydrogenous compounds in coal were those most readily extracted by heat. The hydrogen did not leave the coal without bringing with it a portion of carbon, but notwithstanding their union, the hydrogen was still the more easily inflammable of the two. Neither of them would burn without the presence of atmospheric air; if, therefore, there was only a limited quantity of air, the hydrogen was burnt without the carbon and it formed smoke. It was, however, practically found, that it required less heat to consume the hydrogen, than to burn away the solid carbonaceous portions of the coal. There were, consequently, two methods of preventing the occurrence of smoke; either to force, or to admit to the inflaming hydrogen, a sufficient quantity of air to render the combustion of

both it and its gasified carbon complete, and to continue this only so long as it was necessary; or to delay the explosion of the hydrogen and gaseous carbon, so that the ordinary flow of air, admitted through the bar, for the combustion of the solid carbon, should suffice for the combustion of the hydrogenous compounds. Now the latter process could not be easily effected in locomotive fires, but in land and stationary boilers, it was not difficult, with the cheapest small coal, so to damp the coal and to lay it on the fire quickly in a thick heap, that the charge of coal should ignite only from the face in contact with the previous fire. The combustion of the gaseous part was slackened to the rate of the admission of the air from the fire grate, and a smokeless process was maintained. This was not a theory, for he had now, for some years, practised the plan, at the rate of 4,000 tons per annum, with perfect success as to smoke, and with excellent results as to efficiency, as the following Table would show:—

TABLE showing the amount of fuel employed in four sets of boilers, the comparative proportion of small coal contained in it, (the rest being gas coke,) and the evaporative power obtained per lb., at an average temperature of 80° ; during the first half of the year 1859:—

	Fuel, in Cwt.	Per Centage of Small Coal.	Water Evapo- rated, in lbs.
No. 1 . .	16,259	92.97	7.91
„ 2 . .	14,248	89.71	8.32
„ 3 . .	4,265	90.00	8.16
„ 4 . .	4,578	82.98	8.61
„ 5 . .	5,701	88.68	7.59

The following were the results during the second half of the year 1859:—

No. 1 . .	17,814	91.63	7.81
„ 2 . .	12,186	84.03	8.00
„ 3 . .	7,435	86.75	8.13
„ 4 . .	8,083	88.35	7.82
„ 5 . .	1,367	85.73	8.18

The average rate of combustion of the whole, was about $6\frac{1}{4}$ lbs. per square foot of grate per hour.

The relative calorific power of coal and coke had been, similarly, the subject of patient investigation, and reliable results had been obtained from trials extending over a long period. Taking the small of the best coal as a standard of comparison, the use of gas coke gave a result superior to it, of 14 per cent. The combustion of those coals usually called steam coals, as Holywell Main, West Hartley, South Peareth, &c., only produced an effi-

ciency from 5 to 10 per cent. more than that of small coal, which, on an average, cost about 11s. 6d. per ton. The steam coal was burnt without any attempt to avoid smoke.

He regretted, that it should ever have been stated, that coke would do more work than the coal from which it was made. His experience had, unequivocally, proved, that under boilers commonly called Cornish, worked at high pressure, at a moderate rate of thorough combustion and with every advantage, one ton of coke would perform more work than one ton of such coal as that from which coal gas was made. Indeed it would, he thought, be generally admitted, that the calorific power of that part of the coal, which produced the hydro-carbons, or the gaseous matter, was lower than the calorific power of the carbon, which burned by direct combustion with oxygen. Both coal and coke, as ordinarily burnt, contained about 7 per cent. of waste; and coal, burnt as coal, appeared to afford 65 per cent. of solid combustible matter, and 28 per cent. of combustible gas. Now picked and clean coke would evaporate ten and a half times its weight of water, at a temperature of 80°; therefore 0.65 lb. of the pure carbon in 1 lb. of coal would evaporate 7.34 lbs. of water. But 1 lb. of coal afforded 4.46 feet of gas, and 100 feet of gas only evaporated 28.3 lbs. of water, at which rate, 4.46 feet would evaporate 1.26 lb.; and admitting, that a larger proportion of carbon was burnt in a gaseous form with an open fire, than with retorts, the result was 7.34 lbs. and 1.26 lb., which with an addition of, say 0.65 lb., made the total efficiency of good coal 9.25 lbs., as compared with 10.5 lbs., the efficiency of good coke, at a temperature of 80° Fahrenheit. The relative values of 9.25 lbs. for coal and 10.5 lbs. for coke, on special trials, with care and exactness, were about equivalent to 8 lbs. for coal, and 9 lbs. for coke, in ordinary practice and with long trials; and from his own experience, he adopted the latter quantities as their relative value by weight. Generally speaking, all trials of relative value, by the cost of the fuels, were much more favourable to coal. There were, unfortunately, no good Reports of the calorific power of gas, used in a practical way; a good series of experiments would be extremely valuable.

He had the highest opinion of the plan advocated by the Author, the control of the acting force, being, under the peculiar and varying conditions in which locomotives were placed, the point to be aimed at; increasing the admission of air up to the irregular requirements of the coal gases, rather than delaying the explosion of gases to the minimum rate of admission of air, through the ordinary fire grate.

Mr. D. K. CLARK said, the result of his experience was, that so far as locomotive engines were concerned, a saving of coal to

the extent of 18 per cent. had been effected, by properly consuming the smoke. If coal was burnt in a locomotive, without any provision for the admission of air above the fuel, certain unconsumed gases were sent through the chimney. In 1856, he had made a comparison of the cost of coke and coal, and he found, that coke cost, upon an average, 18s. 2d. per ton, and coal 8s. 7d. per ton; the recent introduction of coal-burning in locomotives had slightly reduced the price of the coke. The prices at Crewe, were 14s. 11d. for coke, and 7s. 10d. for coal. On the Oxford, Worcester, and Wolverhampton Line it was 21s. for coke and 10s. 6d. for coal, and on the Midland Railway 20s. for coke and 9s. for coal. On the North Eastern Railway the price of coking coal was not more than 6s. per ton, its conversion into coke would raise it to 9s., and allowing 1s. per ton for labour and the maintenance of furnaces, the total cost would be 10s. per ton. The difference in price as compared with coal, was thus only about 4s. per ton, scarcely sufficient to induce the company to use the latter.

In reply to remarks by Mr. G. Berkley,—that the coke was made from the refuse of the coal, and by Mr. T. E. Harrison, that a large quantity of coke was supplied to the North Eastern Railway Company, at 9s. per ton,—Mr. Clark said, that the information he received was, that the coking coal was sold at 6s. per ton. He had not been able to give in detail, the particulars of Mr. Jenkins's apparatus, as employed upon the Lancashire and Yorkshire Railway, but he had understood, that the air was admitted to the furnace, through rows of tubes, at different levels, varying from each other as much as 12 inches, in their height above the grate; if the lower rows were covered by the fuel, the air so introduced below the level of the fuel did not contribute towards the prevention of smoke. As regarded the comparative performances of Mr. Jenkins's apparatus and that of the Author, on the Brighton Railway, the two systems were applied in the same passenger engine No. 89, and by the Official Report of Mr. Craven, the locomotive superintendent, the respective results were as he had stated them in the Paper. The number of miles run by the engine under Mr. Jenkins's system was 1,949, with a consumption of coal of 33·6 lbs. per mile; and under his own system, on the same duty, 878 miles, with a consumption of 29·3 lbs. of coal per mile. It should be observed, that the longer mileage run in the first case, was a favourable circumstance in the trial of Mr. Jenkins's system, as the excessive consumption of fuel incidental to the trial of new systems, during the first few days, was arranged over a greater number of miles in his case, than in Mr. Clark's.

He might state, that the greater part of the information demanded by Mr. Yarrow, for the purpose of enabling a proper com-

parison to be made, was given in the Tables ; that it was of no use to give the altitudes simply, unless the particulars of the inclines, and the length of the trips were also given ; that the average speed of the goods trains on the Caledonian Railway, deduced from the service time tables, was 12 miles per hour, not 20 miles per hour, and on the Great North of Scotland Railway, it was, by the time tables, $14\frac{1}{4}$ miles per hour, not 13 miles ; further, that the average distance run between stopping stations was, on the Caledonian, $6\frac{3}{4}$ miles, and on the Great North of Scotland, 4 miles.

May 8, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

No. 1,025.—“On Indian Railways: with a Description of the Great Indian Peninsula Railway.”¹ By JAMES JOHN BERKLEY, M. Inst. C.E.

THE importance of Railways in British India is more correctly represented by the extent of the country, its population, and the well-known richness and variety of its produce, which constitute the source of all railway traffic, whether internal, or foreign, than by its commerce. This distinction in favour of railway prospects is illustrated to a remarkable degree, by the statistics of India; for though its commerce in the year 1858-59, amounted only to £60,219,660 sterling, its population is estimated at 185,000,000, of which 132,000,000 are in the British dominions. Its area is 1,484,367 square miles; and its principal producing districts are as rich and extensive as the possessions of any civilised nation, which has made the growth of commerce an important element of its policy. The cause of the commerce of India having continued so incommensurate with the resources of the country, which has been under British rule for nearly a century, may be chiefly assigned to the want of proper communications. Its roads, with a few recent exceptions, have been little better than bad tracks, fit only for native conveyances, which have been contrived merely for rough usage and the slowest travelling. The great physical barriers have been left unsurmounted, except by these rude means, and the traffic across them has been obstructed by almost every possible kind of impediment; while the rivers have, in only a few instances, been made navigable. The usual conveyances of the country have been carts, elephants, camels, pack-bullocks, and asses.

Indian railways will not, therefore, as in England, be the substitution of a perfect system of conveyance, for other convenient means, at the demand of a prosperous nation; but they

¹ The discussion upon this Paper extended over portions of two evenings, but an abstract of the whole is given consecutively.

will be, at least in many districts, the first introduction of any communication whatever, adapted for the requirements of the country. Their effects in the creation and development of traffic must, consequently, be very considerable. It is not, however, from these considerations alone, that the subject of Indian railways may claim the attention of the Institution of Civil Engineers. It is rather in the remoteness of the country, and the novelty of its physical character, in the peculiarities of its labouring classes, of its soil and climate, and of the materials it produces, that matter worthy of being placed upon record, may be found.

In the year 1849, the Indian Government took the first decided step, towards the establishment of a system of railways in India, by entering into arrangements with the East Indian and the Great Indian Peninsula Railway Companies, for the construction of railroads in the Presidencies of Bengal and Bombay. Since that date the formation of 4,821 miles has been sanctioned; 636 miles have been opened for traffic; 765 miles are expected to be opened in 1860, and 864 miles in 1861. The estimated capital required to complete these lines, amounts to £52,450,000; of which sum the expenditure of £31,133,300 has been sanctioned by the Indian Government, and £27,129,622, has already been subscribed.

The arrangements under which the Indian railways, now in course of construction, are established, consist of a Government guarantee, as the means of raising the requisite capital; the agency of Incorporated Companies, to design, execute, and manage them; and Government supervision to define the projects, and control the proceedings and expenditure.

The terms of the contract between the Government and the Companies, may be thus briefly stated:—

The Government guarantees, for ninety-nine years, a dividend of $4\frac{1}{2}$ to 5 per cent. upon the estimated cost of the railway.

They supply, free of cost, all the land requisite for the line and the stations.

They have power of approval, or disapproval of all lines laid out, and of their estimated cost; and they also exercise a general control over the affairs of the Company.

They appoint a Director, who has, with one, or two exceptions, a general veto on the proceedings. The cash is paid into the Government Treasury; accounts are kept as directed, and are always accessible.

They regulate the tolls, time tables, &c.; and stipulate for mails being carried free, as well as for officers, soldiers and Government stores being conveyed on advantageous terms.

When lines pay more than the guaranteed dividend, one-half

the surplus is to be retained. until the arrears of interest paid by Government have been repaid ; and terms are made for the purchase of lines, rolling stock, &c. under certain conditions, previous to the termination of ninety-nine years, when, if the Railway Company have given no notice for sale thereof as arranged, the property will pass into the hands of Government.

These terms and the system they constitute, may, so far as they have been brought into operation, be pronounced to have been successful ; although attended with some disadvantages. On the one hand, the following beneficial results may be assigned to the guarantee system. The requisite capital has always been forthcoming with remarkable facility, and Indian railway stock has occupied a firm position in public confidence ; the finished portions of the undertakings have been made at a comparatively low mileage cost ; the land has been provided with unusual despatch ; there has been scarcely any litigation ; (this enormous item of English railway expenditure has, in fact, been almost eliminated from the accounts ;) the public traffic has been carried on, for seven years, with perfect safety ; and lastly, Indian railways stand well in public estimation, and will, most probably, continue to do so, in consequence of the thorough and independent scrutiny to which they are constantly subject.

On the other hand, it must be acknowledged, that these many and important advantages have not been obtained, without some countervailing sacrifice. There has, undoubtedly, been great loss of time, which has occasioned serious inconvenience to the public, and especially to the State, during the late mutiny. In the earlier operations of the Companies, supervision was exercised with an unnecessary degree of minuteness, which was one cause of misunderstanding and delay. It must, however, be remembered, that railway practice was new to the controlling authorities, and that they had to acquire experience, to satisfy themselves of the capabilities of the Companies' agents, to store up precedents for future guidance, and to note many details, to which peculiar value attached, so long as Indian railways were regarded only as experimental. The most protracted delays, however, arose from the multifarious authorities, whose opinions had to be brought into unanimity, before any important business could be proceeded with, and from the great quantity of official labour, which was involved by every reference to so many widely-scattered tribunals. There was a Local Committee of Railway Directors, or a Managing Agent ; the Local Government ; the London Board of Directors ; the Supreme Government ; the Honourable Court of Directors ; and the Board of Control. Amongst so many authorities, dif-

ferences of opinion frequently arose, discussion and correspondence were freely had recourse to, and unfortunately, the more urgent and important the subject, the greater was the delay. Ten years have elapsed, since the Great Indian Peninsula Railway Company was in a position to commence operations; yet only a small proportion of that period has been occupied in the construction of any of the several sections of the line, and in every case, considerable time was spent in preliminaries. The periods occupied in constructing those sections have varied, from only one year and a quarter to four years; and it is, therefore, evident, that the slow progress has not been occasioned by a want of energy in constructing, but of promptitude in deciding upon the projects, and of simultaneousness in carrying them out to the full extent to which they were needed by the country. It is solely to the working of the guarantee system, that these defects can, it is thought, be attributed. Recently, however, the transaction of railway business has been simplified and improved, for less delay than was formerly experienced, is now found to attend it, and an effectual co-operation generally subsists between the several Companies and their supervising authorities.

It has been asserted, that the interest accruing from delay in constructing the Indian railways, will seriously affect economy. Though this may be a sound argument for despatch, it is an exaggeration of the actual evil; for the capital being called up only by instalments, as the various sections of the line come forward for construction, and the time spent in making them not being unusually long, interest is rarely allowed to accumulate for any lengthened period. A similar charge for interest on unproductive capital has really stood to the debit of all railways, at the date of their completion; and it would be unfair to charge Indian railways with unusual excess under this head, except in those instances where delay has occurred between the raising of capital, and the commencement of active operations upon the lines for which it was provided. There is, however, another item of expenditure attaching to the guarantee system, which ought not to be passed unnoticed, the disbursements which the Local and Home Governments have had to incur, in the performance of those special functions which appertain to them under the supervising system.

It has been considered important, that uniformity should be established, as far as possible, on Indian railways; the following standard dimensions have, therefore, been adopted. The uniform gauge is 5 feet 6 inches, and the minimum clear width between the tracks is 6 feet. The minimum clear distance of the platform wall from the rail, is 2 feet 6 inches, and the projection of the nosing of the platform, not more than 3 inches. The minimum clear height for all over-openings above the rails, in the

[1859-60. N.S.]

centre of each line, is 14 feet 6 inches. The breadth from centre to centre of the buffers is 6 feet 5 inches, and the height of the centre of the buffers above the rail is 3 feet 6 inches, for all the rolling stock. The goods-loading gauge for interchange of traffic is to be settled to a pattern. In the passenger carriages, the extreme width of the body is 8 feet 6 inches; of the roof, 10 feet 6 inches; and of the steps, 10 feet. The maximum length between the centres of the wheels of both carriages and waggons is 11 feet, and they are to have spring buffers and draw springs at both ends. The minimum diameter of the turn-tables is 15 feet, and the engine and tender are to turn on tables, 40 feet in diameter.

Thus far attention has been directed to the subject of Indian Railways in general.

THE GREAT INDIAN PENINSULA RAILWAY will now form the special subject of this communication. The principal lines of this railway are proposed to extend from the port and city of Bombay, to join the East Indian Line at Jubbulpore on the north-east, with a long branch to Nagpore, and to join the Madras Line on the south-east, at, or about the River Kristna.

Bombay, the western terminus to which the trunk lines and all others connected with the undertaking, converge, is a justly celebrated port. Its population now numbers about 700,000, consisting of Europeans and Asiatics of all castes and races. The advantages of its situation, in the centre of the western coast of the peninsula of India, and of its safe and capacious harbour, are obvious; and these, as well as the recent preponderance of trade, distinguish it as the commercial capital of India. It is the depôt of the Indian navy, and the number of square-rigged vessels and native craft which entered and departed from the harbour in 1858-59, amounted to

1,200 square-rigged vessels and steamers . .	892,218 tons
10,873 native craft	460,714 „
<u>12,073</u>	<u>Totals . . . 1,352,932 „</u>

It exported, in 1859, 206,915,874 lbs. of cotton, valued at £3,957,639 sterling. The produce of the customs has risen from £3,024,000 to £6,169,200 per annum, more than double, in only five years. Its commerce in merchandise and treasure amount, for 1858-59, to a total of £34,332,423, the imports being £18,381,541, and the exports, £15,950,882, or £9,000,000 more than the whole foreign commerce of India, in the year 1848.

The chief imports are;—cotton and woollen goods, machinery,

metals, wine, spirits and malt liquors, military and naval stores, railway materials, ivory, spices, silk, sugar, tea, coffee, tobacco, horses, drugs and dyes, fruits, precious stones, books and stationery, grain, seed, oil, timber, ice, apparel and treasure. These are derived from the United Kingdom, Africa, China, Penang, Singapore, the Straits of Malacca, the Persian Gulf, Suez, Calcutta, Malabar, North America, the Arabian Gulf, Batavia and Java, Ceylon, France, the Mauritius, Aden, Cutch and Guzerat.

The chief exports are :—cotton, hides and skins, oils, saltpetre, seeds, Cashmere shawls, wool, opium, coffee, dyes, sugar, tea, grain, provisions, precious stones, beads, metals, spices, salt and fruits ; and they are generally consigned to the same places whence the imports are derived.

Bombay is already an established field for industrial enterprise, and many large manufacturing establishments are in active operation. It now stands in urgent need of the following additional accommodation :—new docks,—enlarged custom-house premises,—extended wharfage and quay accommodation, with covered sheds,—large warehouses,—and an improved system of landing cargo.

So crowded, indeed, is the city, that one of the most difficult matters to be decided is, where to find a commodious site for the railway terminus, which has, hitherto, been only of a temporary character. There has, in the course of the last few years, been an extraordinary rise in wages, varying from 25 to 100 per cent., and a corresponding increase in the prices of native materials requisite for public works. These and other signs of prosperity it is impossible to call in question, and they clearly indicate a great and rapid improvement.

With this port and metropolis as a western terminus, the Great Indian Peninsula Railway will command a portion of the great traffic of the north-western provinces of India ; the opium fields of Malwa ; the grain and seed provinces of the valley of the Nerbudda ; the vast cotton fields of Berar, of the Nizam's dominions and of Sholapore ; and the trans-peninsular traffic from Calcutta and Madras. The Syhadree Mountains, or, as they are commonly called, the Ghauts, intercept the channel of communication for this vast and important traffic. They lie parallel to the coast, from which they are distant about 30 miles, and, in a range of about 100 miles, there are only two main roads by which wheeled traffic can cross them ; the Agra road up the Thul Ghaut, and the Poonah and Calcutta road up the Bhore Ghaut. The rest of the passes are rough tracks, fit only for the use of pack-bullocks.

When regarded as a means of conveyance for a rich and extensive tract of country, capable of sending to a central port like Bombay, a large amount of produce, to supply the demands of distant competing markets, the principal characteristics of the existing

roads and means of transport in the Bombay Presidency are ;— extreme slowness, only about twelve miles per day, —uncontrollable irregularity, —great cost of conveyance, amounting to $3\frac{1}{2}d.$ to $4\frac{1}{2}d.$ per ton, per mile, —physical obstructions, and the crowded condition of the roads, —the short duration of the favourable season for conveyance, —the limited extent of the present means of carriage, —and the damage to goods, and losses by theft and from bad weather, upon the journey.

These difficulties particularly affect the very important export of cotton, the beneficial growth, package, conveyance, and sale of which, are much influenced by the want of the facilities which will be afforded by the railways in course of construction. This railway system will operate most beneficially upon the whole of the commercial transactions relative to cotton. It will give facilities for local superintendence, and thus help to bring about the improvement of cultivation, to promote greater cleanliness in the subsequent process, to neutralise the influence of the Brahmins, and to prevent fraud by the middlemen. It will free commerce from loss and damage on the journey, by either preventing, or giving compensation for them. It will establish one regular continuous journey from the depôts to Bombay, and will insure perfect independence of the seasons, so far, at least, as regards conveyance. It will afford the Bombay merchants great advantages for shipment and freight to England, and it will give more independence to this species of commerce, by balancing the return loads. The aggregate benefits of the railway system, and its legitimate consequences upon the cotton supply of Western India, have been estimated at as much as $2d.$ per lb., and there is little doubt, that a considerable part of this economy will be effected in the course of a few years, by its various beneficial operations upon that important branch of Indian commerce.

The first section undertaken by the Great Indian Peninsula Railway Company, was that from Bombay to Callian, 33 miles, with a branch to Mahim of $1\frac{1}{2}$ mile ; it was called the ' Experimental Line.' It was begun by Messrs. Faviell and Fowler in February, 1851, and was finished by them, in conjunction with Messrs. Wythes and Jackson, and Mr. Jamsetjee Dorabjee, a Parsee, in April, 1854. The portion from Bombay to Tannah, a distance of 20 miles, was the first railway opened in India for public traffic, which event took place on the 16th of April, 1853. From Bombay to Callian, a double line of rails has been laid. Its steepest gradient is 1 in 150, and the radius of the sharpest curve is 40 chains. The Bombay terminus is at Boree Bunder, having the advantage of a quay frontage to the harbour, and although it occupies an area of 19 acres, it is already overcrowded. The site not being permanent, all the buildings are of

a temporary character. The Company's depôt for working the lines is situated at Bycullah, about 2 miles from the terminus. It covers an area of $18\frac{3}{4}$ acres, and contains steam sheds, erecting and fitting shops, smithies, iron and brass foundries, saw mill, carriage-repairing and waggon-building sheds, stores, warehouses, coke shed, a timber-preserving establishment, offices, and workmen's and engine-drivers' dwelling-houses.

The principal works upon the Experimental Line are the crossing of the Sion Marsh, which is effected by an embankment; the crossing of the arm of the sea from the island of Salsette to the Concan, comprising two viaducts, the length, respectively, of 111 yards and 193 yards, in the latter of which there is an opening for navigation, of 84 feet, spanned by wrought-iron plate girders; beyond this, there are two tunnels of the respective lengths of 103 yards and 115 yards. The railway is protected by post and rail fences, and prickly-pear and cactus hedges. The station buildings are of masonry. The permanent way is chiefly laid with transverse wooden sleepers, and 6 miles of it with iron pot-sleepers. The rails, which are of the double T form, weigh 84 lbs. per lineal yard, as far as Tannah; beyond which place, they weigh only 65 lbs. and 68 lbs. per yard. The lighter rails extend along the whole of the Company's main lines, except the two Ghaut Inclines, on which will be laid rails of 85 lbs. per yard. From Callian diverge,—the South-Eastern Extension to Poonah and Sholapore, and its proposed prolongation to the River Kristna and the Madras Railway,—and the North-Eastern Extension to Nassick and Jubbulpore, to join the East Indian Railway from Calcutta, by which a communication will also be effected with the North-Western Provinces of India.

The first section of the South-Eastern Extension is from Callian to Campoollee, at the foot of the Bhoze Ghaut mail-road. It is $37\frac{3}{4}$ miles in length, of which $30\frac{1}{4}$ miles, to the foot of the railway incline, are permanent, the remainder having been designed for temporary use, until the Ghaut Incline was opened. This portion of the railway was begun by Mr. Jamsetjee Dorabjee in 1854, and was finished in 1856. It has been constructed for a double line, but only one road has been laid. Its ruling gradient is 1 in 115 on the permanent, and 1 in 85 on the temporary portion. The radius of its sharpest curve is 40 chains. It contains no work of any special character, but it is remarkable for the extraordinary floods and rapid torrents to which it is exposed on both sides. The bridges and culverts are built of rubble masonry, with coursed facework; and in one, or two instances, cast-iron girders were made use of. The average cost of this section, exclusive of rolling stock, was only £4,500 per mile.

BORE GHAUT INCLINE.—Four years were spent in preliminary surveys of this incline, and in laying out and preparing cross sections, to the number of about two thousand, and perhaps, the most difficult that have ever been taken. Four years more have already expired since the contractor commenced operations upon it. The works were begun by Mr. Faviell in January, 1856, and taken up in November, 1859, by Mr. S. Tredwell, whose melancholy death within a month of his arrival in India, many members of the profession must sincerely lament. It is expected to be finished about three years hence. It is 15 miles 68 chains in length, and the total rise is 1,831 feet. Its average gradient is 1 in 48. The steepest gradients are 1 in 37 and 1 in 40, for a total length of 9 miles and 44 chains; 1 in 37 extends in one length for 1 mile 10 chains, and 1 in 40 for 5 miles 6 chains. Short lengths of level gradients and of 1 in 330 are introduced into this incline, to facilitate the ascent of the engine. The radii of the curves upon it range from 15 chains to 80 chains; but as much as 12 miles 45 chains have a radius of more than 30 chains, and 5 miles 33 chains are straight. It comprises twenty-five tunnels, of a total length of 3,585 yards. The longest is 437 yards; and the longest without a shaft, which is carried through a mountain of basalt, is 346 yards. There are eight viaducts of a total length of 987 yards. The two largest are 168 yards long, and respectively, 163 feet and 160 feet above the foundations. The viaducts are being built, up to the surface of the ground, of solid block in course masonry, and above, of block in course facework, strongly tied through by header bonds of block in course, to the internal work of sound rubble, and with coursed rubble arches. The contract also comprises a large quantity of retaining walls. The total quantity of cutting, chiefly rock, amounts, by calculation, to 1,263,102 cubic yards. The maximum depth of cutting is 70 feet, and the greatest contents, 75,000 cubic yards of trap rock. The embankments amount to 1,849,934 cubic yards, the maximum height being 74 feet; and the greatest contents are 209,000, and 263,000 cubic yards. The slopes average about $1\frac{1}{2}$ to 1. There are twenty-three bridges of various spans, from 7 feet to 30 feet, and sixty culverts from 2 feet to 6 feet wide. The rails weigh 85 lbs. per yard, and are laid with fish joints, with small cast-iron saddles under the joints, resting upon longitudinal planks, the ends of which bear upon, and are secured by fang bolts to transverse wooden sleepers. The estimated cost of this incline is £750,000. The upper 2 miles, from Khandalla to Lanowlee, with gradients of 1 in 40 and 1 in 50, were opened on the 14th of June, 1858, and have since been worked with safety and regularity.

At the eleventh mile, the incline is divided into two banks, by

what is called a reversing station. This subdivision, however, was not adopted for the purpose of making two banks of the incline, but of increasing the length of the base, in order to flatten the gradient and to reach a higher level, where it encountered the great features of the Ghaut margin, near Khandalla. Without the necessary expedient of the reversing station, the practicability of changing the direction of the line would have been confined to making curves of small radius; but with the device of the reversing station, the direction was altered at a very acute angle, by means of points and crossings. In consequence of its adoption, the incline is prolonged by nearly the difference between the length of the two sides of an acute-angled triangle, and that of its base.

The limits of this communication will not admit of the proper treatment of this subject, nor of that of the Thul Ghaut Incline, either of which alone demands a separate Paper.

The following Table presents a comparison between the Bhore Ghaut, and the Giovi and Sömmering Inclines :—

Name of Incline.	Length.	Total Ascent.	Average Gradient.	Maximum Gradient.	Sharpest Curves.	Total length of Tunnelling.
	Miles.	Feet.				Miles.
Giovi Incline . . .	6	889	1 in 36	1 in 29	{ 20 chains radius. }	2·55
Sömmering Incline :— Ascent from Payerback to Sömmering . . .	13½	1,325	1 in 47	1 in 40	{ 30 curves of 10 chains radius, and 38 curves of 14 chains radius. }	2·66
Descent from Sömmering to Murzschlag . . .	8½	705	1 in 50	1 in 50	{ 1 of 15, and 2 of 20 chains radius. }	
Bhore Ghaut Incline	15½	1,831	1 in 48	1 in 37		1·44

The peculiar difficulties upon this incline are the unfavourable nature of the hot and rainy seasons; the fatal epidemics which dismay and disperse the people employed upon it; the lofty and precipitous character of the ground, which impedes the haulage of materials and harasses all who are engaged in the operations; the extensive and sudden slips upon the mountain sides; the extreme hardness and solidity of the rocks; the scarcity of water; and the want of necessaries and comforts for the men.

Setting aside all consideration of the design, the Bhore Ghaut Incline is, in point of construction, a stupendous mass of works, crowded upon an unhealthy, desolate, and inaccessible mountain scarp; and it can only be completed by a display from all parties employed, of resolute endurance and unflagging energy.

The next section of the South-Eastern Extension is from Lanowlee, the summit of the Bhore Ghaut Incline, to Poonah and Sholapore, and is 205½ miles in length. The first 42 miles were begun by Mr. Faviell in January, 1856, and were finished in June, 1858. The remaining 163½ miles were commenced by Mr. J. Bray, in March, 1856, and 143 miles were completed on the 27th of December, 1859. Its engineering character is very similar to that of the-Concan section. Its ruling gradient is 1 in 132, and the radius of its sharpest curve is 40 chains. The cuttings are in trap rock, moorum, and soil; and the embankments are chiefly composed of soil and moorum. There are twenty-two viaducts, three hundred and fifty-nine bridges, and four hundred and fifty-four culverts, all built of substantial masonry. The largest works are the viaducts over the Beema, 441 yards long and 60 feet high, consisting of twenty-eight segmental arches 40 feet in span, with a flood stream 46 feet deep, and rock foundations, the cost of which was £24,246; and that over the River Seena, 190 yards long, 54 feet high, consisting of twelve segmental arches 40 feet in span, with a flood stream 41 feet deep, and foundations partly in rock and partly on hard clay. The fences are dry rubble walls, and cast-iron posts and iron-wire rails. One peculiarity of this district is the violence and suddenness of the floods, which descend with scarcely an hour's notice, and gather into torrents on spots upon which there is no trace, or warning of any stream. In the uncommenced portion of the South-Eastern Extension, from Sholapore to the junction with the Madras Line, in the Raichore district, there will be two very large viaducts over the Rivers Beema and Kristna. Upon the South-Eastern Extension large quantities of cotton and country produce are now carried, and it is evident, that an immense traffic must soon be accommodated. The earth-work has been executed for a single line, and the viaducts and bridges for a double line.

Returning to Callian, the first section of the North-Eastern Extension, which there diverges towards Jubbulpore and Calcutta, is from Callian to Kussarah, 26 miles, gradually climbing, by steep gradients, of which a great portion are 1 in 100, the flank of a long mountain spur, which projects from the Ghaut range, and divides the valley of the Basta on the south, from the Wyturnee on the north. This section is full of heavy work; but to obtain even such a line, demanded a long and minute study of the rugged and jungle-covered district. The works have been executed by Mr. Jamsetjee Dorabjee. The radius of the sharpest curve is 30 chains. It contains 520,493 yards of cutting, chiefly trap and basaltic rock, and 1,353,317 cubic yards of embankment. It comprises four viaducts, of which the two largest are, respec-

tively, 124 yards and 143 yards long, and 127 feet and 122 feet high; forty-four bridges from 7 feet to 30 feet in span, and one hundred and seventeen culverts. By means of this section, 849 feet of the ascent have been surmounted to the summit of the Ghaut, and thus the altitude to be overcome by the Thul Ghaut Incline is reduced to only 972 feet.

THUL GHAUT INCLINE.—The Thul Ghaut Incline extends from the village of Kussarah to Egutpoora, and is in course of construction by Messrs. Wythes and Jackson. It is $9\frac{1}{2}$ miles in length, and has a total ascent of 972 feet. At the end of $3\frac{3}{4}$ miles there is a reversing station, similar to that upon the Bhore Ghaut Incline, by which the base was lengthened, the gradient flattened, and the incline divided into two banks. The steepest gradient is 1 in 37, for a length of 4 miles 30 chains; and the same introduction of a level portion is adopted here as on the Bhore Ghaut. The radius of the curves ranges from 17 chains to 100 chains; but of 7 miles 12 chains the radius exceeds 30 chains, and 3 miles 28 chains are straight. There are thirteen tunnels, of a total length of 2,652 yards. The longest are, one of 474 yards, in black basalt, with two shafts, and another of 483 yards, without a shaft, in greenstone. There are six viaducts, of a total length of 741 yards, the largest of which are, respectively, 144 yards and 250 yards long, and 83 feet and 182 feet high. The latter is designed for three spans of triangular iron girders measuring 150 feet, with a pair of semicircular abutment arches measuring 40 feet at each end. There are fifteen bridges, of which the span varies from 7 feet to 30 feet, and sixty-two culverts. The cost of the incline will be about £450,000. The preliminary surveys and studies occupied four years, and the works were commenced in October, 1857.

The next section of the North-Eastern Extension runs from the summit of the Thul Ghaut Incline, at Egutpoora, by Nassick, across the fertile valley of the Godavery, and the Indyahadree range of mountains, along Khandeish to Bhosawul, the point of junction with the Oomrawuttee and Nagpore Branch. The character of this line is very similar to the corresponding section of the South-Eastern Extension, from the Bhore Ghaut to Sholapore, and the nature of the earthwork is much the same. The principal works upon it are:—a viaduct over the Godavery, 145 yards in length, consisting of nine arches 40 feet in span, with a flood stream 36 feet deep, and foundations upon rock, excavated through sand; the Kadoo Viaduct, 242 yards in length, with fifteen arches 40 feet in span; the Munnair Viaduct, with five openings spanned by triangular iron girders, and two pairs of abutment

arches ; and the Waugoor Viaduct, with ten openings also spanned by triangular iron girders. This section contains twenty viaducts, two hundred and seventy-nine bridges, and four hundred and thirty-five culverts. It was commenced by Messrs. Wythes and Jackson, in October, 1857.

The last section of the North-Eastern Extension runs from Bhosawul to Jubbulpoor. It is 328 miles in length, and was contracted for by Messrs. Duckett and Stead, in January, 1859. As the operations are in a preliminary state, it is only necessary to notice the two very large viaducts over the Rivers Taptee and Nerbudda. The Taptee Viaduct is 875 yards long, consisting of five openings of 138 feet and fourteen openings of 60 feet, and twenty arches 40 feet in span, with a flood stream 70 feet in depth, and foundations upon rock. The Nerbudda Viaduct will be about 387 yards long, 100 feet high, with a flood stream 90 feet deep.

The Oomrawuttee and Nagpore Branch is about 263 miles in length. It was let to Messrs. Lee, Watson, and Aiton, who are now just commencing operations. As the line has not yet been entirely staked out, no details can, at present, be given ; but its general character is known to be favourable, and the works are light. The largest works will be the viaducts over the Rivers Nalgunge and Wurdah.

There is no tunnel beyond the Ghauts, upon any of the lines now under construction, comprising a length of 782 miles.

The general style of design for these trunk lines is derived from the model of the late Mr. Robert Stephenson's English railways, with such modifications as were rendered necessary by special circumstances. The propriety of adopting a line of a character so substantial and durable, has been called in question by parties who profess to be strict economists, having a thorough knowledge of India. It has been argued by them, that the American system, or even tramways, would have been far more suitable and politic ; but the following considerations will show, that this opinion is unsound. The Great Indian Peninsula Railway runs parallel to the main communications of Western India, and must, therefore, not only be adapted for the conveyance of the present large traffic of the country, but be capable of meeting the demands of that immense increase which such facilities of transport are intended, and cannot fail, to generate. It may be admitted, that long before India has been covered with such a labyrinth of lines as figure upon the map of America, cases will frequently present themselves where a cheaper style of construction will be appropriate. But an agricultural line, which might be economical and convenient in the

plains of America, and even in many provinces of India where cultivation may only become profitable when a railway is completed, would be insufficient along those main routes, where 100,000 tons per annum of bulky and valuable goods, and a large travelling population already stand in need of conveyance, and where the increase of the traffic is beyond all definite calculation. Unsubstantial lines of railway, costly to work and to maintain, would also be inappropriate for the conveyance of an extensive traffic across the ravines, and through the basaltic mountains of the Ghauts; and in the Presidency of Bombay they would prove an utter failure, as they could never withstand the four months' deluge of the monsoon, and the torrents that annually rush down from the mountains and flood the country.

The following fundamental points are observed and carried out with the greatest practicable economy and despatch, in the completion of the railway system. The character of the main lines is plain, substantial, and durable, and such as will provide for the regular and expeditious conveyance of a heavy and increasing traffic in goods, and the accommodation of a great number of passengers, at a moderate working cost, and at a reasonable expenditure in maintenance.

The geological nature of the country is volcanic; the hills and mountains consist of trap rock and laterite, a kind of ferruginous clay, so called from its frequent resemblance to brick. Granite hills protrude in the southern Mahratta country below Sholapore. The trap is of various sorts, more, or less, earthy, or crystalline, and the hills have, almost invariably, either a crest, or axis of basalt; their surface is bare, or covered with what is called in India, moorum. The basalt, sometimes highly porphyritic, is nodular, rectangular, tabular, and columnar. Moorum is of a reddish, or gray colour, and is, no doubt, decomposed rock of a very earthy nature; in the cuttings, it is found both hard and soft. In the valleys, there is a great depth of vegetable soil.

As favouring engineering operations, the destructible nature of the slopes of cuttings and embankments made of the black soil; the facility of excavating moorum, its firmness for slopes and embankments, and frequent suitability for ballast; the advantage of having rock foundations for the crossings of rivers and streams, and also that of making tunnels without either lining, or faces; and the fine quality of the stone for building purposes and the facilities of quarrying it, are worthy of special notice. As a set-off to these advantages, there are the extreme hardness of the black basalt, which renders progress both tardy and expensive, and the precipitous altitude of the mountains, which, in many cases, prevents the sinking of shafts, and thus limits the mining of tunnels to the two faces only.

Among the geological features, the existence of large quantities of kunkur, a variety of fresh-water limestone, and the want of good brick earth, must be mentioned.

In excavating the Ghauts, a valuable store of zeolites have been exposed; they are found in the bubbles, or cavities of the rocks. This class of minerals is peculiar, as they contain a great quantity of water, which evaporates with more, or less rapidity in each species, thus rendering them perishable when exposed to the atmosphere. Among them, the mesolite, poonalite, apophyllite, stilbite, and laumonite, are the most abundant. Professor Maskelyne has already placed some fine specimens in the British Museum, and their number will, no doubt, shortly be increased. They are not only beautiful, but rare, and are esteemed, in museums, as second only to the specimens of gold from Australia and California.

The physical geography of the districts of Western India traversed by the Great Indian Peninsula lines, may be thus briefly described. First, the plain of the Concan, elevated very little above the level of the sea; then the abrupt scarp of the Syhadree Mountains, the least altitude of which above the sea, is about 2,200 feet; and beyond that, the plain of the Deccan on the South-Eastern Extension, gradually sloping down towards the eastern coast of India: whilst upon the North-Eastern Extension, the country presents the bold features of the Rivers Taptee and Nerbudda, with three parallel chains of mountains called the Indyahadree, the Santpoora, and the Vyndhya ranges. The altitudes of various known spots along the railway are:—

Altitudes.	Height above High-water Mark at Bombay.
	Fect.
Level of the ground at the Bombay Terminus . . .	11
Ditto at the Callian Station . . .	16
Foot of the Bhor Ghaut Incline at Padusdhurree . . .	196
Top of ditto at Lanowlee . . .	2,027
Level of the ground at Poonah Station . . .	1,811
Ditto at Sholapore Station . . .	1,481
Ditto at Kristna, near Narrainpoor . . .	1,291
Foot of the Thul Ghaut Incline at Egutpoora . . .	940
Top of ditto . . .	1,912
Level of the ground at Jubbulpore . . .	1,218
Ditto at Oomrawuttee . . .	1,134
Ditto at Nagpore . . .	997

The physical character of the country is less favourable to the Railway Engineer than it might appear, in consequence of the extraordinary quantity of rain which falls upon the western coast during the monsoon, a period of four months from June to September. The line of the Ghauts is the axis of these rains, and

the rivers and streams which rise in it are either dry, or stagnant, during the fine weather, and become deep and violent torrents during the monsoon. The height of known floods, where the railway crosses some of the principal rivers, varies from 25 feet at the Waldhur, to about 70 feet at the Taptee, and about 90 feet at the Nerbudda. Extreme difficulty has been experienced in ascertaining the maximum heights of floods, not merely in the rivers, but in most of the numerous streams which it was necessary to cross. There is nothing upon the ground to indicate them, and the information obtained by careful and extensive inquiry has, generally, been of the most inconsistent nature. Some idea of this difficulty may be conveyed by the fact, that in the year 1837, the River Taptee, where the railway crosses it, rose 30 feet above the highest level it has since attained.

The following Table exhibits the amount of rainfall during the year 1858, at different places upon and near the western coast:—

	Inches.		Inches.
Malligaum	19	Poorundhur	41
Dhoolia	19	Baroda	41
Poonah	21	Alibag	80
Gogo	24	Bhewndy	80
Ahmednuggur	28	Bombay	85
Nassick	29	Tannah	91
Surat	30	Rutnagherry	106
Sholapore	31	Panwell	107
Suttara	31	Narel	121
Dharwar	31	Khandalla	156
Belgaum	36	Matheran	255
Kolapore	39	Mahableshtar	265

The railway materials procurable in India are, iron, coal, timber, stone, bricks, lime, gunpowder, and ballast. Indian coal and iron are very seldom seen in the Bombay market. In many parts of the Nerbudda valley, coal exists of excellent quality and in great abundance, and it lies in a favourable position for being worked. Iron ore, too, abounds, especially on the right bank of the Nerbudda. The principal mines of the district are those at Tenderkaira, near Nursingpoor, about ten miles from the Great Indian Peninsula Line. They are now worked in the rudest fashion, but the iron produced is of excellent quality. There are furnaces also at Paneghur and Gosulpoor, close to which the railway passes, and although the iron is deemed inferior to that of Tenderkaira, it is good, and forms an article of export from those towns. Valuable iron mines also exist at Poonassa and Chandghur, and there are five mines within twenty miles of Jubbulpore. Productive, however, as these may hereafter prove, it is evident, that active and successful operations in the manufacture of Indian iron, or the supply of coal, depend more upon the completion of

railway communications, than the railway depends upon a local supply of these materials.

The various kinds of wood procurable for railway purposes are of unusually good quality. The properties of some which have already been extensively used, are represented in the following statement, the specimens experimented upon being 7 feet in length, by 2 inches square in section, a size adopted in previous experiments :—

No. of Experiments.	Names of the Woods.	Specific gravity in Ounces.	Breaking weight in Pounds.	Ultimate deflexion in inches.	Cost per Cubic Foot.
5	Northern Teak . . .	880	672	5 75	s. d. 5 0
3	Southern Teak . . .	768	1,012	6·87	6 0
2	Blackwood . . .	896	924	6·75	8 0
4	Khair . . .	1,168	966	4·00	2 6
3	Errool . . .	1,008	718	4·50	2 6
3	Red Eyne . . .	1,088	559	2·50	2 6
2	Bibla . . .	896	560	5·62	2 6
3	Poon . . .	624	425	3·62	..
3	Kullum . . .	656	569	6·12	2 0
3	Hedoo . . .	624	630	4·25	2 0
2	Jungle Teak . . .	656	537	3·00	2 0

It has not been found necessary to make use of timber in the permanent bridges. The following woods have been successfully converted into sleepers :—teak, blackwood, khair, errool, and red eyne.

The cost of a sleeper 9 feet 9 inches long, 10 inches wide, and 4½ inches thick, has varied from 4s. to 7s. 7½d., and the average price has been about 6s. It was, at first, feared, that wooden sleepers could not be used in India, on account of the ravages of the white ants, but it is a curious and important fact, that although they have been very often seen, eating the sapwood of sleepers lying on the surface of the ground by the side of the railway, those laid in the ballast of the permanent road have not been injured by them.

The whole of the rolling stock, except the engines and tenders, and the first and second-class and composite carriages, is chiefly built of teak in the Company's workshops, and is strong and durable.

Excellent building stone is generally procurable, and is easily quarried. Various qualities are met with suitable either for hammer, or chisel dressing, whilst the sharpness of the rough blocks and stones adapt them for strong rubble masonry. It is of the usual hard and compact nature of the trappite, or green-stone class, and when properly selected in the quarries, it is never found to be injured by exposure to the weather. The large size of the blocks has been found most useful, for the foundations of

machinery in the Company's shops, where sound stones, containing 44 cubic feet, have been laid. Good brick earth is rarely to be met with, and the fuel for making the bricks, which commonly consists of grain husks, is very sparingly used. Consequently, although bricks are cheap and abundant, they are seldom of suitable quality for railway works. Moderately good bricks have been, occasionally, procured and used in arches, but to obtain them, a proportion of only 23 per cent. has been selected from the best native kilns. The price ranges from 10*s.* to 24*s.* per thousand. The former rate is for small-sized bricks of inferior quality; the latter are good bricks made of the English size. The lime is of a remarkably fine quality, and is hydraulic. In making chunam, or mortar, one part of lime is mixed with two parts of sand. It sets rapidly, so as to give immediate stability to the work, and continues to do so for twelve, or fifteen months, until it becomes as hard as the rock itself. Saltpetre and charcoal being easily procurable in the country, gunpowder is largely manufactured. It is very strong and suitable for blasting, and costs, when made upon the spot, about £34 per ton. The ballast consists of sand, broken stones, gray moorum, and nodular basalt.

The Bombay and district markets have greatly varied, and have been sometimes found unfavourable for extensive dealings. The railway demands being unusually large, have, occasionally, been met by a combination of native merchants, who find it easy to establish a monopoly amongst themselves, and to work it to their profit. This movement has been defeated by a variety of expedients, such as obtaining supplies from the Government stores, procuring the articles direct from the dépôt, making them by the Company's own agency, or importing them from England. The result, however, has been a great augmentation of prices, and considerable irregularity in obtaining supplies. It is one remarkable feature in Indian railway practice, that a very large, and certainly the most expensive, portion of the materials, have to be supplied from England; a circumstance which not only affects the cost, but also the progress of the works. Experience has already established the fact, that the period requisite for finishing a line for the use of public traffic in the interior of India, is not determined by the local execution of the works, but is dependent upon the delivery along the line of the permanent-way materials, station machinery, and rolling stock, which have to be procured in England, shipped to Bombay, and thence transported to the various districts of the railway.

During the year 1858-9, the shipments to Bombay of permanent-way materials, rolling stock, machinery, and miscellaneous stores, amounted to 66,937 tons, against 51,386 tons shipped in the previous year. The average sum paid in the year, for freight,

was £2. 0s. 9d. per ton of dead weight; the lowest rate, 20s. per ton, having been paid in July, 1858, and the highest rate, 60s. per ton, in June, 1859; an increase of 29 per cent. upon the average rate of the previous year.

Many of the articles sent from England have been specially designed for Indian use, their main principles being strength, simplicity, and durability, with as much regard to facility of transport as those essential properties would admit.

Greaves's iron sleepers, as used by Mr. Robert Stephenson in Egypt, have been laid on portions of the Great Indian Peninsula Railway, not, however, on account of any objections to the wooden sleepers procured in the country, but because of the difficulty of obtaining a sufficient and timely supply of them. A store of English iron sleepers has, therefore, been found convenient for meeting emergencies, and experience has shown, that they are handy for transport, that they are quickly and well laid by native labourers, and that they make a good and durable permanent way. A few consignments of creosoted sleepers have also been despatched to Bombay. They proved to be more expensive than Indian wooden sleepers, and were also very liable to split during exposure, between the time of landing and their being laid down. A complete apparatus has been sent out, and fitted in Bombay, for dressing timber with corrosive sublimate; as the climate has proved favourable for kyanising. The Indian sleepers which have been dressed, have absorbed about $3\frac{1}{4}$ gallons of corrosive sublimate; and the cost, including haulage, has been 1s. 6d. per sleeper.

An iron goods-shed and an iron booking-office were supplied from England, in 1853. Their comparative dearness may, perhaps, be compensated for, by their durability and cheapness in repairing, and they are convenient for removal; but, on account of their great heat, they have been unsuccessful, notwithstanding well-devised means of ventilation. In future, any iron buildings imported from England, should consist merely of framework, the large surfaces in the sides and upon the roof being afterwards filled in with non-conducting Indian materials.

Native labour by which these works have been executed, is cheap, but very inferior to that of England. Nearly one hundred thousand men have been employed upon the Great Indian Peninsula Railway lines at the same time, and as many as forty-two thousand upon the Bhoze Ghaut Incline, which is 15 $\frac{1}{2}$ miles in length. This great force has not been collected without considerable trouble; it is not entirely supplied by the local districts, but is gathered from distant sources. Labourers sometimes tramp for work as in England, and on the same work may be seen men from Lucknow, Guzerat, and Sattara. The wants of the works have, however, been supplied by unusual exertions in sending

messengers in all directions, and by making advances to muckadums, or gangers, upon a promise to join the work with bodies of men at the proper season. Country artisans and skilled labourers have their own methods of doing work, but are capable of improvement and are not averse to change their practice. For operations requiring physical force, the low-caste natives who eat flesh and drink spirits, are the best; but for all the better kinds of workmanship, masonry, bricklaying, carpentry, for instance, the higher castes surpass them. Miners are, on the whole, the best class in the country. The natives strictly observe their caste regulations, yet will readily fall into an organisation upon particular works, to which they will faithfully adhere, and in which they are by no means devoid of interest. Although they cling closely to their gangers, they will attach themselves to those European inspectors who treat them kindly. The effective work of almost every individual labourer in India, falls far short of the result obtained in England. This is a disadvantage upon works, the dimensions, and proper mode of execution of which, limit the number of men that can be employed at one time, because the rate of progress is proportionally diminished. The fine season of eight months is favourable for Indian railway operations, but on the other hand, fatal epidemics, such as cholera and fever, often break out, and the labourers are, generally, of such a feeble constitution, and so badly provided with shelter and clothing, that they speedily succumb to those diseases, and the benefits of the fine weather are, thereby, temporarily lost. They work under the immediate control of a ganger, or muckadum, and the various gangs, under a maistry, or native foreman; and the whole under the inspection of a European overseer of works, by whom interpreters are also usually required. Not only men, but women and children, are employed upon Indian works; and thus, although the wages of the individual are small, the earnings of his family are by no means inconsiderable. The present rates of wages, per day, of the several classes of native labourers are:—

Native Maistries, or Foremen of Masonry,	s.	d.
Brickwork, or Carpentry	2	6
Masons	1	9
Bricklayers	1	3
Carpenters	1	6
Smiths	2	0
Miners (a very large class)	0	9
Excavators	0	7½
Labourers	0	6

These rates are very low as compared with English wages, but allowing for the comparatively small effectiveness of Indian
[1859-60. N.S.] 2 s

labour, the following may be safely taken as the relative cost of each kind of labour, in England and the Bombay Presidency.

Classes of Labour.	Proportion of Work done by each.		Relative cost of Labour in each Country.	
	England.	Bombay.	England.	Bombay.
Masons	2½	1	1½	1
Bricklayers	4	1	1	1
Carpenters	3	1	1½	1
Miners.	3	1	2½	1
Excavators	4	1	1½	1
Labourers	3½	1	1½	1

In examining this Table, it should be borne in mind, that the comparison is between simple labour only, and that it does not represent the cost of finished work; for the economy in favour of India suffers, from the expense of obtaining the powerful aid of English appliances, from defective and clumsy methods, and from a variety of drawbacks and disadvantages peculiar to native customs. Strikes, although of rare occurrence, have, occasionally, taken place, and the truck system, commonly discountenanced at home, is beneficial in India.

The whole of the Great Indian Peninsula Railway has been successfully executed by contract. This adoption of the English example has elicited a bona fide competition in the public market, and has effected remarkable economy in the construction of the lines. It has insured the execution of the works with considerable despatch, for although the allotted periods have been short, all the contracts have been completed with a fair degree of punctuality. It has enabled the Company to draw largely and advantageously upon the resources of the country, both in labour and materials, without suddenly, or unduly affecting the public markets, which must have been the case, had the Railway Company conducted its own enormous transactions, instead of easing the extraordinary demand by dividing it among so many contracting agents, with such different connections and modes of negotiating business. It has, probably, been more successful than any other system, in making use of European mechanical appliances for the execution of railway works, and in effecting an extensive and powerful organisation of native labour, improved and controlled by European superintendence and co-operation; it has, moreover, introduced a class of wealthy and useful men, who have formed the strongest link in India that could be established,—the conviction that it affords them a profitable field of enterprise for the employment of their capital, experience, and agency.

A very large engineering establishment, containing some most

competent and rising young Engineers, has been created for the performance of the required professional duties, and considering the severity of the service in this climate, where they have had to execute extensive works of a novel description, with untrained labour, it is remarkable how little change, or casualty has occurred among the Company's officers, many of whom have, during many years, acquired very valuable experience.

The introduction of the contract system into India on a wide scale, is a very important effect of railway enterprise, and its advantage cannot be long confined to railway construction, for it will prove the best and most effectual agency for the execution of the many large public works, which will, ere long, be projected. Speaking generally, the European contractors have, as a rule, been more successful than natives, because the native tenders for the principal contracts have, usually, been either too high, or unreasonably low, so that, looking to their inexperience of such works, it has not often been thought desirable to take advantage of their agency, in the construction of the main lines. There has, however, been one remarkable instance of the employment of native enterprise; a Parsee contractor, Mr. Jamsetjee Dorabjee, has executed four main-line contracts as satisfactorily, as expeditiously, and as cheaply, as any of the European firms, and is now about completing his fifth, which comprises some of the heaviest works on the lines.

In other respects, native agency has been employed and encouraged, as much as possible. The Company's Engineers, Assistant Engineers, and Surveyors are generally Europeans, but one native Engineer has won his way to the office of Assistant Engineer, and has skilfully discharged its duties for three years. In the office establishment of draftsmen, accountants, and clerks, all the situations have been held by natives. As inspectors of work, natives have been chiefly employed. As district inspectors of the line, when opened, native agency is already partially adopted, and is, by encouragement, gradually becoming more useful. The principle to be kept in view is, that only by means of European and native co-operation, can the great railway undertakings which are required in the Bombay Presidency, be accomplished with due despatch. European skill, experience, and management, are of primary importance, but native agency has proved much more valuable and efficient than was anticipated, and will, undoubtedly, be found capable of considerable and rapid growth, if it is adopted without prejudice, and is treated with equity; and if native employes of all classes are stimulated to improve themselves, by the assurance of their gradual advancement, according to merit.

In the methods of executing the works two objects have to be

kept in view; first, to turn those of the natives to the best account; and secondly, to introduce English appliances where it can be done with advantage. It has not always been obvious which of these measures would, under the circumstances, be the more effectual, and experience has taught, that some Indian modes of doing work, which seemed barbarous and clumsy, were the cheapest and quickest means which could be employed.

Tunnels may be said to be an entirely new description of work in Western India, and the whole process, except blasting and excavation, was unknown to native workmen. In the earliest tunnels, where the top was heavy, it was found, at first, impossible to keep native miners in the heading, and the timbering was done chiefly by Europeans and one, or two Parsee carpenters, and the arch was keyed in by the former alone. Native miners use the churn drill, with which they are very handy, and they have sometimes been brought to work in pairs with the hammer, and strike with dexterity. They will work hard in close contact, and in the foulest atmosphere. They are careless in blasting operations, and consequently, the loss of life has been considerable; miners have been seen to fire a shot with a bamboo, and lie upon the ground while it exploded.

In building viaducts and bridges, there is a mixture of appliances and operations. In pumping foundations, English pumps only are sometimes used, but they are often aided by the native methods of the Persian wheel and by the Mahratta moat, a leather bag containing about 35 gallons of water, which is lowered and lifted over a pulley by bullock power. The natives also have various hand devices for scooping, or baling water from the bridge pits, all of which are occasionally resorted to. Where water has to be brought, it is carried in leather bags called puckals, laden in pairs on bullocks' backs. In some districts, it is thus conveyed more than two miles, to the masonry.

In staging and scaffolding it is only rarely, and in very large works, that the English example has been followed, nor are crabs and derricks so often met with as might be expected. The reason for this is afforded by experience, which has taught how cheap and expeditious it sometimes is, to use the native process. The bamboo coolies, or carriers of heavy weights, will lift their loads up the roughest staging, and the masons and labourers require but little help, to find their way to the work at the top of the highest piers. The centering commonly adopted in the country, was to fill up the arch with stone and earth, and to shape the top to the form of the soffit, or at other times, to use almost a forest of jungle wood in scaffolding a rough centre. For these, centres of English construction have invariably been substituted, with, as may be conceived, immense advantage to the work.

The native sawyers always work in pairs, even in the smallest jobs. The sawing is so inferior, that a great deal of adzing is requisite, and much of the work that would be planed in England, is turned out roughly with the adze in Bombay. In planing there is the same peculiarity of working in pairs; carpenters squat to their work, and it is with extreme difficulty, that a few of them have been brought to stand to a bench. It is remarkable to observe how freely they call in the aid of their feet; a carpenter may be seen resting his weight upon one foot, and cleaning his axe with the sole of the other.

In making embankments, the Hindoo custom of carrying the earth in baskets upon the head, is, owing to the cheapness of land for side cuttings, found more economical than waggon roads with long 'leads', and, judging by the result, it is attended with very little sacrifice of despatch. Within one month, 30,000 cubic yards have been put, by this means, into an embankment, only 24 chains in length.

Smiths' and foundry work is moderately good, but the class being so small and the material being English, almost all supplies of manufactured iron have had to be procured from England. Plate-laying was of course entirely a new operation, yet a large quantity of excellent work has been turned out by native labour under close inspection.

Among the mechanical improvements and innovations made by railway construction, may be enumerated, the use of barrows, dobbin carts, waggons and waggon roads, lorries, both wooden and iron, water pumps, crabs, moulding tables for brickmaking, stationary-engine power for sawing, pumping, and working tunnel shafts, hammer drilling, bridge centering, pile driving, timber pickling, and various kinds of iron superstructure for bridges.

The average cost of the opened portions of the line has been from about £8,000 to £9,000 per mile.

The prices of the principal kinds of work, including all the usual contract stipulations, have ranged as follows:—

		£.	s.	d.	£.	s.	d.
Earthwork in embankment under a quarter of a mile lead } per cubic yard, from		0	0	6	to	0	0
Cutting in earth, or moorum, ditto	"	0	0	7½	"	0	0
Ditto in rock, ditto	"	0	1	1	"	0	2
Tunnel	"	21	10	0	"	33	0
Brickwork in arches	"	0	15	0	"	1	10
Coursed rubble masonry in ditto	"	1	7	0	"	1	16
Ashlar	cubic foot	0	1	7½	"	0	3
Block in course	cubic yard	0	16	0	"	1	15
Coursed rubble	"	0	14	0	"	1	4
Rubble	"	0	9	0	"	0	14
Woodwork (teak)	cubic foot	0	4	0	"	0	6
Ballast	cubic yard	0	1	1½	"	0	1
Laying permanent road	lineal yard	0	2	0	"	0	2
Post and rail fence	"	0	1	6	"	0	2
Dry rubble wall	"	0	2	6	"	0	4

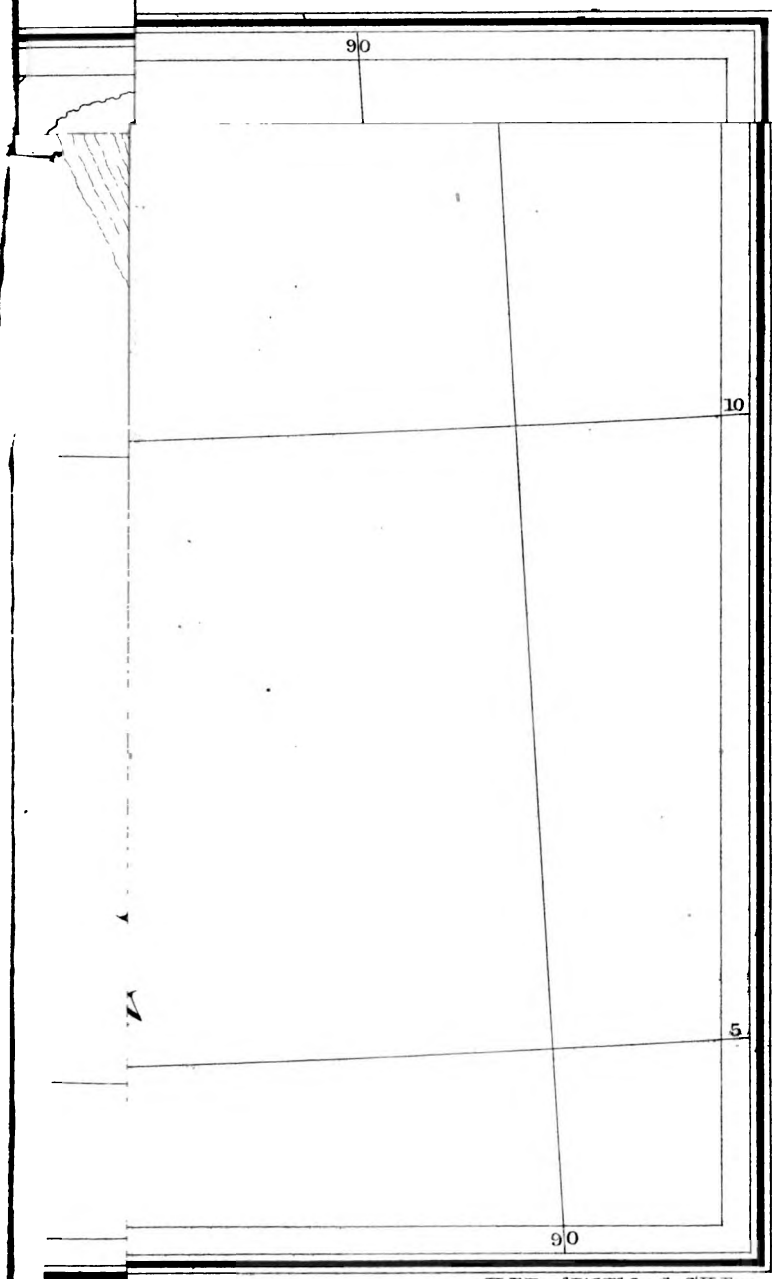
This does not include the Ghaut Inclines, which are exceptional.

Railway enterprise has already produced important effects in Western India. It has earned, at a remarkably low tariff, more than the guaranteed dividend. The working expenses have been moderate, notwithstanding the dearness of imported fuel and European superintendence. It has afforded the advantages of the best mode of conveyance, to immense numbers of the poorest and lowest orders of the people. It has generated for itself new sources of traffic. It has achieved its success in competition with water carriage, and when it was in only a fragmental state. Although constructed in what is, erroneously, called an expensive style, the traffic has already demanded the partial laying of a second line of rails. Within the Bombay Presidency, it has given simultaneous employment to one hundred thousand natives. It has raised the wages and increased the effectiveness of native labour, by encouragement, improvement, and organisation. It has, necessarily, employed thousands of the carriers of the country. It has greatly increased the tonnage of the port. It is expending about £1,000,000 sterling per annum in the districts, among small traders, ryots, artisans, and village labourers. It has opened quarries and brick-fields, has impelled trade into unwonted activity, and has drawn largely upon the resources of the country. It has introduced into India, hundreds of new patents and ingenious contrivances. It has lessened the expenditure of the State, by its cheap conveyance of mails and troops, and has augmented its income by large payments of tolls and duties.

To conclude, in the words of the Right Hon. James Wilson, in his late financial statement to the Council of India :—" I never saw greater signs of industrial vitality, all full of promise for the future prosperity of India. Those who have seen the rush of third-class passengers on a holiday in Belgium, can, from that, form the best idea of the use made of these railways. What we require is their completion at any cost, but quickly, and we shall see a result for which the most sanguine are not prepared."

The Paper is illustrated by several diagrams, from which Plate 7 has been compiled.

[Mr. GEORGE BERKLEY,



Kell Bro^s Lith^rs Castle St Holborn.



Mr. GEORGE BERKLEY, as the representative of the Author of the Paper, and as officially connected with the Great Indian Peninsula Railway Company, stated, that when he received the Paper from his Brother, now in India, it was accompanied with a request, that he would make any additions to it which he thought desirable. He believed he had exercised a wise discretion in submitting the Paper to the Institution, almost exactly as it was written by the Author; because it was complete in itself, and the Author, who had borne the brunt of the work in India, and upon whom the chief responsibility rested, should have, therefore, all the credit that attached to it. He had also been requested to illustrate the Paper, and he had done so by maps and tables, to which he would briefly allude, because, in addition to the Paper itself, which dealt chiefly with the works of the Great Indian Peninsula Railway, they referred, generally, to the system of railways which had been sanctioned by Government, and were under construction in India.

In the absence of Mr. Rendel, the Consulting Engineer of the East Indian Railway, he would give some particulars respecting that line, which extended from Calcutta to Delhi, with a long branch to Jubbulpore and three shorter branches to Raneegunge, the Barrackur river, and the Singarow valley. The length of the line was 1,338 miles, and the length already opened for public traffic, 295 miles. On the East Indian Railway, although it was of so great a length, there was only one tunnel, 300 yards in length. The principal works consisted of viaducts. In the crossings of the Rivers Soane and Jumna and two others, the railway was carried at the top, with a roadway underneath for the ordinary traffic, in a somewhat similar way to the High-Level Bridge at Newcastle. These four bridges were constructed upon the wrought-iron lattice principle, and were extensive works. The bridge over the Soane consisted of twenty-eight openings, with a span of 150 feet; that over the Jumna of fifteen openings, of which the span was 205 feet. The bridge over the Adjai consisted of thirty-two openings, with a span of 50 feet; that over the Keeul, of nine openings, with a span of 150 feet; and that over the Tonse, of seven openings, with a span of 150 feet. In the prosecution of this railway, a coal field of considerable extent had already been tapped; the coal was used as fuel for the locomotive engines, and was carried in large quantities along the line. This was the first instance, in India, of the development by railways of that most valuable mineral, coal.

The estimated capital of the East Indian Railway was £19,000,000, rather more than £14,000 per mile. The cost of the finished portions of the line had been £12,500 per mile: there was, therefore, every probability, that the whole of the works

would be carried out within the estimates. The cost of these railway works had been largely increased by the recent mutiny, to the extent, it was estimated, of not less than £3,000,000. The following were the rates of wages per day, both before and after the mutiny:—foremen carpenters, from 9*d.* to 13½*d.*; ordinary carpenters, from 6*d.* to 7*d.*; blacksmiths' foremen, from 9*d.* to 13½*d.*; ordinary blacksmiths, from 5*d.* to 7*d.*; masons, from 4*d.* to 7½*d.*; coolies, from 1½*d.* to 2½*d.* He hoped on a future occasion, that the Institution would be favoured with a Paper descriptive of the works on the East Indian Railway.

The Author had given so full a description of the Great Indian Peninsula Railway, that it was unnecessary to do more than call attention to the following points: that the total length of the line was 1,266 miles, of which 246 were already opened; that the estimated cost was about £10,000 per mile; and that the portion already opened, had been constructed at an average cost of £8,758 per mile, so that there was every reason to believe, that the estimate would not be exceeded.

The materials sent from this country for the use of Indian railways, were similar to those employed on English lines. The engines had cylinders 15 inches in diameter, with a stroke of 22 inches, and four coupled wheels, each 5 feet 6 inches in diameter. Those intended for use upon the inclines were tank engines, having cylinders 15 inches in diameter, with a stroke of 2 feet, and four wheels, each 4 feet in diameter, with skid breaks which did not pass under the wheels, but were pressed upon the rails between the wheels, on each side of the engine. The specimens of permanent way materials which he had brought for the inspection of the Meeting, showed, that they were the best that this country could, at any reasonable price, afford. It was of great importance, where the freight was so heavy an item, and the question of renewal was also so serious, to have the best quality of materials.

It might be interesting to learn how largely wages had increased in Bombay. Comparing a report from an inspector in that Presidency, of the rates of wages in 1853, with those in 1859, given in the Paper, he found, that the increase of wages in Bombay had been larger than in Bengal, and he believed, than in the other Presidency also. The wages of masons had risen, from 9*d.* to 1*s.* 6*d.*; of carpenters, from 9*d.* to 1*s.* 6*d.*; of miners from 7½*d.* to 9*d.*; and of bigaries, or labourers, from 4½*d.* to 6*d.* It must not be inferred, that the cost of the works had risen in the same proportion; because, as the workmen became accustomed to the use of mechanical appliances, they earned more money than before, they were better fed and protected, and their work, consequently, became more valuable. He considered a

material element of prosperity to have arisen from the introduction of these large works into India ; for it was an undoubted gain to a country, when the amount of labour an individual workman could perform, was largely increased, or doubled. With reference to the salaries and wages paid to persons who went out to India from this country, he might state, that there were three classes of Resident Engineers. The first class,—he did not mean those who had the chief superintendence, but those to whom districts were assigned,—received £780 per annum : the second class, £660 : and the third class, £540. The salary of the Locomotive Superintendent, was £1,080. The foremen of the several mechanical departments were paid various rates, from £20 to £35 a month. Engine fitters, engine drivers, coppersmiths, general smiths and coachmakers, received £16, £18, and £20 per month, respectively, for three years : permanent-way inspectors had £14 per month, with no addition during the three-years' engagement. He did not know whether that circumstance influenced the supply, but this was the most difficult class to procure. He believed, that it was desirable, in order to induce persons to go out from this country, that they should have the stimulus of an annual increase in their salary ; unless they had that prospect before them, they were not tempted to leave England. It was satisfactory to be able to state, that with but few exceptions, the whole of the engineering staff of the Great Indian Peninsula Company had saved money. Living was very cheap in India. One engine driver who went out as a fireman, at £9 per month, and who, six months afterwards, was raised to the position of engine driver, at £16 per month, in addition to liberal allowances under special circumstances, had saved, during the sixteen months he had been engaged, £100. This driver stated, that he had everything necessary for a working man, and lived luxuriously for £3 per month ; his definition of luxury being curries and beer. He hoped these statements would induce able men to go out to India, to aid in the development of the valuable system of railways in that country.

The Madras Railway started from Madras to join the Great Indian Peninsula at the River Kristna, thereby connecting Bombay and Madras : it also connected Madras and Bepore. It was unnecessary to enter into any particulars respecting it, as he saw present, not only the Consulting Engineer, but also the late Chief Resident Engineer of the line, who had done himself great credit in the management of that undertaking. The Madras Railway as well as the Bombay and Baroda Railway, and the Great Southern of India, were in course of construction, without the intervention of contractors. It would be interesting to hear the reasons, which had induced the able and experienced Engineers of

those railways to adopt that course. From the Paper it appeared, that very good practical results had been derived from the introduction of the contract system on the Great Indian Peninsula Railway. He need not dwell for more than a moment, upon the importance and value of subdivision of labour, as practised in the manufactures of this country, where the principle was universally adopted, in the most minute, as well as in the greatest works. One of the most striking examples of this, was afforded by the introduction of the contract system in railways : the Engineers designed, estimated, and superintended the works, whilst men whose practical knowledge fitted them for the task, undertook the organisation of labour, the purchase of materials, and the actual execution of the works. That the system had been productive of great good in this country, was not doubted ; it would, therefore, be interesting to learn why it had not been applied in those parts of India, where the railways he had named, were in course of construction.

The Scinde Railway, although it consisted of three companies, —the Scinde Railway Company, the Indus Steam Company, and the Punjab Railway Company,—was, in point of fact, but one system. There was also a line under survey, from Lahore to Delhi which was not indicated on the map, (Plate 7), as the route was not yet determined. He hoped, that in the course of the discussion, some information would be elicited, with regard to the difficulties that had been experienced in the navigation of the Indus, and the manner in which it was proposed to surmount them.

It would also be highly interesting to have some information respecting the bridges on the Bombay and Baroda Line, which were made entirely of iron, somewhat similar to the celebrated bridge in Wales, and consisted of spans of 60 feet, which he understood were duplicates.

The Eastern Railway of Bengal, and the Calcutta and South Eastern Railway, were also works of interest. In connection with the latter, he believed dry docks were to be constructed.

The average cost of railways in England, up to the end of 1858, was about £38,700 per mile ; in Scotland, about £27,500 per mile ; and in Ireland, about £15,000 per mile ; making an average of £34,243 per mile. That amount applied to 9,323 miles, of which about 3,100 miles were single lines. On the Indian railways it was proposed, in the first instance, to lay down single lines of permanent way, except on such portions as those between Bombay and Callian, and the two Ghaut Inclines, where double lines would be laid. Their estimated cost was from £6,000 to £14,000 per mile, giving an average of £11,000 per mile, and there was every reason to hope, that

this would be found sufficient for their construction, as whilst the East Indian Railway was estimated at £14,480 per mile, the part now constructed had only cost between £12,000 and £13,000 per mile; of the Great Indian Peninsula Railway, which was estimated at £10,000 per mile, the parts completed had cost less than £9,000 per mile; and of the Madras Railway, which was estimated at about £8,500 per mile, the part constructed had only cost about £7,000 per mile. But it would be unjust not to mention, that in India there were no charges for Parliamentary expenses and land, whilst in England, there was high authority for estimating them at 25 per cent. of the total cost of railways. There were also many other important matters, such as the permanent and extensive character of the station accommodation on English railways, which should be taken into consideration, if it were desired to institute any comparison between the two systems of railway.

It appeared from the Paper, that as many as one hundred thousand men had been employed upon the Great Indian Peninsula Railway, the average number being about forty thousand. Looking at the length of that line and comparing it with the whole system, the conclusion would be arrived at, that within the last four, or five years, there had been a minimum of one hundred thousand, and a maximum of three hundred thousand, or four hundred thousand persons employed upon railway works in India. Within the same period of time, about £14,000,000 had been expended in labour, the effect of which had been to raise the rate of wages, and to increase the capacity of the men employed. Experience proved, that no native prejudices, or other circumstances would interfere to prevent railways from being used for the benefit of India, and that they would be extensively employed for the conveyance of goods, as the best and cheapest modes of transit.

But the beneficial effects of the introduction of railways into India, had not been limited to the people and trade of that country. It was true, that England had found the greater part of the capital for these undertakings, but her manufactures at home had been very largely benefited by them. Within the last four, or five years, 700,000 tons of railway material had been sent from this country to India, independently of contractors' materials. Those 700,000 tons of material had cost about £10,500,000. In 1859, 208,000 tons, chiefly of iron, were shipped for India, which was equal to about one-sixth of the whole exports of the country, during the previous year.

He had spoken of the number of persons temporarily employed in the construction of these railways; he would now notice the number that would, probably, be permanently employed, and here

the comparison with England was one of some interest. In England, in 1858, there were employed upon 9,323 miles of railway, 109,328 persons or an average of 11·7 men per mile. In India, in 1859, there were 590 Englishmen and 7,855 natives employed on 586 miles of line, giving 1 Englishman and 13·5 natives, or 14·5 men per mile. When all the lines at present sanctioned were completed, viz., 4,821 miles, there would be about 70,000 people permanently employed upon the Indian railways.

The question of fares for passenger traffic was one of some interest: upon this subject he would confine himself to the Bombay Presidency. On the Great Indian Peninsula Railway, the fares were, per mile, for the first class, 2·25*d.*, or 2¼*d.*; for the second class, ·75*d.*, or ¾*d.*; and for the third class, ·37*d.*, or ⅜*d.* per mile. In England, Scotland, and Ireland, the fares varied but little: in England they were respectively, for the three classes, 2·16*d.*, 1·48*d.*, and ·88*d.*; in Scotland, 1·70*d.*, 1·51*d.*, and ·88*d.*; in Ireland, 1·82*d.*, 1·36*d.* and ·80*d.* The speed in the Bombay Presidency was from sixteen miles to twenty miles per hour, including stoppages, while in England it was from twenty miles to the express speed of thirty-eight miles per hour. The mean number of miles open in Bombay in the year 1859, was 195, and in the United Kingdom in 1858, it was 9,323 miles. The total number of passengers in Bombay was 1,161,501; and in the United Kingdom, 139,193,699. The average distance travelled by each passenger in Bombay, was 32·4 miles, while in England, the average distance travelled was only 12·7 miles. The number of passengers, per mile of railway, was larger in Bombay than in Great Britain and Ireland; in the former, it was 192,974, and in the latter, 189,611. The total number of passengers by railways in India in 1859, was 2,822,382, 93 per cent. being third-class passengers, probably consisting chiefly of the lowest classes of the native population. On the Great Indian Peninsula Railway, the receipts for passengers amounted to £453 per mile, and for goods, £464 per mile; in Great Britain and Ireland, £1,112 per mile for passengers, and £1,458 per mile for goods. Notwithstanding the very high price of fuel in Bombay, coals being £3. 3*s.* per ton, the working expenses did not exceed those in England. The dividend earned by the Bombay Line was £5. 14*s.* per cent., and by the East Indian Line, more than 7 per cent.; in England, the average dividend was about 4 per cent.

He could not leave this part of the subject without remarking, that the greatest credit was due to those gentlemen who had gone to India, and had devoted their best energies to the development of the traffic and to working it in the cheapest and best way; who had set aside all prejudices and personal feelings; and who, in all

cases, had laboured for the good of Her Majesty's subjects in her Indian dominions.

With regard to the future prospects of these enterprises, he looked again to English experience. The Author of the Paper had remarked, that the necessity for railways in India, was not to be measured by the present extent of the commerce of that country. England afforded a striking illustration of the truth of this statement; for from the time that railways were first introduced and developed in this country, say from 1830 to 1858, the population had increased about 42 per cent., whilst the exports of the principal articles of manufacture had increased from about 200 to 1,000 per cent. The exports of cotton twist, &c., had increased during this period, from about 65,000,000 lbs. to 200,000,000 lbs.; of calicoes, &c., from about 245,000,000 yards to 1,517,000,000 yards; and of iron and hardwares, from about 131,000 tons to 1,349,000 tons. He did not maintain, that these results were entirely owing to the construction of railways, but he ventured to assert, that they would never have been obtained without railways, and they led him to hope for similar results from the development of the railway system in India, in opening out the vast resources of that country.

He had now been connected with Indian Railways for ten years, and he was desirous of communicating all the information he could, in the hope, that the attention of Engineers would be directed towards extending and perfecting the railway system in India, and thus promoting the development of the resources and the civilisation of the large native population of that country.

In answer to a remark from the President,—that without a statement of the earnings per train mile in India, there could be no basis for the comparison of the working expenses there and in England,—Mr. Berkley added, that he had not intended to institute a comparison between the working expenses; as although the cost of fuel was large in India, the railways were working with new plant; and although there was some expensive labour, there was also a great deal of cheap labour. He did not consider, that the circumstances admitted of a direct comparison.

Mr. BRUCE stated the reasons which induced him, in the construction of the Madras Railway, to associate, in his own person, the apparently double functions of Engineer and Contractor, a course so different from that generally adopted in this country. He had never ignored the benefits to be derived from the division of labour, nor did he condemn the employment of contractors; but no general rule, could, or ought to be laid down upon the subject; under certain circumstances, and indeed most frequently, the works must, of necessity, be undertaken by contractors. But in Madras there were no large contractors available; it became, therefore, a

question, whether he should begin the works at once, within two months of landing, or whether he should allow a year, or two years, to be consumed in correspondence with the administrative body in England. If he had gone direct from England to Madras, he should not have deviated from the ordinary rule; but having previously had, in Bengal, some experience of the ease with which natives might be organised for railway work, and seeing that contractors in India were, at that time, in no better position than the Engineers, he did not hesitate to begin the works of the Madras Railway on his own responsibility. He did not, for a moment, call in question the propriety of others adopting a different plan; but he had no doubt, that under the circumstances in which he was placed, he had pursued the best course. Another fact must also be taken into consideration; in carrying out works in India, very little plant was required; it might almost be said, that the less there was, the better. He began, at Madras, without any materials from England, and before it was possible to import any, the natives had found their own tools and earth baskets. As there were no long 'leads' from cuttings into embankments, there was no necessity for temporary rails; all that was required were good labourers, to whom a certain length of work was let. The first party engaged, undertook six miles of earthwork, the first out of Madras, at a price rather less than one penny per cubic yard. The other parts of the line were also let, wherever it was possible, to sub-contractors who were paid weekly; and he never had any difficulty, within a reasonable distance of Madras, in getting people to take these contracts. In so doing, he had only followed, after all, the old method, the plan on which Smeaton built the Edystone Lighthouse, and on which Stephenson constructed the Stockton and Darlington, and the Liverpool and Manchester Railways. At the same time, he perfectly admitted, that the intervention of contractors was desirable, whenever it was possible to get responsible men to undertake the work at fair prices. By that system, the Engineer relieved himself of a great amount of responsibility and continued anxiety, which sometimes led to fatal consequences.

He added his testimony to what, he was sure, was the sense of the Meeting, that this Paper was a most valuable one, as the Author, in the popular description he had given of the Bombay Railway, had also sketched the general features of all the Indian railways. With most of the points mentioned, he cordially agreed, and with regard to those from which he differed, he acknowledged the fairness and ability with which the Author had stated his case. There was one topic alluded to, which called for remark; it was stated, that the substantial system had been adopted in contradistinction to what was called, the American system. It always appeared to him, that there was a popular

delusion in the minds of the public upon that subject. Engineers were repeatedly asked, why the American system was not adopted in the Indian railways, and why so much money was spent upon them. Now the fact was, that what had been incorrectly called the American system, had, in reality, been followed, if by that term was meant the practice of taking advantage of the cheapest and best materials to be found upon the spot. It would be folly to maintain, that because timber was used in America, where it was cheap, it ought also to be employed in India, where it was dear, but where there was abundance of good stone, and bricks.

The prices of labour in Bombay were much higher than in Madras. In the latter Presidency, the ordinary prices per day were ; —coolies, 3*d.* ; women and boys to assist in carrying the earth, 1½*d.* ; carpenters, from 9*d.* to 10½*d.* ; bricklayers, 7½*d.* to 10½*d.* These rates were about the same as those now paid upon the Great Southern of India Line. The ordinary price of earthwork was 1½*d.* to 2*d.* per cubic yard ; and of masonry, 7*s.* 6*d.* to 10*s.* per cubic yard ; and works, which were usually let to contractors, such as making embankments, building bridges, laying the permanent way, and ballasting the road, cost about £1,500 per mile, exclusive, of course, of materials and stations.

He could confirm, in every respect, the observations in the Paper, that the railway works of India had materially contributed to the improvement of the people. Their whole appearance was changed for the better ; there was plenty of money amongst them, and they had now the means of living well. The coolies were daily increasing in size and strength, as also in mechanical ability. He ought to have mentioned, when speaking of the system he had adopted in Madras, that he never made a written contract. Every arrangement took place by word of mouth, and no money was ever advanced before it was earned. The ordinary practice in India was to pay in advance, one-half, or more, of the value of the article ordered, which was proof of the absence of proper confidence between the employers and employed. He had been warned, by men long acquainted with India, that the system he proposed to adopt, would fail. He found, however, that when the natives were once impressed with the honesty of those who employed them, and of their readiness to fulfil all their engagements, there was not the least difficulty in procuring labour. But his reason for not entering into written contracts, was the possibility, from the well-known mania of the natives for litigation, of his being continually involved in lawsuits.

Allusion had been made to the advantages of the guarantee system. It had been his fate to have been occasionally placed in a rather prominent position of hostility to that system : he was

glad, therefore, to take this opportunity of stating, from the experience of some years past, that the manner in which the Government supervision of the works had been exercised, was all that could be desired. The course pursued by Captain Johnson in connection with the lines in Madras, had been in the highest degree satisfactory to those concerned; and he was sure, that all those who had been connected with Indian railways, would not do justice to their own feelings, if they failed to express, on every occasion, their admiration of the extraordinary ability and the high gentlemanly bearing which had always been manifested by Sir James Melville, in the management of that department of the Indian Government in this country.

Mr. LONGRIDGE said, the line in India with which he was connected was comparatively so insignificant, that it offered little interest in an engineering point of view. It possessed, however, great commercial interest, the chief object of that line being to relieve the over-crowded and somewhat dangerous port of Calcutta, by the creation of a subsidiary port on the Mutla. The trade of the province of Bengal had increased so rapidly, and the dangers of the navigation of the Hooghly were so much enhanced, partly, perhaps, owing to the increased size of the vessels, that the necessity for a new port had become evident. When he went to India in 1856, to survey this line,—the Calcutta and South Eastern, from Calcutta to the River Mutla,—he was struck with the capabilities which that river, or rather arm of the sea, presented for the formation of a new port. At low water there was, throughout, a minimum depth of 24 feet, whereas in the Hooghly there were many shoals, with not more than 15 feet of water over them. Those shoals were at such distances from each other, that it was impossible to get over more than one, or two of them, in a tide. Unlike the Hooghly, the Mutla was free from bores and dangerous eddies; it was kept open by the tidal flow, which did not exceed, at any time, four miles per hour, through the channels of the Sunderbunds; and as no fresh water entered it, there had been little, or no change in the depth of water; there was, moreover, no bar. Ships of all burthens would be able to come alongside the intended wharves, and discharge their cargoes, at once, into the railway trucks. The traffic from Calcutta, for about three months in the year, was, at present, brought down the Nuddea Rivers, running nearly due north from Calcutta, but except during the rainy season, those rivers were not navigable; at other times, the whole of the traffic, which, in 1856, amounted to 1,667,000 tons, had, therefore, to be brought round, through the most intricate navigation of the Sunderbunds, to the head of the River Mutla, thence it passed up the Biddiadhurree River and then again through the Circular Canal to Calcutta. The scenes which took place upon that canal, must be seen to be believed.

At high water it was, frequently, so crowded with vessels, that when the tide fell, they crushed up together, and the weakest went to the bottom. He had himself occupied from four hours to five hours in getting two miles, in a dingy. The port on the Mutla was the only one in India capable of receiving the 'Great Eastern'; it could be moored alongside the wharf at the railway station to discharge both passengers and cargo direct for Calcutta, which was at a distance of $28\frac{1}{2}$ miles. There was no necessity for wet docks; graving docks only would, therefore, be constructed by the Company, who also intended to erect wharf walls, with hydraulic cranes for discharging cargo.

The system of executing the works without the intervention of contractors had been adopted, because the line was short, and the works were, generally, of such a nature as not to require any plant. There were only two bridges of any importance; one with a span of 170 feet, the other of 125 feet. The foundations were not difficult, and he proposed that the bridges should be formed of iron girders resting on brick abutments. It must not be inferred, that he was systematically opposed to the employment of contractors; on the contrary, he thought it would have been almost impossible to have carried out many large works, without their intervention.

Mr. HAWKSHAW, V.P., as the Consulting Engineer of the Madras Railway Company, had seen no occasion to differ from the mode adopted, of carrying on the works by small sub-contracts, and dispensing with the intervention of large contractors. It was unwise to enunciate general rules, as to whether railway works should be executed by large, or small contracts, or be conducted under the immediate control of the Engineer; everything must depend upon the circumstances of each individual case. In England there existed the means of obtaining fair competition for contracts, from a valuable class of men, educated to the business and possessing a thorough knowledge of the manner in which the work ought to be executed; and he believed, that most Engineers had arrived at the conclusion, that in England, this was the best mode of conducting public works. But it was not always possible to adopt the same plan in a foreign country. There was great difficulty in obtaining proper competition for contracts in a country like India. He had under consideration, at the present time, the best mode of carrying out railway works in some of the Colonies, yet with all his experience, he found it an embarrassing question. There were so many difficulties in the way of obtaining offers from a competent number of persons, to perform works of the cost of which the Engineer himself could have no very precise knowledge, that the contractors were tempted to add a considerable sum, to provide against the contingencies which might arise. He had also the honour to be the Consulting Engineer of

[1859-60. N.S.]

the Eastern Bengal Railway, which was carried on upon the contract system ; thus having railways in India under construction on both systems, he should, in a few years, be able to give a sounder opinion as to their relative superiority. But with his present experience, he approved the course which had been pursued, in commencing the Madras railway, and judging merely by the cost of that line, experience had pronounced in its favour.

The question had been mooted, whether Indian railways ought to be constructed substantially, or on the principle which was adopted, some years ago, in America. It was his decided opinion, that as a rule, temporary works were proper for temporary purposes only ; but that, occasionally, from want of funds, it might become necessary to adopt temporary structures for permanent objects. In an old country like India, where there was an immense population, a large amount of traffic, and the prospect of a fair return upon the outlay, the best materials only should be employed. It was as bad policy in India, as it was in England, to use inferior sleepers, or bad rails ; because those portions of a railway wore out sooner than the rest. The same remark applied to bridges and other similar works ; yet, in some cases, where the timber could be depended upon to last fourteen, or fifteen years, it might, with propriety, be employed for viaducts, provided that by using timber, the structure could be erected for about one-half the cost of iron, or stone, which would permit of its being rebuilt at the expiration of that time ; but if the timber was liable to decay in five, or six years, that course would be inapplicable.

He thought that, at present, there were no fair grounds for comparison between the cost of railways in India and in England. His own experience proved, that the cost of many of the English railways had been nearly doubled, during the last fifteen years. No useful conclusions could, therefore, be drawn, from a comparison between the cost of a new line of railway and that of an old one. On the lines with which he was connected, almost all the stations had quadrupled in size and cost, during the period they had been under his charge. It would be the same, hereafter, in India ; the rolling stock also would become more extensive as the traffic increased ; and thus the ultimate cost, per mile, would be proportionately augmented. It must not be forgotten, when instituting comparisons of this kind, that there was not a single old railway that had not cost much more per mile, than when it was first opened.

He could not refrain, in conclusion, from expressing the great pleasure which this Paper had afforded him. It was a clear and lucid statement of what had taken place under the Author's own observation, and he felt, that the Institution was much indebted to him, for having favoured them with this communication.

Mr. BIDDER,—President,—said, that the great experience of the Author in Indian railway work, had enabled him to present to the Institution one of the most instructive Papers that had ever been laid before it. For his own part, he regarded it as a perfect model for such communications; he had never listened to a Paper with more pleasure and interest, or to one from which he had derived more information.

It was a subject for congratulation, that the results of the non-contract system had been so satisfactory on the Madras Line. Those who invested their money in Indian railways merely looked for interest on their capital, and although great supervision had been exercised over the expenditure by the Government, there was no personal interest to insure economy. Contractors might be said to be both bold and timid; bold, when the work was fairly understood, but timid, when there were contingencies in the background. In introducing a new class of labour into a new country, it was necessary, that the Engineer should pioneer the way, so as to ascertain the elements on which contractors might, subsequently, found their estimates; fair competition might then be relied on, and the contract system could be introduced with advantage. The remarks which had been made relative to the increased cost of railways in this country, had demonstrated, that no useful result could be drawn from a comparison of the cost of lines in England and in India. But there was one point of comparison which led to inferences of a useful character, that of the fares, and the average distance travelled by each passenger. The third-class fares in India seemed to be only about one-half as much as in England, whilst the average distances travelled by each passenger were, respectively, 32 miles and 12 miles. If the distance each passenger was conveyed in England could be increased, either larger dividends would be realised, or lower fares could be charged. It was well known, that the most intricate complications had arisen, from the contests of railway companies with regard to long fares; whereas it appeared to him, that the real prosperity of a railway was more dependent upon its local traffic, and that, in general, railway companies should endeavour to facilitate the construction of lines in the districts they traversed, so as to lead, ultimately, to an increase in the accommodation of the immediate population and in the conveyance of traffic.

Another subject had been incidentally alluded to, to which he more particularly referred, in the hope that Mr. Longridge would be prevailed upon to present a Paper to the Institution, embodying the results of his experience as to the local circumstances connected with the navigation of the Hooghly and the Mutla. The Hooghly received a very large amount of inland water, and

was uncertain and changing in its depth, whilst the Mutla was kept open by the tidal water only, and preserved a uniform and invariable depth. There was no subject of greater national importance than that which concerned the régime of navigable rivers, and the effects of scour and tidal waters upon their channels; and a discussion upon that subject, independent of technical points, could not fail to be interesting and useful to the profession.

May 15, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

The discussion upon the Paper, No. 1,025, "On Indian Railways," &c., by Mr. J. J. Berkley, occupied the whole evening, to the exclusion of every other subject.

May 22, 1860.

GEORGE PARKER BIDDER, President,
in the Chair.

The following Candidates were balloted for and duly elected :—
GEORGE HIGGIN, as a Member ; JOHN GLADSTONE DAVENPORT,
CHARLES JAMES FREAKE, SYDNEY LAWRENCE, and JOSEPH LEE
THOMAS, as Associates.

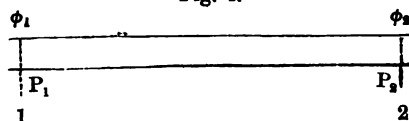
No. 1,027.—“ On a Method of computing the Strains and Deflections of Continuous Beams, under various Conditions of Load.”¹

By JOHN MORTIMER HEPPEL, M. Inst. C.E.

1. THE method which forms the subject of this Paper is, in its main features, identical with that given in Moseley’s “Mechanical Principles of Engineering and Architecture,” and so successfully followed, in the deduction of practical results, by Mr. Edwin Clark, (M. Inst. C.E.) and others. In one, or two points, however, the Author believes it to be new ; and as some of the formulæ arrived at, appear to be of more general applicability, and of easier adaptation to the purposes of numerical computation than any he has yet seen, he is induced to offer to the notice of others what he has found useful.

2. The chief points in which the following method of investigation differs from others are, that in estimating the action of forces, more aid is derived from the theory of couples, and that in considering the pressures on the bearing points, the two parts of each such pressure, arising, respectively, from the portions of the beam on either side of it, are always kept separate and distinct. Much advantage will result from this separation, by the aid of which, chiefly, all the conditions of a continuous beam can be treated, with little greater labour, or complexity of calculation, than if it were a succession of independent beams.

Fig. 1.

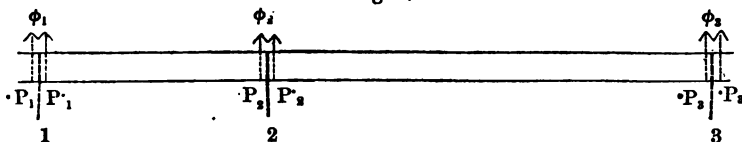


3. Let 1. 2 (Fig. 1), represent an opening, or span, of a continuous beam, and call the elastic forces in action at the sections over the bearings 1 & 2, ϕ_1 & ϕ_2 , the length l , and the load per unit of length μ . It is evident, that since all the forces in action on this

¹ This Paper, being of a purely mathematical character, was read in abstract.

part of the beam are in equilibrium, the sum of the two upward pressures P_1 & P_2 must be equal to the weight. It is true, that if the strain over either of the bearings, as ϕ_1 , is produced by the action of an adjacent portion of the beam, which also carries a load, an additional pressure due to such adjacent load will be brought to bear on the support 1. In estimating the total pressure on this support, it would be necessary to take it into account, but its amount is deducible, exclusively, from the mechanical conditions of the adjacent span, from which it alone arises, and whether great, or small, it in no way affects the amount of P_1 , which continues, as before, to be the vertical force arising from, and in equilibrium with, the other forces in action in the span 1. 2.

Fig. 2.



4. To make this more clear, as it is a point of some importance to what follows, let Fig. 2 represent two adjacent spans of a beam, and represent by P_1 , P_1 , P_2 , P_2 , P_3 , P_3 , the vertical forces in action, at sections of the beam exceedingly near to each other, on either side of the points of support. It must be obvious, that whatever may be, in other respects, the mechanical conditions of the two spans, the sum of the vertical forces P_1 & P_2 must be equal to the weight of the part of the beam lying between 1 & 2; and the sum of P_2 & P_3 will, in like manner, be equal to the weight of 2.3; also, that P_2 and P_2 together make up the whole pressure on the support 2.

Returning to Fig. 1, it will be evident, that the sum of the pressures P_1 & P_2 can in no way be affected by the mechanical conditions of the adjacent spans. By affecting the amount of the strains ϕ_1 & ϕ_2 , a change in the condition of the adjacent spans, or of either of them, may, indeed, become productive of a corresponding change in P_1 & P_2 individually, but their sum nevertheless remains constant throughout, and their individual changes are simple results of the changes in ϕ_1 & ϕ_2 , and would be exactly the same, whether these latter had been brought about by the action of the adjacent spans, or in any other way.

5. Taking account of all the forces in action on the span 1.2, (Fig. 1), there will be found to be three different couples of forces in action, any one of which may, according to the special conditions of the case, tend to turn the span under consideration, in either direction; and in order to avoid ambiguity in the use of the positive, or negative sign, by which the direction must be indicated,

it will be necessary, in the first instance, to determine the rule to be followed.

In the case of couples which act on one portion of the beam only, there is no difficulty about the sign, which will be + when the tendency is to turn forwards, or in the direction of the hands of a clock, and - when the tendency is backwards, or in the opposite direction. With reference, however, to such couples as ϕ_1 , ϕ_2 , &c., which act both on the span under consideration and on one, or other of the adjacent ones, and whose action, if positive on the one, is negative on the other, and *vice versa*, there may be some risk of confusion, unless the conditions determining the sign, and its precise meaning, are kept in view throughout.

Each such couple then, must be considered as having its own proper, or essential sign, according as its action is direct, or retrograde, considered with reference to the portion of the beam lying always towards the same side of it; and it will be found most convenient, that this should be the left side; that is to say, that a couple whose action is to turn the portion of beam lying to the left of it, forwards, or in the direction of the hands of a clock, should always have the proper sign +, and one whose action is the reverse, the proper sign -. This is only saying, in other words, that those in which the top is extended and the bottom compressed, are positive, and those in which the reverse obtains, are negative. This sign is, however, indeterminate, until, from the solution of an equation, or otherwise, the particular value of the moment of the couple becomes known. In the mean time, ϕ_1 , ϕ_2 , &c., represent quantities which may be positive, or negative, according to circumstances.

In bringing these quantities into equations, another circumstance must be considered; whether they are regarded with reference to their action on the part of the beam to the left, or on that to the right of them. In the first case, their action will always be accurately represented by their proper sign, whatever that may be; but in the second case, it will be the reverse. Both cases will, therefore, be provided for by writing the sign +, before all couples which are considered with reference to their action on the portion of the beam lying to their left, and the sign - before those considered with reference to the right. The proper signs of the values of the couples resulting from the solution of such equations, will always accurately indicate the direction of their action.

Bearing these things in mind, there are acting in the span 1.2, the couples ϕ_1 & ϕ_2 to the first of which must be prefixed the sign -, inasmuch as its action is considered with reference to a portion of the beam lying to its right, and to the second, the sign +, for the converse reason. Next, there is the couple whose components are the whole load acting through its centre of gravity,

and the equal and opposite force $P_1 + P_2$, acting through the centre of pressure of P_1 & P_2 . The moment of this couple is easily shown to be $(P_1 - P_2) \frac{l}{2}$, and as it acts on the span 1.2 alone, and on no other portion of the beam, there is no ambiguity as to its direction, which is positive when P_1 is greater than P_2 , and negative in the contrary case. Therefore, the positive sign may always be prefixed to the above expression, and the proper sign of the result, obtained by assigning any particular values to P_1 & P_2 , will accurately indicate the corresponding direction of its action.

7. Now since these three couples are the only forces acting on the span 1.2, they must mutually balance, and

$$\text{then,} \quad -\phi_1 + \phi_2 + (P_1 - P_2) \frac{l}{2} = 0;$$

$$\text{also,} \quad \mu l - (P_1 + P_2) = 0.$$

$$\text{Hence,} \quad P_1 = \frac{\mu l}{2} + \frac{\phi_1 - \phi_2}{l} \quad \dots \dots \dots (1)$$

$$P_2 = \frac{\mu l}{2} + \frac{\phi_2 - \phi_1}{l} \quad \dots \dots \dots (2)$$

in which ϕ_1 & ϕ_2 may be properly positive, or negative, in any particular case, according as their tendency is to turn the portion of the beam lying to the left of them forwards, or backwards.

P_1 & P_2 , it will be observed, may also either of them become negative, indicating an upward, instead of a downward pressure on the bearing; but never both of them together, as their sum is always μl , whatever ϕ_1 & ϕ_2 may be.

8. Returning now to Fig. 2, call the lengths of the two contiguous spans l_{12} & l_{23} , and the loads per unit of length μ_{12} & μ_{23} ; also call the tangent of the inclination of the deflection curve over the bearing 2, T_2 . Let the strain at any distance x from the point 2, in the span 2.3, be called ϕ .

Then, with reference to the portion of the beam between 2 and x ,

$$-\phi_2 + \phi + P_2 x - \mu_{23} \frac{x^2}{2} = 0.$$

Hence, by a well-known process, remembering that where y is the

deflection at x , $\phi = EI \frac{d^2 y}{dx^2}$; and that at the point 2, $\frac{dy}{dx} = T_2$,

$$EIT_2 = -\phi_2 \frac{l_{23}}{2} + P_2 \frac{l_{23}^2}{6} - \frac{\mu_{23} l_{23}^3}{24} \quad \dots \dots \dots (3)$$

Similarly, if the position of the beam be reversed, that is, turned right for left, and the conditions of the span from 2 to 1 be investigated in the same way,

$$-EIT_3 = -\varphi_2 \frac{l}{2} + P_2 \frac{l^2}{6} - \frac{\mu l^3}{24} \dots \dots \dots (4)$$

But from 1 & 2,

$$P_2 = \frac{\mu l}{2} + \frac{\varphi_2 - \varphi_3}{l} \dots \dots \dots (5)$$

$$P_2 = \frac{\mu l}{2} + \frac{\varphi_2 - \varphi_1}{l} \dots \dots \dots (6)$$

Substituting these values in (3) & (4), adding them together, and clearing the fractions,

$$8 \left(\frac{l}{2} + \frac{l}{2} \right) \varphi_2 + 4 \frac{l}{2} \varphi_1 + 4 \frac{l}{2} \varphi_3 = \mu \frac{l^3}{12} + \mu \frac{l^3}{24} \dots \dots (7)$$

In a continuous beam resting on any number of bearings, at equal, or unequal distances, and loaded in any way, so that the load on each span is uniform, it is evident, that an equation may be formed, such as the above, between any three consecutive φ 's, as $\varphi_1 \varphi_2 \varphi_3$, $\varphi_2 \varphi_3 \varphi_4$, $\varphi_3 \varphi_4 \varphi_5$, &c.; and that the number of equations thus obtained, will always be two less than the number of bearing points. If then, the strains over any two of these be known, there are as many equations as unknown quantities, whence all may be determined. The most frequent case, and in fact, almost the only one occurring in practice, is where the first and last strains are known, and both are equal to nothing, being at the free ends of the beam.

The three equations, (5), (6), (7), will be found sufficient to determine the strains over the bearings and the pressures on the supports, in every case of a continuous beam of uniform section, and whose normal condition is straight, *i. e.*, which would, if relieved from all load, either extraneous, or arising from its own weight, just touch all the bearings.

9. If these conditions do not obtain, if the different spans are of different sections, and if the normal condition is not straight, then the expression requires the following modification, of which it is not necessary to give the demonstration at length, as it is arrived at by a method precisely analogous to that followed in determining the conditions of the Britannia Tube.¹ Put I for

¹ Vide "The Britannia and Conway Tubular Bridges, &c." By Edwin Clark. Page 774, *et seq.*

the moment of inertia of the section of the first span, and $\frac{I}{23}$

that of the second, and let $\frac{I_{12}}{I_{23}} = t_2$.

Moreover, if the different spans of the beam do not range in a straight line when all load is removed, the consequence is, that any two spans in which the condition obtains, as, for instance, 1.2, and 2.3, instead of having a common tangent to the inclination of their deflection curves at the point 2, will have two different tangents, which, call T_2 & T'_2 , and let $T_2 - T'_2 = t_2$.

The modified equation then becomes

$$8 \left(\frac{l}{12} + \frac{l}{23} \right) \phi_2 + 4 \frac{l}{12} \phi_1 + 4 \frac{l}{23} \phi_3 = 24 EI t + \frac{\mu l^3}{12 \cdot 23} \left(\frac{1}{12} + \frac{1}{23} \right) \quad (8)$$

Expressions (5) & (6) require no modification, being absolutely true for all conditions.

The equations (8), (5), & (6) then, are sufficient for the determination of all the conditions of any continuous beam of equal, or unequal spans, with equal, or unequal loads, of equal, or unequal sections, and either normally straight, or in which the strains over the bearings are regulated, as in the Britannia tubes. The mode of employing them is, first to form a set of equations of the form of (8), between each three consecutive ϕ 's, inserting the known numerical values of the other quantities. From them, all the ϕ 's are determined. When these are found, insert their numerical values in equations of the form of (5) & (6), two for each span, from which all the P 's are determined. It is plain, that these things being known, the whole of the conditions of each span, as to strain and deflection, become at once ascertainable, by the simplest processes, quite independent of the others; just as if it were a simple beam.

11. It may be useful, however, to enumerate the expressions to be employed, putting them in their most convenient form for the purpose of computation. The working out is not, however, given from a wish to avoid encumbering the Paper with a mere detail of well-known processes.

Taking then the span 1.2 as an example, the equation to its deflection curve

$$y = \frac{\mu}{24 EI} \times x \times \left(x - \frac{l}{12} \right) \times \left(x^2 + \left(x + \frac{l}{12} \right) \times \left(\frac{l}{12} - \frac{4 P_1}{\mu} \right) + \frac{12 \phi_1}{\mu} \right) \quad (9)$$

which, if it is the deflection on the middle of the span that is required, becomes, by making $x = \frac{l}{2}$,

$$y = \frac{l^3}{384 EI} \times \left(\frac{24}{12} P_1 \frac{l}{12} - 7 \frac{\mu l^3}{12 \cdot 12} - 48 \phi_1 \right) \dots (10)$$

The position of the points where the deflection curve becomes horizontal may be obtained from the cubic equation,

$$\frac{\mu}{6} x^3 - \frac{P_1}{2} x^2 + \phi_1 x + \frac{4 P_1 - \mu \frac{l}{12}}{24} \frac{l^3}{12} - \frac{\phi_1 l}{2} = 0 \dots (11)$$

the roots of which show the points where the tangent to the curve is horizontal. The corresponding signs of y & $\frac{d^3 y}{d x^3}$ will show whether any particular one is a point of greatest elevation, or greatest depression. This will, however, generally be obvious from other circumstances. The points where the deflection curve cuts the horizontal line through the bearings, will be given by the quadratic equation,

$$x^2 + \left(\frac{l}{12} - \frac{4 P_1}{\mu} \right) x + \left(\frac{l}{12} - \frac{4 P_1}{\mu} \right) \frac{l}{12} + \frac{12 \phi_1}{\mu} = 0 \dots (12)$$

whose roots show the positions of such points.

The point of maximum strain will be found from

$$x = \frac{P_1}{\frac{\mu}{12}} \dots (13)$$

and for the strain at any distance x , from the point 1,

$$\phi = \frac{\mu}{2} x^2 - P_1 x + \phi_1 \dots (14)$$

Lastly, for the position of the points of contrary flexure,

$$x = \frac{P_1 \pm \sqrt{P_1^2 - 2 \phi_1 \mu}}{\mu} \dots (15)$$

Of these expressions, the only ones ordinarily requisite in practice are Nos. (5), (6), (7), (8), (10), (13), (14), & (15), all of which are tolerably well adapted to numerical computation.

12. It may be desirable to mention another modification of the general expression, for the case where, in addition to the dis-

tributed loads, there are concentrated loads W W , &c., at the centres of the spans. It then becomes, as shown in the Appendix, (page 639,)

$$16 \left(\frac{l}{12} + \frac{l}{24} \right) \phi_2 + 8 \frac{l}{12} \phi_1 + 8 \frac{l}{24} \phi_3 = 2 \frac{\mu}{12} l^3 + 2 \frac{\mu}{24} l^3 + 3 W \frac{l^2}{12} + 3 W \frac{l^2}{24} \quad (16)$$

If, as in the case of a rail with supports at equal distances, the distributed load is altogether inconsiderable as compared with the concentrated load, this reduces to

$$4 \phi_2 + \phi_1 + \phi_3 = \frac{3}{8} l \left(\frac{W}{12} + \frac{W}{24} \right) \quad (17)$$

or, if one span only, as 1.2, be loaded,

$$4 \phi_2 + \phi_1 + \phi_3 = \frac{3}{8} l W \quad (18)$$

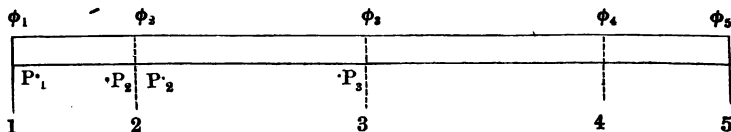
This is an exceedingly simple expression, by which the amount and direction of the strains on a rigidly-fixed rail, (supposing for a moment the possibility of its existence,) produced by the action of a load in the middle of any one of its spans, may be computed. If it is also desired to know the pressures, upward, or downward, on its supports, the expressions (5) & (6) must be thus modified:—

$$P_1 = \frac{W}{2} + \frac{\phi_1 - \phi_2}{l} \quad (19)$$

$$P_2 = \frac{W}{2} + \frac{\phi_2 - \phi_1}{l} \quad (20)$$

13. To give an example of the application of the above to the purposes of computation, let the case of the Britannia tube, before referred to, be taken.

Fig. 3.



The following quantities are given:—

$$\begin{array}{llll} \phi_1 = 0 & l = 230 & l = 460 & l = 460 \\ & 12 & 23 & 34 \\ \phi_2 = \phi_4 & \mu = 2.6 & \mu = 3.38 & \mu = 3.38 \\ & 12 & 23 & 34 \\ I = 962 & I = 1,584 & I = 1,584 \\ & 12 & 23 & 34 \end{array}$$

$$\frac{I}{I_{23}} = t_2 = 0.607324$$

$$\frac{I}{I_{34}} = t_3 = 1$$

$$T_2 - T'_2 = t_2 = 0$$

$$T_3 - T'_3 = t_3 = -0.00407$$

$E = 10,000 \times 144$, the dimensions being taken in feet.

Substituting these in the general equation (8), first taken for the spans 1.2 and 2.3, and next for the spans 2.3 and 3.4, and reducing, the following equations are obtained:—

$$\phi_2 + 0.2743 \phi_3 = 56,810$$

$$2 \phi_3 + \phi_2 = 118,250$$

Whence $\phi_2 = 47,030$, $\phi_3 = 35,610$: (the values given by Mr. Clark, are 46,920 & 35,650). Substituting these values of ϕ_2 & ϕ_3 in equations (5) & (6), first for the span 1.2, and next for the span 2.3, there is obtained:—

$$P_1 = 94.5 \cdot P_2 = 503.5$$

$$P'_2 = 802.3 \cdot P_3 = 752.7$$

$$P_2 \text{ (whole pressure on 2)} = P_2 + P'_2 = 1,305.8$$

$$P_3 \text{ (whole pressure on 3)} = P_3 + P'_3 = 1,505.4$$

(Mr. Clark gives $P_1 = 96$ $P_2 = 1,305$ $P_3 = 1,504$).

It is unnecessary to pursue the comparison further; but the deflection from expression (10), and the points of contrary flexure from (15), will be found to compare equally well.

In order to show, that there is nothing formidable in the calculations necessary for obtaining numerical results from these formulæ, the entire calculation of the quantities involved in the above expression, is given in the Appendix, (page 641). The logarithms are only taken to four places of decimals, which gives the results with sufficient exactitude, and will, probably, account for the slight differences between the results and those given by Mr. Clark. In the majority of cases in practice, the slide rule is sufficiently accurate for making these calculations.

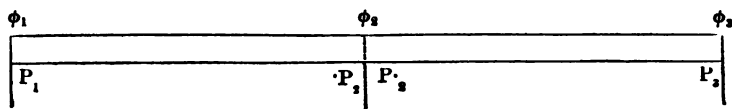
14. In simpler cases, such as that of the Torksey Bridge, investigated by Mr. Pole,¹ (M. Inst. C.E.,) the formulæ given above will be found to bring out the results with great ease and rapidity. As an example, however, one of the bridges on the Madras Railway, which presents the same features, though on a smaller scale, may be taken. The bridge over the River Cauvery consists of twenty-two openings, each 64 feet in span, with piers 6 feet in width, making the distance from centre to centre of the piers, 70 feet. The superstructure consists of a double line of wrought-iron boiler-

¹ Vide Minutes of Proceedings Inst. C.E., vol. ix., page 257, et seq.

plate girders, each 140 feet long and 4 feet deep. Each such length rests on three bearing points, being fastened to the middle one, and free to move at the two ends. The strain and the deflections of this girder will now be examined; first, when both spans are loaded; and secondly, when one only is loaded.

For the sake of simplicity, let it be supposed, as in the case of the Britannia Tube, that the spans are brought close together, and the breadth of the pier diminished to a line, or edge.

Fig. 4.



Then, in the first case,

$$\frac{l}{12} = \frac{l}{12} = 64$$

$$\mu = \mu = \text{half the weight of the structure per foot run} + \text{half the rolling load} = 0.166 + 0.5 = 0.666 \text{ tons}$$

$$\text{also, } EI = 1,356,000.$$

The general equation (7) then becomes

$$\begin{aligned} \frac{16}{12} \frac{l}{12} \phi_2 &= 2 \frac{\mu}{12} \frac{l^3}{12} \\ \phi_2 &= \frac{\mu}{8} \frac{l^3}{12} = \frac{0.666 \times 64^3}{8} = 336 \end{aligned}$$

$$P_1 = \frac{\mu}{2} \frac{l}{12} - \frac{\phi_2}{l} = 21.33 - 5.25 = 16.08$$

$$P_2 = 21.33 + 5.25 = 26.78$$

$$P_3 = P_1 = 16.08 \quad P_2 = 2 \cdot P_1 = 32.16$$

For the points of contrary flexure, the general equation (15) becomes

$$x = \frac{P_1 \pm \sqrt{P_1^2}}{\frac{\mu}{12}} = 0, \text{ and } 48.24 \text{ feet from point 1.}$$

For the deflection at the middle of span, the general equation (10) becomes

$$y = \frac{l^3}{384 EI} (24 P_1 - 7 \frac{\mu}{12} \frac{l}{12}),$$

which, by substituting the above values, gives

$$y = 0.045 \text{ feet} = 0.54 \text{ inch.}$$

In the second case, where one span only is loaded, $\mu = 0.166$, the other quantities being as before. The general equation (7) then becomes

$$16 l \phi_2 = \left(\frac{\mu}{12} + \frac{\mu}{24} \right) l^3$$

$$\phi_2 = \frac{\frac{\mu}{12} + \frac{\mu}{24}}{16} l^2 = 213$$

$$P_1 = \frac{\mu l}{2} - \frac{\phi_2}{l} = 21.333 - 3.333 = 18$$

$$\cdot P_2 = \quad = 21.333 + 3.333 = 24.67$$

$$P_2 = \frac{\mu l}{24} + \frac{\phi_2}{l} = 5.333 + 3.333 = 8.67$$

$$P_3 = \quad = 5.333 - 3.333 = 2$$

$$P_2 = \cdot P_2 + P_2 = 33.34.$$

For the point of maximum strain in span 1.2, the general equation (13) becomes

$$x = \frac{P_1}{\frac{\mu}{12}} = \frac{18}{0.666} = 27 \text{ feet from point 1.}$$

For the amount of maximum strain, the general equation (14) becomes

$$\phi = \frac{\mu}{2} x^2 - P_1 x$$

putting $x = 27, \quad \phi = -243.$

For the point of contrary flexure,

$$x = \frac{2 P_1}{\frac{\mu}{12}} = 54 \text{ feet from point 1.}$$

For the deflection at the centre of span, proceeding as before,

$$y = 0.0685 \text{ feet} = 0.822 \text{ inch.}$$

Now, the weight of the unloaded bridge is just one-fourth of that of the loaded bridge. The deflection, therefore, when unloaded, will be one-fourth of that when loaded, or $\frac{0.54}{4} = 0.135$ inch, in the middle.

The difference between this and that last determined, $0.822 - 0.135 = 0.687$, is, evidently, the deflection from its ordinary position, which should be occasioned by the passage of a rolling load of 0.5 ton to the foot run.

The actual deflection caused by the passage of a train of locomotives over a pair of these girders, erected for the purpose of the experiment in the station yard at Madras, was, on the mean of two trials, 0.755 inch. The weight was, however, a little in excess of a ton to the foot, or half a ton to each girder, and some slight further allowance should be made for the concentration of a portion of the weight on the driving wheels.¹ As these girders have been referred to, a peculiarity in their construction may be mentioned, which, as far as the Author is aware, was, at the time, new. For convenience of transport, the large beams, 140 feet in length, had to be made in several pieces, and the question arose, where the joints should be made.

The points fixed upon were at 52 feet distance from the supports 1 & 3, or about the mean between the positions of the point of contrary flexure, in the two conditions of the beam above considered. In neither case is the joint more than 2 feet distant from this point; consequently, the transverse strains are exceedingly small in both. In the case, however, of the unloaded half of the girder, in the second illustration, the point of contrary flexure recedes very considerably from the joint, and in fact, comes very near the bearing 3. It is desirable, therefore, to see to what amount of cross strain the joint may, probably, be exposed. Suppose the beam to be reversed, or turned right for left, then, at 52 feet from 3,

$$\begin{aligned}\phi &= \frac{\mu}{2} \times 52^3 - P_3 \times 52 \\ &= \frac{0.166}{2} \times 52^3 - 2 \times 52 = 121,\end{aligned}$$

This corresponds to a strain of 1.8 ton per square inch on the top and bottom fibres.

A more accurate examination, however, taking into account the breadth of the pier, showed, that the strains could not exceed 1.23 ton per square inch.

15. As a curious case, the strains and pressures at the supports

¹ A subsequent and more exact calculation of I , in which the loss of section from the rivet holes is taken into account, gives 1,264,000 for the value of EI , instead of 1,356,000, as above. The effect of this would be to increase the calculated deflection 0.687, by about one-thirteenth. It thus becomes 0.74, or almost exactly the observed deflection.

of a continuous rail of infinite length, supported at equal intervals of 3 feet, and loaded with a given weight at the centre of one of the spans, taking also into account the effect of the weight of the rail itself, will now be examined. Let the weight per yard of the rail be 75 lbs., or 0.0335 ton, and let the load be 5 tons; then, numbering the bearings from the one on the left of the load, and adopting the previous notation,

$$32 \varphi_2 + 8 \varphi_1 + 8 \varphi_3 = 4 \mu l^2 + 3 W l,$$

observing, that φ_1 evidently is equal to φ_2 , and taking dimensions in inches:—

$$40 \varphi_2 + 8 \varphi_3 = 4.8 + 540 = 544.8$$

$$\text{or,} \quad 5 \varphi_2 + \varphi_3 = 0.6 + 67.5 = 68.1$$

$$\text{also,} \quad 4 \varphi_3 + \varphi_2 + \varphi_4 = 0.6$$

$$4 \varphi_4 + \varphi_3 + \varphi_5 = 0.6$$

$$4 \varphi_5 + \varphi_4 + \varphi_6 = 0.6, \text{ \&c.}$$

$$\text{Hence,} \quad \varphi_2 + 0.2 \varphi_3 = 13.62$$

$$\varphi_3 + 0.263 \varphi_4 = - 3.48$$

$$\varphi_4 + 0.268 \varphi_5 = 1.095$$

$$\varphi_5 + 0.268 \varphi_6 = - 0.133$$

$$\varphi_6 + 0.268 \varphi_7 = 0.197$$

$$\varphi_7 + 0.268 \varphi_8 = 0.132$$

$$\varphi_8 + 0.268 \varphi_9 = 0.125$$

$$\varphi_9 + 0.268 \varphi_{10} = 0.127$$

$$\varphi_{10} + 0.268 \varphi_{11} = 0.127, \text{ \&c.}$$

After this there is no further variation in the coefficients, at least when only taken to three places of decimals, and consequently the successive φ 's may be taken to be equal.

$$\text{Hence, as } \varphi_9 = \varphi_{10},$$

$$\text{then,} \quad \varphi_9 = \varphi_{10} = \varphi_{11} \text{ \&c.} = 0.100$$

$$\varphi_8 = 0.098$$

$$\varphi_7 = 0.106$$

$$\varphi_6 = 0.169$$

$$\varphi_5 = - 0.178$$

$$\varphi_4 = 1.143$$

$$\varphi_3 = 3.782$$

$$\varphi_2 = 21.184$$

Hence,

$$\left. \begin{aligned} P_2 &= 0.017 + \frac{2.5}{36} = 0.517 \\ P_2 &= 0.017 + \frac{24.96}{36} = 0.711 \end{aligned} \right\} P_2 = 3.228 \text{ down}$$

$$\left. \begin{aligned} P_3 &= 0.017 - \frac{24.96}{36} = -0.677 \\ P_3 &= 0.017 - \frac{4.923}{36} = -0.120 \end{aligned} \right\} P_3 = -0.797 \text{ up}$$

$$\left. \begin{aligned} P_4 &= 0.154 \\ P_4 &= 0.017 + \frac{1.321}{36} = 0.054 \end{aligned} \right\} P_4 = 0.208 \text{ down}$$

$$\left. \begin{aligned} P_5 &= -0.020 \\ P_5 &= 0.017 - \frac{0.347}{36} = 0.007 \end{aligned} \right\} P_5 = -0.013 \text{ up}$$

$$\left. \begin{aligned} P_6 &= 0.027 \\ P_6 &= 0.017 + \frac{0.063}{36} = 0.019 \end{aligned} \right\} P_6 = 0.046 \text{ down}$$

$$\left. \begin{aligned} P_7 &= 0.015 \\ P_7 &= 0.017 + \frac{0.008}{36} = 0.017 \end{aligned} \right\} P_7 = 0.032$$

$$\left. \begin{aligned} P_8 &= 0.017 \\ P_8 &= 0.017 \end{aligned} \right\} P_8 = 0.034, \text{ \&c., \&c.}$$

It thus appears, that a load of 5 tons placed on the middle of one of the spans of a rail weighing 75 lbs. per yard, exerts an appreciable influence in modifying the pressures and strains up to about the eighth bearing on either side; after which its effect is entirely imperceptible, and the pressures and strains become, sensibly, those simply due to the weight of the rail itself.

APPENDIX.

EQUATION 16.

As the derivation of this may not be quite obvious, it is here given :—

From 1 to $\frac{l}{2}$:—

$$EI \frac{d^2 y}{dx^2} = \phi_1 - P_1 x + \mu \frac{x^2}{2} \dots \dots \dots (1).$$

From $\frac{l}{2}$ to l :—

$$EI \frac{d^2 y}{dx^2} = \phi_1 - P_1 x + \mu \frac{x^2}{2} + W \left(x - \frac{l}{2} \right) \dots \dots \dots (2).$$

Integrating (1) :—

$$EI \frac{dy}{dx} = \phi_1 x - P_1 \frac{x^2}{2} + \mu \frac{x^3}{6} + EIT_1 \dots \dots \dots (3).$$

Integrating (2) :—

$$EI \frac{dy}{dx} = \phi_1 x - P_1 \frac{x^2}{2} + \mu \frac{x^3}{6} + W \frac{x^2 - lx}{2} + C.$$

When $x = \frac{l}{2}$, then the two last are the same :—

$$\therefore EIT_1 = -W \frac{l^2}{8} + C;$$

$$\therefore C = \frac{Wl^2}{8} + EIT_1 ;$$

$$\therefore EI \frac{dy}{dx} = \phi_1 x - P_1 \frac{x^2}{2} + \mu \frac{x^3}{6} + W \left(\frac{x^2 - lx}{2} + \frac{l^2}{8} \right) + EIT_1 (4).$$

Integrating again (3) :—

$$EIy = \phi_1 \frac{x^2}{2} - P_1 \frac{x^3}{6} + \mu \frac{x^4}{24} + EIT_1 x.$$

Integrating again (4) :—

$$EIy = \phi_1 \frac{x^2}{2} - P_1 \frac{x^3}{6} + \mu \frac{x^4}{24} + W \left(\frac{x^3}{6} - \frac{lx^2}{4} + \frac{l^2 x}{8} \right) + EIT_1 x + C_1 (6);$$

but when $x = \frac{l}{2}$ (5) and (6) are the same ;—

$$\therefore -C = W \left(\frac{l^3}{48} - \frac{l^3}{16} + \frac{l^3}{16} \right) = W \frac{l^3}{48};$$

$$\therefore EIy = \phi_1 \frac{x^2}{2} - P_1 \frac{x^3}{6} + \mu \frac{x^4}{24} + W \left(\frac{x^3}{6} - \frac{lx^2}{4} + \frac{l^2 x}{8} - \frac{l^3}{48} \right) + EIT_1 x;$$

but when $x = l$ $y = 0$:—

$$\therefore 0 = \phi_1 \frac{l^2}{2} - P_1 \frac{l^3}{6} + \mu \frac{l^4}{24} + W \frac{l^3}{4} + EIT_1 l;$$

$$\therefore \text{EIT}_1 = -\phi_1 \frac{l}{2} + P_1 \frac{l^2}{6} - \mu \frac{l^3}{24} - W \frac{l^3}{48};$$

$$\therefore \text{EI}y = \phi_1 \frac{x^3 - lx}{2} - P_1 \frac{x^3 - l^2 x}{6} + \mu \frac{x^4 - l^2 x}{24} + W \left(\frac{x^3}{6} - \frac{lx^2}{4} + \frac{5l^2 x}{48} - \frac{l^3}{48} \right).$$

When $x = \frac{l}{2}$:—

$$y = \frac{1}{\text{EI}} \left(-\phi_1 \frac{l^3}{8} + P_1 \frac{l^3}{16} - \mu \frac{7l^4}{384} - W \frac{l^3}{96} \right)$$

For span 2.3 :—

$$\text{EIT}_2 = -\phi_2 \frac{l}{2} + P_2 \frac{l^2}{6} - \mu \frac{l^3}{24} - W \frac{l^3}{48} \dots \dots \dots (7).$$

Similarly for span 1.2 :—

$$-\text{EIT}_1 = -\phi_2 \frac{l}{2} + P_2 \frac{l^2}{6} - \mu \frac{l^3}{24} - W \frac{l^3}{48} \dots \dots \dots (8):$$

$$\text{but } P_2 = \frac{\mu l + W}{2} + \frac{\phi_2 - \phi_1}{\frac{l}{2}}$$

$$\text{and } P_1 = \frac{\mu l + W}{2} + \frac{\phi_2 - \phi_1}{\frac{l}{2}}$$

adding (7) and (8), and reducing :—

$$16 \left(\frac{l}{12} + \frac{l}{23} \right) \phi_2 + 8l \phi_1 + 8l \phi_2 = 2 \mu \frac{l^3}{12} + 2 \mu \frac{l^3}{23} + 3 W \frac{l^3}{12} + 3 W \frac{l^3}{23}.$$

As a verification of Equation (16), the result obtained by it may be compared with the case investigated by Moseley, in his "Mechanical Principles of Engineering and Architecture." Article 379.

In this case it becomes

$$16 \left(\frac{l}{12} + \frac{l}{23} \right) \phi_2 = 3 W \frac{l^3}{12} + 3 W \frac{l^3}{23};$$

$$\therefore \phi_2 = \frac{3 W \frac{l^3}{12} + 3 W \frac{l^3}{23}}{16 \left(\frac{l}{12} + \frac{l}{23} \right)}.$$

$$(A) P_1 = \frac{W}{2} + \frac{\phi_2}{\frac{l}{12}} = \frac{W l \left(\frac{8l}{12} + \frac{5l}{23} \right) - 3 W \frac{l^2}{23}}{16 l \left(\frac{l}{12} + \frac{l}{23} \right)}$$

$$P_2 = \frac{W}{2} - \frac{\phi_2}{\frac{l}{12}}$$

$$P_3 = \frac{W}{2} + \frac{\phi_2}{\frac{l}{23}}$$

$$(B) P_2 = \frac{W}{2} - \frac{\phi_2}{\frac{l}{23}} = \frac{W l \left(\frac{8l}{12} + \frac{5l}{23} \right) - 3 W \frac{l^2}{23}}{16 l \left(\frac{l}{12} + \frac{l}{23} \right)};$$

adding together P_1 and P_2 , substituting and reducing :—

$$(C) P_2 = \frac{1}{2} \left\{ W \left(1 + \frac{3l}{8l} \right) + W \left(1 + \frac{3l}{8l} \right) \right\}.$$

Expressions (A), (B), and (C), being identical with Moseley's, 576, 577, and 578.

These expressions are only used for the purpose of comparison. To obtain the numerical results in any particular case the process is very much more simple; thus suppose

$$\begin{aligned}
 & l_{12} = 10 \text{ feet } l_{23} = 15 \text{ feet,} \\
 & W_{12} = 6 \text{ tons } W_{23} = 8 \text{ tons,} \\
 \text{then } \phi_2 &= \frac{3 \times 6 \times 10^3 + 3 \times 8 \times 15^3}{16 \times 25} = 18. \\
 P_1 &= \frac{6}{2} - \frac{18}{10} = 1.2; \\
 P_2 &= \frac{6}{2} + \frac{18}{10} = 4.8 \\
 P_3 &= \frac{8}{2} + \frac{18}{15} = 5.2 \quad \left. \vphantom{\begin{matrix} P_2 \\ P_3 \end{matrix}} \right\} P_2 = 10; \\
 P_3 &= \frac{8}{2} - \frac{18}{15} = 2.8.
 \end{aligned}$$

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The general equation (8) for spans 1.2, and 2.3, becomes

$$\begin{aligned}
 & 8 \left(l_{12} + \frac{l_{23}}{2} \right) \phi_2 + \frac{4}{3} l_{23} \phi_3 = \frac{\mu_{12} P_{12}}{12 \cdot 12} + \frac{\mu_{23} P_{23}}{2 \cdot 23 \cdot 23} \\
 & l_{12} = 460 \quad 2.6628 \\
 & \frac{l_{23}}{2} = 0.607324 \quad 1.7834 \\
 & \frac{l_{23}}{2} = 279.4 \quad 2.4462 \\
 & l_{12} = 230.0 \\
 & \quad 509.4 \\
 & \quad 8 \\
 & \quad 4,075.2 = \text{coefficient of } \phi_2. \\
 & \frac{l_{23}}{2} = 279.4 \\
 & \quad 4 \\
 & \quad 1,117.6 = \text{coefficient of } \phi_3. \\
 & l_{23} = 2.6628 \\
 & P_{23} = 7.9884 \\
 & \mu_{23} = 3.38 \quad .5289 \\
 & \frac{l_{23}}{2} = 1.7834 \\
 & \quad 199,850,000 \quad 8.3007 \\
 & l_{23} = 2.3617 \\
 & P_{12} = 7.0851 \\
 & \mu_{12} = 2.6 \quad .4150 \\
 & \quad 31,630,000 \quad 7.5001 \\
 & \quad 231,480,000 \text{ absolute term.}
 \end{aligned}$$

To clear of coefficient of ϕ_1 :—

	1,117.6	3.0483
	4,075.2	3.6101
	0.2743	1.4382
Coefficient of ϕ_2	231,480,000	8.3645
		3.6101
absolute term,	56,810	4.7544
$\phi_1 + 0.2743 \phi_2 = 56,810$		

The general equation (8) for spans 2.3, and 3.4, becomes, remembering that $\phi_2 = \phi_4$ and $l = l_2$

$$16 l_{23} \phi_2 + 8 l_{23} \phi_2 = 24 E I_{23} + 2 \mu l_{23}^3$$

$$\text{or, } 2 \phi_2 + \phi_2 = \frac{12 E I_{23} + \mu l_{23}^3}{4 l_{23}}$$

$\frac{l_{23}^3}{23}$	7.9884
$\frac{\mu}{23}$	5.289
329,080,000	8.5173
$l_2 = - 0.00409$	3.6096
$I = 1,584$	3.1998
$E = 10,000$	4.0000
$\times$	
144	2.1584
12	1.0792
- 111,430,000	8.0470
217,650,000	8.3377
$4 l_{23} = 1,840$	3.2648
$2 \phi_2 + \phi_2 =$	118,250
	5.0729

The equations (5) and (6) for span 1, 2, become

$$P_1 = \frac{\mu l_{12}}{2} + \frac{-\phi_2}{l_{12}}$$

$$P_2 = \frac{\mu l_{12}}{2} + \frac{\phi_2}{l_{12}}$$

$-\phi_2 = - 47,030$	4.6724
$l_{12} = 230$	2.3617
204.5	2.3107

l	2.3617
l_{23}	
μ	4150
μ_{23}	
	<u>2.7767</u>
2	3010
	<u>299.0</u>
	<u>2.4757</u>
	94.5 = difference P_1 ,
	503.5 = sum P_2 .

The equations (5) and (6) for span 2.3, become

$$P_2 = \frac{\mu l_{23}}{2} + \frac{\phi_2 - \phi_3}{l_{23}}$$

$$P_3 = \frac{\mu l_{23}}{2} + \frac{\phi_3 - \phi_2}{l_{23}}$$

$$\phi_2 = 47,030$$

$$\phi_3 = 35,610$$

$$11,420$$

l	4.0577
l_{23}	2.6628
	<u>24.8</u>
	<u>1.3949</u>
l	2.6628
l_{23}	
μ	5289
μ_{23}	
2	3.1917
	<u>3010</u>
	<u>777.5</u>
	<u>2.8907</u>
	802.3 sum P_2 ,
	752.7 difference P_3 .

No. 1,028.—“On Breakwaters.” Part. II.¹ By MICHAEL SCOTT, M. Inst. C.E.

IN December, 1858, a Paper by the Author was read before the Institution, “On the Breakwater at the Port of Blyth, and Improvements in Breakwaters, applicable to Harbours of Refuge,”² which gave rise to a long and animated discussion. Having been requested to contribute another Paper, in continuation of this subject, and believing that a notice of the portion of the Blyth Breakwater, which has been completed since the date of the previous communication, will be interesting to the Institution, the Author gladly acceded to the request.

It was originally intended, that the present Paper should contain a discussion of some important questions, relating to harbours, including entrances, jetties, and quays; but it has been considered best to postpone this part of the subject to another opportunity.

THE COMPLETION OF THE BLYTH WORK.—To quote the words of the former Paper:—“In the case of the portions of the Blyth Pier just referred to, the site was either dry, or nearly so, at low-water spring-tides; but the line upon which it was to be continued, led into a depth of 5 feet, or 6 feet, at lowest spring-tides, and about 22 feet at high-water spring-tides.”³ During last year, this extension was carried out, and the breakwater has been completed, with entire success. After the particulars given in the former Paper, it will be unnecessary to enter into any lengthened description of the part of the work now referred to; it will be sufficient to direct attention to one, or two points of importance. In the course of the discussion on the former Paper, the two main objections to this system of constructing a breakwater of timber and stone, were the assumed difficulty of erecting large frames in deep water, and an alleged deficiency in strength, when completed. It does not often happen, that the experience of a single season affords so complete an answer to both of these objections.

First; as to the difficulty of erecting frames in deep water. The frames at Blyth are 10 feet apart from centre to centre; but

¹ The discussion upon this Paper was resumed on the first evening of the following Session, 1860-61, but an abstract of the whole is given consecutively.

² *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., p. 72.

³ *Ibid.*, p. 74.

notwithstanding this projection of each successive frame beyond the end of the work, and of the existence, at times, of a cross current of about $3\frac{1}{2}$ miles per hour, they were erected with the greatest ease, and generally, between half-tide and high water. So far from a still greater depth of water being any objection, it is believed, that it would facilitate the work. The Author, therefore, considers, that there is no longer room for doubt, that the work described last year, and designed for erection in 10 fathoms of water, could be constructed with expedition and success. It should be remembered, that in the larger work, the frames are only 6 feet apart, instead of 10 feet, as at Blyth.

The method by which the panels of planking, which were fixed with great facility, were hauled down under water, was somewhat novel. At each end of the panel, a hook was fixed, from which a hauling line was carried through a single block below, up to the water level. This block was held in position by another line attached to it, and passed through a staple, driven into the frame before being sunk, and temporarily made fast to the work above. There was a similar arrangement at each end of the panel. The panel being placed between two uprights, and the ropes made fast to it, by means of the hooks, it was hauled down to the bottom and secured; the upright bars serving, laterally, as guides. When the panel was fixed, the line was cast off, and the block and ropes were hauled to the surface. This system of planking was the most suitable in such cases as Blyth, but the Author would not recommend it for adoption in deep water; for not only would the large panels be difficult to handle, but this form of construction is not so strong as the arrangements formerly described.

With respect to the time occupied in the erection of a work of this description, the experience at Blyth shows, that two frames, or 20 lineal feet of breakwater, could be completed per day during the season. With regard to cost, it has been proved beyond doubt, that the system of depositing frames is much cheaper than piling.

Secondly; as regards strength. The power of the sea on the coast of Northumberland during a storm, is so well known, that it is unnecessary to adduce proofs of the fact. As formerly mentioned, the long fetch of the North Sea must, occasionally, produce a very heavy sea, and yet the weakest part of the work described, has now been exposed for four winters, without injury. The storm which occurred on the 25th of October, 1859, was stated to be the most severe that had been felt on the eastern coast, for many years; and the damage done to many sea works attests the truth of the statement. But although a tremendous sea broke against the Blyth breakwater, it was not injured in the slightest degree. An important consideration, which should not be lost

sight of, for it affects both the questions of facility of construction and of strength, is, that the frames at Blyth are 10 feet apart; whereas in the designs exhibited last year, which were objected to as deficient in strength, the frames were only 6 feet apart, which would, of course, render the work proportionately stronger than that at Blyth. The Author is still of opinion, that such breakwaters are stronger, and that they can be erected for less money and in less time, than any arrangement previously in use. The round end constitutes an elegant termination to the breakwater, and has proved, as was anticipated, to be a work of great strength. The planking is interlaced in such a manner, that the planks become a series of hoops, tending to bind the whole work together, and imparting great strength, in proportion to the material employed. Scarcely any difficulty was experienced in the erection of this part of the work. The cost per lineal foot of the breakwater, including the round end and the lighthouse, was about £11.

The Author would again refer to the importance of saving time in construction; for surely, the time which is required to inclose a harbour, is a matter of the greatest consequence. Every year saved represents not only commercial advantages, but, in refuge harbours, life itself. Now the system of construction described, possesses these qualifications in an eminent degree. The harbour is speedily inclosed, and a large saving of money is effected, by deferring an important part of the expenditure.

It might have been thought impossible to question the rigid correctness of this principle. But it has been questioned; and therefore, the Author would refer to the Tables printed with the former Paper,¹ to the evidence given by him, last year, before the Royal Commission on Harbours of Refuge, to their Report thereon, and to the Author's reply, by which it was clearly proved, that by erecting a breakwater, like those described, in the first instance, and subsequently, removing the timber, and facing the work with stone, the result would be a considerable saving both in time and money. With regard to the durability of creosoted timber, it may be stated, that whilst at Blyth, the timber in its natural condition is rapidly destroyed by the worm, the creosoted timber has undergone no change during the past four years.

PROPOSED FORM OF BREAKWATER.—Whilst such arrangements, as have been described, present many advantages, there are situations to which neither they, nor any ordinary kind of breakwater, would be applicable, and conditions which none of them

¹ *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., pp. 154-158.

fulfil. It was the consideration of this point, which led to the designing of the breakwater now to be described.

In order to explain the advantages which are claimed for this work, it is necessary to advert to certain questions of principle involved in the formation of harbours, and in the construction of breakwaters. As it will be unnecessary to repeat all that was advanced upon the subject in the previous Paper, and as it is intended to avoid nautical and commercial questions, so far as possible, attention will only be directed to the following points, which should be kept in view by the Engineer:—

First; A desirable site, offering sufficient area and depth of water, having been pointed out, the facility for obtaining suitable materials for constructing the breakwater, becomes the principal question.

Secondly; Although the direction of the entrance, or entrances, is chiefly a nautical question, and may be considered as partly included in that of suitability of site, yet the Engineer must have regard to it, as it affects his work, and it might, for example, involve silting.

Thirdly; The object of a harbour being to afford shelter from the sea, the inclosing work will be efficient, in proportion as it produces freedom from undulation in the harbour.

Fourthly; The depth of water in the harbour must be maintained, and silting prevented, as far as possible. Then with regard to the inclosing work, or breakwater, the following considerations will weigh with the Engineer, in addition to those previously mentioned.

What is the magnitude of the waves to be encountered, and will they be so large, or the depth so limited, that the waves will break on the work?

How can the work be most rapidly executed?

Is it possible to apply mortar work, in place of dry work, the former being so much stronger?

Having premised thus much, the Author will refer, for illustration, to a well-known instance, in which great difficulty has been experienced in designing a suitable breakwater, and to which none of the forms hitherto employed would appear to be applicable, —Table Bay, Cape of Good Hope. In this case, the principal difficulties arise from the immense magnitude of the rollers, which are waves of the first order, or waves of translation, and from the risk of silting, which there is reason to apprehend, would be produced by the current round the bay.

A brief notice of the leading peculiarities of the ordinary forms of breakwater, will suffice to show, whether any one of them would meet the requirements in the above instance. The first form of breakwater, taken in the order of time of its introduction, is the

slope, of which the work at Plymouth is a type. (Plate 8). The advantage which may be claimed for this form, where stone is very abundant in the immediate vicinity, is the supposed cheapness; but this advantage is neutralised, to a great extent, by the necessity for employing large blocks of dressed stone, to form the face of the slope. From the great quantity of stone used, it really becomes costly to construct, and is expensive to keep in repair; and, except in special cases, it is not so efficient as some other forms, as the sea partly rolls over it, producing disturbed water inside, which would be seriously felt in a harbour of limited area. It is clear, that for the situation in view, even although rough stone be abundant and cheap, this form would not be suitable.

The second form, is the vertical wall on a rubble bank, the bank sloping from low-water mark downwards to the bottom, like the breakwater at Alderney. (Plate 8.) This form would be objectionable, because the waves would break on the work; and how far it would be likely to stand the blows of the tremendous seas at the Cape, is problematical. It would be objectionable in a minor degree, because it requires squared and well-set stones in the face of the wall, which, together with the necessity of building from one end, renders it expensive and tedious in construction; and this form would not prevent silting.

The same observations apply to the third form, which is that adopted at Holyhead, and also at Portland, (Plate 8,) where there is a vertical wall on a rubble bank, which slopes from a few feet above high water downwards. If such a work failed to resist the seas at Table Bay, not only would the breakwater be destroyed, but the harbour would be ruined, by the large stones being strewed over the anchorage. Of course, this form is only applicable where stone is abundant, and even then, it becomes a tedious and comparatively expensive work.

The fourth form is the vertical wall, built either from the bottom, or from the depth below low water, at which the action of the waves ceases to be felt; the pier at Dover, (Plate 8,) is an example of the former. These two modifications are both correct in principle, when the sea is unbroken. But in the locality referred to, Table Bay, the waves are said to be breaking waves; and even if they were not, the height of the wall above high water, necessary to receive the rise of such enormous waves, would render this form inapplicable. Moreover, the hydrostatic pressure of the water against so high a wall, would require great width of structure to afford the necessary stability. For, it must be remembered, that a wave rises against a vertical wall to twice its height; and if the wall is not high enough, unbroken water is sent over into the harbour, doing damage on its way, and reproducing a wave inside. Neither of these forms would prevent silting.

It would, consequently, appear, that none of the ordinary kinds of breakwater would be applicable to Table Bay; and none of them would provide in any way against the silting up of the harbour and entrance, a point of importance, not only in Table Bay, but in other situations.

The Author has now to submit to the notice of the Institution, the proposed new kind of breakwater, (Plate 8,) which, it is believed, will be found to possess several advantages, and which, he thinks, would be applicable in such localities as the Cape. It will be observed, that it is a modification of the wave screen, described in the Paper contributed by the Author in 1858. But whilst the same principle is involved, the new arrangement differs from the screen of 1858, in some important particulars, such as being applicable in deep water, &c. In the case of deep water, and where stone is to be readily obtained, a bank of rubble might be deposited, rising to within, say 15 feet, or more, of low-water mark, the height of the bank varying with the circumstances of the locality. Upon this bank it is proposed to build a face wall, up to low-water mark, and behind this wall, long counterforts, the upper surface of which would rise from low water, at an inclination of about 2 to 1, and extend back for a distance dependent upon the amount of slope, rendered necessary by the magnitude of the waves. These counterforts would be placed at sufficient intervals, say of 20 feet, as to be conveniently spanned by iron girders, and the whole of the sloping surface would be converted into a sort of gridiron, by girders laid from pier to pier, the upper flanges being about 1 foot wide, and the girders laid at intervals of about 18 inches.

Supposing such a breakwater to be erected and exposed to a heavy sea, if the waves are not breaking, the water would be projected up the slope, and would drop through the spaces between the girders; and if the waves are breaking, they will rush up the slope as a confused mass of water, dropping through on their passage. But, although it is anticipated, that the great bulk of the water would pass through the grating, and not return to the foot of the slope, the operation would be gradual and be diffused over a considerable surface, so that a wave would not be reproduced inside. The only effect would be a stream of water into the harbour, and in this particular, the proposed form differs in principle from all vertical screens, or gratings, which, by permitting the waves to pass through, at the same instant of time, have not the effect of destroying the undulation. The breakwater should, if possible, be placed at an angle with the direction of the greatest sea, so that a wave should not only run up the slope, but along it, diffusing itself in this manner, over a much larger area; and this would be especially desirable in a case like Table Bay, where the waves are of such

magnitude. It is supposed, that this form of breakwater possesses further advantages; it would not only be efficient as a breakwater, but it would also, in the opinion of the Author, be sufficiently strong to resist waves which would probably wreck any other form, arranged to afford equal protection. Moreover, it will be observed, that the surface exposed to the blows of the waves, is covered with mail, so to speak, and could not be disintegrated. It would be durable, for the iron is not of the nature of thin plates readily destroyed by oxidation; but it is in solid masses, the whole of which is above water, and could, therefore, be preserved by painting. From its simplicity, it would be easily and quickly constructed. The face wall and counterforts might be formed of large rubble blocks prepared on shore. The risk of interruption to the progress of the work, would then be confined to the period during which the blocks were being deposited; and with the facilities described in the former Paper, a great length of breakwater might be erected in a single season. It should also be noticed, that as each counterfort constitutes a complete and separate work, the risk of damage, by storms during erection, is reduced to a minimum. It would be cheap, and it would cost considerably less than the breakwaters at Dover, Alderney, Holyhead, or Portland. A breakwater for Table Bay would stand in water, on the average, about 7 fathoms deep. Calculated at prices current in this country, the cost of such a work, in 7 fathoms of water, would not amount to £80 per foot run. Moreover, whilst ordinary rubble work will withstand the action of the sea for an indefinite time, it requires the best work to resist the blows of the waves. In this respect, it will be obvious, that the new work possesses great advantages, so that comparatively inferior material might be employed with success in its construction. Again; in the case of the work being erected abroad, where skilled labour was scarce and expensive, the whole of the iron work, in a finished state, could be advantageously supplied from this country. Finally; and this is a novel feature of the new form, whilst it would operate successfully as a breakwater, it is thought, that it would tend to prevent the silting which arises from sand brought in by the tide.

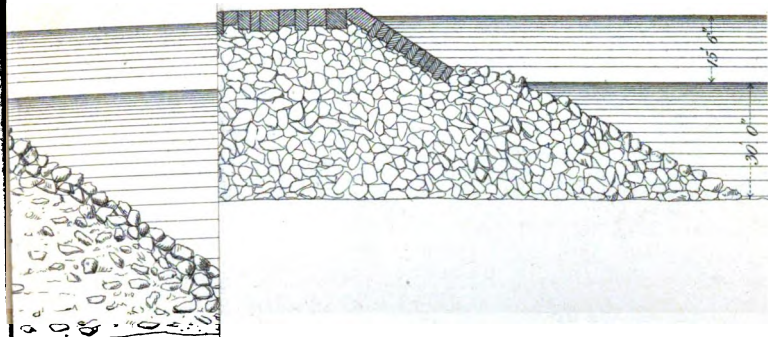
This question, however is too important to be dealt with incidentally, and belongs to the next Paper, in which it will be discussed.

The Paper is illustrated by diagrams and models, from which Plate 8 has been compiled.

[Mr. J. SCOTT RUSSELL

Harbour Face

Harbour Face



Sea

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SEC GRIDIRON BREAK

Minutes

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Mr. J. SCOTT RUSSELL said, that the former Paper had given rise to a valuable and interesting discussion. In the present Paper, the Author had recapitulated some of his arguments, and had confirmed his views of the efficiency of the breakwater at Blyth Harbour ; he had also submitted a plan of a gridiron, or wave screen, on the top of a rubble bank, to stop the progress of the waves. It was difficult to estimate truly the effect, in general terms, of the proposed arrangement, without knowing the depth of water, the dimensions of the works, and the height and length of the sea, in which it was intended to be placed. It was also necessary to consider the various uses of breakwaters, the various nature of the action of the sea, and the various ways in which the difficulties were now met by engineering expedients, before it was possible to decide whether this plan was, or was not better for practical use, than those hitherto acted on.

On a former occasion, the late Mr. Rendel had wisely cautioned Engineers, that they must not imagine, because any one method had been successful under one state of circumstances, it would, necessarily, be successful elsewhere, under others ; for every different locality and every different coast required, from the presence of materials, the nature of the sea, the conditions of the shore, and the prevalence of the winds, a construction of harbour peculiar to that place. Mr. Scott Russell, by experiments upon a moderate scale, and from observations wherever he had been able to make them, under circumstances approximate to the present case, had ascertained, that the progress of a wave of the first order was not retarded, by interposing an obstacle parallel to the ridge of the wave. Now it was proposed to substitute a wave screen, for the usual perpendicular, or sloping pier of masonry which formed the top of the breakwater. There were, no doubt, certain circumstances under which the propagation of a wave would be materially impeded by such a screen, and if another was placed at a certain distance behind it, so that the first might retard the wave till the superfluous water of it got into the hollow of the following wave, such an arrangement would be a great approximation towards preventing the continued propagation of the wave. But in sea works there were, practically, two classes of waves to deal with, of such different, if not, opposite natures, that what was beneficial in the one case, was often useless in the other. Now even supposing a structure of this kind to be durable, and economical in construction, it did not seem adapted to successfully resist a large ground swell, or wave of the first order, or wave of translation. There was every reason to believe, that the great ground swell of the Cape of Good Hope was of that character ; solid columns of water, sometimes as high as 30 feet, rolling in with an enormous progressive motion, and giving to the water a vast force of horizontal translation, extending

to a great depth. The interposition of a screen, even 80 feet in length, as proposed, would scarcely stop the translation of one of these enormous waves, which would, probably, pass very nearly over it, as if it were a solid slope, because the openings would soon get filled. In other words, the quantity of water that could be forced, in a short time, through these openings, would not be sufficient to relieve the pressure of the enormous mass of superincumbent water, even on the most advantageous supposition, that the wave impinged upon the structure at right angles. But if the wave impinged obliquely upon it, it would receive upon every one of its counterforts such an enormous pressure, that it was impossible to conceive, that any ingenious combination of them would enable them to resist the attack. He would resume, in a few words, his views upon the system of gridirons, or wave screens of open timber and iron piling. They were, no doubt, efficient in a degree, but he was not able at present, to fully appreciate the value of their effect in relation to their cost. The momentum of the ground swell was not, however, to be destroyed by any structure of small weight and great specific cost, and vertical piles would not oppose resistance to the vertical part of the motion of oscillating waves. For waves of translation, a gridiron, or series of piles, would only offer resistance, proportioned to the number and distance of the rows of the piles placed behind each other, and to the length between them. In regard to wind waves, so long ago as 1847, he had presented to the Institution, a Paper "On the Practical Forms of Breakwaters, Sea Walls, and other Engineering Works exposed to the action of Waves,"¹ in which he expressed a strong opinion on the expediency of certain forms of section, very different from one another. Where the water was not deep, and the object was merely to render the surface oscillating waves harmless to the work on which they broke, the cycloidal form he then recommended was, as he still thought, well fitted for the purpose. By this form, the action of the waves was rendered tolerably equal over the whole surface, thereby diminishing the chance of special injury at any one point, and stones of a moderate size, uniformly distributed, were each enabled to bear their portion of the power of the wave, and so together to withstand, without injury, the whole shock of the surface waves. But this form was only suited for this limited purpose. It was chiefly applicable to long shallow fore shores, liable to a sharp, short, but severe, set of waves, breaking at moderate heights, on account of the shallowness of the water in the offing; the oscillating wave being merely allowed to rise and fall along the surface, with the least possible injury to the structure of the sea wall.

¹ *Vide Minutes of Proceedings Inst. C.E., vol. vi., p. 135.*

But a true breakwater was a structure of a totally different nature, and designed for a totally different purpose, that of producing comparatively smooth water, not in the case of oscillating waves in shallow water, but of heavy waves and in deep water. Now in deep water, there were not only the oscillating surface waves to be encountered, but also those which he had termed waves of translation, forming what were called rollers at the Cape of Good Hope, and when on a smaller scale, known as ground swell. These were a much more troublesome class of waves; it was mainly with them that the Engineer had to deal, in places open to the Atlantic; and after a storm of some duration at sea, they became the deadliest enemies in the cases of deep water, against which breakwaters for harbours of refuge had to contend. These rollers, or ground swell, did not merely oscillate up and down, and backwards and forwards; and they could not be eluded, or turned back, by giving to the wall a particular curve, suited to the form of a cycloidal oscillation.

These great waves of translation constituted a vast mass of solid water, moving in one direction with great velocity, and this action was nearly as powerful at a great depth, as at the surface. They resembled the tidal bore of the Hooghly, of the Severn, or of the Dee; they formed a high and deep wall of water, of great weight, moving horizontally with great force, and causing all floating bodies they met with, to travel with them with great velocity, in the same direction. As he had before mentioned, they could not be eluded, or diverted, they must be stopped; and therein consisted the difficulty. The only certain way of effecting it, was to oppose to these waves a mass of matter so much heavier than themselves, that they could not move it. By so doing, the waves were compelled, either to roll over the obstacle, in which case they would create a new wave inside; or they must be made to break on themselves backwards, which required enormous power; or they must be completely reflected, which perhaps, required the greatest force of all.

To reflect, or send back the roller was the most effectual plan. For this purpose, nothing more was necessary than a deep perpendicular face of perfect masonry, and so long as it stood firm, it was faultless, and the water inside was smooth as in a millpond; for the reflection really converted the whole effect of the roller on itself, into a simple pressure of water. When such a wave was reflected on a perpendicular wall, it merely produced a hydraulic pressure, equal to that due to little more than double its own height. A roller, 20 feet in height, would produce a pressure of about a ton per foot, and it would be reflected by a vertical wall of moderate dimensions. He had, therefore, no hesitation in saying, that, cost apart, a wall of vertical masonry was the best, while it lasted. The primary cost of erecting a vertical wall of

[1859-60. N.S.]

2 x

perfect materials was, in most cases, so great, as to put it out of the question, setting aside the important point of durability, which was also cost in a serious form. A vertical wall of masonry had, however, this great disadvantage ; if the sea found out a weak place, it would enlarge it much more rapidly than in an inclined wall. He had seen a stone of small size show symptoms of crumbling at the beginning of winter, and in one week the little hole of single defective stone was converted into a circular breach, nearly 30 feet in diameter. An inclined wall, he had ascertained, would reflect a roller, or deep wave, nearly as well as a vertical wall, down to an inclination of 45° . This observation was of value, because at 45° , large blocks, judiciously placed, would not move out of position, even although a considerable breach was made in the wall near them. He had carefully watched the action of the sea, when approaching this slope, and his opinion was favourable to the trial of a wall of heavy rubble blocks fairly laid, so as to form a tolerably even face, or slope of 45° , when the nature of the materials and the local circumstances would permit.

When any approximative attempt at reflection of the wave, by walls not too remote from the vertical, had to be abandoned, recourse must, generally, be had to breaking the wave. This was a formidable thing to attempt on a deep-sea roller ; and it could only be done by opposing mass to mass. For this purpose, he had ventured to recommend, that the fore foot of the breakwater should be rounded off, and that the shape of the breakwater should be convex. The intention of this was, to cause the front wave to begin breaking at the earliest moment, to make the breaking last as long as possible, and thus to render the diminution of its momentum as complete as was practicable. In breaking a wave it was very important, that it should be made to break on the water, and not on the stones ; and the convexities given to the foreshore accomplished this object, by causing the waves to begin breaking, as far out as possible in deep water. The form for breaking a deep roller, should be entirely distinct from that used for meeting a superficial oscillating wave ; the one should be concave, the other convex. The Digue, or breakwater of Cherbourg,¹ showed an approximation to this form. A breakwater, to succeed in breaking the long waves of translation, rolling in from deep water after a storm, should have a long convex slope.

The practical construction of breakwaters was, however, scarcely confined either to resisting deep-sea rollers, or waves of translation, or to procuring the means of stopping the progress of common sea waves, or mere surface oscillation ; it usually combined both of these objects. It might, therefore, be considered as

¹ *Vide* Minutes of Proceedings Inst. C.E., vol. xviii., plate 2.

the general problem of a breakwater; first, to stop out the great wave of translation, and secondly, to still the oscillating surface wave. A vertical wall effected both these conditions, and so also did a convex sea-slope, with a vertical pier. But the best plan of all was, first to carry up a convex sea-slope of rubble, to near the surface of the water, and thus break the force of the heavy ground swell, that would sweep ships from their anchors, and lay them high and dry on a lee shore. This would allow the wave to break to pieces on itself, and to expend its force in raising so much water as represented its momentum, to a height above its former place, sufficient to exhaust its power; in other words, to expend its power on water. Secondly, to carry up from the top of the rubble slope, a wall slightly inclined, to reflect the waves of oscillation near the surface, which could do no harm, if quietly resisted and sent back. This plan seemed to be that which had been found to answer best in practice. He further considered, that making a step backwards in the upper work of a breakwater, was of great value in preventing the tops of the waves from going over. He had carefully watched the effect at Marseilles, and his observation had confirmed the opinion he expressed in favour of it, in 1847.

These views of the nature of the forces to be resisted in breakwaters, and the methods of dealing with them, showed the inefficiency of what were called floating breakwaters. Large floating masses would, certainly, intercept oscillating waves of a small depth, and in moderate weather, they would often still the water. The lee side of the 'Great Eastern,' when lying at Holyhead, afforded excellent anchorage for small vessels, in light breezes. But when the great roller, the one great wave of translation came, the anchors snapped at once, showing the danger which would have been incurred, had she been moored broadside to the roller, instead of offering to it the small resistance of her fine bow. No known force could effectively secure a large floating breakwater broadside-on to a heavy ground swell. It would move horizontally with the wave of translation, which would propagate itself along the bottom, just the same as if the breakwater was not there.

Mr. LONGRIDGE thought it had been erroneously conceived, that this breakwater could be put down in blocks, at certain intervals; but the real intention appeared to be, to make it a continuous structure. He thought, that when a wave had once broken upon the breakwater, any propagation of it inside, would be of a very small and insignificant character. It would only consist of the quantity of water that passed through the grating; it could not, therefore, be a wave of translation; it would only be a wave of oscillation of a very minute character, which could not produce any injurious effect. But the distinction which had

been made between the two kinds of waves, was not satisfactory to his mind, so far as works of this nature were concerned; because the same work might, at one time of tide, meet waves of oscillation, and at another time, waves of translation. It could only be in very deep water, that pure waves of oscillation would occur. He did not see, how it was possible, in designing a breakwater for a given locality, to foresee where the wave of translation, and where the wave of oscillation would arise.

Mr. W. A. BROOKS said, that he considered justice had not been done to the works of former Engineers. No notice had been taken, for instance, of Kingstown Harbour, constructed entirely upon the long-slope principle, the slopes being at 5 to 1, and the total cost having been only 1s. 6d. per ton, which amount had been exceeded by the cost of the scaffolding alone of some of the modern public harbours. The sections of breakwaters gave only that of the inner pier of Portland Harbour, which was partly constructed of vertical masonry, but the great breakwater was altogether constructed upon the long-slope system, and the present Engineer, Mr. Coode, (M. Inst. C.E.,) had given evidence, that he did not believe it would be necessary to construct any vertical masonry, except for quays, or wharves on the inside. Mr. Brooks considered Kingstown Harbour as a perfect work, for its locality, but for more exposed situations it might, probably, be necessary to increase the slope from 5 to 6, or even 7 to 1, near the level of high water of spring tides, where the stroke of the wave was the heaviest. He was satisfied with the soundness of the system of constructing deep-water piers with a long slope, in preference to vertical walls, and he entirely agreed with the arguments used by the advocates of that system, at the former discussion on the subject.¹ He regretted to say, that in the execution of modern harbours of refuge, the science of construction appeared to have retrograded, instead of advancing.

He was well acquainted with Blyth Harbour, and he was confident, that the pier which had been constructed there, could not, with propriety, be adduced as an example of the success of vertical walls, when exposed to a stormy sea. The pier was constructed on a ledge of rock, extending about 4,700 feet from the shore, and it covered about 4,400 feet of that length. The surface of the rock was from 10 feet to 6 feet above the level of low water of spring tides, and at the outer end, or seaward of the pier, the rock was 6 feet in height above low water. The seaward margin of the rock was called the 'Sow and Pigs,' and it was situated at the great distance of 1,100 feet from the foot of the pier constructed by Mr. J. Abernethy, (M. Inst. C.E.,) act-

¹ *Vide Minutes of Proceedings Inst. C.E., vol. xviii., p. 91, et seq.*

ing as a breakwater to protect the pier from heavy seas. The mass of rock on which the pier was built, had a breadth of about 400 feet seaward of the pier, between which and the Sow and Pigs rocks, or outer barrier, there was a pool from 5 feet to 8 feet deep at low water. Heavy seas broke at the distance of half a nautical mile from the line of pier. In addition to this great natural protection to the pier, offered by the presence of the rocks between it and the sea, there also existed, at a distance of 60 feet seaward of the pier, one of those old-fashioned, but useful long-slope piers, varying from $4\frac{1}{2}$ feet to 1 foot in height, and this extended for a length of about 700 feet. He considered, that the Engineer who constructed Blyth Pier, was not warranted in treating it as one which was exposed to heavy seas; it was, however, a very cheap and proper work for the situation, and so far, did credit both to the Engineer and to the Contractor.

Mr. M. SCOTT expressed his dissent from the statements which had just been made, relative to Blyth.

Mr. J. COCKBURN CURTIS could not agree with the Author of the Paper, as to the adaptation of the form and structure of the breakwater under discussion, to the special conditions of Table Bay, Cape of Good Hope. Whilst in the Royal Navy he had been, for some years, on the Cape station, and on several other occasions he had visited Table Bay. He was, therefore, well acquainted with the physical features of this peculiar case. Table Bay might be described as a bight, or recess, with a northern aspect, on the eastern side of the mass of mountainous and rocky land, which formed the peninsula of the Cape of Good Hope. This peninsula was the turning point of the wind and wave systems of the Indian and Atlantic Oceans, and its littoral portions, including Table Bay, were, at certain periods, subject to all the sudden and violent atmospheric and oceanic changes incidental to such a situation. The circle of wind influence around the Cape of Good Hope, had been variously estimated to extend from 300 miles to 600 miles from the land, and hence the 'lead' of the waves, or the length which they travelled in the course of their development, before reaching Table Bay, might be taken to be of nearly equal extent. The prevailing winds in Table Bay and near the Cape of Good Hope, were from the southward and eastward. In the summer months, from April to October, these winds were generally accompanied by settled weather; but in the months of January, February, and March, they sometimes blew, for several days together, with such violence, that vessels were frequently driven out of Table Bay, with all their anchors down, and were unable to regain their anchorage, until the gale had abated. The chief danger, however, to shipping and to any breakwater in course of, and after, erection,

arose from the gales from the N.N.W. These occurred, occasionally, in the summer months,¹ but more generally, during the winter months of June,² July, and August. They were always accompanied by a heavy swell, the waves of which, from the configuration of the bottom over which they passed, assumed the character of heavy 'shoal-water waves.' The bay having a northern aspect afforded no shelter from these gales, to the vessels moored in the anchorage, and being exposed to the full force of the wind and the sea, they were generally driven on shore. During the time he was on the Cape station, it was a common occurrence, after a north-westerly gale, to see the beach literally strewn with wrecks.

The breakwater was required to prevent the recurrence of these disasters, and to afford shelter for receiving and discharging cargoes, during fresh breezes and moderate gales. To answer these purposes, the greater portion of it would have to be constructed in 6 fathoms, or 7 fathoms of water, or a short distance outside of the breaking point of the crest of the waves, in north-westerly gales. Looking at the model of the breakwater proposed by the Author of the Paper, and bearing in mind the volume and the peculiar character of the waves, the condition in which they would arrive at the proposed structure, the momentum and velocity with which they would be travelling, as well as the possibility of north-westerly gales occurring during the progress of the work, he could not but concur with the opinions which had already been expressed, that such a mode of structure was inapplicable. He believed, that even if the breakwater could be completed as proposed by the Author, so far from the waves being broken up, or neutralised by it, they would, to use a sailor's expression, 'go clean over it;' and that by the action of a succession of such waves, the entire fabric would be speedily reduced to the state of a dangerous shoal-water reef, in the entrance of the bay.

He would add that, perhaps, there was no branch of duty of a Nautical Engineer which required so much discrimination as designing a breakwater, in reference to the peculiar character and condition of the waves which it was intended to reduce, or neutralise. The character of the waves and the many modifying circumstances of each case, could only be properly studied in the locality. It was, therefore, not surprising, that the Author of the Paper had selected a somewhat unfortunate illustration of the efficacy of the new system which he proposed; and although Mr. Curtis felt

¹ H.M.S. 'Sceptre,' of 64 guns, and several other large ships were driven on shore and wrecked, by a severe north-westerly gale in the month of November, 1799.

² The Dutch fixed the 10th of May, as the period beyond which it was not safe for vessels to stop in the bay. Vessels, however, ventured into the bay, in the winter months, for refreshment and for trading purposes, notwithstanding the risk of north-westerly gales.

confident, that such a breakwater would be unsuitable to Table Bay, yet, on the other hand, he felt bound to state, that he considered the Institution was under great obligations to the Author, for bringing forward the present Paper, as he could readily conceive, that there were other localities, where such a mode of construction might prove economical and efficient. So far, however, as Table Bay was concerned, he thought, that if an economical structure was proposed for simply neutralising the waves, a system of iron cylinders, or columns, as proposed by Mr. Brunlees, (M. Inst. C.E.), if erected in detached sections, would be the most effectual.

In 1847, when the subject of defences and breakwaters was discussed before the Institution,¹ he had stated that, notwithstanding he had been employed by the Admiralty on coast surveys in nearly every part of the world, and had paid much attention to the subject of the character and action of waves, he knew of no uniform rule which could be laid down as to the best form and material for maritime works; but that the only safe course which the Engineer could pursue, was carefully to acquaint himself with the peculiar circumstances of the locality and the special character and mode of action of the waves with which he had to deal, and then to adapt his structure to the conditions of the individual case. Notwithstanding all that had been written and said, and the costly experiments made during the last eleven years, with the view of generalising certain special systems of construction, he was still of the same opinion. They were merely attempts to arrive at conclusions without a careful study of the premises; for it might be safely stated, that the comparative stability, economy, and duration, of any hydraulic work, would be almost, if not entirely, proportionate to the amount of knowledge and skill, which had been bestowed on the preliminary survey of the physical features, and hydraulic phenomena of the locality.

Amongst other sources of error, he included the present theoretical division of waves into the two classes, waves of the first, and waves of the second order,—or waves of translation, and waves of oscillation. However useful such a classification might be in the tentative processes of mathematical research, he contended it was comparatively useless as regarded practical execution. The term, ‘great wave of translation’ was, no doubt, well adapted to denote the great wave of the first order, or tidal wave, but he found that misconceptions arising from application of these terms to the wind waves of the ocean and sea coast had, in no small degree, contributed to the many crude and impracticable schemes which, from time to time, were brought forward as ‘universal’

¹ *Vide Minutes of Proceedings Inst. C.E., vol. vi., p. 125.*

modes of constructing breakwaters and sea defences. He thought, that all waves not occasioned by earthquakes, tides, currents, or other extraneous causes, should be called 'wind waves,' as being solely due to the immediate, or latent action of the wind. For subordinate classification, he suggested, that it would be better to use names which gave a definite idea of the genesis, antecedent history, and existing character of the wave, such as, 'active surface-wind waves,' 'shoal-water wind waves,' 'latent shoal-water wind waves,' 'ground swell,' &c. Indeed, the ordinary terms used by seafaring men, conveyed complete ideas of every detail connected with a wave, and by the combination of them, any wave could be minutely and correctly described. When it was further considered, that the use of these terms would, at all times, place the Engineer 'en rapport' with his nautical informants, it certainly seemed a pity, unnecessarily to depart from a nomenclature derived from experience, and improved and consolidated by long-established usage.

In aid of a classification of this character, he would submit the following division and definitions of the terms more generally used by nautical men.

CLASS I. ACTIVE WIND-WAVES, or waves rapidly generated by the active, or immediate force of the wind.

1. Catspaw.—The first ripple produced by the wind.
2. Wind Waves.—Small waves with an unbroken top, or crest.
3. Crested Waves.—When the wind had increased its force sufficiently to push over, or break the top, or crest, of the wave.
4. Sea.—An accumulation of several crested waves, with a larger crest of its own.
5. Heavy, or Mountainous Sea.—An accumulation of several smaller seas.
6. White Waves.—When, by a sudden squall, or the violence of the wind, they were broken and laid low, as fast as they were generated.

CLASS II. LATENT WIND-WAVES, or waves slowly generated by the previously exercised force of the wind.

1. Cross Seas.—When, by a change of wind, or other cause, two systems of seas were crossing each other.
2. Chopping Sea.—Formed by two systems of seas, crossing each other at acute angles.
3. Swells.—Extensive waves without crests, and often, without existing wind, generated by gales which had subsided, or had taken place in some other locality from which the swell had travelled.

CLASS III. SHOAL-WATER WAVES.

1. Breakers.—Produced by the active wind-wave suddenly breaking against the coast, or on detached shoals, or reefs.
2. Surf Waves.—Periodical waves, formed when the ocean wave of either class in its progress to the shore, suddenly encountered a continuous shelving bottom.
8. Ground Swell.—Produced when the ocean swell got into shoal water ; it was generally without a crest.
4. Rollers.—Rolling waves generated when the ground swell, in its progress to the shore, encountered a long line of continuously sloping bottom.
5. Blind Breakers.—Waves rising suddenly at intervals, and breaking ; so called, from their not being perceptible, unless watched for. They occurred over bars of channels, and were, no doubt, produced by the ground swell.

Apart from the question of the terms which had been adopted for the purposes of classification, the profession, he considered, were much indebted to the scientific men who had so ably investigated the mathematical theory of waves. It was pleasing to note, how frequently the conclusions at which they arrived, agreed with the every-day observations and experience of practical men. The little step, or hummock, suggested by Mr. J. Scott Russell as an addition to a sea wall, in the case of a surf wave, was the actual shape assumed under the action of the surf wave, by the 'pierre perdue' of the Madras Bulwark ; and its correctness in principle was further borne out, by the lagoon configuration of the coast of Coromandel, and of Central America, localities where, probably, the heaviest surfs in the world existed. With reference to the surface, or ocean wind-wave, it was very remarkable how small an object was sufficient to break its crest and prevent its rolling onwards. The knowledge of this fact was most important, as the force of the wave was always concentrated in the onward rolling motion of its crest, and when once the crest was broken, the force of the wave was destroyed. In former days, the Mount's Bay boatmen, when caught in a gale, used to avail themselves of this property of the ocean wave. They made a kind of loose raft of their oars and spars, and hung on to it by a rope to leeward, with as much security as if they were at anchor ; the crest of the wave was thus broken by the raft, instead of continuing its course and rolling over the boat. Covering the surface of the water with oil, also prevented the formation of a crest. The Esquimaux, who had a similar custom to the Mount's Bay boatmen, availed themselves, in addition, of this property, by attaching to their spars large pieces of blubber, or seal's fat, the oil which drained from it stilling the surface of the water ; and by these means, their little kayacks were enabled to ride out, with

safety, the heaviest gales. Although the smallest boat might ride securely, if the crest of the wave was broken before it reached it, yet the safety of the largest ships was endangered when, from bad steering, or any other cause, they were struck by the crest. Under such circumstances, it was no uncommon occurrence for the vessel to have the whole, or the greater part of her upper works carried away, and the men and everything on her deck, swept overboard. In illustration of his previous remarks, he would add, that to call the 'active ocean wind-wave,' or 'sea,' which was the ordinary cause of this class of accidents, a simple 'wave of oscillation,' certainly did appear to him a misnomer greatly calculated to give erroneous ideas, in designing any special structure for the resistance, or neutralisation of this character of wave.

Mr. A. GILES said he did not think, that wooden breakwaters were adapted to deep water. They might, in some situations, be useful and economical, but he questioned their durability. The duration of timber, when not creosoted, was very limited. He had seen creosoted timber show but little signs of decay after ten years, but how much longer it would last, he was unable to say. The Author's model had a stone base, which showed, that he also did not consider a wooden structure adapted for a deep-water breakwater. It was evident, that the amount of water stopped by the gridiron breakwater, would be in proportion to the openings between the gratings; if these openings were of the same size as the solid intervening parts, he apprehended the water would go through, and form a wave of translation of nearly half the magnitude of that outside, although not so powerful. He concurred in the opinion, that if a breakwater of that kind was struck in an angular direction by a sea, the counterforts would receive the whole force of the blow. Instead of a series of counterforts, it would be better, he considered, to make the breakwater solid, with a larger quantity of an inferior material; this, with its great weight, would be superior, more economical, and more durable than a breakwater made of counterforts of squared blocks. With regard to the slope of the old breakwaters, he agreed in all that had been advanced respecting the convex curve; but he doubted whether the sections of the various breakwaters exhibited, were quite correct. Mr. Rendel had informed him that, at Holyhead, the sea within range of the heavy waves, had trimmed all the outer slope down to 6 to 1, but that in deep water, below the action of the waves, the stones rested at their natural slope of $1\frac{1}{2}$, or 2 to 1; so that he considered the convex form of the outer slopes of breakwaters, to be more the result of accident than of design.

Mr. HAWKSHAW, V.P., said that, assuming the high-water mark to be 50 feet from the bottom of the sea, a vertical line, a cycloidal line, or a line representing the slope formed by the sea, as at Holyhead,

would all be applicable for the purposes of a breakwater. The discussion resolved itself into two separate questions ;—the theoretical question, or which was the best form of wall to resist a deep-sea wave,—and the question of cost, which often compelled the adoption of a less perfect form than that deduced from theory. He should prefer a vertical wall, if it could be built at the same cost as the others, for he believed it to be the best form, and not liable, as had been objected, to be breached, any more than the other forms he had mentioned. Engineers had, therefore, to consider which of the three forms should, under all the circumstances of each case, be adopted. A vertical wall, in 50 feet of water, would require about 2,000 feet of masonry per foot forward : but if stone could be cheaply obtained in the locality, a rubble breakwater, though requiring ten times the amount of material, might be less expensive. But where stone was scarce and dear, the latter plan would be inapplicable. He had recently examined the breakwater at Marseilles, in which a steep slope had been adopted, and the requisite vis inertiae had been obtained, not by building a vertical wall, but by throwing blocks of béton, or concrete, weighing from 20 tons to 25 tons each, pell-mell into the sea, and by that means, a sufficient slope had been secured, to resist the shock of the waves. But each case would have to be considered separately, and a decision arrived at, not merely from a theoretical preference for any given form, but from a careful review of all the incidental circumstances which the Engineer would have to take into account.

Mr. J. ABERNETHY said, it was of the greatest importance to arrive at fixed principles, as regarded the forms of breakwaters. It had been urged, that local circumstances were so different, as to render it almost impossible to lay down any general rule on the subject. But although local circumstances might vary, the hydraulic laws which regulated the motion of waves were fixed and immutable : he thought, therefore, that a definite conclusion could be arrived at, as to the best form of breakwater ; first, for the deep-water oscillating-wave, and secondly, for the shoal-water wave of translation, or percussion. With regard to the first, he conceived that the long rubble slope, say of 7 to 1, between the levels of high and low water, which converted the deep-water oscillating-wave into a wave of translation, was an error in construction, not only as regarded the original cost, but also as to the future maintenance of works. It must be obvious, that the long seaward slope was exposed, not only to the percussive action of the waves of translation, but also to the recoil of the sea, or what sailors termed the 'undertow' of the wave. The effect of this constant action must be the conversion, by attrition, of the face on the seaward side, into a mass of boulder stones, and after every storm those stones would be dis-

turbed and drawn out, and the slope lengthened. To preserve that portion of the work, it would be found requisite to pave the whole with massive ashlar pitching, which would have the effect of giving the waves still greater force upon the vertical superstructure. As to the question of cost, (an important consideration in the cases of Portland, Plymouth, and Alderney, where such enormous masses of rubble were required to form the mound,) he thought the vertical form of structure would be found to be the cheapest. But there were other circumstances under which he would prefer the long slope. In shallow water, with insecure foundations, he believed the cycloidal form to be the best. At Aberdeen, the pier head was first constructed as a vertical wall in shoal water, but the foundation being insecure, it fell, more than once, from the constant percussion, and the effect of the 'undertow.' The cycloidal, or long-slope form was then adopted, by which a gradual resistance was offered to the force of the waves, and the pier head had now stood the test of nearly half a century. But it appeared to be manifest, that for deep-water breakwaters, required to resist the simple oscillating wave, the vertical wall was the proper form of structure; it was also the most economical and durable. Whatever fault might be found with the Dover breakwater, on the score of cost, he believed it to be the best example round the coast, as regarded form, for a deep-water breakwater.

Mr. BRUNLEES had already expressed some doubts as to the capabilities of the structure proposed by the Author, and to the durability of timber for such a purpose in a warm climate. He then exhibited specimens of the hardest wood of Brazil, which had been in use on the coast of Pernambuco, one of them during one year, another for two years, and another for three years. They had all been very much attacked by the teredo. The same deterioration had taken place in situations a long way down the same coast, and there was little doubt, that it would also occur in Table Bay.

He had designed a cast and wrought-iron breakwater and pier, which he considered presented several advantages. By placing the piles zigzag, at an angle of 90° , the greatest strength was obtained, for resisting the impact of the waves from any direction. He proposed, that each pile should be provided with a Mitchell's screw, which, in sandy, or loose foundations, would take the form of a disc, hydraulic power being used for sinking, as in the Morecambe Bay Viaducts. On reaching stiffer soils, or where the foundations were altogether of stiff material, the piles could be screwed down in the usual way. The louvre boards could be readily screwed and cemented into the grooves, thus adding to the strength of the structure, and, from the spaces between the piles being short, cast iron could safely be used. The space between low water and

the ground being comparatively open, there would be little obstruction to the run of the tides, and hence, there would be little tendency to silting up inside the harbour. The cost in England for a breakwater, 40 feet above the ground line, and 15 feet wide, would be £53 per foot run; and the cost of a breakwater and pier combined, of the same dimensions, would be £63 per foot run; including the angle pile, which should be made of boiler plate and be filled with timber, all the other cast-iron piles being filled with concrete.

Mr. A. J. ROBERTSON said, that as the model with an opening below, exhibited by Mr. Brunlees, constituted a type of a class which had been much commended, he would point out what he conceived to be an error in the principle of these breakwaters. Any wave impinging upon such a breakwater, would be propagated through the space underneath, and thus there would not be still water within the harbour. The piles were so arranged, that there were re-entering angles; consequently, the momentum of a wave, instead of being distributed over the whole space between the piles, would be concentrated into the middle angle, thereby occasioning undue pressure at that point. Again; the breakwater was vertical and had louver boards so placed, that they would divert the water, as it rose, from its natural path, and thereby produce considerable resistance. Mr. Scott Russell had spoken in favour of the cycloidal form of breakwater, because the wave would produce an equal pressure over the whole surface, but he did not state the mode in which he had arrived at that conclusion. He also said, that although the vertical form was highly desirable,—the true principle, in fact,—still he considered it had more to bear, and that there was greater risk of its being destroyed, than a wall which was on the top of a sloping foreshore. Mr. Robertson agreed with the Author, that the vertical form was the true principle, as laid down in his first Paper. With regard to the objection urged against the gridiron system, that the wave would be propagated through the openings, he believed it was impossible for a body of water which passed through the gridiron in detail, to produce a wave of any magnitude; at the most, it would only cause a number of small undulations, of no importance whatever.

Mr. BURNELL,—through the Secretary,—exhibited the section of a pile, said to have been creosoted, which was driven in one of the banks on the sea shore of North Holland, and had been, subsequently, attacked by the teredo navalis. This circumstance had attracted great attention, for it was considered to prove, that the process of creosoting did not protect wood from the attacks of the worm; and the Dutch Government had appointed a Commission to report upon the case. A cursory examination of the section would, however, suffice to demonstrate, that this particular pile had never been properly creosoted. Had a piece of timber of a similar description

been treated like the sleepers for the East Indian railways, it would have been thoroughly impregnated with creosote, even to the pith. The destruction of this pile, therefore, did not detract from the value of creosote, as a protection to timber from the ravages of the teredo ; it simply established a *prima facie* ground of accusation against those who undertook to perform the process.

He was also desirous of calling attention to another circumstance, which might have a far more important bearing upon the solidity of works in sea water, than might be suspected. Green-heart timber had, of late years, been much used, because it was believed to contain some peculiar acid, or flavour, which rendered it distasteful to the boring worms, and conferred on it an immunity from their attacks. Now on the 28th of April, he had noticed, that the surface of a remarkably fine stick of green-heart timber, then lying in the West India Docks, had been deeply furrowed by a worm, resembling the *lymexylon* : the heart wood was untouched. This fact did not, of course, necessarily imply, that the green heart did not possess immunity from the attacks of the teredo ; but it certainly appeared probable, that a wood, liable to be attacked by one class of boring insects, would also be liable to be attacked by others ; and he thought, therefore, that before green-heart timber was more largely used for Hydraulic Engineering, some experiments upon its qualities ought to be instituted. Piles of this wood should be driven in places notoriously infested by the worm, such as Lowestoft, Gravesend, Portsmouth, &c., and they should be carefully watched.

Mr. JOHN BETHELL said, that an experience of about fourteen years had shown him, that where timber had been properly impregnated with creosote, to the extent of 10 lbs. of oil per cubic foot, it perfectly withstood the action of the worm. But contracts were sometimes issued for the erection of piers, or other marine works, within a given time, and the builders were obliged to take timber which might have been floating for years in timber ponds, and had become soddened with water. Now it was impossible to force 10 lbs. of oil per cubic foot into wood, the pores of which were completely filled with water ; quantities of timber had, therefore, been sent out as creosoted, which had not been impregnated with a sufficient quantity of oil, and instances of failure had, consequently, occurred.

Having been engaged in diving operations, some time ago, he was able to state, that the action of the wave was usually confined to about 6 feet beneath the surface ; and below that depth there was little, or no oscillation. He thought, therefore, that if the open portion of the breakwater referred to, was below the lowest part of the level of the wave, it would not be propagated on the other side.

In answer to questions from Members, Mr. Bethell said, that his

diving operations had been carried on, at different places round the coast of England, and he had invariably observed, that there was very little oscillation just below the hollow of the wave. The operations were sometimes continued in rough weather, yet the waves had no effect upon the air pipes and signal lines below.

Mr. J. COCKBURN CURTIS considered, that the action of a wave upon the bottom, depended materially upon the age of the wave, and the other circumstances under which it arrived in the locality. Those who had been much employed in sounding, dredging, coast-line examinations, and the other details of marine surveying, saw in the different objects brought up by the dredge, the heavy bodies which were sometimes cast on shore; and the circumstance of submarine shoals and banks, at great depths below the surface, being occasionally perceptible by the increase of the wave action immediately above them, gave evidence of a disturbance to a much greater depth than that stated. He believed, that in the cases which had been cited, when the disturbance was confined to a small depth, the 'active wind-wave' was that which had existed at those times, and not the ground swell, or 'latent wind-wave.' So far as he had been able to observe, the action of a wave upon the bottom, did not depend so much upon the height of a wave, as upon its volume, and the expanse of surface over which it was diffused.

His previous remarks appeared to have been misunderstood. He did not absolutely condemn the principle of the structure proposed by the Author of the Paper, but he had endeavoured to show, that it was inapplicable to the neutralisation, or resistance of waves of such a character as those which entered Table Bay, during the north-westerly gales. The proposed structure might, no doubt, break smaller waves, if they were of great height in proportion to their volume.

Mr. BIDDER, —President,—remarked, that there were a variety of ways in which an efficient breakwater could be constructed, but the great desideratum was to find an efficient method of construction, more economical than the systems hitherto employed. The first datum required was the *bonâ fide* cost of existing breakwaters; he hoped, therefore, Mr. Cooper could give the Meeting some information as to the cost per yard, or per foot, of the Alderney breakwater, at the present depth of 20 fathoms of water. The works there, were carried on economically, so far as the carriage and placing of the material was concerned.

Mr. JAMES COOPER said, he was not prepared, at that moment, to give the cost of the Alderney breakwater; he could only state, that the works were now being carried on in 21 fathoms of water, and the cost was found to increase in proportion as the depth became greater.

Mr. JOHN MURRAY agreed with the President, that the ques-

tion was, above all, one of cost. There were, of course, various methods of constructing breakwaters, but the system to be adopted depended greatly upon the locality. It was requisite to take into consideration the action of the tide, the direction of the prevailing winds, and the force with which the waves would strike the breakwater. Now, there was a very powerful sea running round the Cape of Good Hope, and the force exerted by those tremendous rollers, in very deep water, was evinced, by their stirring up the sand bank off the Cape; no ordinary structure, therefore, would be of any use in that locality. But breakwaters were generally placed in a more sheltered position, either in bays, or in shallow depths. The water at Alderney was about the deepest in which a breakwater had been, hitherto, constructed. At the discussion upon the Author's first Paper, Mr. Murray had gone into the question of cost,¹ from which he had arrived at the conclusion, that the cheapest method of construction was to form a deposit of stone from the bottom, to a certain distance under low water, varying from 6 feet to 15 feet. He did not approve of the practice of using the stones taken from the quarry, indiscriminately, and putting into the heart of the breakwater the large stones mixed with the small ones, as had been done in several of the breakwaters already completed. He thought it would be preferable to make a nucleus of small stones at the bottom, where they could not be affected by the action of the waves, to cover them with large stones, gradually increasing in size, up to about a depth of 12 feet at low water, at a slope of 2, or $1\frac{1}{2}$ to 1, and in some cases of 1 to 1, and then to commence the superstructure. Now it was the latter that was the great source of expense, when carried out, as had been, hitherto, the practice in this country. A lesson in this respect might be learnt from the French. At the breakwater at Algiers, large blocks, many of them weighing from 10 tons to 15 tons and 25 tons, were used from the very bottom. They were manufactured on shore, where they were allowed to dry; they were then placed upon a railway and tipped over the end of the breakwater into the sea. At Marseilles, on the contrary, the plan he had just recommended had been adopted; gradually increasing the size of the blocks to a certain height, when blocks of *béton* were used as an outer covering. The breakwater at Marseilles was in a depth of water from 5 fathoms to 7 fathoms, and had cost about £215 per lineal yard; it was the cheapest structure, he believed, of the kind ever erected. At Algiers, where large blocks were used at the bottom, where labour was dearer, and the warmth of the climate impeded the progress of the work, the cost had been £360, or £370 per lineal yard. The depth of the works there

¹ *Vide Minutes of Proceedings Inst. C.E., vol. xviii., pp. 105 et seq.*

was 7 fathoms. Of late, the system had been altered at Algiers; large blocks were now no longer used, till the structure had been raised to within 20 feet of the surface, with small stones. There was considerable saving by this method of throwing in artificial blocks, in large masses; one-third of the cubical section of the breakwater might be deducted for the interstices between the blocks, and these interstices were also valuable in another way. They allowed of percolation taking place through the structure, and they were filled up by the sand, which cemented the whole into one solid mass. They were also valuable in the upper portion of the work, by allowing a passage for the waves, and thus stilling the water inside the breakwater. In constructing harbours of refuge, the main object was to break the great force of the waves, and to endeavour to secure smooth water within, so as to enable trade to be carried on in the interior harbour, alongside of quays. A large superstructure, therefore, was not required, and there were many advantages in keeping it low; if raised to the level of high water of equinoctial spring-tides, the waves would roll over, and create an agitation in the interior of the harbour, which would prevent its silting up. In a military point of view, such a low breakwater would act as a glacis to vessels of war inside, which could deliver their fire over it, and yet be protected by it, to a great extent, from an enemy outside the harbour.

With regard to the plan under consideration, in which the superstructure was to be made with a series of counterforts at a slope of 2 to 1: if the rise of tide was 20 feet, the length of each of the counterforts would be, necessarily, 40 feet. It was proposed to place them 20 feet apart, and if they were 10 feet in width, the distance would be 30 feet from centre to centre. Now the cost of erecting such counterforts would be enormous, because they must be built by manual labour, and be sufficiently strong to withstand the shock of the sea. The expense would be the same as that of a pier with two faces, at a distance from each other of 20 feet, and would be greater than building two longitudinal walls. It was then proposed to connect the counterforts by iron girders, with the view of producing still water within. But a heavy wave striking the gridiron surface, spanning the interval between the counterforts, would have a greater tendency to run upon the work in a body, than to pass through the intervals of the grating.

It had been stated, that there was no agitation from waves, below a depth of 6 feet from the surface. Now at Sunderland, he had considerable experience in diving-bell operations during twenty-five years. On going, one day, to the works at the north pier, where the diving bell was usually in operation at a depth of 20 feet, for cutting off the piles, he was astonished not to find it at work, as the day was calm, and there was but a slight roll on the surface of the

[1859-60. N.S.]

2 Y

water. Upon making inquiry, he was told, that the bell had been let down, but the water was found to be so much agitated, that the men could not work. To verify this he went down himself, and from the rolling of the bell he was soon convinced, that it was impossible to work without danger. The cause was soon discovered, for, two, or three tides afterwards, there was a tremendous rolling sea and a storm from the north-east. It appeared, that there had been a storm in the Atlantic, on the west of Scotland, which had agitated the water at the bottom, and had carried forward the agitation to the North Sea, between Scotland and Norway, and round the English coast, to Sunderland. This action of the sea at considerable depths, was still more prominently brought under his notice in another way. In the coal trade, the ballast of the vessels returning to the north, which sometimes consisted of chalk and flints, was usually discharged at a distance of from 7 miles to 10 miles from port, instead of 5 miles as formerly: yet after a violent storm, the whole coast was strewn with ballast which had been cast into water, 10 fathoms, at least, in depth, and the flints were easily recognised as coming from the Thames.

Mr. M. SCOTT said, it had never been his intention to propose the employment of timber at Table Bay, but exclusively stone and iron. With reference to the statements respecting the magnitude of the waves, and the great ease with which they would pass over this structure, he remarked, that as the model he had exhibited was very small, it had tended, perhaps, to give erroneous ideas of the actual dimensions of the work itself. It was 80 feet in height, and the superstructure was 80 feet in width; and he could not conceive, that a large wave could pass over a structure of that size, with such facility as had been suggested. But if 80 feet were not sufficient, the dimensions might be increased to 100 feet, or even 120 feet, and the cost of the structure would still be less than that of any other form of breakwater. He might be permitted to congratulate the Members, upon the progress this subject had made since he had presented his former Paper. There seemed now to be very few dissentients from the principle of the vertical wall, which, on that occasion, was very much disputed; but the main objection to it now, was the great cost it involved. He concurred in the remark of the President, that cost was the question which must, eventually, determine the form and method of construction of every breakwater; it was, therefore, important to show that, in this respect, the vertical wall had the advantage over every other form, hitherto applied. He believed, as he had endeavoured to show in his previous Paper, that it was possible to construct a breakwater with a vertical face, by employing, in the first instance, timber work, and subsequently, facing it with large blocks of stone, at less cost than any of the sloping forms.

The principle of the proposed structure had been tried on a small scale, and found to be successful. The undulation of the wave was destroyed, and the water passed through the breakwater, in something like a stream; there was a broken surface, but no undulation, and no wave. He had suggested this form precisely, because, in such a situation as the Cape, there were, evidently, great difficulties in applying any of the ordinary forms of breakwaters, and the waves were very heavy. The supposed difficulty with regard to the counterforts had arisen, he thought, in consequence of the details of that portion of the structure not having been given; but the way in which they were proposed to be constructed, would show, that the expense would not be great. They were supposed to be constructed of large and heavy blocks of rubble, cemented together, and so interlocked, that each counterfort should become one whole mass. He was quite satisfied, that the cost of the work would not exceed the sum he had stated. The forms adopted at Plymouth and Portland would not withstand the force of the waves at the Cape; and he thought the plan he had proposed, was the only one capable of dealing with such great masses of water.

Mr. Scott Russell appeared to think, that these great rollers could only be broken by opposing mass to mass. If that principle were recognised in the case under consideration, the mass must be very great to oppose such waves as these, and necessarily, be very expensive. He proposed to place before the breakwater a detached mass of rubble stone, at a considerable distance seaward, and parallel with the breakwater, in order to destroy the wave. This, no doubt, would be the effect, but, practically, it was tantamount to constructing two breakwaters. He also advocated the system of projecting a very long foreshore; the effect he anticipated from it would inevitably result, but it could only be obtained at excessive cost.

It had been objected, that the water passing between the girders, would strike the counterforts obliquely, and that they would receive the whole force of the blow. But that assumed, that the sea came nearly end-on to the breakwater, which could never be the case; and even if this were possible, counterforts of solid masonry, 12 feet in thickness, were not likely to be destroyed by the water that would pass through spaces, 20 feet in length, and 1 foot in breadth. The only surface exposed to the blow of the wave would be of iron, and the girders would be capable of supporting the pressure of 100 feet of water. It was also objected, that the water would press through the openings and produce a wave inside the breakwater. He dissented from that view, for the worst that could happen would be for the water to pass through a number of the spaces at the same moment, each

producing a wave ; but even in that case, the waves would be small, and by their succession, they would neutralise each other. The misconception arose from considering a sloping screen, with horizontal bars, to be the same as a vertical screen with vertical bars, and ignoring the fact, that whereas, in the latter, the water passed through at the same moment, leaving the continuity of the wave unbroken, in the former it passed through in detail, and at successive periods of time, whereby the continuity was destroyed. The principle of a screen with horizontal bars had been tried on a small scale, and had been successful ; the wave was entirely broken : if the bars had been placed vertically, he had no doubt it would not have been destroyed. The only forcible objection that had been taken to the new form was, that the wave might go over the top of the slope. Now it was evident, that the whole would not go over, under any circumstances ; the quantity would entirely depend upon the magnitude of the work. But if a slope 80 feet in width was not sufficient to prevent it, it could be extended to 100 feet, or to 120 feet ; and even then, it would only cost £126 per foot run, which was less than any other form equally effective. The breakwater could first be built 80 feet in width, and any addition could be made afterwards. Every addition to the efficiency of a vertical wall, by increasing the height, diminished its strength, whilst every increase to the efficiency of the proposed work, added to its strength, because every foot added to the height, involved an increase of 2 feet in the width.

With respect to the breakwater at Blyth, alluded to in the Paper, there had been some misconception regarding the principles upon which it had been constructed. The straight part of this portion of the work was so like, in external appearance, some other works, that it had been assumed by many to be similar, whereas in principle and action it was quite different. The fundamental idea had been, that the work should rest on, and not be attached to the ground ; but that its stability should be entirely dependent on the weight of the stone filling, and be independent of any ground fastening. The second object was so to combine the timber and stone, that they should form one whole. Both these objects had been effected, but it must not be forgotten, that an apparently slight change in either the form, or in the details of construction, would be attended with great danger to the structure. There were several other peculiarities in that work, one being the great facility afforded by the system of framing the timber on shore, and depositing it complete in its place. He thought it necessary to make these observations, because this system of construction was being adopted elsewhere, and if attention was not directed to these details, apparently unimportant, but in reality involving principles, success could not be expected.

He feared that his observations respecting some of the existing works, had been construed into reflections upon the Engineers who constructed them; this he exceedingly regretted. In introducing any new system, the only means of fairly bringing it under notice and criticism, was to compare it with examples, where other systems had been applied. This was all he had intended to do; and it could scarcely be regarded as reflecting upon the Engineers engaged, to point out, that works constructed many years ago, were not executed upon principles which had only recently been recognised.

Mr. BIDDER,—President,—said, that in this case, the Institution had departed from the usual rule of abstaining from the discussion of projects, and he thought some inconvenience had been occasioned thereby. The discussions were generally confined to works that had been already tested and tried; and the great difficulty in considering the subject of breakwaters was the absence of important facts concerning them, for the whole question, after all, mainly turned upon the cost, which must be dependent on the peculiarities of the various localities and the materials which they afforded. The breakwaters at Holyhead, Dover, and Alderney, could not be expected to be constructed at the same cost as that at Portland, where there was an inexhaustible supply of stone. He was, personally, inclined to doubt whether, granting all the positions assumed by the Author of the Paper, the wave screen would be an efficient breakwater in any situation, and whether any real economy would result from its use. It certainly could not at Alderney, or Portland, or Holyhead: it might, possibly, be applied at Dover, but even of that, he was very doubtful. But the advisability of introducing this breakwater must depend, in a great measure, like all other questions of engineering, upon the confidence which capitalists reposed in the Engineer. The question of breakwaters still remained in a very unsatisfactory position, and Engineers had been unreasonably blamed for it. At Alderney, within the last year, a first-class frigate had struck upon an apparently unknown rock in the centre of the harbour. This, certainly, was not the fault of the Engineer; but it did reflect, and not very creditably, upon some department, or another. With regard to Alderney, he thought the Author of the Paper had not mastered all the peculiar features of that locality. The sea rose there in a most peculiar manner. Mr. Rhodes, the late Resident Engineer of that work, had told him that, on one calm day, when he was at the end of the breakwater with Captain Washington, they saw a swell coming from the north-west. Mr. Rhodes advised Captain Washington to retire as quickly as he could, yet before he had left the pier, the sea was already making a breach over the end of it. In such a situation he thought, if there was to be a breakwater at all,—a question which could not be dis-

cussed at this Institution,—there was scarcely any choice, and that the mode now adopted was the only one likely to be effectual. He hoped Mr. Cooper, would, on some future occasion, be able to give the information for which he had asked,—the cost per yard of the walls in 20 fathoms of water, at Alderney, where the stone had to be quarried by the Contractor, as contrasted with the cost in 10 fathoms of water, at Portland, where the stone was quarried by convicts. He believed it would be found, that at Alderney, the quarrying cost less than at Portland. If such should prove to be the fact, it would serve still further to demonstrate the advantage of ascertaining the whole of the circumstances attaching to each case, even when the actual cost was known; for there was no apparent reason, why the works at Portland should not be constructed as cheaply as those at Alderney. The Institution had no data upon which to arrive at a satisfactory conclusion. With respect to breakwaters, he considered, therefore, that the profession was fairly entitled to ask, that those Engineers who were employed in constructing important national works, which were paid for out of the public exchequer, should be called upon to furnish detailed particulars, in reference to their cost, and the mode in which they were carried out.

MEASUREMENT OF GAS.

It was announced that a Paper had been received, which would, probably, be read, early in the ensuing Session, “On the Measurement of Gas, and the Classes of Gas Meters, in general use,” by WILLIAM CROSFLEY, Assoc. Inst. C.E.

This Paper stated, generally, the facts connected with the early introduction of gas lighting, and its progress, up to the present day, when the rental amounted to about five millions of pounds sterling per annum,—and the consumers, to several millions of persons, supplied by upwards of one thousand companies, corporations, or private proprietors of gas works. With a view of ascertaining the quantity consumed, otherwise than estimating by burner, the gas meter was introduced by Mr. Clegg, (M. Inst. C.E.,) and the late Mr. Samuel Crosley. After this instrument had been in use for some time, it was found that, although it had been considerably modified by other persons, it could not always be relied upon for correct registration. The general complaints from the consumers of gas led to the passing of an Act, in the last Session of Parliament, to insure a more constant inspection of the instruments for measuring gas. The details of a series of experiments on meters

of various designs and construction were given, showing how great an amount of error prevailed. At the same time, remedial measures were pointed out, and discussion was invited upon the best instruments for use, and the most effectual systems of supervision.

At the conclusion of the last Meeting, Mr. BIDDER,—President,—said, he would take that opportunity of congratulating the Members, upon the proceedings during the Session which had now, practically, terminated. The finances were in a flourishing condition, many new Members and Associates had been admitted, and the attendance at the Meetings had been, at least, equal to, if it did not actually exceed the usual average. The future prosperity of the Institution depended upon the Members and others, contributing Papers upon subjects, not only of interest to the profession, but to all who were engaged in scientific pursuits.

June 5, 1860.

The Session was concluded by a CONVERSAZIONE, at which the President received the Members of the Institution, and a numerous circle of distinguished visitors. The rooms were decorated with many choice works of art, and there was also exhibited a large and interesting collection of mechanical models.

I N D E X

TO THE

MINUTES OF PROCEEDINGS,

1859-60.

ABEL, F. A., remarks as to the comparative chemical qualities of the materials for cannon, 372.

Abernethy, J., remarks as to the form and construction of breakwaters, 663.

Accounts, auditors of, appointed, 131.—Abstract of receipts and expenditure from the 1st of December, 1858, to the 30th of November, 1859, 166.—Remarks as to ditto, in the Annual Report, 164.—Statement as to the invested property of the Institution, 490.

Adams, W. B., remarks as to the construction of artillery, particularly of rifled ordnance, and its assumed advantages, 428.—Ditto as to the respective merits of the Armstrong and the Whitworth guns and projectiles, 430.—Ditto as to breech-loading cannon for naval purposes, 432.

Address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 214.—Notice of the deaths of Mr. Brunel and of Mr. Robert Stephenson, and especially as to Mr. Brunel's exertions in accelerating the progress of ocean steam navigation, 214.—Progress of the profession of the Civil Engineer, its scope and object, 218.—Present state of hydraulic engineering, 219.—Relative effects of tidal scour and of upland waters upon rivers and harbours, 219.—The effects of the under-draining of land, 220.—As to the prospect of the sewage of towns being employed for the fertilisation of land, 220.—Offer of a premium for Paper on 'the régime of rivers,' 221.—Extension of submarine telegraphs, 221.—The national defences, especially in connection with the design and construction of steam ships, of rams, or vessels for running down, of harbours of refuge, of artillery, and of railway access to the coast, 222.—The points requiring investigation as regards rams, 223.—Harbours of refuge as bases of operations for the navy, and especially as to the principle of construction employed at Dover, Alderney, Jersey, Holyhead, and Portland, 225.—The progress of government works and those undertaken by private enterprise, contrasted, illustrated by reference to the New Westminster bridge, and the bridge over the river St. Lawrence, at Montreal, 227.

Aird, J., jun., elected associate, 130.

Amos, C. E., on the Government Waterworks in Trafalgar Square, 21.—Remarks as to the quantity and level of the water in the wells at ditto, 30, 51.

Anderson, J., remarks as to the best principle of constructing a gun, so as to afford the artillerist the full development of the gunpowder used to throw the
[1859-60. N.S.] 2 z

- projectile, 357.—Ditto as to the cost of the Armstrong 12-pounder gun, 358.—Ditto that the gun of the future must be a built gun, 358.—Ditto as to the Ordnance Select Committee, 359.
- Annual General Meeting, 131.
- Report, 133.—Ditto read and ordered to be printed, 131.—*Vide* Report.
- Anstruther, Major General, remarks as to the want of uniformity in the strength of cast iron, and in the force of gunpowder, 394.
- Appold, J. G., remarks as to the experiment of drilling a cracked bell, 19.
- Aqueduct for the Wolverhampton Level canal, at Tividale, and another over an occupation road, near Dudley Port, on the line of the Netherton tunnel branch of the Birmingham canal.—*Vide* Canal.
- Armstrong, Sir W. G., C.B., remarks as to his system of rifled ordnance and projectiles, 407.—Ditto as to the three kinds of projectiles hitherto used in the field, 407.—Ditto as to the segment shell, 407.—Ditto as to the time fuze, 409.—Ditto as to the concussion fuze, 411.—Ditto as to the question of long ranges, 412.—Ditto as to the construction of his gun, the description of the sights, and the process of loading, 413.—Ditto as to the form of carriage originally used, 414.—Ditto as to what effects were required to be produced by heavy artillery, and what species of projectile would best answer the purpose, 414.—Ditto as to the conditions upon which the attainment of long range chiefly depended, 415.—Ditto as to the proposed 100-pounder gun, and the cast-iron shell designed for it, 417.—Ditto as to the difference between Mr. Whitworth's and Sir W. Armstrong's guns, 418.—Ditto as to the pressure required to force the shot through the contracted part of the bore of his gun, 420.—Ditto as to experiments against wrought-iron plates, 420.—Ditto as to the Nyctoscope, an instrument for enabling gunners to maintain a fire upon any given object after nightfall, 421.—Ditto as to the cost and durability of his 12-pounder gun, 423.—Ditto as to the comparison of ranges of Mr. Whitworth's and Sir W. Armstrong's rifled ordnance, 427.
- Arterial drainage and outfalls, on, 53.—*Vide* also Drainage.
- Artillery, on the construction of, and other vessels, to resist great internal pressure, 283.—Investigation of the method of making a gun, which gunpowder, or any other explosive compound, cannot burst, 284.—Difficulty experienced in the manufacture of cylinders of hydraulic presses, when of large size, to resist a comparatively moderate pressure, 284.—Proposal to construct a vessel, by wrapping successive layers of wire round an internal core, and coiling them on with the proper initial tension, 284.—Similar principle, though different in detail, adopted by Capt. Blakely, Mr. Mallet, and Sir W. Armstrong, 285.—Experiments as to the ultimate force of gunpowder, 286.—Cast iron alone does not possess the requisite tensile strength, permanently to resist the internal strain caused by ditto, 288.—Causes of the failure of cast-iron guns, 289.—One source of weakness arises from the variation in the thickness, causing a difference in the rate of cooling, 289.—Amount of strain to which guns are now subjected, 291.—Guns of wrought iron and of steel, 292.—Experiments with Herr Krüpp's forged steel gun, 292.—Ditto with a wrought-iron gun made at Gospel Oak, 293.—Ditto to determine the tensile strength of the material of the gun which burst on board the U.S. frigate 'Princeton,' 293.—Ditto, ditto, the iron from which the Mersey Company's monster gun was made, 295.—Construction of a gun in successive layers, with an increasing and definite initial strain from the centre to the circumference, impossible by means of hoops, 296.—Successive states of stress with two, three, and four rings, 299.—Result of a very trifling error in the workmanship, 300.—Method adopted to coil on a

quantity of wire with the exact strain indicated by theory, 301.—The same accuracy impracticable with hoops, which also have the defect of want of continuity of substance, 301.—Extract from Capt. Blakely's specification for a method of making guns, by the application of rings shrunk on an inner tube, 303.—Capt. Blakely's experiments with an 18-pounder, constructed on this plan, and with a 9-pounder turned down and strengthened from the trunnions to the breech on the same system, 304.—Amount of initial strain to be put on each coil of wire, 306.—Result of experiments to ascertain the effect of powder on brass cylinders without any wire, and with different thicknesses of iron wire, 306.—Ditto with a brass cylinder, of nearly the same internal dimensions as a 3-pounder mountain gun, 308.—Report from the Ordnance Select Committee as to the failure of ditto on second trial, 309.—The failure attributed to the method of mounting, by which the cylinder had been torn asunder by the recoil, but was not burst, 310.—Experiments to determine the ultimate resisting strength of the fragments of the brass cylinder, 312.—Second series of experiments to try the effect of wire, in enabling hard cast iron to resist a bursting strain, and to ascertain whether the force of gunpowder could be transmitted through a thin breech of cast iron, to a yielding substance placed between that breech and the carriage of the gun, 313.—Further trials as to the strength of cylinders without wire, and with different thicknesses of wire, 316.—Experiments with a small gun constructed on the principle recommended, prematurely stopped by the shot getting wedged in the chase, 317.—Application of the same principle to the construction of the cylinders of hydraulic presses, 319.—APPENDIX :—1°, Force of gunpowder, 322.—Ditto 2°, Effect of twist, 325.—Ditto, amount of lateral deviation due to the axis of the trunnions being 1° out of the horizontal line, 329.—Ditto, Investigations by C. H. Brooks, of the conditions of stress of a cylinder built up of concentric rings, 329.—Ditto, ditto, of the effect of rotation in correcting the deviation due to want of symmetry of the projectile, 335.—Ditto, ditto, of the amount of error produced by the axis of the trunnions not being horizontal, 336.

Axles, railway, remarks as to the fracture of, 16.

Ayris, J., elected member, 489.

Bamborough, —, remarks as to Mr. Newall's and Mr. Fay's continuous breaks, 518.

Barlow, W. H., remarks as to a contrivance in operation on the Midland railway, for coal-burning in locomotive engines, 567.

Barrett, J., memoir of, 185.

Barry, E. M., elected associate, 263.

Barry, F., remarks as to the arterial drainage works in Ireland, 104.

Barton, E., elected associate, 263.

Bassett, A., elected member, 461.

Bateman, J. F., remarks as to the construction of thirteen miles of tunnels for the Glasgow waterworks, 279.—Ditto of a tunnel for the Manchester waterworks, 279.—Ditto as to a comparison between Mr. Newall's and Mr. Fay's breaks, 524.

Bazalgette, J. W., remarks as to the supply of water for the metropolis, 42.

Beams, Continuous, on a method of computing the strains and deflections of, under various conditions of load, 625.—Differs from others in deriving more aid from the theory of couples, and in keeping separate and distinct the two parts of the pressures on the bearing points respectively, from the portions of the beam on either side, 625.—Equations for determining the strains over the

- bearings and the pressures on the supports, in every case of a continuous beam of uniform section, whose normal condition is straight, 629.—Modification where the spans are of different sections, 629.—Enumeration of the expressions to be employed for the purpose of computation, 630.—Modification of the general expression, for the case where, in addition to the distributed loads, there are concentrated loads at the centres of the spans, 631.—Application to the purposes of computation in the case of the Britannia tube, 632.—Example, wrought iron, boiler-plate girder bridge over the river Cauvery, on the Madras railway, 633.—Strains and pressures at the supports of a continuous rail of infinite length, supported at equal intervals, and loaded with a given weight at the centre of one of the spans, 636.—Appendix, derivation of the equations, 639.
- Beardmore, N., remarks as to government supervision of drainage operations, 114.—Ditto as to the connection between tidal outfalls and land drainage, and the effect of the improvements of the rivers Nene and Thames, 114.—Ditto as to the section of the strata passed through, in the construction of the Nether-ton tunnel, Birmingham canal, 278.—Ditto as to works recently constructed upon the river Severn, at the Upper Lode, near Tewkesbury, 534.—Ditto as to the method which should be pursued in canalising a river, 535.
- , W., elected associate, 461.—Ditto as to the saving in fuel by the use of superheating apparatus, 474.
- Beattie's, (J. H.,) system of coal-burning and feedwater heating in locomotive engines, 550.—*Vide* also Locomotive engines.
- Bedford level, the, its extent, and the characteristics of the rivers draining the district, with an account of the means adopted to reclaim the South, the North, and the Middle levels, 65.—*Vide* also Drainage.
- Bells, in the Clock Tower, at the New Palace, Westminster, on the process of raising and hanging the, 3.—Particulars relating to the clock and bells, from the papers and correspondence ordered by the House of Commons, 4.—Extract from the specification for supplying the five bells, prepared by Mr. E. B. Denison, Q.C., 5.—The first large bell, cast by Messrs. Warner and Sons, 6.—The second large bell, cast by Messrs. Mears, 6.—The four quarter-bells, cast by Messrs. Warner, 6.—The hoisting of the four quarter-bells to the bell-chamber, 6.—Arrangements for raising the large bell, by means of a treble-purchase, with large blocks, and a new chain-fall, 6.—The cradle on which the bell rested, 7.—Iron chain-stopper for holding the chain, during the fleeting, on the drum of the crab, 8.—The process of hoisting, and the time occupied, 8.—The carriage from which the bells are suspended, 9.—Experiments upon the bells, to ascertain the right weight and proper fall of the hammer, 9.—Discovery of two fractures, 10.—The Rev. W. Taylor, F.R.S., on the other large bells of Europe, 10.—At Lucerne, 10.—At Cologne, 10.—In the church of St. Gudule, Brussels, 11.—At Moscow, 11.—The Isar Kolokot, 11.—At Erfurt, 11.—At St. Peter's, York, 11.—Appendix, containing details of the time occupied in raising the Great Bell, and the heights lifted, 12.
- , restoration of cracked, by tin soldering, 229.
- Bennett, S., memoir of, 186.
- Bequest of Robert Stephenson, noticed in Annual Report, 164.
- Berkley, G., remarks as to coal-burning in locomotive engines, 566.—Ditto as to Indian railways, 611.—Ditto as to the extent, character of the works upon, and cost of the East Indian railway, 611.—Ditto as to the rate of wages in Bengal, before and after the mutiny, 612.—Ditto as to the cost of the Great Indian Peninsula railway, the class of locomotive engines employed upon it, and the increase

- in the rate of wages in the Bombay Presidency from 1853 to 1859, 612.—Ditto as to the salaries and wages of persons sent from this country, 613.—Ditto as to the Madras railway, and to the fact of the contract system not having been adopted in its construction, 614.—Ditto as to the average cost of railways in England, Scotland, and Ireland, compared with those constructed in India, 614.—Ditto as to the number of persons temporarily employed in the construction of Indian railways, and of those who would probably be permanently engaged, 615.—Ditto as to the beneficial effects of the introduction of railways into India, 615.—Ditto as to the passenger fares, and the receipts, both for passengers and goods, compared with the same in Great Britain, 616.—Ditto as to the future prospects of these enterprises, 617.
- Berkeley, J. I., on Indian railways, with a description of the Great Indian Peninsula railway, 586.
- Bessemer, H., Telford Medal awarded to, 155, 193.
- Bethell, J., remarks as to creosoting timber, 666.—Ditto as to the depth to which the action of waves extends, 666.
- Bidder, G. P., President, remarks as to the arterial drainage works in Ireland, 105.—Ditto as to the necessity for a complete record of the phenomena attending works for the improvement of rivers, and as to the effect of the Eau Brink works, 105.—Ditto as to the effect of under-drainage on the supply of water to rivers and streams, 105.—Ditto as to the depth to which under-drainage should be carried, 125.—Ditto as to the effect of upland and tidal water on the scour of rivers, especially as to the case of the river Thames, 127.—Ditto as to government interference with the drainage of land, 128.—Ditto in reading an extract from a pamphlet by Edmund Burke, as to the duties of the State, 129.—Address on taking the chair, for the first time, after his election, 214.—Remarks as to the construction and enlargement of the Lindal tunnel, on the Furness railway, 239.—Ditto as to the cost of a bridge on the Mangaratiba Serra road, Brazil, 255, 261.—Ditto as to the relative value of free and slave labour, as to the materials to be employed in tropical countries, and as to the prosecution of railways under guarantee, 262.—Ditto as to the construction of tunnels, for railways and canals, and as to the Netherton tunnel on the Birmingham canal, 281.—Ditto as to the construction of artillery, 338.—Ditto as to the tensile pressure evolved by the explosion of gunpowder, 338.—Ditto as to the Whitworth and the Enfield rifles, 400, 457.—Ditto as to the capability of vessels to resist great internal pressure, especially as to the construction of guns, 448.—Ditto as to the tensile force of metal, 448.—Ditto as to the Armstrong and the Whitworth breech-loading rifled ordnance, 448.—Ditto as to cast-iron guns, 449.—Ditto as to the present practice of gunnery, 449.—Ditto as to the initial pressure of gunpowder, and its *modus operandi* upon the shot, 450.—Ditto as to the element of atmospheric resistance, 452.—Ditto as to the Armstrong and the Whitworth systems of rifling, 453.—Ditto as to the ratio of the velocity of the retardation of the shot per element of distance, 454.—Ditto as to the results of experiments, with spherical shot, with the Whitworth rifled cannon, and with the Armstrong gun, showing what has been accomplished by rifling, 454.—Ditto as to a comparison of the present results of the Whitworth and the Armstrong guns, 456.—Ditto as to the mode of breech-loading in the two guns, 456.—Ditto as to experiments with Mr. Whitworth's rifled musket, 457.—Ditto as to the Emperor Napoleon III. on the 'Past and Future of Artillery,' 459.—Ditto as to the state of ship-building in the public establishments, 460.—Ditto as to the rationale of the advantages from superheating steam, and as to the use of combined steam, 487.—Ditto as to railway breaks,

- 525.—Ditto as to works on the river Severn, at the Upper Lode, near Tewkesbury, 544.—Ditto as to Indian railways, 623.—Ditto as to the form and construction of breakwaters, 667.—Ditto as to the breakwater at Alderney, 673.—Ditto on the close of the Session, 675.—Notice of the President's conversazione, 675.
- Blakely, Captain, R.A., extract from specification of, for a method of making guns, by the application of rings shrunk on an inner tube, 303.—His experiments with an 18-pounder constructed on this plan, and with a 9-pounder turned down and straightened from the trunnions to the breech on the same system, 304.—*Vide also* Artillery.
-
- , remarks as to the construction of artillery, particularly as to building up a gun of concentric cylinders, 348.—Ditto as to experiments with guns made on his system, 348.—Ditto as to a gun cast on a hollow core by Lieutenant Rodman, 371.
- Boiler explosions, remarks as to the causes of steam, 16.
- Bombay, population of, preponderance of trade to, and exports and imports, 590.—*Vide also* Railway, Great Indian Peninsula.
- Boxer, Captain, R.A., remarks as to the construction of artillery, 368.—Ditto as to the Ordnance Select Committee, 369.—Ditto as to the strength of gunpowder, 369.—Ditto as to cast iron as a material for guns, 370.
- Braithwaite, F., remarks as to the deep wells of the metropolis, the gradual depression of the level of the water in them, and the rainfall and evaporation in the London basin, 33.—Ditto as to the analyses of water from wells on the north side of London, and from that at Plumstead, 37.—Ditto as to the quantity of water that could be abstracted from the deep wells under London, more particularly on the north side, 43.—Ditto as to the wells at Trafalgar Square, and the analyses of the water from, as well as from the chalk at Tring and Watford, 43.—Ditto as to the levels of the water in Combe and Co.'s well, and in that in Orange Street, 44.
- Bramwell, F. J., remarks as to the composition of the metal of the bells at the New Palace, Westminster, 15.—Ditto as to the objections to casting guns solid, as to the principle, introduced into the American service by Lieutenant Rodman, of casting guns hollow, and as to the result of comparative experiments with a solid and with a hollow gun, 363.—Ditto as to the area of the section of the gun which bears the strain, 365.—Ditto as to railway breaks, 519.
- Bray, W. B., appointed one of the scrutineers of the ballot for Council, 131.—Vote of thanks to, 132.
- Brazil, on the means of communication in the empire of; chiefly in reference to the works of the Mangaratiba Serra road, and to those of the Mauá, the first Brazilian railway, 240.—Civilisation of the country effected by Jesuit missionaries, 240.—Extent of coast, 240.—Internal lines of communication by bridle paths, 241.—Character of the Province of Rio de Janeiro, 241.—Paved descents of the 'Serra do Mar,' 241.—Carriage road up the face of the Serra to Petropolis, 242.—The Mauá railway, 242.—Absence of any correct map, labour of making the survey, and of taking the levels for the railway, 242.—Mode of prosecution of the works, particularly of the excavations, 243.—Slave labour and its cost, 244.—Great inequality in the character of the timber, and its liability to decay, as instanced in the bridges on the line, and in the pier forming the Southern terminus in the Bay of Rio, 244.—Advantages of the double-headed rail, 245.—Unhealthiness of the district through which the line was carried, and cost of the works, 246.—The Mangaratiba Serra road, 246.—Land traffic to and from the port of Mangaratiba, 246.—Contract with the Government for

- the construction of a carriage road up the Serra, 247.—Chinese labourers proved to be useless, 247.—Employment of Portuguese labourers, 247.—Character of the strata met with in cutting the road, 248.—Works executed in a number of small contracts, 249.—Supply of the necessities of life to the men a serious consideration, 249.—Quality of the works, particularly of the masonry, and cost of a cut stone bridge, 249.—Result of employing English workmen in a country like Brazil, 250.—Comparative scarcity of slaves, 250.—Dimensions and cost of the Mangaratiba road, 251.—Brazil possesses little internal water communication, 251.—The railways now in progress, 252.—The supply of labour for public works and facilities for colonisation should be considered together, 253.—Two methods of guarantee in operation for the railways, 253.—Brazil little known in England, 254.
- Brazil, railway system in, noticed in Annual Report, 152.
- Breaks, railway, on the efficiency of various kinds of, with experimental researches on their retarding powers, 490.—Colonel Yolland's report to the Board of Trade relative to the steam break of Mr. McConnell, the continuous break of Mr. Fay, the continuous and self-acting break of Mr. Newall, and the self-acting break of M. Guérin, 491.—Further investigation, at the instance of the Rolling Stock Committee of the Directors of the Lancashire and Yorkshire railway, to ascertain the retarding power of different breaks, and to obtain data relative to the rapidity with which a train could be brought to a stand, when travelling at a high rate of speed, so as to determine the value of the continuous self-acting breaks, as compared with others, 492.—Dynamical laws affecting the case of trains on a level, and when the breaks are applied on an incline, 492.—Description of Mr. McConnell's break, 494.—Ditto Mr. Newall's and Mr. Fay's breaks, 494.—Ditto M. Guérin's break, 496.—Experiments with Mr. Newall's and Mr. Fay's continuous breaks, 497.—Ditto on the friction of the carriages, 501.—Ditto with slide breaks, with the engine detached from the train, 502.—Ditto with flap breaks, with ditto, 504.—Ditto with the engine attached to the train, 504.—General summary of results of experiments with Mr. Newall's and Mr. Fay's breaks, and conclusions deduced, 505.—Colonel Yolland's experiments compared with these results, 506.—Ditto on M. Guérin's self-acting break, 511.—Experiments on Mr. Ingram's auxiliary break, by the Manchester, Sheffield, and Lincolnshire railway, 512.—Final reduction of retarding force to units of weight of the different break carriages, 513.—General summary, 515.—Practical formulæ, 515.
- Breakwaters, on, Part II., 644.—Completion of the Blyth work, 644.—As to the assumed difficulty of erecting large frames in deep water, 644.—Method by which the panels of planking were fixed and hauled down under water, 645.—As to the alleged deficiency in strength, 645.—Proposed form of breakwater, 646.—Points to be kept in view in the formation of harbours, and in the construction of breakwaters, 647.—Notice of the leading peculiarities of the ordinary forms of breakwater at Plymouth, Alderney, Holyhead, Portland, and Dover, 647.—The proposed breakwater, a modification of the wave-screen, 649.
- Breech-loading rifles, McKenzie and Wentworth's, notice of, 461.
- Brereton, R. P., elected member, 214.
- Bricks, experiments as to the power of, to resist a crushing force, applied by a hydraulic press, 276.
- Bridge, Londonderry, noticed in Annual Report, 141.
- , Victoria tubular, at Montreal, noticed in Annual Report, 154.
- , wrought-iron boiler-plate girder, over the river Cauvery, on the Madras railway, calculations relative to a, 633.—*Vide also* Beams.

Bridges, towing path and road, over the Netherton tunnel branch of the Birmingham canal.—*Vide* Canal.

Britten, Bashley, remarks as to the construction of artillery, and particularly as to endeavours to improve the efficiency of the existing stock of cast-iron guns, by converting them into rifled cannon, 349.—Ditto as to the results of experiments with 32-pounder guns, as smooth bores, and when rifled, 350.—Ditto as to the character of the projectile to be used with rifled guns, 352.—Ditto as to the comparative cost of cast and wrought-iron guns, 352, 355.—Ditto as to the opinions of practical artillerists on the subject of range, 352.—Ditto that the performance of different guns should not be compared for range alone, 353.—Ditto as to the presumed advantages of small-bore guns and projectiles of great length, 353.—Ditto as to a Table giving the results of experiments as to the range, at different elevations, of rifled cast-iron guns and of wrought-iron breech-loaders, 354.

Brooks, C. H., investigations by, of the conditions of stress of a cylinder built up of concentric rings, 329.—Of the effect of rotation in correcting the deviation due to want of symmetry of the projectile, 335.—Of the amount of error produced by the axis of the trunnions not being horizontal, 336.

——, W. A., remarks as to the levels taken for the drainage of the eastern fen districts, 106.—Ditto as to placing weirs across navigable rivers, and as to works on the river Severn, 534.—Ditto as to the breakwaters at Kingstown, Portland, and Blyth, and as to the situation in which the latter had been constructed, 656.

Broughton, F., remarks as to the application of Mr. Newall's break on the Ulster railway, 524.

Brown, T., R.N., elected associate, 461.

Browning, F. R., elected associate, 263.

Bruce, G. B., remarks as to the inexpediency of interfering with the ordinary native methods of doing work in foreign countries, 258.—Ditto as to the difference in the nature of the guarantees, causing the guaranteed shares of some Brazilian railways to be at a discount, 268.—Ditto as to the reasons which induced him to depart from the usual contract system in the construction of the Madras railway, and to the manner in which the works were executed, 617.—Ditto as to the adoption, on Indian railways, of the substantial system of construction, in contradistinction to what was called the American system, 618.—Ditto as to the prices of labour in the Madras Presidency, and on the Great Southern of India railway, 619.—Ditto as to the improvement of the people due to the railway works, 619.—Ditto as to the guarantee system, 619.

Bruff, P., remarks as to the supply of water to the town of Colchester, from a well sunk into the chalk, with a list of the strata passed through, 38.—Ditto as to the artesian boring at Harwich, and the strata passed through, 39.

Brunel, I. K., V.P., notice of the death of, by Mr. J. Locke, M.P., President, at the opening of the Session, 1.—Ditto, in Annual Report, 164.—Ditto, in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after election, especially as to Mr. Brunel's exertions in accelerating the progress of ocean steam navigation, 214.—Mémorial, 169.

Brunlees, J., remarks as to the cost of a cut stone bridge on the Mangaratiba-Serra road, Brazil, 256.—Ditto as to the deterioration of timber in sea works in warm climates, 664.—Ditto as to a proposed cast and wrought-iron breakwater and pier, 664.

Burgoyne, General Sir J. F., remarks as to the construction of artillery, and as

to the effect which long range rifled ordnance would have upon fortifications, 356.

Burnell, G., remarks as to creosoted timber, and as to the capability of green-heart timber to resist the attacks of the teredo, 665.

Canal, Birmingham, description of the works on the Netherton tunnel branch of the, 263.—Brief account of the navigation belonging to the Birmingham Canal Company, 263.—Range of hills through Rowley Regis, Dudley, and Sedgley, pierced by the Old Dudley tunnel, 264.—The original canal from Birmingham to Autherley, 264.—The Tame Valley canal, 264.—The Dudley tunnel afforded a means of communication between the Birmingham and the Dudley canals, and process of propelling boats through the tunnel by 'legging,' 265.—Act obtained for the construction of a new canal and tunnel, on the Birmingham Level, from Netherton to Dudley Port, 265.—General description, extent, and time occupied in the construction of the works, 266.—The towing-path walls, 267.—The basins, or docks, 268.—Towing-path bridges over the entrances of three of these basins, 268.—Road bridges over the canal, and details of the Birmingham and Sedgley turnpike road bridge, 268.—Towing-path bridges across the canal, 269.—Brick aqueduct for the Wolverhampton Level canal, at Tivdale, 269.—Brick aqueduct over an occupation road near Dudley Port, 269.—The stop locks, 269.—Sinking of the shafts for the construction of the tunnel, their total depth, and the rate of progress, 270.—Dimensions of the tunnel, and thickness of the brickwork, 271.—Forcing up of the invert, 272.—The trough of the canal in the tunnel, 272.—Process of excavation and construction of the tunnel, 272.—Nature of the strata passed through, 273.—Casualties during the execution of the works, 274.—Character of the mortar and brickwork, and strength of the bricks employed, 274.—APPENDIX :—Experiments as to the power of bricks to resist a crushing force, applied by a hydraulic press, 276.

Cannon.—*Vide* Artillery.

Cantwell, R., memoir of, 186.

Chambers, A., remarks as to the results of the application of a continuous break on the North London railway, 524.

Civil Engineer, progress of the profession of the, its scope and object, noticed in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 218.

Clark, D. K., on coal-burning and feed-water heating in locomotive engines, 546.—Remarks as to ditto, 564, 566.—Ditto ditto, and as to the comparative cost of coke and coal, 583.—Ditto as to Mr. Jenkins' apparatus for burning coal in locomotives, as employed on the Lancashire and Yorkshire, and as to the performances of the system on the Brighton railway, 584.

Clarke, J. W., elected associate, 461.

Clay, W., remarks as to the construction of artillery, and as to the Mersey Company's large forged gun, 359.

Clutterbuck, Rev. J. C., remarks as to the supply of water to be obtained from wells sunk in the chalk and sand formations, and as to the general depression of the water level under London, 30, 37, 45, 49.—Ditto as to tidal water finding its way, by filtration, into wells at London, Ramsgate, Portsmouth, and Liverpool, 32.—Ditto as to water from the chalk at Brighton, 47.

Clutton, John, remarks as to the effects of an obstruction to the drainage of a district near Maldon, 88.—Ditto as to the arterial drainage works in Ireland, 89.

———, W. J., remarks as to the difficulties in draining large areas of land for

- farming purposes, owing to the want of compulsory powers for dealing with the outfalls, 113.
- Coal and coke, remarks as to the comparative heating powers of, 568, *et seq.*—*Vide* also Locomotive engines.
- Coal-burning and feed-water heating in locomotive engines, on, 546.—*Vide* also Locomotive engines.
- Colburn, Z., remarks as to coal-burning locomotives in the United States, with descriptions of Phleger's, Dimpfel's, and Boardman's boilers, and of the plans used on the Boston and Providence, and Iowa Central railroads, 572.
- Communication in the empire of Brazil, on the means of; chiefly in reference to the works of the Mangaratiba Serra road, and to those of the Mauá, the first Brazilian railway, 240.—*Vide* also Brazil.
- Conversazione, President's, notice of the, 675.
- Conybeare, H., remarks as to the construction of artillery, 380.—Ditto as to the disruptive force of the powder, and the inertia and friction of the projectile, 381.—Ditto as to the use of cast iron for the construction of ordnance, especially as to experiments made by the officers of the U.S. Ordnance Department, with iron guns cast hollow on Lieutenant Rodman's plan, and with Captain Dahlgren's gun, 382.—Ditto as to the experiments recorded in the United States reports, on the extreme proof of cast-iron ordnance under hydrostatic pressure, and of casting guns hollow on chilled cores, 384.—Ditto as to the cast-iron gun of the future, and the form of rifling it, 388.—Ditto as to the present tendency of naval armament, and as to coast defences, 389.—Ditto as to a common standard of ratios being adopted in recording experiments on various descriptions of rifled arms, and in registering the elements of each, 389.—Ditto as to a Table giving the elements of twelve of the most approved rifled arms, 391.—Ditto as to the principles of effect in rifled projectiles, and the classes of projectiles in use, 393.—Ditto as to the effect of twist in rifles, 400.
- Cooper, J., remarks as to works for the improvement of that part of the Bedford level called the Middle level, 101.—Ditto as to a general legislative enactment on drainage, 102.—Ditto as to the works on the Netherton tunnel branch of the Birmingham canal, 278.—Ditto as to the breakwater at Alderney, 668.
- Corlett, H. L., elected associate, 461.
- Cottam, E., elected associate, 130.
- Council, list of the attendances of the members of, read, 131.—Ballot for, 131.—Annual Report of, read, 131.—Annual Report, 133.—Council and Manby Premiums, and Telford and Watt Medals awarded, Session 1858-59, 131, 155, 193.—List of subjects for premiums, Session 1859-60, 194.—Vote of thanks to, 131.—List of Council and officers for Session 1859-60, 132, 213.—Remarks in Annual Report as to balloting-list for members of Council, 165.
- Cowper, E. A., remarks as to the Swedish percussion shell, 373.—Ditto as to some of the principal distinctions between different guns, 374.—Ditto as to the wire-bound gun, 375.—Ditto as to the attainment of long ranges, 376.—Ditto as to the extent of the loss which occurred with different amounts of expansion, when no steam jacket was used, and as to the advantage of superheating steam, 483.—Ditto as to the combustion of fuel, with regard to the prevention of smoke, and as to the relative values of coal and coke, 580.—Ditto as to feed-water heaters, 580.—Ditto as to the admission of air, through hollow stays, into the fire boxes of locomotives, 581.
- Crosley, W., on the measurement of gas, and the classes of gas meters, in general use, 674.
- Crosse, A. C., elected member, 130.

Cudworth, W., elected member, 546.

Cudworth's system of coal-burning in locomotive engines, 550. *Vide also* Locomotive engines.

Curl, W. H. R., elected associate, 130.

Curtis, J. G. Cockburn, remarks as to the improvement of tidal drainages, 107.—Ditto as to the Norfolk Estuary works, 107.—Ditto as to the treatment of rivers, 535.—Ditto as to the use of weirs in the improvement of rivers, 536.—Ditto as to a series of observations on the river Severn, in 1848, 536.—Ditto as to oblique weirs, 540.—Ditto as to the physical features of Table Bay, Cape of Good Hope, 657.—Ditto as to a proposed wave screen breakwater for ditto, 658, 667.—Ditto as to the impossibility of generalising, as to the best form and material for maritime works, 659.—Ditto as to the present theoretical division of waves into waves of translation and of oscillation, 659, 667.—Ditto as to a suggested classification of waves, 660, 667.—Ditto as to the expedients adopted by the Mount's Bay boatmen, and by the Esquimaux, to break the crest of the surface, or ocean wind waves, 661.

Cuthell, A., elected associate, 263.

Daft, T. B., elected associate, 263.

Dahlgren's, (Capt.), gun, remarks as to, and as to the mode by which the forces tending to disruption were measured, 383.

Davenport, I. G., elected associate, 625.

De Grey, Earl, memoir of, 168.

Defences, the National, especially in connection with the design and construction of steam ships, of rams, or vessels for running down, of harbours of refuge, of artillery, and of railway access to the coast, noticed in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 222.

Denison, E. B., Q.C.—*Vide* Bella, in the Clock Tower, at the New Palace, Westminster.

Denton, J. Bailey, remarks as to the want of some comprehensive measure for the control of arterial drains and outfalls, 86.—Ditto as to the necessity of regulating the discharge of the refuse of towns into the arterial drains, 88.—Ditto as to the appointment of a central commission to administer a general Act for the improvement of outfalls, 120.—Ditto as to the cost of obtaining special Acts of Parliament for different drainages, 121.—Ditto as to the effect of under-draining in increasing floods, and in diminishing evaporation, 122.

Dickinson, J. A., elected member, 489.

Dinnen, J., remarks as to the condition of common steam before superheating, and as to the results of trials in H.M.S. 'Dee,' with common steam, with steam simply dried or superheated, and with mixed or combined steam, 476.—Elected member, 489.

Dock and harbour works in progress, noticed in Annual Report, Grand Surrey, 137.—Swansea, 138.—Newport, 138.—Silloth, 138.—Penarth, 139.—Southampton, 139.—Copenhagen, 151.

Docks, or basins, details of the, constructed on the Netherton tunnel branch of the Birmingham canal, 268.—*Vide also* Canal.

Donaldson, G. G., memoir of, 186.

Dorman, W. H., remarks as to superheated steam, 484.

Douglas', (G.), system of coal-burning in locomotive engines, Birkenhead railway, 551, *et seq.*—*Vide also* Locomotive engines.

Drainage, arterial, on, and outfalls, 53.—Extract from Mr. Simpson's address,

- as to land drainage, 53.—Want of a combined system of operation prevents the adoption of arterial drainage, 54.—Pipe or underground drainage, the advantage and extent of, 54.—Obstructions caused by mills, weirs, &c., 56.—Proposal for draining marsh land at Wareham, and the estimated cost of the several projects, 56.—Results of the drainage works in Ireland, with detailed particulars of five examples, 62.—The Bedford level, its extent, and the characteristics of the rivers draining the district, 65.—Means adopted to reclaim the South level, 66.—Ditto the North level, 67.—Ditto the Middle level, 70.—Drainage of the Ancholme level, 75.—Principles of improving rivers in flat countries, 76.—Drainage districts in Yorkshire, 77.—Reclamation of Hainault forest, by underground and arterial drainage, 78.—System of dealing with rivers in India, particularly in the Madras Presidency, and the irrigation works in connection with them, 80.—The improvement of rivers in Italy, especially those discharging into the plains of Lombardy, and the irrigation works in Northern Italy, 82.—Principles of improving upland rivers and streams, 83.—Commissioners of Sewers for repairing sea-banks, 84.
- Drainage, main, of the Metropolis, progress of the, noticed in Annual Report, 133.
- Dunn, J. B., elected associate, 130.
- Eardley-Wilmot, Colonel, R.A., remarks as to the construction of artillery, especially as to Mr. Longridge's wire-bound gun, Capt. Blakely's hooped gun, and Sir William Armstrong's and Mr. Whitworth's breech-loading guns, 403.—Ditto as to the results of experiments with a Spanish cast-metal 32-pounder, 404.—Ditto, ditto with an American shell gun, 404.—Ditto as to cast-iron guns, and as to the Royal Gun Foundry, at Woolwich, 405.—Ditto as to the rifling of the old service guns, 406.—Ditto as to brass guns, 406, 423.—Ditto as to Sir W. Armstrong's new breech-loading gun, 422.
- Engineer appointments in the Public Works Department, India, noticed in Annual Report, 148.
- Engines, locomotive, on coal-burning and feedwater heating in.—*Vide* Locomotive engines.
- Errington, J. E., remarks as to the advantages of the double-headed rail, 257.
- Evans, J., elected associate, 130.
- Fairbairn, W., on the efficiency of various kinds of railway breaks, with experimental researches on their retarding powers, 490.
- Fay's continuous break, report of Colonel Yolland relative to, referred to, 491.—*Vide* also Breaks.
- Fisher, J., jun., elected associate, 130.
- Fletcher, L. E., remarks as to early experiments with superheated steam, particularly as applied to locomotive engines, 480.—Ditto as to the rationale of the economy resulting from the use of superheated steam, 481.—Ditto as to steam-induced air currents for coal-burning in locomotive engines, 506.
- Forrest, J., Secretary, vote of thanks to, 132.
- Fortescue, H. E., memoir of, 173.
- Fowler, J., V.P., remarks as to suggested commission for carrying out-works of arterial drainage, 99.—Ditto as to the principles of arterial drainage, illustrated by reference to the Ancholme, the Witham, the Nene, and the Ouse, 100.—Ditto as to the construction and enlargement of the Lindal tunnel, on the Furness railway, 238.—Ditto as to the recommendation that tramways should

- be introduced into Brazil, 259.—Ditto as to the construction of the Netherton tunnel on the Birmingham canal, 280.
- Fox, Sir C., remarks as to the construction of artillery, 371.
- , D. M., elected member, 489.
- Fox, F., elected member, 546.
- Freake, C. J., elected associate, 625.
- Frodsham's system of coal-burning in locomotive engines, on the Eastern Counties railway, 554.—*Vide* also Locomotive engines.
- Fry, A., remarks as to the Bells at the New Palace, Westminster, 13.
- Fuzes, time and concussion, remarks as to the, used with the Armstrong artillery, 409.
- Gardiner, R. B., memoir of, 188.
- Gas, on the measurement of, and the classes of gas meters, in general use, 674.
- Geological nature and physical geography of the districts of Western India traversed by the Great Indian Peninsula railway, 599.—*Vide* also Railway, Great Indian Peninsula.
- Giles, A., remarks as to the form and materials for breakwaters, 662.
- Girders—on a method of computing the strains and deflections of Continuous Beams, under various conditions of load, 625.—*Vide* also Beams.
- Grantham, R. B., on arterial drainage and outfalls, 53.—Remarks as to ditto, and as to the general principles to be adopted in carrying out such works, 122.—Ditto as to the want of legislation being a great impediment to land drainage upon a large scale, 123.—Ditto as to pipe drainage, and the depth to which it should be carried, 124.
- Greaves, C., remarks as to the Bells at the New Palace, Westminster, 15.—Ditto as to the circumstances under which superheating steam, or clothing and jacketing the steam passages would be advantageous, 482.—Ditto as to the progress in the economy of fuel, especially in stationary engines, and as to the philosophy of the production of smoke, 581.—Ditto as to the amount of fuel employed in four sets of boilers, the comparative proportion of small coal, the rest being gas coke, and the evaporative power per lb., 582.—Ditto as to the relative calorific power of coal and coke, 582.
- Gregory, C. H., V.P., remarks as to the construction of artillery, particularly as to the principles suggested and introduced by Capt. Blakely, Mr. Longridge, Sir William Armstrong, Mr. Mallet, Mr. Lancaster, and Mr. Whitworth, and as to the Ordnance Select Committee, 344.
- Grew, N., elected associate, 461.
- Guérin's self-acting break, report of Colonel Yolland relative to, referred to, 491.—*Vide* also Breaks.
- Gunpowder, experiments as to the ultimate force of, 286, *et seq.*—*Vide* also Artillery.
- Guns.—*Vide* Artillery.
- Haddan, J. C., remarks as to the construction of artillery, and as to the results obtained by rifling the existing service gun, and by using particular projectiles, 361.
- Hainault Forest, reclamation of, by underground and arterial drainage, 78.—*Vide* also Drainage.
- Harbours.—*Vide* Breakwaters.
- of Refuge, viewed in relation to the National Defences, as bases of operations for the navy, and especially as to those at Dover, Alderney, Jersey,

- Holyhead, and Portland, noticed in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 225.
- Hartree, W., memoir of, 174.
- Harwich, artesian boring at, remarks as to the, and the strata passed through, 39.
- Hawkshaw, J., V.P., remarks as to the yield from water-bearing strata, and as to evaporation and rainfall, 48.—Ditto as to the construction and enlargement of the Lindal tunnel, on the Furness railway, and as to tunnelling operations generally, 238.—Ditto as to the durability of permanent way, and as to the materials of which it should be composed, especially in tropical countries, 258.—Ditto as to railway breaks, 523.—Ditto as to Jenkins's system of coal-burning in locomotive engines, as applied on the Lancashire and Yorkshire railway, 565.—Ditto as to the mode adopted of carrying on the works on the Madras railway, by small sub-contracts, and generally as to the best plan of executing similar works in foreign countries, 621.—Ditto as to the general style of design of the works on Indian railways, 622.—Ditto as to the cost of railways in India and in England, 622.—Ditto as to the form and materials for breakwaters, 662.
- Hawksley, T., remarks as to the under-drainage of land, and the depth to which it should be carried, 115, 126.—Ditto as to drainage outfalls, and the impolicy of government interference, 117, 126.—Ditto as to the relative value of upland and tidal waters in estuaries, and in rivers discharging into tidal estuaries and within tidal action, 118.—Ditto as to the effect of the removal of Old London bridge, and as to the quantity of water flowing down the Thames in dry weather, 118.—Ditto as to the formation of bars, and the advantage of curved channels, 119.—Ditto as to the difference between the Inclosure Commission, and a Government commission to deal with rivers, mills, &c., 126.—Ditto as to the amount of rainfall, and the quantity of water flowing into rivers, 127.—Ditto as to town drainage, 127.—Ditto as to railway breaks, 520, 524.—Ditto as to oblique weirs, as to the construction and position of weirs on tidal rivers, and as to the works near Tewkesbury, on the river Severn, 539.—Ditto as to coal-burning in locomotive engines, particularly as to the plan introduced by Mr. S. Hall (of Basford) on the Midland railway, 569.—Ditto as to experiments upon the relative values of coal and coke, 569, 579.—Ditto as to the consumption of smoke, and as to experiments on the combustion of smoke, 570.
- Hay, General, remarks as to the relative merits of the Enfield and the Whitworth rifles, 401.
- Haywood, W., appointed one of the scrutineers of the ballot for Council, 131.—Vote of thanks to, 132.
- Hemans, G. W., remarks as to the sinking of a well, in a district of cavernous limestone, in the west of Ireland, 37.—Ditto as to the arterial drainage works in Ireland, 103.—Ditto as to the works on the Netherton tunnel branch of the Birmingham canal, 278.—Ditto as to Mr. Fay's and Mr. Newall's continuous breaks, 518.—Ditto as to the object of experiments on ditto, 523.—Ditto as to the effect of weirs erected on the river Shannon, 540.
- Heppel, J. M., on a method of computing the strains and deflections of Continuous Beams, under various conditions of load, 625.
- Higgin, G., elected member, 625.
- Highton, E., memoir of, 188.
- Hobbs, A. C., appointed one of the scrutineers of the ballot for Council, 131.—Vote of thanks to, 132.—Remarks as to superheated and combined steam, 469, 483.
- Homersham, S. C., remarks as to the Trafalgar Square wells, their yield, and the

- level of the water in them, 35, 45.—Ditto as to the supply of water obtained from the chalk, by the Charlton, Plumstead, and Woolwich, and the Kent waterworks companies, 36.—Ditto as to the statements as to the general depression of the level of the water in the wells sunk into the chalk in the north of London, 45, 48.
- Hopkins, Evan, remarks as to carrying on large works in the interior of South America, near the equator, particularly as to the cost of labour, and the quality of the timber, 256.
- Houldsworth, J., memoir of, 189.
- Howard, T., remarks as to the rationale of superheating steam, 474, 476.
- Hughes, E., memoir of, 190.
- Huish, Captain, remarks as to railway breaks, especially as to the application of continuous breaks, and as to Mr. McConnell's steam break, 522.
- Humphrys, E., remarks as to the use of superheated steam, and as to the results of its application in the 'Ceylon,' the 'Alhambra,' the 'Nepaul,' &c., 470, 473.
- Hutton's (Dr.) experiments for ascertaining the force of gunpowder, remarks as to, 360.
- Hydraulic engineering, present state of, noticed in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 219.—Relative effects of tidal scour and of upland waters upon rivers and harbours, 219.—Offer of a Premium for a Paper on 'the régime of rivers,' 221.
- presses, cylinders of, difficulty experienced in the manufacture of, when of large size, to resist a comparatively moderate pressure, 284, *et seq.*—*Vide* also Artillery.
- Inclines, Bhore and Thul Ghauts, on the Great Indian Peninsula railway, principal works upon the, and comparative Table of the Bhore Ghaut, and of the Giovi and Sömmering inclines, 594, 597.—*Vide* also Railway, Great Indian Peninsula.
- India, list of public works on which interest has been guaranteed, with the amount in each case, noticed in Annual Report, 143.—Railways in progress, ditto, 143.—Madras irrigation and Canal Company, ditto, 146.—Madras pier, 147.—Engineer appointments in the Public Works Department, ditto, 148.
- Indian railways, on, with a description of the Great Indian Peninsula railway, 586.—*Vide* also Railways.
- Ingram's auxiliary break, notice of, and experiments on, by the Manchester, Sheffield, and Lincolnshire railway, 496, 512.—*Vide* also Breaks.
- Irrigation and Canal Company, Madras, noticed in Annual Report, 146.
- works in connection with rivers in the Madras Presidency, 80.—*Vide* also Drainage.
- Lombardy, 82.—*Vide* also Drainage.
- Iron, cast and wrought, experiments as to the tensile strength of, employed in the construction of guns.—*Vide* Artillery.
- plates, remarks as to the results of trials with the Whitworth and the Armstrong artillery and projectiles against wrought, 397, *et seq.*
- Isaac, T. S., Council Premium of books awarded to, 155, 193.
- Jackson, M. B., Council Premium of books awarded to, 155, 193.
- James, J., on the process of raising and hanging the Bells, in the Clock Tower, at the New Palace, Westminster, 3.—Remarks as to the composition of bells, in reference to ditto, 18.—Ditto as to superheated steam, 482.
- Jameson, J. W., Watt Medal awarded to, 155, 193.

Jenkin, F., elected associate, 130.

———, S. W., elected member, 489.

Jenkins's system of coal-burning in locomotive engines, 551.—*Vide also* Locomotive engines.

Jervia, Capt., B.A., remarks as to the service bullet used with the Enfield rifle, 376.

Jones, J. Hodgson, remarks as to the depth to which the under-drainage of land should be carried, 125.

Johnston, T. M. H., elected member, 263.

Kimber, J., elected associate, 130.

Kingsbury, W. J., appointed one of the auditors of accounts, 131.—Telford Medal and Manby Premium in books, awarded to, 155, 193.

Knight, G., remarks as to the cost of the construction of the Lindal tunnel, on the Furness railway, 238.

Krüpp's forged steel gun, experiments with, 292.—Remarks as to the circumstances in connection with the bursting of, when tried at Woolwich, in 1853, 343.—*Vide also* Artillery.

Lancaster, O. W., remarks as to the construction of cannon of large calibre, with extract from Paper on this subject by Professor Treadwell, U. S., 376.—*Ditto* as to the Lancaster oval guns, 377.

Land drainage.—*Vide* Drainage, arterial.

Latham, J. H., elected associate, 130.

Lawrence, S., elected associate, 625.

Leach, S. W., remarks as to the improvements of the river Thames, during the last thirty years, 111, 112.

Lees' system of coal-burning in locomotive engines, East Lancashire railway, 553.—*Vide also* Locomotive engines.

Locke, J., M.P., Past-President, remarks, on the opening of the Session, as to deaths of I. K. Brunel and R. Stephenson, M.P., 1.—*Ditto* as to the variation of the level of the water in chalk, 51.—*Ditto* as to legislative interference in the drainage of land, 86.—*Ditto* as to a chart showing the improvements on the river Clyde since 1842, 111.—*Ditto* as to the effect of recent improvements in the river Thames and the river Clyde, 112.—Vote of thanks to, 131.—Remarks as to the construction of the Netherton tunnel, on the Birmingham canal, of the Woodhead tunnel, on the Manchester and Sheffield railway, and of a railway tunnel at Liverpool, 277.—*Ditto* as to Sir W. Armstrong's and Mr. Whitworth's breech-loading rifled ordnance, 421.

Locks, stop, details of the, on the Netherton tunnel branch of the Birmingham canal, 269.—*Vide also* Canal.

Locomotive engines, on coal-burning and feedwater heating in, 546.—Qualifications of the ordinary locomotive boiler for the combustion of coal, 547.—Methods recently introduced of admitting air above the fuel, near its surface, and amongst the combustible gases, 550.—Adaptation of large extended fire boxes and combustion chambers, by Mr. McConnell, Mr. Beattie, and Mr. Cudworth, 550.—*Ditto* of existing engines, for burning coal without smoke, by simple means, independent of extensive structural alterations, by Messrs. Gray and Chanter, Mr. Dewrance, Messrs. Dubs and Douglas, Messrs. Evans and Dubs, Mr. Yarrow, and Mr. Jenkins, 551.—System of steam-induced air currents, for admitting air above the fuel, through the sides of the fire box, and application of system on different railways, 552.—Other systems for the admis-

sion of air into the fire box, 553.—Class of contrivances acting by the deflection of a body of air introduced through the doorway, upon and over the surface of the fuel, by Mr. Douglas, Messrs. Lee and Jacques, Mr. Sinclair, and Mr. Frodsham, 553.—The steam blow-pipe, or auxiliary jet, in the chimney, common to all plans for consuming coal without smoke in locomotives, 554.—Contrivances for dealing with air in bulk, usually attended by the escape of a considerable quantity of unconsumed air through the flue-tubes, and a difficulty in keeping up steam at high speeds, 556.—The various forms of doorway deflectors objectionable, in facilitating the friction of particles of coal through the tubes, and the burning of the smoke box, unless counteracted by an internal arch, 557.—The direct and efficient combustion of coal, in the ordinary locomotive boiler, depends upon the intermixture of a plentiful but regulated supply of air with the ascending smoke, or combustible gases, at or near to the surface of the fuel, 557.—Performances of coal and coke-burning engines compared, 559.—Comparison of the results of coal-burning practice on different lines, 560.—Feed-water heaters, 562.

Longridge, J. A., on the construction of artillery, and other vessels, to resist great internal pressure, 283.—Remarks as to ditto, 338.—Ditto as to the deterioration of cast iron in the interior of a gun, by cooling, 340.—Ditto as to the pressure which would burst the cylinder of a hydraulic press, 341.—Ditto as to the state of tension of the particles of a homogeneous cylinder, strained by an internal pressure, 341.—Ditto as to the construction of iron girders, 343.—Ditto as to the Ordnance Select Committee, 370.—Ditto ditto, and as to the manner in which his invention was tested, 433.—Ditto as to the effect of twist in rifled guns, 435.—Ditto as to the want of some means of adjusting the trunnions of all ordnance perfectly horizontal, 436.—Ditto as to the force of gunpowder, 437.—Ditto as to the statement, that the effect of a resisting medium is to prolong the range of the shot, 440.—Ditto as to the actual and the calculated parabolic ranges for some of the experiments made with the Armstrong and the Whitworth guns, 442.—Ditto as to cast iron as a material for ordnance, 443.—Ditto as to a mixture of certain alloys ditto, 444.—Ditto as to wrought iron and steel ditto, 445.—Ditto as to building up guns with concentric rings or hoops, or binding them with wire, 446.—Ditto as to the economy in the use of superheated steam, 468.—Ditto as to the temperature of the gases in the up-take before the chimney, in a marine boiler, as ascertained during experiments relative to the consumption of smoke, and as to the application of a heater for abstracting a portion of the heat, 469.—Ditto as to coal-burning in locomotive engines, particularly as to the plan introduced on the Midland railway, by Mr. Hall, of Basford, 567.—Ditto as to experiments upon the consumption of smoke, 567, 577.—Ditto as the comparative heating powers of coal and coke, 568, 576.—Ditto as to the heat generated by 1 lb. of coal, and by 1 lb. of coke, 568.—Ditto as to the quantity of air required for the perfect combustion of 1 lb. of Hartley coal, 577.—Ditto as to the results of ten experiments on Hartley coal, tending to show, that the admission of air, in such quantity as was required to prevent smoke, was to increase the temperature of the chimney, and to decrease the consumption of fuel, 578.—Ditto as to experiments on board the 'Bustler,' showing the advantage of the use of ventilating doors for the prevention of smoke, 578.—Ditto as to the Calcutta and South-Eastern railway, 620.—Ditto as to the capabilities of the river Mutla for the formation of a new port, 620.—Ditto as to the form and construction of breakwaters, and as to the classes of waves to which sea works were exposed, 655.

[1859-60. N.S.]

3 A

- Longdon, A., remarks as to the circumstances in connection with the bursting of Mr. Krüpp's gun, at Woolwich, in 1853, 343.
- Lovegrove, J., elected associate, 461.
- Mackain, D., memoir of, 175.
- Mallet, R., Telford Medal awarded to, 155, 193.
- Manby, C., Honorary Secretary, vote of thanks to, 132.
 ——— and Council Premiums, and Telford and Watt Medals, awarded, Session 1858-59, 131, 155, 193.—Lists of subjects for, Session 1859-60, 194.
- Manning, B., remarks as to the arterial drainage works in Ireland, 119.
- Maudslay, H., vote of thanks to, 131.—Appointed one of the auditors of accounts, 131.
- May, C., remarks as to the Bells at the New Palace, Westminster, and as to the atomic proportions of different alloys of copper and tin, 13, 18.—Ditto as to the superiority of charcoal-smelted copper, 14.—Ditto as to the depth of, and the supply of water from the chalk at Colchester and at Harwich, 48.—Ditto as to the depression of the water in the wells of London, 48.—Ditto as to the use of superheated steam, 476.
- McConnell's (J. E.) steam-break, report of Colonel Yolland relative to, referred to, 491.—*Vide* also Breaks.
 ——— system of coal-burning in locomotive engines, 550.—*Vide* also Locomotive engines.
- McKenzie and Wentworth's breech-loading rifles, notice of, 461.
- Mears, G., remarks as to the composition of the metal of the Bells at the New Palace, Westminster, 15.
- Medals, Telford and Watt, and Council and Manby Premiums awarded, Session 1858-59, 131, 155, 193.—List of subjects for, Session 1859-60, 194.—Extract from Minutes of Council, February 23rd, 1835, on the principal subjects for, 199.
- Memoirs of deceased members, 168.
- Mills, G., memoir of, 191.
- Minutes of Proceedings published during the past year, 165.
- Moorsom, Capt. W. S., remarks as to the duties which devolve upon an Engineer in the management of works in distant regions, especially in tropical countries, 255.—Ditto as to the size of the bore in rifled small arms, and as to the construction of artillery, 400.
- Murphy, J., remarks as to the composition of the metal, the casting, and the shape of bells, in reference, particularly, to the great Westminster Bell, 19.—Ditto as the bell selected to strike the hours, in the centre of the hall of the Great Exhibition, 19.
- Murray, E. F., elected member, 489.
 ———, John, remarks as to works recently constructed upon the river Severn, at the Upper Lode, near Tewkesbury, 534.—Ditto as to the form and materials for breakwaters, 668.—Ditto as to the use and cost of béton blocks at Algiers and at Marseilles, 668.—Ditto as to the proposed substitution of a gridiron, or wave screen, for the usual perpendicular or sloping pier of masonry, 669.—Ditto as to the depth to which the action of the waves extends, 669.
- Neilson, W. M., elected member, 489.
- Newall's continuous and self-acting break, report of Colonel Yolland relative to, referred to, 491.—*Vide* also Breaks.
- Noble, Captain, R.A., remarks as to the computed initial velocity of the Armstrong projectile, 421.

Norfolk Estuary, works noticed in discussion on arterial drainage, 91, *et seq.*
 Nyctoscope, an instrument for enabling gunners to maintain a fire upon any given object after nightfall, remarks as to the, 421.

Ordnance Select Committee, remarks as to the, 344, *et seq.*—*Vide* also Artillery.
 Original communications received between June 30, 1858, and June 29, 1859, 201.
 —Instructions as to preparing ditto, 199.

Ormiston, T., remarks as to the improvements of the river Clyde, and their effect on the land floods, 110.

Outfalls, on arterial drainage and, 53.—*Vide* also Drainage.

Papworth, J. W., remarks as to striking bells, and as to the composition of old bells, 18.

Parkes, W., remarks as to that portion of the river Severn, between Worcester and Tewkesbury, and as to the system pursued for its improvement, 538.

Parson, G. J., remarks as to the system of superheating steam by means of pipes, 473.

Patridge, D., remarks as to the use of superheated steam, 471, 476.

Pearson, S., remarks as to the construction of guns to withstand the greatest amount of internal pressure, 394.

Phillips, J., elected associate, 461.

Pier, Madras, noticed in Annual Report, 147.

Pole, W., remarks as to the musical quality of the Bells at the New Palace, Westminster, 15.—Vote of thanks to, 131.—Remarks as to the application of a continuous break on the North London railway, 524.

Pollard, W. B., elected associate, 489.

Poole, J., notice of a parallel motion safety-valve, 52.

Premiums, Council and Manby, and Telford and Watt Medals, awarded, Session 1858-59, 131, 155, 193.—List of subjects for, Session 1859-60, 194.—Extract from Minutes of Council, February 23rd, 1835, on the principal subjects for, 199.

Presents particularly noticed in the Annual Report, 162.—Catalogue of, received between June 30th, 1858, and June 29th, 1859, 203.

Projectiles.—*Vide* Artillery.

Purdon, W. H., elected member, 130.

Purser, E., elected member, 263.

Quarm, T., remarks as to the Bells at the New Palace, Westminster, 13, 14.

Railway breaks, on the efficiency of various kinds of, with experimental researches on their retarding powers, 490.—*Vide* also Breaks.

———, Furness, on the construction and enlargement of the Lindal tunnel, on the, 229.—*Vide* also Tunnel.

———, Great Indian Peninsula, on Indian railways, with a description of the, 586.—Population of Bombay, preponderance of trade to, amount of exports and imports, 590.—Traffic which will be commanded by the railway, 591.—The Syhadree mountains, 591.—Effect of the railway system upon the commercial transactions relative to cotton, 592.—The principal works upon the experimental line from Bombay to Callian, 592.—Ditto upon the south-eastern extension from Callian to Campoollee, 593.—Ditto upon the Bhore Ghaut incline, 594.—Comparative Table of the Bhore Ghaut and of the Giovi and Sömmering inclines, 595.—Principal works upon the south-eastern extension from Lanowlee to Sholapore, 596.—Ditto upon the first section of the north-eastern extension from Callian

- to Kussarah, 596.—Ditto upon the Thul Ghaut incline, 597.—Ditto upon the north-eastern extension from Egutpoora, by Nassick, to Bhosawl, 597.—Ditto upon the last section of ditto, from Bhosawl to Jubbulpoor, 598.—Ditto upon the Oomrawuttee and Nagpore branch, 598.—General style of design for these trunk lines, 598.—Geological nature of the country, 599.—Physical geography of the districts of Western India traversed by the railway, 600.—Amount of rainfall, during the year 1858, at different places upon and near the western coast, 601.—Railway materials procurable in India, 601.—Properties of some of the various kinds of wood, 602.—Cost of sleepers, and immunity from the ravages of the white ant when laid in the ballast, 602.—Building stone, bricks, and lime, 602.—Rise in the prices of materials, and irregularity in obtaining supplies, 603.—Use of Greaves's iron sleepers, and of creosoted sleepers, 604.—Iron goods' shed and iron booking office, 604.—Character of the native labour, 604.—Rates of wages per day of several classes of native labourers, and relative cost of each kind of labour in England and in the Bombay Presidency, 605.—Adoption of the English contract system, and effect of its introduction into India, 606.—The engineering staff, 607.—Methods of executing the works, 607.—Average cost of the opened portions of the line, and prices of the principal kinds of work, 609.—Effects produced by railway enterprise in Western India, 610.—*Vide also Railways, Indian.*
- Railway, Mauá; on the means of communication in the empire of Brazil, chiefly in reference to the works of the Mangaratiba Serra road, and to those of the Mauá, the first Brazilian railway, 240.—*Vide also Brazil.*
- system in Spain, noticed in Annual Report, 148.
- Brazil, —————, 152.
- Railways in progress, noticed in Annual Report:—Metropolitan, 135.—Victoria Station and Pimlico, 135.—Charing Cross, 135.—West London extension, 136.—Portsmouth, 136.—Shrewsbury and Crewe, 136.—Severn Valley, 136.—Salisbury and Yeovil, 137.—Castle Douglas and Dumfries, 140.—In Ireland, 140.—Egyptian, 142.—East Indian, 143.—Great Indian Peninsula, 144.—Madras, 145.—Scinde and Punjab, 145.—Eastern Bengal, 146.—Great Southern, 146.—Calcutta and South-Eastern, 146.—In the Mauritius, 148.—The Northern of Spain, 149.—The North-Eastern of Spain, 149.—The Biscayan, 149.—Riga and Dunaberg, 151.—Don Pedro Segundo, 152.—Pernambuco, 152.—Bahia, 152.—San Paulo, 153.
- , Indian, on, with a description of the Great Indian Peninsula railway, 586.—Importance of railways in British India, 586.—Arrangements entered into by the Government for the establishment of a system of railways, and terms of the contract between the Government and the companies, 587.—Advantages of the guarantee system, and disadvantages of the minute supervision exercised by the authorities, 588.—Standard dimensions adopted on Indian railways, 589.—The Great Indian Peninsula railway, 590. *Vide also Railway, Great Indian Peninsula.*
- Rainfall during the year 1858, at different places upon and near the western coast of India, 601.—*Vide also Railway, Great Indian Peninsula.*
- Rams, steam, or vessels for running down, the design and construction of, noticed in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 223.
- Ramsbottom's (J.) system of coal-burning in locomotive engines, on the London and North-Western railway, 553.—*Vide also Locomotive engines.*
- Rawlinson, R., remarks as to the yield from water-bearing strata, the depth at which water is to be obtained, and the caution which should be exercised in

relying, for large communities, upon a supply of water from wells, 49.—Ditto as to sinking wells near the sea coast, and as to that at Worthing, 50.—Ditto as to the fountains in Trafalgar Square, 51.

Rennie, G., remarks as to superheated and combined steam, 478.

——, Sir John, Past-President, remarks as to the difficulties in carrying out measures for arterial drainage, 90.—Ditto as to the history of the drainage of the fens, called the Bedford level, 91.—Ditto as to the main principles of drainage and navigation, 98.—Ditto as to Italian and Indian rivers, and as to the value of works of irrigation, 98.—Ditto as to the improvements in the river Ouse, 101.—Ditto as to the alteration in the high-water level in the Thames, by the removal of Old London bridge, 112.

Report, Annual, read and ordered to be printed, 131.—Notice of some of the principal works in progress, 133.—Main drainage of the metropolis, 133.—RAILWAYS :—Metropolitan, 135.—Victoria Station and Pimlico, 135.—Charing Cross, 135.—West London extension, 136.—Portsmouth, 136.—Shrewsbury and Crewe, 136.—Severn Valley, 136.—Salisbury and Yeovil, 137.—Castle Douglas and Dumfries, 140.—In Ireland, 140.—DOCKS :—Grand Surrey, 137.—Swansea, 138.—Newport, 138.—Silloth, 138.—Penarth, 139.—Southampton, 139.—Glasgow waterworks, 139.—Galway packet station, 141.—Londonderry bridge, 141.—Submarine telegraphs, 141.—Egyptian railway, 142.—INDIA :—Public works on which interest has been guaranteed, with the amount in each case, 143.—RAILWAYS :—East Indian, 143.—Great Indian Peninsula, 144.—Madras, 145.—Scinde and Punjaub, 145.—Eastern Bengal, 146.—Great Southern, 146.—Calcutta and South-Eastern, 146.—Madras Irrigation and Canal Company, 146.—Madras pier, 147.—Engineer appointments in the Public Works Department of India, 148.—Railways in the Mauritius, 148.—Railway system in Spain, 148.—The Northern, 149.—The North-Eastern, 149.—The Biscayan, 149.—Reclamation of land and extension of harbour at Copenhagen, 151.—Altona waterworks, 151.—Riga and Dunaberg railway, 151.—Railways in Brazil, 152.—Don Pedro Segundo, 152.—Pernambuco, 152.—Bahia, 152.—San Paulo, 153.—Victoria tubular bridge at Montreal, 154.—Principal papers read during the Session, 154.—List of Telford and Watt Medals and Council and Manby Premiums of books awarded for ditto, 155.—Notice of some of the principal papers, 155.—Presents particularly noticed, 162.—Statement of the arrears of subscriptions for 1859, and for 1857 and 1858, 162.—Tabular statement of the transfers, elections, deceases, and resignations of members of all classes, during the years 1857-58 and 1858-59, 163.—Ditto of the annual increase of members and associates for the past six years, 163.—Notice of the deceases of I. K. Brunel and R. Stephenson, M.P., 164.—Bequest of R. Stephenson, M.P., 164.—Remarks as to statement of receipts and expenditure, 164.—Minutes of Proceedings published during the past year, 165.—Remarks as to the balloting list for members of Council, 165.—Abstract of receipts and expenditure from the 1st December, 1858, to the 30th November, 1859, 166.—Appendix, memoirs of deceased members, 168.

Resolution to adjourn over Easter Tuesday, 489.

Rickards, J. L., elected member, 263.

Rifled ordnance and projectiles.—*Vide* Artillery.

Rifles, breech-loading, McKenzie and Wentworth's, notice of, 461.

——, Whitworth, remarks as to the strength of the material used for the, their cost, the form of rifling, and as to the results of competitive trials between the Enfield and the Whitworth rifles, 398.

Rigby, W., elected associate, 489.

River Clyde, remarks as to the improvements of the, and their effect on the land floods, 110.

— Severn, an account of the works recently constructed upon the, at the Upper Lode, near Tewkesbury, 527.—Scheme of improvement laid down by Sir William Cubitt, in 1841, 527.—Parliamentary contest as to ditto, and unsatisfactory result of experiment, forced upon the promoters, to dredge that portion of the river between Diglis and Ryall, instead of constructing a lock and weir at Ryall, as proposed, 528.—Determination to construct a lock and basin at the Upper Lode, 528.—Dimensions and details of ditto, 529.—Mode of carrying out the excavations for the lock pit, 529.—Details of the concrete foundations for the walls and invert, 530.—Ditto of the lock chamber, invert and wing walls, 531.—Ditto of the weir, 531.—Process by which the base of the permanent dam was carried across the river, 532.—Cost of the works, 533.—The tugs used upon the river, 532.

— Thames, remarks as to the improvements of the, during the last thirty years, 111.

Rivers in flat countries, principles of improving, 76.—*Vide* also Drainage.

— India, system of dealing with, particularly in the Madras Presidency, and the irrigation works in connection with them, 80.—*Vide* also Drainage.

— Italy, the improvement of, especially those discharging into the plains of Lombardy, and the irrigation works in Northern Italy, 82.—*Vide* also Drainage.

'Rivers, the régime of,' premium for a Paper on, offered by Mr. G. P. Bidder, President, in his address, on taking the chair, for the first time, after his election, 221.

Road, Mangaratiba Serra, on the means of communication in the empire of Brazil, chiefly in reference to the works of the, and to those of the Mauá, the first Brazilian railway, 240.—*Vide* also Brazil.

Roberts, E., elected associate, 461.

Robertson, A. J., remarks as to a proposed cast and wrought-iron breakwater and pier, 665.—Ditto as to the form and materials for breakwaters, 665.

Rodman, Lieutenant U.S., remarks as to his principle of casting guns on a hollow core, 363, *et seq.*—*Vide* also Artillery.

Rouse, H. J., elected member, 489.

Russell, J. Scott, remarks as to breakwaters, as to the two classes of waves to which sea works are exposed, as to proposals for substituting a system of gridirons, or wave screens, of open timber and iron piling, for the usual perpendicular or sloping pier of masonry, and as to the practical forms of breakwaters, sea walls, and other engineering works exposed to the action of waves, 651.

Safety-valve, parallel motion, notice of a, 52.

Samuel, L. W., elected member, 546.

Scott, Capt., R.N., remarks as to the construction of artillery, the rifling of the existing cast-iron service guns, and the mode in which new guns should be cast, 366.

—, M., Telford Medal awarded to, 155, 193.—Remarks as to the form of the weir constructed upon the river Severn, at the Upper Lode, near Tewkesbury, 539.—On breakwaters, Part II., 644.—Remarks as to the situation of the Blyth breakwater, 657, 672.—Ditto as to a proposed form of breakwater, 670.

Sewage of towns, remarks as to the prospect of employing the, for the fertilisation

- of land, in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 220.
- Sewell, J., remarks as to the fracture of railway axles, and as to the cause of steam-boiler explosions, 16.—Ditto as to the fracture of the first Great Bell at Westminster, being due to the meeting of the waves of sound, 17.
- Sharpe, R., elected associate, 489.
- Shaw, H. R., elected associate, 546.
- Shell, segment, remarks as to the, used with the Armstrong artillery, 407.
— Swedish percussion, remarks as to the, 373.—*Vide* also Artillery.
- Shipbuilding in the public establishments, remarks as to the state of, 460.
- Siemens, C. W., remarks as to the construction of artillery, especially as to Krüpp's cast-steel gun, 378.—Ditto as to experiments and apparatus for determining the pressure upon the interior of a gun when fired, and the resistance of the atmosphere to shot, at different velocities, 379.—Ditto as to superheating steam, and as to experiments for determining the rate of expansion, 479.—Ditto as to mixing ordinary saturated steam with superheated steam, 480.—Ditto as to his regenerative engine, 480.
- Simpson, J., Past-President, remarks as to the difficulties in obtaining outlets for land drainage, 89.—Ditto as to a project for the drainage of low lands near Wareham, 90.
- Sinclair, R., remarks as to railway breaks, especially as to Mr. Fay's and Mr. Newall's continuous breaks, 518.
- Smith, W., remarks as to the composition of the Bells at the New Palace, Westminster, 14.
- Smoke, consumption of, remarks as to experiments upon the, 567, *et seq.*—*Vide* also Locomotive engines.
- Spain, railway system in, noticed in Annual Report, 148.
- Sperling, R. A., elected associate, 263.
- Stanley, Walmsley, jun., elected member, 130.
- Steam, combined, on, 462.—Application of ordinary and superheated steam, mixed, 463.—Objections to the use of steam simply dried, or superheated, 464.—Practical illustration of the efficiency of mixed steam in the 'Avon,' 464.—Experiments conducted by order of the Lords of the Admiralty, 465.—Examples of the application of combined steam, with the results obtained, 465.—Advantages of this system, 466.
— navigation, ocean, Mr. I. K. Brunel's exertions in accelerating the progress of, noticed in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 214.
- Stephenson, R., M.P., Past-President, notice of the death of, by Mr. J. Locke, M.P., President, at the opening of the Session, 2.—Ditto in Annual Report, 164.—Bequest of, 164.—Memoir of, 176.—Notice of the death of, by Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 216.
- Stileman, F. C., on the construction and enlargement of the Lindal tunnel, on the Furness railway, 229.—Remarks as to ditto, particularly as to the cost, 238, 239.
- Stokes, J. F., elected associate, 130.
- Storey, T., memoir of, 182.
- Strode, W., remarks as to the Enfield rifle, 376.
- Subscriptions, arrears of, statement of the, for 1859, and for 1857 and 1858, 162.
- Superheated steam.—*Vide* Steam, combined.

Tabular statement of the transfers, elections, deceases, and resignations of members of all classes, during the years 1857-58, and 1858-59, 163.

Taylor, J., elected associate, 130.

——, Rev. W., F.R.S., on the large bells of Europe, 10.—*Vide* also Bells, in the Clock Tower, at the New Palace, Westminster.

Telegraphs, submarine, extension of, noticed in Annual Report, 141.—Ditto in the address of Mr. G. P. Bidder, President, on taking the chair, for the first time, after his election, 221.

Telford and Watt Medals, and Council and Manby Premiums awarded, Session 1858-59, 131, 155, 193.—List of subjects for, Session 1859-60, 194.

Tennant, Professor, remarks as to the supply of water in London, 37.

Thomas, J. L., elected associate, 625.

Thwaites, J., elected associate, 130.

Timber, creosoted, remarks as to, and as to the capability of green-heart timber to resist the attacks of the teredo, 665.

——, properties of some of the, procurable for railway purposes, in India, 602.
—*Vide* also Railway, Great Indian Peninsula.

——, quality of the, in the lower region of the tropics, remarks as to the, 256.

Topham, J., elected associate, 214.—Remarks as to the results of the application of superheating apparatus, in vessels belonging to the Intercolonial Royal Mail Company, 472.

Townsend, G. B., elected associate, 546.

Treadwell, Professor D., U. S., extract from a Paper by, 'on the practicability of constructing cannon of large calibre,' 376.

Tugs used upon the river Severn, details of the, 533.—*Vide* also River Severn.

Tulloch, Colonel, R.A., remarks as to the advantage of the small muzzle of the Armstrong gun, 423.

Tunnel, Lindal, on the construction and enlargement of the, on the Furness railway, 229.—Circumstances under which the tunnel for a single line was constructed, 229.—Various modes proposed for executing the works, 230.—Rules adopted for working the traffic during the operations, 231.—Explanation of the manner in which the works of enlargement were carried out, 231.—Difficulties encountered and overcome, 233.—Number of men employed and time occupied in the execution of the several works, 235.—Total cost of the tunnel, 236.

——, Netherton, branch, of the Birmingham canal, description of the works on the, 263.—*Vide* also Canal.

Varley, S. Alfred, restoration of cracked bells by tin soldering, 229.

Vignoles, C., remarks as to the Woodhead tunnel on the Manchester and Sheffield railway, 278.—Ditto as to Dr. Hutton's experiments for ascertaining the force of gunpowder, 360.—Ditto as to railway breaks, 518.—Ditto as to the friction, or adhesion on a railway, 521.

Walker, C. R., elected associate, 461.

——, J., Past-President, remarks as to the works for the construction and enlargement of the Lindal tunnel, on the Furness railway, 239.

——, J. R., description of the works on the Netherton tunnel branch of the Birmingham canal, 263.—Remarks as to the strata passed through in sinking the shafts and in driving the tunnel, 277.—Ditto as to the cost and mode of execution of the works, 281.

Ward, R. J., elected member, 263.

Water supply, metropolis, remarks as to the, in the course of the discussion on the Government Waterworks in Trafalgar square, 30, *et seq.*

Waterworks, Glasgow, remarks as to the construction of thirteen miles of tunnels for the, 279.

— in Trafalgar Square, on the Government, 21.—Undertaken for supplying the fountains in Trafalgar Square, as well as the public offices, 21.—The first well in Orange Street, and the strata through which it passed, 22.—The second well, in the inclosure in front of the National Gallery, and the strata through which it passed, 22.—The engine-house and tanks, 23.—The engines, 23.—Extensions of the works, 24.—Accident in driving the large bore-pipe, 24.—The new steam engine, 25.—The yield from the springs, and the supply obtained, 25.—Levels of the water in the well in front of the National Gallery, from 1846 to 1856, 25.—Well on the premises of Truman, Hanbury, and Co., 26.—Well at Combe and Delafield's brewery, 27.—Appendix, containing a list of the places supplied, with the revenue derivable therefrom, at the rates previously paid for water, 28.

— Manchester, remarks as to the construction of a tunnel for the, 279.

— noticed in the Annual Report :—Glasgow, 139.—Altona, 151.

Watt and Telford Medals, and Council and Mauby Premiums awarded, Session 1858-59, 131, 155, 193.—List of subjects for, Session 1859-60, 194.

Waves, remarks as to the classes of, to which sea works are exposed, 651, *et seq.*

Webb, E. B., on the means of communication in the empire of Brazil, chiefly in reference to the works of the Mangaratiba Serra road, and to those of the Mauá, the first Brazilian railway, 240.—Ditto as to the San Paulo railway, 255.—Ditto as to the cost of a bridge on the Serra road, and as to the use of the double-head rail on the Mauá railway, 256.—Ditto as to the common labour of the country, 260.—Ditto as to the reasons which operated unduly to increase the cost of the bridge of cut stone, 260.

Wells for supplying the fountains in Trafalgar Square and the public offices, details respecting the, and the yield from the springs; also of those at Truman, Hanbury, and Co.'s, and at Combe and Delafield's breweries, 21, *et seq.*—*Vide* Waterworks in Trafalgar Square.

West, F. W. L., elected member, 263.

Wethered, the Hon. J., on combined steam, 462.—Remarks as to ditto, particularly as to experiments in H.M.S. 'Dee,' 468.—Ditto as to superheated steam, and as to the apparatus for applying it in the 'Valetta' and H.M.S. 'Dee,' 485.—Ditto as to the use of mixed steam, 486.—Ditto as to the reports of trials of the system made by the French, the British, and the United States Governments, 487.

Wheatley, Captain, remarks as to experiments in the United States against iron plates, 400.

Whitworth, J., remarks as to his system of rifled cannon, the metal used in their construction, the operation of loading, the projectiles employed, and their range and penetration, 396.—Ditto as to the strength of the material used for the Whitworth rifles, their cost, the form of rifling, and as to the results of competitive trials between the Enfield and the Whitworth rifles, 398.—Ditto as to experiments against iron plates, 397, 399.—Ditto as to the construction of his 80-pounder gun, 398.—Ditto as to the degree of rifling adopted in the Whitworth guns, 399.—Ditto as to the effect of twist in rifles, 400.—Ditto as to the diameter of bore adopted in his rifle, 400.—Ditto as to the rifling of the service brass guns, 406.—Ditto as to suggested competitive trials of Sir W. Armstrong's and Mr. Whitworth's guns, 422.—Ditto as to the difference between Sir W.

[1859-60. N.S.]

3 B

- Armstrong's rifled ordnance and projectiles and his own, 423.—Ditto as to his 12-pounder breech-loading gun, the projectiles used with his rifled ordnance, the metallic cartridge, and the mode of working the gun, 423.—Ditto as to the comparison of ranges of the two guns, 427.
- Wightman, A., remarks as to railway breaks, 519, 524.
- Williams, E. L., an account of the works recently constructed upon the river Severn, at the Upper Lode, near Tewkesbury, 527.—Remarks as to ditto, and as to the construction of weirs in a tidal river, and the position of the lowest weir, so as not to obstruct the tidal flow, and to facilitate the passage of the flood water, 540.—Ditto as to the effect of the oblique weirs on the river Severn, 541.
- , E. L., jun., elected member, 263.
- Wilmot, Colonel Eardley, R.A.—*Vide* Eardley-Wilmot.
- Wilson, J., elected associate, 263.
- Wilson's system of coal-burning in locomotive engines, on the Oxford, Worcester, and Wolverhampton railway, 553.—*Vide* also Locomotive engines.
- Wollaston, C. J., elected associate, 461.
- Wright, A., memoir of, 183.
- , —, remarks as to experiments on coal-burning in locomotive engines, 566.
- Yarrow's system of coal-burning in locomotive engines, 551.—*Vide* also Locomotive engines.
- Yarrow, T., remarks as to experiments on coal-burning in locomotive engines, 571.
- Yolland, Colonel, report of, to the Board of Trade, relative to the steam break of Mr. McConnell, the continuous break of Mr. Fay, the continuous and self-acting break of Mr. Newall, and the self-acting break of M. Guérin, referred to, 491.—*Vide* also Breaks.

END OF VOL. XIX.

