Mr. Hackney said he had only to add that, through the kindness of Dr. Siemens, he had received from M. Euverte some particulars of the mechanical properties of the steel made at Terrenoire, containing from 0·3 to 0·35 per cent. of phosphorus, and 0·16 per cent. of carbon, as compared with ordinary Bessemer rail steel of good quality, such as was made in France.[1] The summary of these was that the limit of elasticity of the steel containing phosphorus was the same as that of good, but rather hard Bessemer steel—about 24 tons per square inch; and that up to that limit the stiffness and extension under strain were the same in the two qualities; but while the ultimate strength of the harder steel was 49 tons per square inch, stretching 7 per cent., that of the metal high in phosphorus was only 33 tons per square inch, stretching 9½ per cent. With reference to Mr. Smith's eminently practical Paper, he would remark that the plan of detecting hard rails, by the greater resistance they offered to punching, might be carried out with much less loss to the manufacturer than would appear at first sight. Few steel-makers would fit indicators to their punching-presses, if they knew that all rails thrown aside, as too hard, were finally rejected, and would be saleable only as rejected rails, at a deduction of £1 or £2 a ton: there would be no need, however, for this, as the hard rails might be annealed, in a conveniently arranged furnace, at a cost of a few shillings per ton, and they would then stand the punching test perfectly well, and would be in all respects as safe and durable in use as rails lower in carbon, used, without annealing, as they left the rolls.

Mr. Smith remarked that notice had been taken of some difference in the table indicating certain strains required to produce certain results in specimens of rails which had been down for eight or nine years. The only difference was, that in the table presented with the Paper the tests were given in the order in which they were made; but he afterwards thought it would be better to alter it so as to allow the tests to follow each other in the value represented by the rails, so far as punching and tensile strains were concerned, in order to show the remarkable continuity of result—that as the carbon and as the tensile or punching or bending strain increased, the results were pretty consecutive with respect to all the strains. He attached no more value to the punching strain than to any other mechanical strain; but as all the rails had to be punched for fish-plate joints, it appeared to him that there was an opportunity, without expense, of testing each rail. He was satisfied

[1] An abstract of M. Euverte's report is given in Appendix D (page 63).

that the companies who principally manufactured steel rails would gladly fall in with any arrangement that would tend to allay distrust as to the uncertainty of steel, if the expense of doing so came within the limits of the price at which they had to sell the article. His brother manufacturers would agree with him that, instead of the unsatisfactory method now adopted of taking a few rails out of each day's make, (which really formed no criterion of the value of the whole batch), it would be better to adopt a system by which every rail should be tested without the inconvenience of having a number of good rails spoiled, with a view to satisfy the requirements of the inspectors. The results were a little variable in one or two instances. He would have preferred a table showing the results from the section of rail recently rolled. That section was now more nearly adhered to than it was nine or ten years ago, when the rails in question were made. They were made for the Furness railway, and perhaps the same care was not taken with regard to the section as should have been taken. Some of them varied 1 lb. to the yard. In all instances where there was a variation the rails were a little over the 73-lbs. standard, some of them being a little over 74 lbs. He had compiled the table from the results shown by the old rails, because he was astonished at the remarkable uniformity of the results obtained with respect to the punching, tensile, bending, and all the mechanical strains, and that they were nearly always in correspondence with the amount of carbon. But there was one test which he considered more important than anything else, and with regard to which for four or five years he had thought some engineers were in error, namely, with respect to the wearing property of the rails. Between the limits of 31 tons and 33 tons tensile strain, there was a marvellous uniformity in all the results; so that if the tensile, or the punching, or the bending strain were obtained, the other properties were known. Having those old rails to experiment upon, the opportunity was afforded of ascertaining whether there was any value in hard rails exceeding 33 tons, and going up to 60 or 70 tons per square inch—whether it was not a mistake to suppose that hard rails were better than soft rails. The experiments made upon rails partially worn out upon an exceptionally heavy traffic during nine years, had conclusively proved that rails within 33 tons of tensile strain would wear as well as the hardest steel rail that it was practicable to put down. It appeared that the rails had lost about 1 lb. per year. In every respect, except that in one or two instances the rails weighed a little more than 73 lbs., the table was thoroughly reliable.

Dr. Percy said Mr. Hackney had in his Paper embraced a very extensive range of subjects; but he would only refer to a few topics. With regard to the proposed definition of steel, he thought it involved some confusion. Mr. Hackney appeared to maintain that not only any kind of steel, whatever its composition, but iron itself, when fused and subsequently rolled out, was to be regarded as steel; so that all the essential distinction between the things called malleable or wrought iron and steel was at once abolished. He had always regarded steel as possessing certain physical properties in a marked and sensible degree, of which the chief were hardening, when exposed to a certain degree of heat and to rapid cooling, and tempering. It was a question for engineers to consider, whether they were willing to continue that definition or not; because it must be given up if this definition were accepted. He thought it would be hardly expedient to abandon it, seeing that it had an intelligible meaning, and was generally accepted. A statement was referred to in the Paper which he had made on a former occasion, that iron and steel were to be distinguished by certain proportions of carbon of a very definite character. The numbers were not his own; and he took especial care to state that, in his view, it was impossible to draw anything like a precise line of demarcation between the thing called iron and the thing called steel, passing as they did, by insensible gradations, into one another. The Author, in his remarks with regard to what he called 'piled' iron, appeared to draw a wide distinction between the molecular arrangement of such iron and that which was the result of fusion, whether of steel or iron. Now, taking the purest kind of iron that could be obtained (perhaps the nearest approach to absolutely pure iron was that which was thrown down by the battery), it could be easily melted, and it would be found to possess a highly crystalline structure. If a piece of fused metal were heated and drawn out into a bar, it would be found to break with a distinctly fibrous fracture. A piece of so-called malleable iron, the result of the operations of puddling, rolling, &c., when drawn out into a similar bar, would be found to possess a similar structure. Wherein consisted the difference between the two? The piled bar might contain certain mechanical impurities, so called; but they might exist only to a small extent. In the matter of structure, however, what reason was there for maintaining that there was a difference between the two? Mr. Hackney had failed to advance any sufficient evidence in proof of such difference. He said that the piled bar was made up of little pieces of iron, stuck together in the puddling operation, and which by compression might be made into a solid, unyielding mass; but was it not true also that fused metal was a mass consisting of molecules sticking together? Was it right to say that in one case the metal was steel, and in the other iron? Such a view would give rise to an awkward predicament. Suppose a number of small pieces of the purest fused iron were exposed to a temperature that would make them stick together and were converted by powerful compression into a solid, coherent mass, and then drawn out into a bar, he saw no reason for maintaining that there was any essential difference of structure between such a mass and a piece which had been actually fused. What difference did the actual fusion make in regard to the structure in that case? Though wrought iron was never actually melted in the ordinary mode of treatment, yet it was greatly softened and became viscous, capable, at a high temperature, of arranging itself in definite crystalline forms; even at a lower temperature, if long continued, sufficient freedom of motion was thereby acquired by the particles to enable them to arrange themselves in a definite crystalline structure. It might be said, "It is all very well to talk of making experiments of this kind, taking small pieces of iron and heating them so as to prevent the production of scale; that cannot be done." True; but platinum could be employed. He was aware that platinum was sometimes actually fused; but by far the larger part of the platinum produced in commerce had never been melted. It was made of simple particles of metal sticking together, exposed to a high temperature, then compressed, and mechanically treated so as to form a solid mass. Would it be said that in the case of platinum, which had been fused, one kind of thing was produced —steel platinum; while in the other, which had never been absolutely fused, but yet formed a compact coherent mass, there was ordinary platinum? He saw no grounds for such a definition. He desired to make a few remarks upon so-called foreign matters found in steel. This was a wide field of inquiry. Not long since little or no attention was paid to it. All that was known was that the steel of such a manufacture had such a tensile strain, and the like. What, however, it was desired to know (and this was the strong part of the Paper) was the relation between the composition and the physical properties of the metal. That indeed was a complex problem; but happily it was in the way of being solved. He remembered the time when a chemist was unknown at any of the ironworks, but at the present time competent chemists were employed at all the great works throughout the country; and in this way a great deal of valuable and accurate information had been collected. All that was needed was to make it available for the use of the engineer. He had no doubt that it would in time become possible, by a simple chemical examination, to predict in a great measure what the mechanical or physical properties of the metal might be. There were several elements to be considered; foremost amongst them carbon, then silicon, sulphur, phosphorus, and manganese. Mr. Hackney had not alluded to a point which, some years ago, excited a great deal of attention—the alleged existence of nitrogen in steel. Many papers were written on the subject by able French investigators, some of whom came to the conclusion that no such thing as steel, properly so called, could exist without the presence of nitrogen. That was a subject he thought deserving of further consideration. He gladly availed himself of the present opportunity of commending the whole subject in a special manner to the Institution of Civil Engineers. It was well worthy of their investigation, and it was a topic on which that great man, Smeaton, would have been rejoiced to dwell, judging from the character of the scientific papers he had left behind him. To investigate the matter properly would require considerable time and expense; but it was possible, and he sincerely hoped that the Institution would be disposed to give the subject its most earnest consideration. He had not touched upon the question of the effect of mechanical treatment. How much depended upon that every one knew. There might be two pieces of metal identical in composition, yet differing enormously in mechanical properties. Dr. Pole had assured him that, in certain experiments made by him, not long ago, he produced, by drawing in a particular way, wire which had the enormous tensile strength of 120 tons to the square inch. That, however, had been controverted, and it was to be the subject of immediate inquiry. It showed how much might depend upon the mechanical treatment of a metal, and this was part of the problem which would have to be carefully considered.

Mr. E. Riley could not agree with the conclusions at which Dr. Percy had arrived. He was certainly of opinion that the two kinds of metal referred to were essentially different. If wrought iron were melted so as to get rid of the carbon, a crystalline mass was obtained somewhat resembling antimony, but when it was worked and hammered at a low temperature it was like copper, and had a silky, fibrous fracture; but after welding it was red-short and useless. Some years ago he tried many experiments upon the subject. So long since as 1857, he had melted wrought iron in clay crucibles, and he found that metallic manganese perfectly restored the metal to its original properties. Dr. Percy had stated that if the oxidation of iron could be prevented, so as to keep cinder from intervening between the particles, perfect adhesion would be obtained, and he had cited the instance of platinum, which he thought was an unfortunate example. Up to the time of Deville's experiments, platinum was simply welded by pressure. In that case a metal was dealt with that was not oxidisable. It should be perfectly welded, and form a solid substance. If welded platinum were used for ordinary purposes it retained its form well, but when exposed to a great heat it always blistered and became porous, so that it was nearly useless. Melted platinum used in the same way would stand great heat, and never became porous or blistered. For seven or eight years he had never used any other platinum crucibles than those that had been melted. He believed there was an essential difference between iron that had been melted and iron that had been welded. He agreed that in the case of the best Yorkshire iron, great care and skill being employed, the iron was practically made to stick together so perfectly as to form almost a perfect weld, as would be seen in the case of a broken tire. He concurred in the remarks of Dr. Percy with regard to the mechanical properties of steel and iron. No doubt good iron might be spoiled mechanically, and bad iron by good mechanical treatment might be made tolerably serviceable. Unquestionably the chemistry of steel had lately received much attention, but more accurate and definite information was yet needed as to the chemical composition and the tensile strength of steel, always taking into consideration the mechanical treatment to which it had been subjected. The relation between silicon and carbon was a subject of the greatest interest, and deserved careful consideration. As silicon increased in pig iron, so carbon had a tendency to decrease.

Mr. Lowthian Bell said an allusion had been made to himself in reference to the amount of heat absorbed in the reduction of iron, and the quantity of heat given off by the combustion of the quantity of carbon required to effect that reduction. He had stated on a former occasion that his own experiments, and the experiments of others, led him to believe that the quantity of heat evolved by the carbon in its combustion under the conditions in question, and the amount of heat absorbed by the oxide of iron to be reduced, were, as nearly as possible, on a par. Mr. Hackney maintained that this was an error, and cited in proof of that view the results obtained in the so-called

Blair process. He was fully aware that these determinations involved a great amount of difficulty and delicacy of operation, and he was prepared to believe that neither his own experiments, nor those of chemists generally, were absolutely correct, but he thought he could show good reason for believing that Mr. Hackney and Mr. Blair must have fallen into an error. The reduction of iron in a blast furnace, the subject to which his investigations had been confined, was effected by carbonic oxide passing into the state of carbonic acid, and the reduction of iron in Blair's process was effected by the conversion of carbon into carbonic oxide. The quantity of heat given off by the amount of carbon necessary for reducing a unit of iron, in the case of the burning of carbon to carbonic oxide, was 800 centigrade heat units; whereas the quantity required for the reduction of a unit of iron, by its equivalent of carbon, was 1,800 units; so that, if Mr. Hackney's statement were correct, he and every other chemist and investigator must have made a mistake to the extent of at least one-half, which was scarcely within the limits of ordinary experimental error. But this opinion was grounded upon the supposed fact that Mr. Blair carried on his process without external heat at all. He gave what he called an initial heating, and that heating once conferred upon the mass under treatment, was said to be sufficient to cause the action to go on *per se.* He was not aware that that was so; but, were it so, the observation of Mr. Hackney was not, he thought, based on true philosophical principles, because he had not determined the quantity of heat so conferred upon the mass; and, moreover (which would be still more convincing), that was not the way in which Mr. Blair, to the best of his belief, carried on the process. He had visited his works within the last few months, and had seen the process in operation, or at least enough of it to convince him that the statement was not correct. If no heat were used, from the combustion of coal, other than that obtained in the process itself, still he would be in error, because the whole of the carbonic oxide generated in the reduction of the oxide of iron, itself, was used for heating the apparatus. But in addition to that, there was constantly kept burning round the annular retort a quantity of carbonic oxide, obtained from coke in a state of combustion, so that there was no doubt that the operation, instead of being carried on as seemed to be supposed, was aided by a notable expenditure of fuel.

Mr. Alex. Wilson remarked that he should be glad to know where a Bessemer blow had been completed in five or six minutes. It appeared to him to be quite impossible. A blow usually lasted from thirteen to sixteen or twenty minutes. Mr. Hackney had not dwelt upon the annealing of steel, one of the most important points in connection with its manufacture, whether the steel was in the shape of castings, forgings, or plates. But, singular to say, the benefits to be derived from a perfect system of annealing were not sufficiently appreciated by many manufacturers—at all events, if recognised, they were not practised—and if they only had been practised hitherto he felt sure that the uncertainty in the minds of engineers respecting the use of steel plates, for example, would never have occurred. Not a single steel plate should under any circumstances be allowed to leave the maker's works until it had been thoroughly annealed. In a plate mill properly laid out for the manufacture of steel plates there should be three or four annealing furnaces, each sufficiently large to contain the produce of one day's rolling, and as these furnaces were successively filled, each one, after being fired up, should be allowed to cool slowly, and be only opened when the contents were cold. Annealing must be carried on with as much systematic care as any other part of the manufacture. It was a remarkable thing that the crop ends of steel rails were sent to Sheffield and Birmingham from all parts of the country, and, after being rolled into sheets and subjected to annealing, they were stamped in dies into dish covers, into what were called 'shapes,' with fastastic designs upon them, and into hundreds of other intricate forms that must be seen to be appreciated. But all this could not possibly be done without annealing. Then, if steel could bear all this extraordinary punishment without fracture, how was it that when the steel plate arrived at the boiler-makers, or at the dockyard, it should immediately bear so bad a name? Depend upon it, annealing had a great deal to do with the question. He looked forward to the time when armour plates should be made out of steel, or, as it had been more properly called, ingot metal, and not only made but used for defensive and offensive purposes. There was another thing he had not lost sight of, and that was the great care necessary in the selection of the raw materials in order to insure a good quality of steel. He believed no process was better capable of making good steel for general purposes than the Bessemer, but the workmen must possess an intimate knowledge of the raw materials. He had known men employed in steel-works who, if asked what silicon was, would have been puzzled to give a reply. He should like to see some institute formed in the steel districts where the working foreman and man could readily obtain an insight into practical chemistry. The injunctions of the analytical chemist on manufacturers were useless unless the foremen and workmen could be made to understand the process thoroughly, and thereby put into practice the theory established in the laboratory. Large works possessed the means already of giving their workmen such a chemical education, but the all-absorbing desire to make money at any cost rendered the attempt futile. The saving alone, to the country, in fuel, would be immense, if the workmen better understood its employment. Mr. Hackney had referred to an experiment with a steel rail, to show the superiority of drilled fish-plate holes over punched holes. He had also tried the same experiment, with totally different results. He believed punching was not detrimental to a steel rail made out of good Bessemer material. The system of inspection steel rails were subjected to was, he considered, a very fair one, and if engineers would adhere to the falling-weight test they could not get wrong. He did not think a safe conclusion could be arrived at by the proposal to be guided by the pressure registered under the punching machine, as the rails might be hard through an excess of silicon or phosphorus, as well as carbon.

Dr. Percy, with reference to the remarks of Mr. Riley, said there were such things as bad platinum crucibles and good ones. He had known platinum crucibles, made from unmelted metal, used for a long time, and often heated to redness, without blistering. A great deal, of course, depended upon the mode of manufacture. He should like to have it clearly proved that all the crucibles which Mr. Riley found so good were made of platinum that had been melted.

Mr. Riley said his observations had been made since the use of Bunsen's burner, and of much stronger flames than were formerly employed. He repeated, that in his experience the only crucibles that had stood were those made of melted platinum.

Mr. Walker observed that twelve or fourteen years ago he had erected some special machinery with the object of turning and finishing locomotive crank-axles, which exhibited the qualities of steel and iron as well as anything he knew. One of the best Bessemer works had sent his firm a crank-axle to be finished. It was so hard that it could not be turned, and it was accordingly sent back. Another was sent that was not so hard, but it was full of blown holes. A third was sent that was much better. The improvement in Bessemer steel had been very gradual. Twenty or thirty crank-axles of Bessemer steel could now be turned without a single waster, and often without a single speck or flaw. Some of the qualities of Bessemer steel were truly wonderful. Six or seven years ago his firm had had a large crank-shaft to finish, on which a heavy boss had been forged for the purpose of making a crank-pin for driving a shear. It was 2 or 3 inches too long, and it had been turned down in a lathe to have the extra length taken off. It had offered the most determined resistance to being broken off, and it had to be actually cut off. When the crank-pin was turned down, a large blown hole was found in the very centre. Of course it was a waster, and had to be returned. Another had been made, which was perfectly sound, but the steel had not half the strength of the former one. He looked with great interest and confidence on the process invented by Sir Joseph Whitworth, which would retain the strength of the first, without the hole in it. A steam hammer piston-rod also showed in a remarkable manner the power to resist impact. Seven years ago, a firm in the Middlesborough district had applied to his firm for a steam hammer, and they had made one. If one thing was needed more than another it was a piston-rod that would not break. Several makers had been suggested; and ultimately it had been made at Barrow. It had been working eight years without an accident. Some rods, however, from Barrow had broken within a month. Another rod, of iron, made by Taylor Brothers, Leeds, put to work at the same time was at work still. If asked whether he would recommend iron or steel, he would say that he could not tell. If it was made of good iron it would do very well, and also if made of good steel. Steel, however, was more uncertain than iron. A steel rod might last a long time and give great satisfaction, or it might give way the first week. One of the most remarkable parts of Mr. Hackney's Paper was the statement that fifty blows could be got in twenty-four hours out of two converters. He did not see how this was possible. But if it were accomplished with two converters instead of, as usual, with four, the only saving was in the cost of the two converters, about £1,500. The same blowing power would be required, the same cupolas, the same ingot moulds, and the same labour. It would seem, from the Paper, as if by changing the lower part of the inside lining of the vessel, these results had been obtained. That was altogether a mistake. The ordinary mode adopted was not that shown in the drawing. The Author appeared not to know that the interior part of the vessel would sometimes last six months, or even twelve; so that the mere power of changing that part could not be of such enormous advantage, in increasing the number of blows obtained out of a pair of converters. It was stated that large rolls were of great advantage, in preventing the cracking of the surface of the ingot, but his experience was that small rolls were better than large ones.

Large rolls required great power, and would not penetrate the iron as readily as the small ones. He had seen Mr. T. Vickers' tire-cogging mill reduce a crucible steel ingot from a thickness of 15 inches to a thickness of 3 inches in not more than seventy-five seconds, the inside roll being only 7 inches in diameter. After rolling, the tire was perfectly free from cracks on the surface. An example was given of a cogging mill with rolls 30 inches in diameter, running from forty to forty-five revolutions a minute. It was also stated that it was difficult to prevent the mill from tearing the steel. No doubt that was so. The speed was too great, and it would be a matter of astonishment if it did not tear open the steel. The Author had stated that the 2-high mill was being abandoned, but in the most successful works for turning out steel mills of that character were used. If he were now constructing a rail-finishing mill he would make rolls 2-high, running from eighty or ninety revolutions per minute. It was stated that at Landore the mill was reversed at thirty-five revolutions per minute. In reversing the mill, the steel was drawn first one way and then another, a process which was very objectionable. In dealing with a short piece, it was not easy to contrive a machine to roll all one way. Mr. Smith's 3-high mill, or the common 2-high, reversed by a clawed clutch, and both mills fitted with Mr. Menelaus' contrivance for putting ingots into rolls, was in his opinion the best. It had been stated that the common mill was expensive and liable to break, but he knew a mill that had been working ten years without having cost 5*s.* for repairs. There was one other point to which he desired to direct special attention. He had found by experience that the power required to roll varied as the cube of the speed of the surface of the rolls. All the laws applying to a vessel moving through water applied to the rolling of iron, and many difficulties would be solved by regarding it as a fluid. This would explain how the ingot was round at the end, and how it came to crack—from the roll being too large, and the speed too great. If the roll were smaller, and travelled at a less speed, so as to allow the particles to arrange themselves, that result would not occur. Mention had been made of Lauth's plate mill, as taking the place of the common reversing mill, but the Author seemed to have forgotten the small roll, which was one of the best features in this mill, in his remarks about large rolls. The small roll worked more easily and penetrated the plate better.

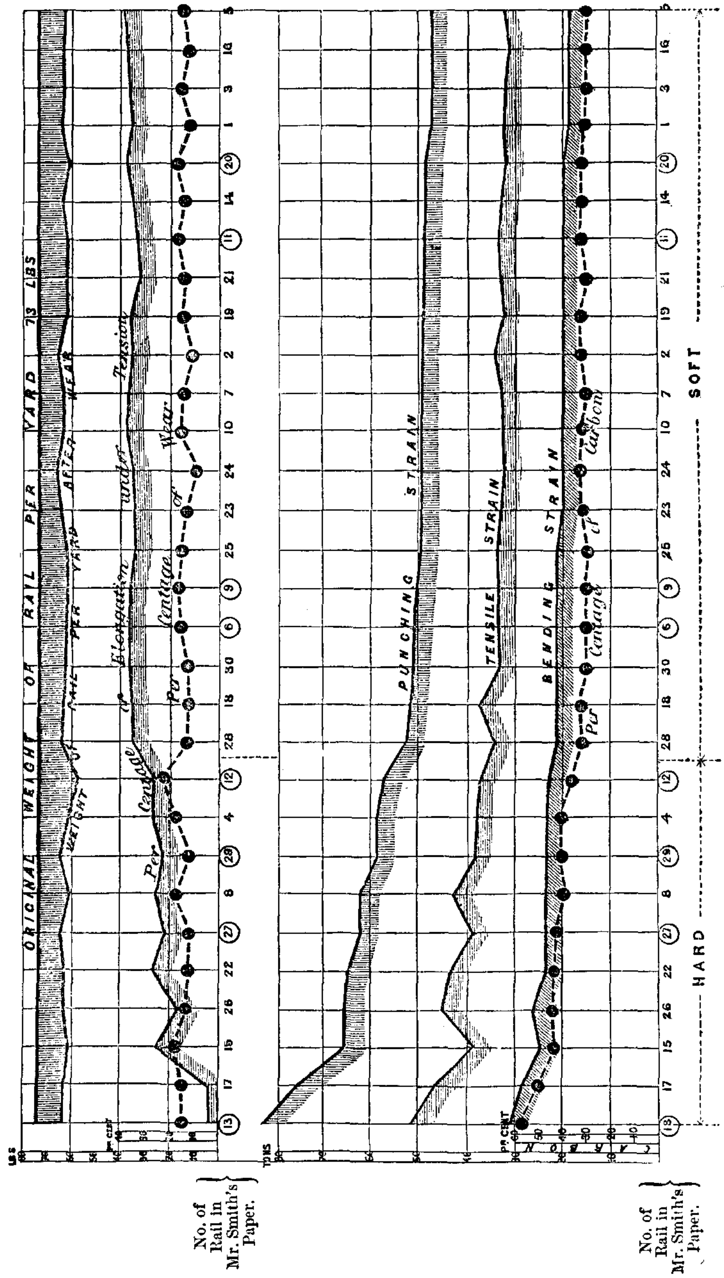
With regard to the testing of rails, he had come to the conclusion, after careful examination, that in shearing with an ordinary square-edged shear, it just required the power to shear a bar of steel, that it took to pull it asunder. This was borne out by Mr. Smith's experiments, although he could not quite see that that test would be conclusive. He had no doubt it would be so on one point, as to the tensile strength of the rail. In from twelve or fourteen years' experience, he had always found Government officials willing to entertain new contrivances, new machinery, and new material; and he was sure there would be no difficulty in convincing the present heads of the War Office or the Admiralty, if a better material could be offered at a lower price. He had been much pleased with the remarks of Dr. Percy, with regard to iron and steel, a point which he thought Mr. Hackney had not well considered, when he had spoken of iron being simply granules or particles sticking together. He had been using steel and iron many years, and he had as much confidence in iron, in the plate form, as he had in steel. He believed, however, that there was a great future for steel. If it was compressed, in its liquid form, the air bubbles which existed in the ingot, and which in rolling out were also found in the plate, would be removed. Mr. Bessemer and Dr. Siemens had given to the world a new material, which was very readily worked into many shapes: engineers should make the best use of it, but they ought not to discard an old friend, like iron, upon which they had so long depended.

Mr. Edward Williams was sorry to differ from Mr. Walker, who was a very skilful designer and maker of machinery for rolling iron or steel, but he thought that the opinions he had expressed with regard to rolls and rolling were the rankest heresy. Instead of small rolls being better than large ones, the very opposite was the fact. It was notorious that as the rolls were turned down in an ordinary iron mill, (and the same thing would apply to a steel mill), less draught had to be put upon the grooves, or they would not bite and take the iron. The larger the roll, the more easily it would take in a piece of iron, and reduce it a certain amount. With regard to the statement that a 2-high pull-over mill was the best for finishing rails, he should like to see rails rolled in three lengths, as he had seen the other day at Barrow, with a 2-high pull-over mill. But to do so was, he believed, impossible. In the case of single-length rails, he was inclined to agree with Mr. Walker; but single-length rails meant a heavy percentage of crop ends. Given a rail of 20 feet in length, it was not easy to work with less than about a yard of crop, and about the same quantity of crop would do for two 20-feet rails. He had seen, at Barrow, rails 24 feet long, rolled in three lengths, and there had not been more than 4 feet of crop in all. It was obvious that there was a great saving by rolling rails in more than one length, which was, in his opinion, impossible with a 2-high pull-over mill. He was inclined to think that the best kind of rolling mill for blooming steel ingots would be obtained by arranging two 3-high mills, one in front of the other, and both running in the same direction, so that at each passage the bloom would go through two grooves, one after the other; and this he proposed to erect at some new works he had now on hand. He believed that the old-fashioned clutch was the best kind of reversing implement. He hardly ventured to say a word about the chemistry of the matter, but all were undoubtedly under great obligations to chemists. Twenty-five years ago, the chemist at an ironworks had been thought to be a harmless young gentleman, pursuing an innocent amusement, but probably doing very little good. Now the state of things was altered, and iron-making, as well as steel-making, had become almost entirely a chemical question. He was disappointed to hear authorities talking of good iron and bad iron. He did not think that there was any such thing as good or bad iron, but that, given a certain chemical composition, and certain mechanical treatment, there must be positive results. He had tried some of the fused wrought iron spoken of by Mr. Riley, and more unworkable stuff he had never met with. One good point in Mr. Hackney's Paper was his definition of steel, a thing which, being cast into an ingot, was malleable afterwards. So far as he was aware, fused wrought iron was not so malleable afterwards. Mr. Hackney had alluded to the alleged weldability of steel having not more than 1½ per cent. of carbon. That, he believed, was a quality which it would puzzle him to find.

Mr. R. Price Williams, having at one time been engaged in the manufacture of steel rails, would offer a few remarks on that part of the subject. Mr. Hackney had stated in his Paper that there was considerable diversity of opinion and practice in regard to the question whether it was best to hammer the ingot before rolling it, or to cog it down and roll it, direct from the ingot. He had been under the impression that, with the light of present experience, the supposed advantage of hammering the ingot had been shown not to exist, and that this diversity of opinion had practically ceased. His own experience was, that so far from hammering being an advantage, its effects were detrimental. He could imagine that if Mr. Walker's views obtained, as to the supposed advantages of only using rolls of small diameter, the rough and severe treatment to which the ingots would be exposed, in passing through the roll drafts, would result in producing those serrated edges to the flanges which, at the outset of steel rail-making, had proved so hard to overcome. He thought, however, there could be no question that with rolls of large diameter, such as Mr. Edward Williams had referred to, suitably arranged as to the draughts, the largest rail ingot might be cogged down and rolled into the finished rail without difficulty. There was this great advantage in rolling the rail direct from the ingot, that the fibre of the steel was better drawn out. As to the distressing effects upon the steel arising, it was said, from passing the ingots the reverse way through the rolls, he entirely disagreed; on the contrary, he considered that it was only by this reverse process that flaws and defects in the ingot could be detected, the tendency being, when the ingot was only passed one way through the rolls, to smooth over these flaws, so that material defects in the structure might escape observation. He had carefully examined the table of the results of experiments on the wear of Bessemer steel rails on the Furness railway, but his conclusions were at variance with those drawn by the Author from the experiments; the results being, according to the Author's views, unexpected and contrary to what might have been anticipated; greater hardness not having conduced to the longevity of the rails, and the softer rails showing a minimum of wear. His experience was that a hard steel rail was the more durable. He thought the somewhat uncertain results, to which Mr. Smith had drawn attention, were not so much due to an overdose of carbon, as to the varying proportions of silicon, and possibly manganese, of which no account was taken. It would not fail to be noticed in the diagram, (Fig. 2, page 90), showing graphically the results given in Mr. Smith's table (page 74), that there was, as had been pointed out, an evident relation between the punching, tensile, and bending strains, and the percentage of carbon in the rail, but as opposed to this view that the soft rail showed the minimum wear, it would not fail to be noticed that, in the instance of the greatest amount of wear amongst the hard rails (No. 12), the percentage of carbon was only 0·36, which was a little above the maximum percentage of carbon in the soft rails, viz., 0·32. Again, in the case of rail No. 27, where the percentage of carbon was as high as 0·43, the amount of wear was only 12 per cent., while in the hard rail (No. 13), which had the largest amount of carbon, viz., 0·57 per cent., the percentage of wear was only 14·47. In the case of the soft rails (No. 11), with a percentage of only 0·30 of carbon, the wear was nearly as high as 16·24 per cent., while in another soft rail (No. 20), with the lowest proportion of carbon, 0·29, the percentage of wear was very high, viz.,

FIG. 2.—STEEL RAILS.

Diagram showing the Relation of the Punching, Tensile, and Bending Strains to the Percentage of Carbon.



15·72. In two other soft rails, Nos. 6 and 9, with a percentage of only 0·30 of carbon, the wear was also very high, being 15·93 per cent. in the one case, and 15·60 in the other. He therefore thought that the conclusion arrived at by Mr. Smith, as to the large amount of wear being due to the excess of carbon in the steel, was rather a hasty one. He was satisfied that a great deal more assistance was wanted from chemists, particularly as to the effect of manganese and silicon, for he did not think the carbon alone was the cause of extra wear.

Dr. SIEMENS regarded Mr. Hackney's Paper as so comprehensive that it offered room for observations on many different branches of the subject. His remarks would be confined to the two first portions into which the subject was divided, namely, the chemical and the manufacturing. The Author had, perhaps, dwelt too much upon the question of name, and had proposed to change the prevailing denominations of mild steel, hard steel, spring steel, and so forth, and to call "steel" all malleable iron which had passed through the fluid condition. This definition would exclude, however, natural, blister, and puddled steel, and was therefore in his opinion inadmissible. On the other hand, Dr. Siemens considered it would be difficult, if not impossible, to define steel by its mechanical qualities. Steel was almost the hardest substance in nature, if treated in a certain way; treated in another way it was the most elastic of metals, if not the most elastic substance in nature; and treated in another way it was nearly the most ductile of metals. It was decidedly the strongest substance in nature. Dr. Percy had said he considered steel a metal in which iron was the chief constituent, and which would harden. If that definition were adopted, a limit would be arrived at where it would be uncertain whether a substance was steel or iron. One rail delivered by a manufacturer would come under the definition of steel, and another rail delivered in the same contract would come under the definition of iron, simply because one had rather less carbon than the other; and after all, the mildest steel, and iron itself, would admit of tempering. Dr. Percy had said, that molten iron, according to Mr. Hackney's definition, ought to be called steel; but it had been already remarked that such iron could not be treated mechanically; it would not stand rolling and hammering, and therefore it was not steel. He thought, however, a definition might be found which would answer all requirements, namely, that steel was a compound of iron with any other substance which tended to give it superior strength. This definition would embrace the different kinds of steel, from the hardest tool steel down to the mildest, and would embrace those compounds in which manganese, tungsten, chromium, phosphorus, or sulphur, replaced the carbon of ordinary steel. It had been objected that steel was uncertain in its nature; that it sometimes was very ductile and very strong, and at other times would break short and was not to be trusted. Mr. Walker had said that, on that account, he preferred iron to steel for structural purposes. Steel was a material of a much higher nature than iron. It was much stronger, and could be made to possess nearly any degree of strength, hardness, and ductility, between wide limits, that it was desired to give it; but it was only natural that such a material should require greater care than iron. If extreme toughness was required, care must be taken that the material was not put partially through a process of hardening or chilling, and that its chemical constitution was uniform. The higher material required a higher intelligence to deal with it, and it was not sufficient that the chemist, or the manufacturer, should possess a knowledge of its characters under different treatments. The engineer, also, had to be thoroughly conversant with its properties, and in giving his orders for a supply of the material should make out a proper specification, defining its chemical constitution and its temper. Sir Joseph Whitworth had proposed a method of defining steel according to its two principal qualities, toughness and strength; he had one number signifying strength, and another toughness, and combining these two expressed the quality of the steel. That was a rule which furnished a fair standard of comparison, and might be worked out with great advantage. He had repeatedly heard it stated, that mild steel, such as was used for engineering purposes, ought to be rejected if it had an absolute strength exceeding 27 or 30 tons. Such a limit was, he thought, objectionable because it stopped the road to improvement. He considered it quite possible that steel might be extremely tough, and at the same time possess very great tensile strength. As an instance of this he would mention wire, which had undergone the process of annealing in oil at certain temperature. He had lately had occasion to experiment on some steel containing 0·4 per cent. of carbon, which, if properly annealed, was extremely strong and yet sufficiently ductile; if hardened, it would take such a temper that it could be put into the form of a punch; and if drawn into wire, and annealed by passing it at a certain temperature through oil, its strength rose from 35 tons per square inch to about 100 tons per square inch. That was an instance of one material presenting itself under very different aspects, in consequence of slightly different modes of treatment.

It was necessary that the engineer, in dealing with steel, should inquire into these different conditions, and should insist upon the utmost care being taken in order to bring the material really into the condition which he required. Cast steel was produced in the Bessemer converter, or in the open hearth of a regenerative gas furnace, in masses of from 5 to 10 tons; and absolute uniformity could be obtained if all conditions were properly carried out. Mr. Hackney, after paying the high compliment which was due to Mr. Bessemer for the introduction of his process, had referred to one process with which Dr. Siemens' name had been associated. That process might perhaps be appropriately called the chemical process, or the cooking process, to use a more vulgar expression; because, given a hot receptacle, such materials were put into that receptacle as would, when melted together, produce steel of the quality required. The beginning of the operation must be a fluid bath, and the material natural for such purpose was pig metal. To this pig metal, when heated to a temperature exceeding 2,000° centigrade, either scrap iron, scrap steel, puddled blooms, or iron sponge might be added, and thus a bath might be obtained, which would gradually come to contain less and less carbon, until it was reduced to the condition of fluid iron. It then received such additions as would give it the necessary manganese, carbon, or other substances, to make steel of the required quality. This process had the advantage that samples could be taken out, from time to time, in order to make sure that the operation was going on properly, and that the chemical and physical qualities of the material were such as might be desired. The essential condition for carrying out such a process was a very high temperature; it was perhaps the highest temperature used in the arts, excepting only the fusion of platinum. Considerable difficulty had existed, at first, in obtaining a vessel to withstand such heat, continuously, and the only substance capable of doing so was silica, containing only from 1 to 2 per cent. of binding material. He noticed diagrams of the furnaces in which the processes were carried out; and one diagram of a modified form of the furnace, by M. Pernot, which was certainly ingenious. The bath was circular, and was rotated on an inclined axis; and the pig metal flowing round and round in that vessel would wash over any solid substance put into it. He however doubted very much whether such a vessel would resist the high temperature which was necessary for the production of mild steel by that process. He understood it was used at St. Etienne, in France, where engineers were content to have steel rails containing 0·8 per cent. of carbon, which melted at a much lower temperature than the mild steel required in this country. Nor would it probably work out altogether satisfactorily for the process of producing steel from pig metal and ore; because, in that process, considerable ebullition ensued, and he should be afraid that the boiling mass would go between the rotating lip of the vessel and the standing part of the furnace, and cause it to stop. Mr. Hackney had alluded to experiments carried on in America by Mr. Blair, who appeared to have found that, if the ore and carbon were heated to a sufficient temperature, and left to themselves, the heat retained in the mass would be sufficient to complete the reduction of the ore. Dr. Siemens maintained that that was an entire fallacy. Although the ore might be hot enough for the reaction to commence, a very large influx of heat would be required, during the operation, to make the whole of the oxygen contained in it combine with carbon. Mr. Lowthian Bell had already proved the fallacy of the assumption that ore, under such circumstances, would combine with carbon and form itself into spongy metal. He had himself paid considerable attention to the reduction of ore, in order to obtain material for the bath, and he had experienced great difficulty in reducing ore into spongy metal. The iron, in that spongy condition, took up sulphur very readily, and sulphur vitiated the steel produced from it. These failures and difficulties induced him to resort to another mode of obtaining metallic iron from the ore, by passing the ore, in a heated condition, through a rotating chamber, and there treating it with reducing material, not at a temperature to produce sponge, but at a melting temperature at which cohesive wrought metal was at once produced. That part of the process was as yet in an experimental stage, and he could not at present reply to Mr. Williams's inquiries regarding it; but before long it would be at work on a complete scale, and he would then be in a better position to speak definitely regarding it. The chemical process of making steel might be varied almost *ad libitum*, and that variety was undoubtedly the best which, from impure ore or impure pig metal, would produce material of comparatively high quality. Hitherto phosphorus and sulphur had been regarded as the enemies of steel, but recent experience had shown that they might be innocent constituents under certain conditions. If phosphorus was contained in the metal used for steel-making, it was necessary that the metal should contain very little carbon; in fact, the carbon should be reduced to a mere trace; and manganese should be introduced in order to produce a malleable metal. This was being carried out very successfully in France and in this country, and the results were very satisfactory. Years ago the use of rich ferro-manganese had been found to afford great assistance, but manufacturers had not succeeded in producing a soft and tough metal containing so large a proportion of phosphorus as had been produced latterly, more particularly at Terrenoire, in France, in using the Siemens-Martin process. He had very few observations to offer with regard to the working of steel. In solidifying from the melted state it had a natural tendency to become honeycombed, either through the generation of gases, or through mere contraction of the semi-fluid mass, and it was necessary to close these cavities as thoroughly as possible, because steel, unlike iron, did not weld; and if a hollow space existed in an ingot, that hollow space would remain a break of continuity, no matter through what process it was put afterwards. Probably hammering was the most efficacious of those contrivances which had hitherto been adopted for closing those bubbles; but Sir Joseph Whitworth had elaborated a plan for remedying the evil at the proper moment. He compressed the steel while it was still in a fluid or semi-fluid condition; Dr. Siemens expected great results from that system. He was not prepared to say whether it would ever supersede hammering and rolling, for ordinary rail-making, but for such purposes as gun-making, he considered that Sir Joseph Whitworth's plan commended itself as one of great importance.

Sir John Hawkshaw, Past-President, wished to add his testimony to the value of the Papers which had been brought before the Institution. There could be no doubt that civil engineers had yet a great deal to learn with regard to steel, with which they dealt so largely. The subject might be divided into two parts—the chemical and the mechanical; and it was the former on which engineers had been hitherto least informed. The question was a most complex one, and it would be very much simplified if Dr. Siemens, or some other gentleman who had devoted himself to this subject, would begin by getting pure iron; but he believed that that was a very difficult thing to do. Pure iron would afford a basis on which to act. He had spoken to Dr. Percy on this matter, and had been told by him that such a thing as pure iron had never yet been seen. All he could say then was, that it would be a great pity if some gentleman did not set to work to make it. His object in rising was, however, chiefly to draw attention to a matter which was rather incidental to the Paper, but which it appeared to him should be mentioned on this occasion. For a long time he had been anxious to apply steel to railway structures. In the construction of the Charing Cross bridge it would have been of very great importance to him to use steel in some portions of the bridge, but he could not do so unless he could induce the gentlemen who were appointed by the Board of Trade to pass railway bridges to say what strength they would allow for steel, after the bridge was built. He had seen the officers of the Board of Trade on the subject, and they had said they did not know enough of steel to fix any co-efficient of strength. His answer had been, that if they did not, it was time they endeavoured to acquire that information. They had entirely agreed with him on that point, and he had then suggested that the Board of Trade should appoint a Committee to consider the subject, and arrive at some decision which would enable them to say to engineers, "If you use steel in railway structures, we will allow you 7 tons to the square inch, or 8 tons to the square inch," or any other figure which they might decide upon. He had written to the Board of Trade, but nothing had come of it. On a subsequent occasion he had had other works, where it would have been of great advantage to use steel. He had again applied to the Board of Trade, but with a similar result. He could get no answer, and he had been obliged to abandon the idea of using steel. Since that time a Committee had been appointed by the British Association, of which Mr. Bramwell, Mr. Barlow and himself were members, and they had endeavoured to induce the Government to come to some conclusion on the subject. Considering that this country made most of the steel that was used in Europe, and had more railway structures than any other country, and Englishmen were not regarded as the most ignorant people in Europe, it might have been supposed to be possible to arrive at some definite conclusion on this point, but hitherto they had failed to do so. This was all the more remarkable, because in Holland, which was a small country, and whose inhabitants were said to be slow, sufficient information had been obtained to enable the Government officers, there, to assign a limit of strength to steel, and it so happened that that limit was nearly the limit that he had asked for. He was sorry there were no officers of the Board of Trade present to hear his remarks; for it was a subject of great regret that the Government could not be induced to turn their attention to this question, so that English engineers might be able to apply steel in railway structures in this country, which would in many cases be a great advantage.

Mr. Bramwell said Sir John Hawkshaw had omitted one circumstance, in connection with the action of the Committee to which he had alluded. They had written a letter to the Board of Trade, and had received an answer, that from the experiments they had carried out, the Board of Trade were inclined to believe that steel was weaker than iron. The Committee had replied that that was contrary to their experience, and that they would be glad to have particulars of the experiments. The answer received had been that the experiments were made somewhere about 1862, and made with puddled steel that had never been in fusion. Upon those experiments they were disinclined to allow the use of steel in railway structures.

Mr. BARLOW, Vice-President, most thoroughly agreed with the remarks of Sir John Hawkshaw as to the great value of the Papers; and it was because of their extreme value that he wished to draw attention to one point, which otherwise might lead to some misapprehension, namely, the brittleness of steel. Mr. Hackney had seemed to imply that steel was a much stronger material than iron, but that the extra strength was acquired at the cost of its toughness, and steel was necessarily brittle. He had been a member of the Committee that had made the experiments on steel, and those experiments had shown that a very large amount of steel was very strong and very ductile. Steel of a tensile strength less than 34 tons to the inch appeared to have a ductility quite as great as that of ordinary iron. Mr. Hackney had referred to falling weights, and had stated that in some cases the cutting out of a small notch in the side of a rail diminished its strength as much as 97 per cent. He had quoted Mr. Bramwell, not exactly as an authority for that deduction, but for a matter for which he did not think him altogether accountable. He was stated to have said that the strength of a steel rail was diminished to a much greater extent than was that of an iron rail by cutting a notch in its side.[1] He well remembered Mr. Bramwell referring to the weakening effect of cutting a notch in metal, but, so far as his recollection served, that applied not only to steel, but to iron. The reduction of the strength 95 or 97 per cent., by cutting a notch, was not stated by Mr. Hackney upon his own observations, but upon experiments made by Sandberg, who had obtained his results by comparing different weights falling different heights. He had taken the weight, multiplied it into the fall, and then compared the numbers he so obtained; but that was no true comparison. The blow which a rail received from a falling weight was due to the velocity with which it was struck by that weight; and Sandberg had taken the actual height instead of the square root of the height. This made a most extraordinary difference

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxxii., p. 205.

when worked out in a practical form. For instance, the weight on the driving-wheel of some heavy engines was about 8 tons; and the road must be very much out of order if that had a fall exceeding $\frac{3}{4}$ inch. Assuming there was such a fall, the 8 tons would have acquired a velocity of 2 feet. The fall of a ram of 1 ton, which would cause an equivalent blow, would be 4 feet, whereas, if Sandberg's view were adopted, it would be only 6 inches. The test of a falling weight was of no value at all, unless account were taken of how much the rail overhung the bearings. He had tested rails, 22 feet long, with a 3-feet 6-inch bearing, and he had found that, while a good many of them had borne the test of a weight falling in the middle, some had broken in the middle, some over one bearing, and some over both bearings at the same time. While that had been evidence, perhaps, of a bad rail, it was evidence also that the strain over the bearing must be something like the strain where the blow fell; and, according to Mr. Sandberg's mode of calculation, the result would be something most extraordinary, although there was no hole or nick in the rail. An experiment made at the Landore Works, as to the effect of punching holes, had been mentioned, where a rail without a hole in it had borne a weight of 1 ton falling 20 feet; yet when it had a hole bored in it, the same rail had broken at 4 feet. He could not, however, conceive that that properly represented the relative strength. The experiment had been made some time ago, when the metal was not so good as that which was now produced. He had tested steel rails not only in the body of the rail, but by fishing the joint and letting the weight fall on the joint, and he had never yet seen a break through the bolt-hole. He had inquired where the rails on the Midland railway generally broke, and he had found that they very rarely broke in the bolt-holes, but, in the majority of cases, in the middle. He was therefore afraid that that point was very much overrated; and, unless it was put right, it might afford the Board of Trade a reason for asserting that this material was not reliable.

He would next refer to Mr. Smith's mode of testing rails. When he had had the honour of presiding over the Mechanical Section of the British Association, at Bradford, he had directed attention to the mode of testing rails that was in use, by testing to destruction two out of one hundred rails, and assuming that the other ninety-eight were like the two that were destroyed. Certainly that was a most fallacious test. The mode suggested was exceedingly simple, most ingenious, and, he believed, most effective, because it indicated the real state of each rail. If rails grew, like apples on a tree, then, if two or three were tested, it might well be assumed that the rest were like them; but each rail was made from a separate bloom. Mr. Smith's process was to ascertain, by the simple force required to punch the rail, what the condition of the material in that rail was; and his remarks as to the general coincidence of any one resistance being carried through all the resistances, in steel, quite agreed with the results of the experiments that had been carried out. He regarded that as a very practical suggestion, and should certainly use it at the first opportunity he had for testing steel rails.

Dr. Pole, with regard to the proposed new definition of steel, said it was a difficult thing to alter an established definition; and it should not be forgotten that there was already a perfectly well-established distinction between iron and steel. For a definition of steel, reference might be made to the authority of chemists, of metallurgists, of general writers, and of engineers and workers in the arts. Taking first a chemical authority: in Miller's "Inorganic Chemistry," the following passage occurred: "The most characteristic property of steel is that of becoming almost as hard as a diamond when heated to redness and then suddenly cooled by plunging it in water or oil." Taking next a metallurgist: Dr. Percy, after describing the combinations which iron might form with different materials, said, "There are various kinds of steel;" and then, referring to them all, he described steel as "a metal which is capable of receiving very different degrees of hardness by tempering, even so as to cut wrought iron with facility." Then in a general work, Brande's "Dictionary of Science," he read, "The peculiarity of steel, upon which its high value in the arts depends, is its property of becoming, if suddenly quenched in water when at a bright red heat, extremely hard, and of being softened again." Then Professor Rankine's "Civil Engineering," which would be regarded by every one present as an engineering authority, said, "Steel is distinguished by its property of tempering; that is, it can be hardened by sudden cooling from a high temperature, and softened by gradual cooling," and so on. Then to pass on from writers to practical men, workers in the arts, if an inquirer went into any workshop in the world, and took a piece of ferruginous metal, and asked a workman to say if it was steel or iron; he would heat it, and put it into water, and if it hardened he would say it was steel, and if it did not, he would say it was iron. Now was it possible or advisable to alter that definition in the face of its universal acceptation? He did not think it was. He was reminded that steel rails would not harden, but he replied that they were only called steel conventionally, and, as he urged, incorrectly. It was said that there might be a point where it was difficult to say whether a metal would harden or would not. The same objection would apply to every distinction. It was impossible to tell exactly, in the solar spectrum, where the blue ended and the green began, or where the red ended and the orange began; but it could not, for one moment, be said on that account that there was no difference between green and blue or between red and orange. It was proposed to abolish the usual definition altogether, and to substitute the definition that steel was iron which was manufactured by melting. It seemed to him that such a definition was unscientific and illogical. The definition of a metal should surely have reference to its properties when made, and not merely to its mode of production. What, according to the Author, were railway springs, or ordinary edge-tools made of? They were not made of ingot metal, and therefore this definition would deny them to be steel, and call them iron! Processes of manufacture were constantly changing, and ought never to be allowed to define the generic name of a metal, or the arts would get into endless confusion. The definition of steel might be a matter of considerable importance, because legal questions might depend upon it; and manufacturers ought to be very careful, for their own sakes, how they introduced a new definition, in opposition to the one generally received, particularly if it had the effect of giving to an inferior material the title of a superior one. If iron prepared by melting, and possessing the qualities of iron and not of steel, required a new name, he did not think it would be difficult to find one. At any rate it should not be called steel, while it lacked the characteristic qualities which everybody whose authority was worth quoting concurred in ascribing to steel, and in defining it by.

However, whatever name it was called by, it was no doubt a very valuable material, and its value might be tested by comparing it with the best kind of iron. All engineers were acquainted with what was called 'Best Yorkshire' iron, a material which had rendered the most important services in the best class of ironwork, and indeed without which the locomotive engine could not have attained its present magnificent development. The tenacity of the best Yorkshire iron varied from 26 to 30 tons, having a mean of about 27½ tons. Of Bessemer metal, judging by the samples in Mr. Smith's tables, the average was about 32·42 tons. The ductility, which was expressed by the elongation, of the best Yorkshire iron was from 20 to 26 per cent., with a mean of 24 per cent. The

Bessemer metal had a ductility of 45 per cent. These two might be combined. Dr. Siemens had mentioned a proposal by Sir Joseph Whitworth for doing so, but that proposal had been anticipated, long ago, by Poncelet and Mallet. Many of those present knew what Mallet's co-efficient was. It was a combination of the tenacity and the ductility, and really expressed a quality which might be called toughness, because the toughness of a metal depended upon both those qualities. Mallet's co-efficient was the quantity of mechanical work which was necessary to break a bar 1 foot long and 1 inch square.[1] Now it had been found that for best Yorkshire iron, the highest Mallet's co-efficient was 9,500 foot-pounds, while the mean co-efficient for the Bessemer metal was 12,500 foot-pounds, the mean of the best Yorkshire being 7,400 foot-pounds. These figures showed the difference in the toughness of the two metals, and afforded a very good idea of the advantage of the one over the other.

The only comparison between the two which might be regarded as doubtful was as to the trustworthiness of the metal. Everybody knew how very trustworthy best Yorkshire iron was. If it was ordered from any of the acknowledged houses, it was as nearly certain as possible what would be received. It was a rare thing to find the best Yorkshire iron fail in the qualities the makers professed it had. Could the same thing be said of Bessemer metal? He did not think it could yet. When steel rails were first made, engineers were obliged to be very cautious how they used them, for it was said that they sometimes broke when they were being unloaded out of the truck. No doubt great improvements had been made since then, and a greater degree of confidence could now be placed in them; and when the same confidence could be placed in Bessemer metal as in the best Yorkshire iron, the superiority of Bessemer metal would be complete. There were some points, however, which raised a little doubt about its trustworthiness. The spiegeleisen was thrown in at the very last moment, and it appeared to be rather doubtful whether in such a short time it could leaven the whole mass so completely and uniformly as to produce perfect homogeneity. He did not profess to enter into the chemistry of the manufacture, but if a small teaspoonful of one fluid were put into a large vessel filled with another fluid, the two would hardly mix so rapidly. For this reason he was inclined to suspect that the resulting material was not always homogeneous. Another thing which had sometimes led him to fear the trust-

[1] *Vide* "Iron as a Material of Construction," by William Pole.

worthiness of Bessemer metal was occasional cracks in the blooms. He had seen blooms coming out of the breaking down rolls with cracks in them, and he had thought, and Dr. Siemens had corroborated his view, that those wounds would not be healed by simply passing through the finishing rolls. Although they might be squeezed so close together that they were no longer visible, they must still be there, and consequently a doubt would arise with regard to the soundness of the metal.

Some experiments he had made on pianoforte wire had been mentioned by Dr. Percy. Some time ago he had had occasion to test the strength of that wire, and had been very much surprised at the result. It was not, however, a new thing to find that a metal had its strength greatly increased by manipulation. That was common to iron wire as well as to steel wire; Muschenbroeck's experiments, which were some of the earliest recorded on iron wire, gave from 30 to 37 tons per square inch of section. Mr. Telford had made experiments on iron wire, and found from 34 to 43 tons. The strength of the wire for the Niagara Bridge, made in Manchester, had been found to be from 45 to 46 tons, being made of good charcoal iron. Pianoforte wire, of very fine quality, would bear the extraordinary strain of from 100 to 120 tons per square inch. Sir Joseph Whitworth had doubted that statement, and therefore he had made some experiments, hoping that Sir Joseph Whitworth would be present when he made them, but unfortunately he was unable to attend. The wire then broke at 100 tons, but it was not of the very best kind. All these experiments seemed to prove that wire would stand something like double the ultimate stress on the same metal in the shape of bars. The elongation, however, of the pianoforte wire was very small. In his experiments it was only 1½ per cent. Mallet's co-efficient for it was only 3,600, or half that of best Yorkshire iron, showing that, as a material for general use, it would be very much inferior.

He regarded Mr. Smith's Paper as very valuable, and that portion of it referring to the wear of rails ought certainly to lead to further inquiry. With all deference, he contended that the rails given on the first examples in the table were not steel but iron—iron prepared in a new and superior way, it might be, but still iron. Mr. Smith's test was a very ingenious suggestion. It was that the general qualities of this new metal might be ascertained by testing its hardness, the softer metal being the best suited for engineering use. At the same time he did not think that that method of testing the hardness by punching was altogether the best. When he was on the Iron Plate Committee, they endeavoured to find a test for hardness, but they could not learn that any had been proposed, except one by Mr. Cowper, and one in America. Mr. Cowper had suggested a test by files, which was very ingenious, but the Committee had not had an opportunity of carrying it out. The American test was by a punch with a point of a pyramidal shape, which was pressed on by a certain weight, and forced into the metal. The amount of penetration could be easily ascertained, and by the amount of penetration the hardness was determined. If some test of that kind, different from the rough one of punching a hole, could be devised, and if the hardness showed the quality, there would then be a perfect and very convenient mode of testing rails and other articles of that kind.

He would make one observation with regard to the Board of Trade: he fancied that the hesitation of the Board of Trade to sanction and provide for the use of the new material was caused by their want of confidence in it. If they could get a material that they could depend upon, having a tenacity of 28 to 32 tons per square inch, it was absurd to suppose that the strain on that would be limited to 5 tons. The limit of 5 tons, he believed, was to provide for possible inequalities in the manufacture. If Mr. Smith and other makers would improve the certainty of the new metal, so that it might always be depended upon, as the best Yorkshire iron could be, no great difficulty need be feared from the Government, or the Board of Trade, or anybody else.

Sir John Hawkshaw said Dr. Pole's observations with regard to the Government would be more to the point, if it was the fact that railway structures were made of Low Moor iron; but it was notorious that the co-efficient allowed for iron by the Board of Trade had little reference to the quality of the iron. Iron from all places, and of all kinds, was used, and yet they had arrived at a co-efficient. Steel was not more various in its character than the different qualities of iron.

Mr. Cowper could not agree with Dr. Pole that Yorkshire plates were so very perfect that bad plates were never met with; because in his own experience, in using plates for boilers, he had found it not a very uncommon thing, to have to return a plate to Yorkshire, though a new one was always quickly sent back for it. Considerable experience had now been gained in the use of steel plates, especially at the Bolton Steel and Iron Works, and at Crewe, where the boiler plates were made of metal of much greater strength than formerly. Ships, too, had been built of steel, which was a tough, useful, ductile material, far more malleable than iron.

A ship made of such soft, tough plates, would be far safer than one made of iron; and Lloyds' surveyors had satisfied themselves that a ship could be made of 25 per cent. less weight, if constructed of steel plates having a breaking strength not exceeding 30 tons to the square inch. He had felt very much humiliated, a short time since, at a meeting in another place, when some one said that steel was used in Government ships with fear and trembling; in fact it was clear that the workmen in the Government yards must be in want of some further instruction on the use of steel, and it was a great pity that any discouragement had been thrown upon the use of steel in the Navy; but after the immense number of boiler-plates and rails that had been made of steel, it was rather late, now, to talk of not being able to use steel with confidence. With regard to steel rails, and the mode of annealing them, and bringing them back to their original toughness after long wear, he thought that there was a certain amount of hardening in the surface of the rails, caused by the rolling of the wheels. Exactly the same effect was produced upon drawn steel wire and upon music wire, which was made in a peculiar manner. It used to be made only in Austria, and the mode of manufacture was not known in this country for many years. It was not softened, but hardened. It was found that by softening steel wire and then drawing it through a die, the surface became 'bench-hardened,' that is, it was so burnished and hardened, by the constant friction of the die, that the outside surface became hard. It was like the scales of a fish, and when drawn apart the inside pulled out, and looked like a bit of dough, while the outside would crack. If steel was hardened, to the same degree as caused by drawing, it was homogeneous and uniform in strength, and would continue to draw well without annealing. He had obtained wire that would bear a strain of 130 tons to the square inch, which he believed was the highest proof ever obtained. He mentioned this, to show that when the steel was perfectly uniform, and slightly hardened to a dark-blue temper, it stood remarkably well. He did not think the subject of the nomenclature would be much improved by a long discussion. His son had suggested that steel might be called No. 1, No. 2, No. 3, or No. 4, according to the number of tenths per cent. of carbon it contained. That was not a perfect description of the steel, because it might contain, also, manganese or silicon, which might modify its properties, but it would give approximately an idea of the hardness. He wished to ask Mr. Smith if he could, by his punching arrangement, ascertain whether one or more of several qualities co-existed, or whether he assumed that they co-existed, in certain proportions, and that therefore, if the punching strain was moderate, the steel possessed all the necessary qualities to a satisfactory extent?

Mr. CRAMPTON said those who were interested in steel-making ought not to try to ignore the difficulties which existed. They would not arrive at the required result by assuming that the material was perfect; the way to succeed was to admit the existing imperfections, and endeavour to induce those interested to find remedies for them. Mr. Siemens had very justly stated that steel was a superior material to iron, altogether. It was also true that it required superior intelligence to manipulate it. Were manufacturers sure of obtaining that superior intelligence in the men who had to manipulate the material? His impression was that, as a rule, the men would not pay proper attention in the practical working-up. He entirely agreed with Sir John Hawkshaw, that an endeavour should be made to get good metal to start with; and from what he had heard in France, lately, it appeared to him that the surest way of obtaining good material, as a base, was to employ the very best ore that could possibly be had, make it into pig iron of the best quality, puddle it as well as possible and get all the cinder out of it, and then melt it in the bath with the proper proportions of the best pig iron, &c. That had been done at Creuzot for the last two years. Mr. Schneider had sent over two engineers, to ascertain if it was true that he did get the phosphorus out of Cleveland pig, which had been accomplished; and they had informed him that they had laid out a large sum in experiments on that subject, and that by taking ore from Algeria, making it into pig iron, puddling it, and turning it into steel in the Siemens bath, they could obtain a better and more regular quality of steel, than by any other means that they knew of. But there still remained that difficulty of careless treatment, though he had no doubt that these difficulties would be got over. Mr. Cowper had said that the iron of 30 tons ultimate tensile strength did not harden, but he had not yet seen any steel of that strength which would not harden if made red hot and put into water. It might be cut by a file, but if it was put into a testing-machine it would indicate 40 or 50 tons breaking strain. If Low Moor iron was taken, at 28 to 30 tons an inch, as mentioned by Dr. Pole, and hardened in water, it would indicate 35 tons, or even more; ordinary iron, breaking with 20 tons to an inch, would indicate 22 tons, and even more occasionally. In experiments upon the common Cleveland iron, made in his furnace, the tensile strain had been reduced to 18 tons an inch, stretching 50 per cent. of its length. Some irons, breaking with a strain of 20 tons, stretched 30 per cent., some only 5 per cent. If iron of 22 tons to the inch were taken, the ultimate stretching as a rule would be 15 per cent. All these materials were much safer than good steel, if the steel were carelessly dealt with, because iron was not altered, to any great extent, by heating it, and cooling it in water. Some remarks had been made about the hesitation of the Government in employing steel, but if the responsibility were thrown upon him he should hesitate to use steel indiscriminately. The Government authorities would be delighted if they could be sure of obtaining steel that could be manufactured with ordinary supervision. He had taken some steel that, in its normal state, broke with a strain of 32 tons to the inch; it was then punched and riveted together; then it was cut into strips, and broken by tension, when the strength was found to be reduced from 32 tons to 14 tons to the inch, while some of the very commonest pig iron, made into wrought iron in his furnace which broke with a strain of 18 tons to the inch, had, when treated in the same manner as the steel, only been reduced in strength 1 ton, or to 17 tons to the inch. Thus the iron, which was the worse, to all appearances, in its natural state, was a safer material than the steel, unless this were annealed after punching. Common ship plates, when so treated, broke with 10 to 11 tons strain. Which, then, was the best for engineering purposes? The 18-ton iron, too, stretched double that of the steel, and treble that of the other. These were some of the reasons why the authorities were not satisfied with regard to the safety of steel, and did not like to use it indiscriminately. He had been rather surprised that Dr. Siemens had not said more about his furnace, because in 1871 he had said he could make steel in the rotating furnace. He did not think the inclined rotating furnace of Pernot would be so good as Dr. Siemens', if the latter were lined with sand. He believed Dr. Siemens would end in making steel in his revolving furnace, would do 50 per cent. more work in the same time and with the same plant, and would save about 30 per cent. of his fuel, as compared with his fixed furnace. He had described, in 1873, that in his revolving furnace, heated with coal dust, cast steel could be made.

Sir John Coode thought Dr. Pole's figures did not bear out the view which he had advanced with regard to the uniformity of Yorkshire iron. If 9,700 lbs. represented the highest, and 7,400 lbs. the average, the minimum must be 5,100 lbs., showing a difference of between 50 and 60 per cent. between the minimum and the maximum.

Sir Joseph Whitworth, in reference to the question whether a piece of metal should be designated iron or steel, observed, through the Secretary, that the decision should, in his opinion, depend upon the tensile strength of the metal, as expressed by the number of tons required to pull it asunder, without regard to the mode of manufacture. He considered engineers ought, after due consideration, to agree upon a limit of tensile strength, so that all metal exceeding this strength should be designated steel, while any metal of inferior tensile strength should be classed as iron. The maximum tensile strength for wrought iron should, he thought, be about 28 or 30 tons per square inch of section. In his opinion, the value of either iron or steel should be represented by the sum of the tensile strength in tons per square inch, and the power of elongation expressed in terms of the percentage of the length. Thus, supposing a metal to have a tensile strength of 40 tons, and to elongate 30 per cent. of its length before fracture, it had a comparative value represented by $40 + 30 = 70$. The power of elongation, which might be represented by the word ductility, was, for some purposes, of the first importance, as in guns, torpedoes, boilers, and wherever severe strains might be suddenly applied; while in the case of certain cutting tools, the strength of the metal was of most importance. Cylinders of steel, to resist with safety the strains produced by gunpowder, should have a ductility of from 30 to 35 per cent.; more than this was unnecessary, for cylinders of such metal would not fly into pieces when burst, but simply opened out or tore like paper; and a metal of greater ductility would not, therefore, be required for any structural purpose. It was now possible to produce with certainty, by the compression of fluid metals, steel that would bear a strain of 40 tons to the square inch, which elongated 30 per cent. of its length before breaking, (the length of the test piece being 2 inches, and its sectional area $\frac{1}{2}$ inch). Such a metal would not harden sufficiently to cut other metals, and yet he thought it worthy of the name of steel. It was desirable that the user should communicate to the manufacturer whether the steel was required to have welding or hardening properties. A general impression prevailed that crucible steel was better than Bessemer or Siemens-Martin steel, or that produced by any other process; but such was not the case, for the quality of steel depended largely upon the quality of the metals used in its production. He had known Bessemer steel much superior to crucible steel in strength and ductility.

Mr. Barlow, Vice-President, remarked, in explanation, that the Committee on the strength of iron and steel had made a number of experiments on riveted steel, with the following results:—In twelve experiments on mild steel, the tensile resistance on rupture was from 27 to 29·92 tons per square inch; in eleven experiments on the next quality of steel, the resistance was from 30·29 to 33·49 tons; in ten experiments on a still higher quality, it was from 31·91 to 41·36 tons per inch; and in fifteen experiments on the highest quality, the tensile resistance was from 31·92 to 46·70 tons per inch. The bars were 3 inches by $\frac{3}{4}$ inch in section, with two bars of 3 inches by $\frac{3}{8}$ inch on each side, riveted with $\frac{3}{4}$-inch steel rivets, and hammered up cold. The difference between the results of these experiments and that of the one experiment described by Mr. Crampton was clearly due to the difference of the manner in which the bars were prepared and the riveting performed.

Mr. Russel Aitken, in reference to the effect of the presence of certain proportions of carbon on the strength of wrought iron and steel, said he had recently assisted in conducting some experiments on the Henderson process. In other modes of making wrought iron, the purifying generally went on at the same rate as the elimination of carbon; but in the Henderson process, the impurities were eliminated, without at the same time removing the carbon. In some experiments, conducted at Bowling, the same mode of working being gone through as usually adopted at that place, the pig iron at first had 1·6 per cent. of silicon, 0·6 per cent. of phosphorus, and the other impurities in proportion. In the wrought iron manufactured, all the phosphorus was eliminated; there was the barest trace of sulphur, and no silicon. In this iron there were left 0·272 per cent. of carbon, and nothing else. It stood 12·7 tons elastic strain, and 30 tons ultimate strain; contraction of area, 42 per cent. This wrought iron was put into a crucible and made into steel with manganese and wood carbon in the usual manner. The elastic strain of the steel was 30 tons per square inch, and the ultimate strain 55·5 tons; the contraction of area being 21·6 per cent. When cooled in oil, the ultimate strain was raised to 76·6 tons; contraction of area, 12 per cent. The steel contained 0·833 per cent. of carbon. In the second bar, when the amount of carbon was 0·86 per cent., the strain was 27 tons elastic, and 50·9 tons ultimate. When the carbon was reduced to 0·60 per cent., the elastic strain went down to 25·9, and the ultimate to 43·3 tons. In some other experiments, with crucible steel made from Staffordshire iron, the amount of carbon was 0·25 per cent., and the strain in that case was only 18·8 tons elastic, and 28·5 tons ultimate. When the carbon was increased to 1·07 per cent., the breaking weight went up to 48·9 tons. Thus it would seem that the strength of steel depended very much on the amount of carbon. He was inclined to fix the amount of carbon which gave the best and strongest wrought iron, by the present modes of manufacture, at about 0·25 per cent. He had tested iron made at Bowling which had contained as much as 0·45 per cent. of carbon, but the results were not superior to what he had mentioned. As to crucible steel, between 0·8 and 0·9 per cent. of carbon would appear to give the best results. He had also tested steel containing as much as 1·07 per cent. of carbon, but it did not give so good a result as steel having only 0·91 per cent. of carbon.

The importance of retaining the carbon in iron, if other impurities could be eliminated, was shown by the fact that the average results of a great number of experiments with wrought-iron plates made from iron purified by the Henderson process, was, that the strength of the plates was 24·7 tons, the contraction of area being 32·4, and the elongation 23·7 per cent. These results, although obtained from metal made from very impure pig irons, were better than those obtained from the best Yorkshire plates, which were only valuable on account of the carbon they contained—and showed that it was not necessary to take out the carbon, in order to make iron which would extend enormously before fracture. By impurity, he meant phosphorus, silicon, &c.; because although phosphorus, for instance, was not an impurity when phosphorised steel had to be made, it was so when it was desired to produce a crystallization of iron and carbon.

Mr. Jabez James said, in allusion to the alleged difficulty of obtaining small steel wire, sound, in this country, that when the late Mr. Larwire introduced his system of perforation, he had not been able to obtain cast-steel wire of English manufacture that would stand the work. He was under the impression that the wire referred to came from Prussia or Switzerland, but he had no doubt Mr. Cowper was right. Steel was difficult to deal with, in the workshop, after leaving the maker. The speck, spoken of by Dr. Pole, would be fatal to cast steel, when used in the engineer's shop. He had with him a specimen of some tools made for work in hand for Mr. Vignoles. A doubt had arisen, after they were made, as to whether they would not break in hardening, and it was resolved to try the effect of using them soft, and they succeeded. It often happened, particularly in the best brands of iron, that the smith found a crack in the metal, after working, and that he was not able to weld it. In the case of iron, sulphur was the cause of the cracks, and gases in the case of steel. He very much objected to working steel under the hammer, for the less it was worked, the sounder it was.

Mr. Rapier observed that he was concerned in cutting into absolute shreds thousands of steel rails every year, and he was beginning to know where the hard places were. He found that more defective rails came from the makers who drilled, than from those who punched them; and in the case of makers who drilled or punched, according to specification, the drilled rails were the harder. Out of five thousand rails cut to pieces last year, for points and crossings, fifty-five rails proved troublesome, of which only five were altogether unmanageable. The difficulty was commonly got over by putting them into the boiler-shop plate furnace and annealing them. The majority of the fifty-five troublesome rails had been drilled. He believed that if the five thousand rails had all been punched, the number of rails which were too hard would not have exceeded twenty-five. The extreme durability of steel rails, as compared with iron rails, would certainly be felt, in the course of the next four or five years, by those railway companies which had adopted steel extensively, during the last few years, and it was of importance that all possible data in reference to them should be obtained and recorded. Mr. Smith had stated, that, after getting beyond a certain percentage of carbon, and a certain degree of hardness, and even a certain degree of tenacity, the rails were not better, but worse. As regarded carbon, that might be expected, because cast iron contained a very large percentage of carbon, and it was well known, in old times, that cast-iron rails wore out very quickly; there must, therefore, be some intermediate point, between wrought iron and the other extreme of cast iron, at which the best result was obtainable. The experiments referred to by Mr. Smith were very valuable, and would be still more so if the area of observation were enlarged. He could not quite accept the claim that the hardening and cutting qualities were a true test of steel. Even in the case of the commonest steel rails, a piece might be cut off, and made into a drill, that would drill the very rail of which it formed a part. He had himself made chisels of malleable cast iron. If a piece of malleable cast iron were taken, and forged into a chisel, and tempered, it would be found to cut very nicely.

Mr. Hackney, in replying upon the discussion, said he should confine himself, mainly, to an explanation of points in the Paper, of which the meaning seemed to have been misunderstood, and to the defence of statements the accuracy of which had been called in question. Dr. Percy had questioned, at some length, the correctness and desirability of what he called Mr. Hackney's definition of steel;

and that definition had been discussed also by other speakers. But the definition was not his; it was that of M. Jordan, of M. Greiner, of Mr. Holley, and, indeed, more or less confessedly, of the great majority of the makers and users of Bessemer steel, and of other varieties of soft ingot metal, in the world. In the Paper, he had avoided putting forward any definition of steel as being the best or the only correct definition. Engineers, as a rule, dealt with facts rather than with words. They found that facts were quite as much as they could manage, and only discussed words or definitions in so far as those might be needed to explain their meaning, and to express the relations that subsisted between facts. Taking this view, what he had said, as to the use of the term "steel," was, in substance, as follows: that the division of all the malleable varieties or alloys of iron into ingot metal and puddled or piled metal, into those that were cast into malleable ingots, and those that were produced in other ways, was at the same time more convenient in practice, and a more natural and better grouping in a scientific point of view, than the old division into iron and steel—into the varieties that were less and more highly carburetted: and he ventured to think that the prominence given to that fact was one of the most important features of the Paper. The subject of the Paper was stated to be the manufacture and the properties of ingot metal, and of ingot metal only, from the soft metal used for boiler plates up to hard tool steel. He had pointed out that all such metal was commonly spoken of as steel, and that whether it was called "steel," or "ingot metal," or anything else, mattered little, so long as the sense in which the term was used was understood; but that as "steel" was at once the shortest and the most commonly used name for ingot metal, the word "steel" would be employed in the subsequent parts of the Paper, as meaning simply this, and nothing else. As a matter of personal opinion, he thought there was little doubt that, within a few years, this would come to be the universally received meaning of the term. Engineers and manufacturers paid a deserved respect to scientific men, to those who took up the study of technical processes from their scientific aspect; but, after all, it was by the practical workers that the meanings of technical terms were fixed, and on this question practical men were daily speaking unmistakably. If a boiler-maker were to order a lot of steel plates, and were asked whether he wished puddled or Bessemer steel, and how hard they should be, would he say, "Certainly, both are steel; either kind will do if it is hard enough. You may learn from

Dr. Percy's 'Metallurgy,' from Miller's 'Inorganic Chemistry,' from Brand, and from Rankine, that no metal can be called steel that does not contain at least 0·5 per cent. of carbon, and that does not become hard and brittle when it is heated to redness and quenched in water. I shall reject every plate that does not come up to this test"? No, he would say, "If I had wished for puddled steel plates I should have asked for them; but you ought to know that puddled steel would be a most unfit material for boiler plates, and that when boiler-makers speak of steel plates, they mean plates that are made from cast ingots. They may be made by any process, so long as they are of good quality, and are sufficiently soft;—every plate must bear bending double, without cracking, after it has been quenched in water from a red heat." It was true, as Dr. Pole had remarked, that if a piece of puddled or cemented ferruginous metal were handed to a workman, and he were asked to determine whether it was iron or steel, he would probably heat it to redness and quench it in water, and if it hardened he would say it was steel; but this was rather an instance of the confusion that existed in men's minds, at the present time, as to what was really meant by the term, than an argument against another and equally general use of it; for the same or a brother workman, if he had been accustomed to work up boiler plates of soft Bessemer metal, would be just as decided in calling these steel plates. In other words, the same name, steel, would be given to two materials that had actually nothing in common except that both were malleable varieties of iron, and that both, though from different causes, had a greater tensile strength than common bar. If these two materials were equally steel, a definition of the kind suggested by Dr. Siemens and Sir Joseph Whitworth, that steel was any malleable variety of iron that possessed a greater tensile strength than common bar, a greater strength, that was, than 24 or 25 tons per square inch, was the only intelligible meaning that could be given to the term; but this again led to a dilemma, for if greater strength was to be the only distinction between iron and steel, it ought to be immaterial by what means this greater strength was attained; and, to be consistent, it would be necessary to say that when, by cold rolling or wire-drawing, the strength of a piece of soft iron was increased from 20 tons to 30 or 40 tons per square inch, the metal was converted, by that merely mechanical treatment, from iron into steel.

Dr. Pole's suggestion, that all varieties of ingot metal that were too soft to be classed as steel, that was, he supposed, all containing less than 0·5 per cent. of carbon, should be called Bessemer metal or Bessemer iron, was still more unfortunate, and would not bear a moment's examination. Such metal, even of the softest qualities, had been made, by fusion in crucibles, by Mushet and others, long before the Bessemer process was known, and it was still made in that way to some extent. Nor did he think that Dr. Siemens would like to hear the soft steel boiler plates and the steel rails, now made largely, in all parts of the world, by the Siemens and Siemens-Martin processes, spoken of as Bessemer plates and Bessemer rails; even if the makers and users of the metal would be content to employ so misleading a term. In defending the old division of the varieties of ferruginous metal into irons and steels, Dr. Pole had remarked, that no distinction in nature was sharp and absolute, that one variety blended into another; but one advantage of the division into ingot metal and piled or puddled metal was that this was a sharp distinction;—there was no more a gradation between the balling-up of particles of iron into a pasty mass, and the pouring of the metal, in a state as liquid as water, into a mould, than there was between moulding a snowball, and pouring out and freezing a glassful of water.

He had been understood by Mr. Barlow to say, that though steel was stronger than iron, its extra strength was gained at the cost of toughness, and that steel was necessarily brittle; and Mr. Barlow had stated, as a correction, that a large proportion of the steel now made was strong and ductile. Here, again, it was necessary, first, to define the sense in which the term "steel" was used;—steel as Dr. Percy or Dr. Pole would define it, containing at least 0·5 per cent. of carbon, was extremely strong, but unless annealed with great care it was apt to be very brittle; but steel, meaning simply ingot metal, might be much stronger than soft puddled iron, and yet extremely ductile, if it contained but little carbon, silicon, and phosphorus. Still it had the peculiarity, well shown by Mr. Price's experiment, quoted in the Paper, that it was weakened, much more than puddled iron was weakened, by cutting a notch in it. Thus the greater strength of soft ingot metal than that of puddled metal, of the same composition, was not gained in any sense at the cost of ductility or safety, except that the ingot metal was more weakened than the puddled metal by cutting holes or notches in it; but the still greater strength of hard ingot metal than of soft ingot metal was attained at the cost of ductility.

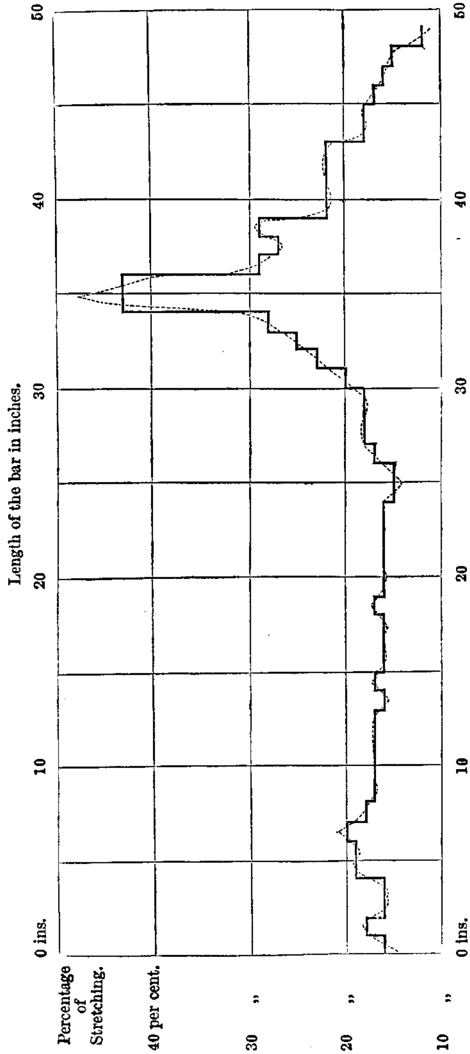
Closely connected with this point, was the question what kinds of steel, meaning by this simply ingot metal, should be used for boiler-making, for bridge or ship-building, or for other engineering purposes, in which perfect safety, and as much strength as was consistent with it, were required. A steel plate or angle might be unsafe from several causes: the metal might contain so much carbon, silicon, or phosphorus, that it could not be made tough and soft by any treatment: or, though not quite so hard as this, it might yet be so hard as not to be safe unless very carefully annealed, and any part of it heated and suddenly cooled might become brittle: or, lastly, though otherwise of the required quality, it might have been hardened, and so spoiled, by mechanical treatment; by having been finished, as plates commonly were, at a very low heat—by having been, in fact, almost cold rolled. The remedy for these sources of uncertainty was very simple. No steel plate or angle should, in his opinion, be passed as fit for use in a boiler, or a bridge, or a ship, that had not been quenched in water when hot from the rolls, and re-heated to moderate redness and quenched again, and that after this treatment would not bear bending up close, or nearly close, without cracking. This treatment would remove all hardening due to cold rolling, and it would insure plates and angles that a smith could not make brittle, by quenching them in his water-bosh. It was probable that it would still be safer to drill the rivet-holes, or, if they were punched, to anneal the plate afterwards, as punching strained the metal, beyond its limit of elasticity, for some distance round the hole, and so weakened it, unless the plate was subsequently annealed; but in other respects the metal might be treated just like soft puddled iron.

Such plates would have a tensile strength of 28 to 32 or 34 tons per square inch; if more strength were required it might be obtained, but only at the cost of their other good qualities; and they would contain not more than 0·2 or 0·22 per cent. of carbon, little or no silicon, and at least from 0·15 to 0·3 or 0·4 per cent. of manganese, according as the metal was more or less free from sulphur. The phosphorus might run up as high as 0·1 or even 0·15 per cent., as in the boiler-plate steel made by the open-hearth process at Trenton, New Jersey, if the carbon were kept low in proportion; so that such metal need not to be made from any exceptionally pure materials. In fact, it was made, and made largely, at Crewe, at Pittsburg and Trenton in America, in France, in Sweden, and elsewhere; the only difference being that, in some of those districts, the plates were sent out just as they left the rolls; they were not reheated, and quenched in water, to toughen them and to remove the hardness due to cold rolling. That was not the place, nor would there be time, to go into the minute details of manufacture. It was enough to say that such metal was quite within reach, and that for the higher classes of work, in which toughness was required, rather than hardness or rigidity, engineers should not be content with anything less. For rails, such metal would be too soft, as well as needlessly tough; and for rail-making it would, probably, be the better plan to keep to the quality of steel, containing 0·3 or 0·4 per cent. of carbon, that was now made for the purpose.

There were many workers now in the field, endeavouring to improve the processes of steel-making; and no doubt much was yet to be done in perfecting the manufacture, more particularly in the way of making a high quality of metal more cheaply, and from inferior materials. In a discussion that had taken place lately, before another institution, mention had been made of an improvement on the Bessemer process, by which it was claimed that, during the blowing, the whole of the sulphur and silicon contained in the metal were removed, and that thus a quality of soft steel never before seen could be produced. The process in question no doubt made good soft steel; he had seen it at work, and had seen specimens of the metal: and it might, for all he knew, effect all that was claimed for it: but equally good metal, to any he had seen, might, he believed, be made, from good materials and with proper working, in other well-known ways.

Much had been said, in the course of the discussion, of the importance of determining the percentage of its length that a steel bar of given quality would stretch under a tensile strain, before it broke, as being a measure of its toughness and of its value for engineering purposes; but no attention had been directed to the very grave sources of discrepancy or error that existed in the common way of estimating this stretching. The elongation of a bar of tough metal, stretched until it broke, was made up of two distinct parts. First, there was the general stretching of every part of the bar, from end to end; and, secondly, there was the greater elongation at and near the point of fracture, that was due to the drawing-out of the metal, and its diminution of area, after it had begun to give way. In illustration of that, he had selected the result of the testing of the steel bar that stretched most, among those experimented on by a Committee of Civil Engineers in 1868. The bar was 50 inches, or 36 diameters, long; 1·382 inch in diameter, or 1·5 square inch area; test number 1,255; ultimate stress 72,187 lbs. per square inch; total extension in 49 inches = 9·57 inches, or 19·5 per cent. The permanent elongation, up to the breaking point, in each inch of its length, was shown graphically in Fig. 3. It would be seen that throughout the greater part of the length of the bar the stretching amounted

Fig. 3.



to 16 or 17 per cent., but that in the 2 inches nearest to the point of fracture the stretching was 43 per cent., and for an inch or two on each side of that part it was 28 or 29 per cent.; and in breaking any long bar of tough metal, by tension, the result was very much the same. Now, which of those percentages of elongation was to be taken as the true measure of the stretching of the bar? Investigators were sadly tempted to cook their experiments a little, now and then;—to get out the best results that they could;—at least he had often felt tempted to do so;—and where cooking was so easy as it was in this case, where all that need be done to make the difference between a very high and a moderate percentage of stretching was to use short test pieces, it was of great importance that some definite mode of testing should be made the rule, in order that tests made by different experimentors might be comparable. The test piece should either be in all cases one fixed number of diameters long, between the datum points; or, what might perhaps be better, it should be at least 20 or 30 diameters long; and all the high percentages of stretching near the point of fracture should be rejected, and the mean percentage of stretching of the rest of the bar,—that is, what the percentage of stretching would be in a bar of infinite length,—should be taken as the final result. That would, of course, give much lower results than many of those now commonly quoted, but it would represent more correctly the real value of the material, because, when a bar began to draw down and yield, at one point, its strength was gone;—the percentage of stretching, before it actually gave way, was the figure that determined the amount of 'work' required to break it.

Passing from those general considerations to points of manufacture, he had first to reply to the criticisms of Mr. Alexander Wilson and Mr. Walker. Mr. Wilson had stated that it was impossible, under any circumstances, to complete a Bessemer blow in five or six minutes; and Mr. Walker had been equally sure that no two 5-ton Bessemer converters in the world ever had made, or ever could make, as many as fifty blows in twenty-four hours, or turn out 1,100 tons of ingots in a week. The error into which those gentlemen had fallen was most natural. The Sheffield district, and the Northern and Midland counties generally, formed, together, the most important steel-making centre in the world, and the workers in such a centre—those who were not at the same time readers and students—were apt to think that the materials, and apparatus, and modes of working, with which they were familiar, were the only materials, the only apparatus, and the only modes of working, that were anywhere in use. If Mr. Wilson had confined himself to the statement that a Bessemer blow was never finished, in Sheffield, in six minutes, he would have been quite right; to say that this was not done anywhere, or under any circumstances, was both rash and wrong. The statement was given as an extreme result; and, as its accuracy had been questioned, he had verified it by reference to the original authority, Director Westman's official report in the "Jern Kontorets Annaler" for the year 1874. The works referred to were the Iggesund Steel Works, the most recently erected in Sweden; and the explanation of the remarkable shortness of the blow, as compared with Sheffield practice, was, first, that the blowing was no doubt direct, or in other words, that it was stopped when the steel was of the required hardness, no spiegeleisen being added, a saving in itself of several minutes; secondly, that, as explained in the Paper, the great anxiety of manufacturers, in working the Bessemer process in Sweden, was to get the steel out as hot as possible, as Swedish Bessemer pig iron, being made with charcoal, and from manganiferous ores, might be very free from sulphur though it contained mere traces of silicon; and one way of getting the steel out at a high heat was to complete the blow rapidly; thirdly, that whereas 10 lbs. of silicon required for combustion 11½ lbs. of oxygen, 10 lbs of manganese required only 4 lbs. of oxygen; so that pig iron containing 3 or 4 per cent. of manganese, and little silicon, such as that worked at Iggesund, required less air to blow it than pig iron that contained 3 or 4 per cent. of silicon, and little manganese. At Iggesund, for charges of only 2½ tons, the blowing power was at least 400 indicated HP., and the tuyeres were twelve in number, with five to seven holes in each, ⅝ of an inch in diameter. Bessemer blows lasting from nine to twelve minutes were by no means uncommon, where the works were provided with sufficient blowing power, and the character of the metal was such as to render quick blows desirable. Thus, at Zwickau, the time taken to blow a charge of 5 tons was only twelve minutes.

Mr. Walker, like Mr. Wilson, had fallen into the error of looking on whatever had not been done in Sheffield as impossible anywhere; but, unlike Mr. Wilson, he had attempted to prove his point by argument. Mr. Hackney had listened carefully to what Mr. Walker had said, but he could only understand his reasoning on the supposition that, familiar as he was with the arrangement of Bessemer plant, he had forgotten for a moment, that an ordinary set of Bessemer apparatus included two converters, and not only one. This was Mr. Walker's argument, in so far as he had been able to follow it. First, he had said, very justly, that, however quickly new bottoms were put into the vessels, at least four hours in the twenty-four must be allowed for those repairs, leaving only twenty hours for actual work. Then, allowing four minutes for running the pig iron into the converter, nineteen minutes for blowing, and four minutes for pouring the steel out, it might be estimated, in round numbers, that each blow would take half an hour; and "how," he had asked, "can you get fifty half-hours in twenty hours? The thing is impossible." But there was the slip: each of the two vessels made only twenty-five blows in the twenty hours; so that, actually, taking Mr. Walker's own figures, the vessels, when doing what he had said was an impossible amount of work, were so far from being overdriven, that each stood idle during seven and a half hours out of twenty, or more than a third of their working time;—an ample allowance, surely, for any contingencies that would be likely to occur in well-arranged works. Of course, as pointed out in the Paper, these large out-puts had only been rendered possible by corresponding changes in other parts of the system. Thus, in an American Bessemer plant, there were four cupolas, each capable of running down at least 6 tons an hour; and in front of those were two ladles, each large enough to hold 10 or 15 tons of metal. The melted pig iron ran from the cupolas into these ladles, and a charge could at any time be poured from one of them, into the converter, in a few seconds, so that the converter was never kept waiting. There were three ingot cranes swinging over the pit, instead of only two, and one of these cranes commanded each converter. And, lastly, the converters were set side by side, closer together than in English practice, so that there was room in the pit to take, at once, moulds enough for two or three casts. Having now, he trusted, shown that what he had said was not impossible, he had only to quote a few instances in which it had been done. At the Troy Steel Works, New York, on February 13th, 1874, fifty blows, yielding 267 tons of ingots had been made from two vessels in twenty-four hours; and at the same works, in the first week of the present year, two hundred and thirty-two blows had been made, in ten turns, yielding 1,140 tons of ingots; and in the following week two hundred and twenty-five blows, yielding over 1,100 tons, fifty-five blows having been made in one twenty-four hours of that week. On September 23rd, 1874, the North Chicago Steel Works had made fifty-two blows in twenty-four hours out of one pair of converters; fifteen of those blows had been made in the six hours between noon and 6 P.M.; and in the night shift five blows had been made in one hour and thirty-five minutes. In August the same works had made, in two consecutive weeks, above 2,011 tons of ingots. And lastly, the Cambria Steel Works, in October last, had made a week's out-put, from two 5-ton vessels, of two hundred and thirty-one blows. Those were, of course, exceptional or "possible" out-puts, and had been quoted in the Paper as such; the standard American practice being, according to Holley, thirty blows out of two vessels per twenty-four hours.

How far, or under what circumstances, those large makes from one pair of vessels were economical, would be matter for a paper in itself; no hard and fast line could be laid down. Any out-put, however insignificant, was too great if it involved hurried or careless work; and, on the other hand, no out-put could be called excessive, if the arrangements for the work were so perfect, and the reserve of power was so ample, that it became a mere matter of routine.

A point of some interest, in connection with the more modern methods of steel-making, had been raised by Dr. Pole. He had asked whether the spiegeleisen, in the Bessemer process, became thoroughly mixed with the charge, in the short time that elapsed between running it into the converter and casting the steel into ingots. The spiegeleisen became mixed through three actions. First, it was run in rapidly, in a strong stream; next, as soon as it began to mix with the metal, the whole mass effervesced, or boiled up violently; and, thirdly, the steel was poured out twice—first into the ladle, and from that into the moulds; and this alone, in the case of a liquid so mobile as melted steel, would cause a pretty fair mixture. In fact, most of those who were conversant with the subject agreed, he believed, that the mixture was practically perfect. In the open-hearth processes the metal was well stirred just before tapping, so that an equal or a better mixture than in the Bessemer process was insured.

It had been remarked by Dr. Siemens that the Pernot furnace, for open-hearth steel-making, might be suited for melting hard metal containing 0·8 per cent. of carbon, but that he did not think it would stand the heat required to melt very mild steel. Mr. Hackney was informed, however, by M. Petin, of Saint Chamond, that the furnace there had been working regularly, not on hard steel, but on the very soft metal made for boiler and ship plates; and if the large girders carrying the furnace roof, which were no doubt the weak parts, should give trouble, there would probably be no great difficulty in keeping them cool by water. The difficulty suggested in working pig iron in the Pernot furnace, from the violent ebullition that would take place on adding ore, did not, he was informed, occur in practice, for, as was mentioned in the Paper, the oxidation of the carbon and silicon in the melted metal was so rapid, through the constant state of agitation in which it was maintained, that little or no oxide need be added. In one sense this was of course a drawback, as, if the carbon and silicon were oxidised by the flame, instead of by oxide of iron, there would be the same loss of weight as in the Bessemer process, instead of a gain from the reduction of iron from the ore.

A few observations had been made on the remarkable process of steel-making that Mr. Blair was engaged in working out. Mr. Lowthian Bell believed that Mr. Blair was wrong in stating that, when a mixture of ore and carbon was once heated up, sufficiently, nothing more was needed, to effect complete reduction, than to keep it for a sufficiently long time from external cooling; and he had pointed out that all the gas passing off from the ore and carbon, and other gas besides, were burned round the retorts to keep up the heat. The whole of the gas from the retorts, however, and part of the other, was burned to give the initial heating, and Mr. Bell had not accounted for what was stated by Mr. Blair as a fact proved beyond question—that the larger retorts gave a greater yield, in proportion to their area, than the smaller; a statement which, if confirmed, would go far to show that the reduction was effected without absorption of heat. Dr. Siemens had pointed out a serious difficulty in the way of the success in this country of any process like Blair's;—the readiness, namely, with which spongy iron took up any sulphur contained in the fuel used to reduce it. In America, the reduction had hitherto been effected entirely by charcoal, and thus the difficulty was avoided; and as to the working of the process in this country, he was disposed to believe, from some experiments made last winter, that if coke containing 10 per cent. of lime were used as the reducing agent, the lime would fix the whole of the sulphur, and the spongy metal would be as pure as if reduced by charcoal.

On the subject of rolling mills, several remarks of interest had been made. Mr. Walker had strongly advocated the use of rolls of small diameter; and, to some extent, he had been right. There was no doubt that, in rolling metal tough enough to bear the dragging action of small rolls, they did their work with less expenditure of power than rolls of larger size; but in many cases, particularly in cogging steel ingots, they were apt to cause cracks in the metal, unless the draft at each pass was very small; and in such cases rolls of larger diameter did the work better. Mr. Edward Williams had advocated the use, for cogging, of two stands of 3-high rolls placed one behind the other, so that the ingot might be reduced, twice, each time it went through. He questioned if there would be any advantage in that, over a single 3-high mill fitted with suitable arrangements for handling the ingots by power, to compensate for the increased cost and complication. There was no need to hurry over the cogging of a big ingot, lest it might cool; and it was stated that a single 3-high cogging mill, fitted with power tables, would cog 1,000 tons of ingots a week; while the draft at each pass might be as small as was found necessary. Mr. Price Williams had expressed some surprise that the question of cogging *versus* hammering, in making rails, had been referred to as being still open. In answer to that, he thought he could not do better than refer to the system of rail-making carried on at Barrow. Every rail made at Barrow was until lately, and he believed still was, rolled from a hammered bloom; and no doubt the authorities there would be able to give good reasons for the practice.

At the commencement of the discussion, Dr. Percy had directed attention, very forcibly, to the imperfection of the present knowledge of steel, and to the urgent need for more complete experiments upon that most valuable material; particularly of experiments to connect the mechanical properties of its different varieties with their chemical composition, and with the different kinds of treatment to which each test piece had been subjected; and Mr. Hackney would most respectfully press upon the members of the Institution the great need for such a thorough set of experiments.

Mr. Harrison, President, said he was sure the members were much obliged to Mr. Hackney and Mr. Smith for their valuable Papers. He could endorse the remarks of Sir John Hawkshaw as to the desirability of obtaining from the officials of the Board of Trade some standard as to the strength of steel, the absence of which was a positive bar to the use of that metal in the construction of bridges. He could not help feeling, that if what had taken place at the meetings of the Institution became known to that department, some standard would be given, whether right or wrong. There were two difficulties in connection with steel: liability to fracture if the steel was too hard; and the tendency to wear away if it was too soft. All who had used steel must have had experience of its liability to fracture. He exhibited two specimens of steel rails taken from a place where they had been subject to a very severe test, not so much from the tonnage passing over them, although that was large (nearly one hundred trains a day), as from the fact that breaks were constantly applied at the place, to stop the trains for watering. One of the samples showed, what he had never before seen, a distinct lamination. It was a 75-lb. rail, and in seven years the weight had diminished 8 lbs. per yard. The rails had been worn on both sides, having been turned after about four years' wear. The original template accompanied the rails, so that a comparison could be made. He had also brought two samples of iron rails from the same line; they were taken up a few days ago, and had been down fourteen and a half years. They came from the main line of the North-Eastern railway, between Newcastle and Berwick. The number of trains passing was only about forty a day, but many of them had very heavy engines, and travelled at the highest speed. These rails were hardened by Dodds' process. About fifteen years ago he had had some furnaces put up for the purpose, and he had laid down a length of 100 miles of the rails hardened by that process. On one occasion, the rails had been so exceedingly hardened that a number of breakages occurred, and the directors had determined not to use any more; the furnaces had then been pulled down, and, except as to the length of 100 miles in question, he had had no experience of that kind of rail. About 60 per cent. of the rails then laid down were still in actual use, and in good condition; and the extent of the case-hardening produced by the process they had undergone was distinctly visible on the broken surface of the rail exhibited. The loss of weight was only about 2 lbs. a yard after fourteen and a half years' wear. The cost of the process had been under £1 a ton; and he was not at all sure that iron so treated might not be made to last as long as steel. There was a great amount of the surface still unworn away; and he believed that by care the iron might, by Dodds' process, be hardened properly, and at the same time that the great brittleness complained of might be avoided. With the single exception to which he referred, the proportion of broken rails in the district referred to had not been greater than the proportion in other parts of the line.

Mr. Sandberg remarked, through the Secretary, that from an experience as Inspector of Rails, for the last fifteen years, for several countries with extremely cold climates, he was certain cold had a most deleterious effect both on iron and steel, and that in the construction of railways as well as for other structures, tenacity and softness were necessary to a much higher extent than for countries with mild climates. Mr. Hackney had touched upon the different modes now in progress for making steel from inferior raw material. The experience of such steel being limited so far to experiments, caution would be required before using such steel extensively. Generally in countries with cold climates, steel was looked upon with greater confidence than iron for railway purposes; but this might be because steel hitherto had been made containing a less proportion of phosphorus than iron. How the so-called phosphor-steel and silicon-steel would answer for railway materials in cold climates was not yet ascertained, but that there were instances where iron had proved safer than steel, might be gathered from the experience of the breakage of rails on the Nijni line of railway in Russia, where the frost affected steel more than iron; but the iron rails in question were of Siberian iron, whilst the steel rails were of English manufacture. On this line last winter, in ten steel rails broken during severe frost, when analysed chemically, the steel was found to contain 0·184 per cent. of phosphorus, 0·343 per cent. of carbon, and 0·178 per cent. of silicon, which amply explained its brittleness. Purity of the metal, whether iron or steel, and particularly freedom from phosphorus, was, therefore, a vital necessity to obtain reliable railway plant for countries with cold climates. He had found Siemens steel as safe as the Bessemer, even in the same works. In fact, the length of time allowed for the decarburization and purification of steel, by the Siemens process, was an advantage, on account of the steel being tested in the furnace before it was cast, and it being possible to alter the quality, by addition of more spiegel to obtain greater hardness, and by allowing time for further decarburization to insure softness. The winter of 1874–5 had been an unusually severe one. In the United States the breakage of rails, and accidents arising therefrom, amounted, according to the "Railroad Gazette," to no less than fifty-three during the month of February, and broken rails were the cause of more than one-fourth of all the accidents put together. In Sweden, with a still harder climate, (inasmuch as quicksilver had been frozen many times during the last winter), there had not been a single accident from broken rails. This, coupled with the fact that England had chiefly supplied the rails to both countries, and that generally the rails for Sweden were subjected to more severe tests and inspection, led to the conclusion of the value of the latter; and although it might be against the inclination of the makers, it must, however, be satisfactory to them, as well as to the inspector, to find from such large experience that in this way safety could be secured at a very small extra cost. Another proof of the value of inspection was found in the rails for Canada, supplied by the Barrow Works. These rails had, in great part, been inspected by himself. Regarding the wear of iron rails, as compared with steel rails, in cold climates, the latter were decidedly preferred both in Canada and Russia. In Scandinavia, however, iron rails made in this country had hitherto given satisfaction, and few, if any, steel rails had yet been introduced. The iron rails had proved both safe and durable, and would probably last eighteen or twenty years. Whether this satisfactory result was due to more careful supervision of the rails, or whether the Scandinavian roads were better drained and maintained than the Canadian and Russian lines, was uncertain, but the fact remained that English iron rails gave a favourable result even under a hard climate, both in respect to safety and to economy, where the traffic was not too great, and where care was taken in the inspection. The amount of traffic generally decided the choice of material from a point of economy. It was, however, satisfactory that steel rails were nowadays much cheaper than formerly, as compared with iron. At the introduction of steel rails, fifteen years ago, their price had been nearly three times that of iron rails. Gradually it had been reduced to 50 per cent. more, and in the latest transactions steel rails had been quoted at a price only 25 per cent. more than iron rails. Reverting to the present system of inspection, characterised by Mr. Smith as 'unsatisfactory' and 'costly,' there was, no doubt, room for improvement, and if practical means could be found, to ascertain the strength and safety of every rail, such as was proposed by the registering punching machine, should such ever be constructed, it would serve as an additional safeguard. But it could hardly be a substitute for inspection, from the same fault as that of the coal-cutting machine, viz., a want of eyes. It would be very costly if the punching machine should be made to break every time the steel was too hard; it would also be very costly to have to reject all rails found too hard. A cheaper plan, when such hard steel was accidentally made, was the present mode of testing a sample ingot, both mechanically and chemically, before the larger ingots were rolled into rails. The Eggertz coloration test, now adopted in nearly all the steel works in England, had proved of the greatest service, and he had supplied nearly all the makers with normal steel, for comparison, which had been obtained direct from Professor Eggertz of the Stockholm School of Mines. Hard steel ingots could be used for springs, tools, &c., and not rolled into unserviceable rails. As for the cost of the present mode of inspection, by sacrificing a great number of rails, it might be said that crop ends and parts of defective rails served for the execution of the falling test, and that by daily testing, and keeping the inspection close up to the manufacture, the rejection of large quantities of rails was very unusual. It would probably be otherwise if testing and inspection were done away with, and the quality of the steel checked at the last operation, viz., the punching. Besides, rails are rejected for other reasons beside being too hard. The result of his inspection at the various works in the kingdom, during 1874, showed a rejection of 10 per cent. at the original inspection, which was reduced to a little under 2 per cent. after dressing for minor faults, such as bad ends and section, imperfect lengths, crookedness, twist, and, finally, bad cracks.[1] The table of the wear of hard and of soft steel rails should have included a column showing the load carried over them in million tons; and so much experience was opposed to the conclusion that the hardest rails were the least durable, that it would be premature to set it aside before further results were brought forward than could be obtained from a single set of experiments. Mr. Barlow's remarks that the effect of the shock was incorrectly estimated by multiplying the weight of ball by the height of fall was no doubt true theoretically. Fifteen years ago he had calculated the effect by the well-known rule of the square of the height; but taking the permanent deflection of the rail to be the true indicator of the force of the blow, and repeating experiments often with two halves of the same rail, say ½ ton and 1 ton, falling 20 feet and 10 feet respectively, the deflections of the halves were similar, and, if anything, the higher fall with the lighter ball gave the least deflection, which, according to that theory, should have given the most. In either case the ball did not fall free, but it was guided by slides so as to hit the rail in the proper place, thus it was possible that the friction on the higher fall and the greater velocity was greater than that with the heavier ball, but the difference was so small that within practical limits a slight difference in the weight of ball, as stipulated in some specifications, might be compensated by the higher fall, and a simple multiplier applied as giving sufficient exactness in practice. The results of these experiments still remained as stated, however such results were expressed, viz., that notching the flange of the rail diminished its strength more than punching bolt-holes in the web, and that the diminution was in proportion to the hardness of the material, so much so, that rails

[1] Results of inspection of iron and steel rails during 1874:—

	Per cent.
Temporary rejections for bad ends	1
„ „ „ too long	1
„ „ „ too short	0½
„ „ „ crooked rails	4
„ „ „ twisted „	1
„ „ „ bad punching	0½
Temporarily rejected, but accepted after mending	8
Final rejection, bad rails	2

made of very hard steel were sometimes broken in the very operation of notching, or even in unloading.

Mr. W. W. Evans, of New York, remarked, through the Secretary, that the first flat-footed rail ever rolled had probably been that known as the Stevens pattern, designed in 1830, and in use on the Camden and Amboy railroad for many years subsequently. In a circular, dated 1830, sent to the different rolling mills in England, Mr. Stevens had called for a rail with the bottom flange, or flat foot, wider than the rest every 2 feet, but finding that in those days there was some difficulty in rolling a rail of that pattern, he had altered his plan to the section previously referred to, now known as the Vignoles pattern. The first approach to a fish-plate had been in connection with this rail; the joints had been linked together by a short plate on each side, with a screw-bolt through the web of each end of the rails; it had varied from the fish-plates of the present day only in being short, and having one bolt in each rail instead of two. Several persons in England had claimed the design of the fish-plate, but he was under the impression that the fish-plate had been due to a man named Barr, and that it had been introduced on the Newcastle and Frenchtown railroad, a short line, constructed in 1832–3, to connect the waters of the Delaware and Chesapeake bays.

Mr. W. H. Maudslay observed, through the Secretary, that the Pernot furnace in use at St. Chamond and Allevard was similar to that patented on the 19th of May, 1858, by his father, the late Mr. Joseph Maudslay, who, however, subsequently found that the principle of the rotary furnace had been brought out in May 1853, although in a much less perfect form, by Messrs. B. P. Walker and J. Warren.

Mr. Hackney replied, through the Secretary, that the arrangement referred to by Mr. Maudslay was a nearer approach to the Pernot mode of making steel than he had been aware of, but it was not the same thing in the one essential point that the furnace was not regenerative, nor even one with forced draft under the grate, but an ordinary furnace with a fire-grate like that of a puddling furnace, so that it would be impossible to maintain in it the high heat needed for making a true steel, fit for springs, rails, or boiler plates. He had made no attempt to give a full history of each of the processes of steel-making, and it would only be in the course of such a history that this furnace of Maudslay, or the early and equally imperfect attempts at the pneumatic mode of steel-making, before the experiments of Bessemer, would call for notice.