Discussion.

Mr. Webster. Mr. WEBSTER said that in testing steel bars by tensile strain the resulting fracture was peculiar. In Fig. 1, it would be noticed

FIG. 1.

Mr. Bell.

that the curve on the top of the bar was very similar to that which was produced when the bar was subjected to impact. Four out of the twelve bars broke as shown in the figure.¹

Mr. LOWTHIAN BELL, M.P., said his experience had been rather in the direction of manufacturing iron than in testing it after it was made, that portion of the business being left to the civil engineer. As a rule, iron manufacturers could form a pretty correct notion of the value of the article they produced without having recourse to elaborate experiments. But he would venture to offer one or two remarks on the mistake which he believed was made when persons spoke of crystalline and fibrous iron. He believed that it had yet to be proved that iron, in its ordinary condition, was fibrous. The structure of iron was necessarily crystalline. Of course engineers were in the habit of judging the quality of iron by the character of the crystals, and they knew well that when iron broke with a face of large crystals, that was a clear indication of inferiority in the quality of the metal. But what constituted the difference between good iron and bad? As a rule, he imagined it was not far from the truth to assume that good iron was pure iron, and that bad iron was impure. It was well known to those who had studied chemistry, that a mixture of materials of different qualities was generally more fusible than any of the substances composing it when taken separately. Iron, associated with 1 per cent. of phosphorus, silicon, carbon, and sulphur, would be more readily affected by heat than chemically pure iron; and it was well known to all practical iron makers, the workmen themselves, that inferior iron was unable to "stand the heat"—to use a phrase well known in the forge. That meant that iron so constituted, approaching more nearly, he would not say to fusion, but at all events to a semi-pasty condition,

¹ Since the reading of the Paper, the Author has had a chemical analysis made of all the samples used in the experiments, the results of which are given in Table X.

in consequence assumed more readily a crystalline fracture in cooling than would a material which had not been softened by the action of heat. The consequence was that there were crystals of considerable size, owing, of course, to the greater mobility of the particles of matter constituting the bar when raised to a high temperature. If, on the other hand, iron was pure, and was not affected by heat to the same extent as impure iron, the opportunity which the metal had of forming large crystals was considerably diminished, and the crystals were small, but in each case there was a crystalline structure. The ordinary mode of testing a bar of iron by fracture, was to make a nick on one side, and to bend it by the action of a hammer; and then commenced what he believed to be the formation of fibre, supposing the quality of the iron to be such as would admit of its formation. If the iron happened to be in large crystals, each crystal presenting a large facet, then, instead of that wire-drawing process going on, there was an abrupt separation, and the iron broke through at right angles to its length—a clean break of crystalline iron. If, on the other hand, the iron was of a ductile character so as to permit the wire-drawing or elongation of its particles, then there was a development of fibre. The late Mr. Brunel had remarked that in the same bar of iron, a fibrous or a crystalline fracture might be obtained at will, ten times, by the application of heat. Mr. Lowthian Bell went further, and said that there might be ten fractures in the bar, five of them crystalline, and five of them fibrous, at the same temperature. If, instead of giving the bar an opportunity of being wire-drawn in the way he had explained, it was nicked all round and then was submitted to a forcible blow, the bar, which under other circumstances would break quite fibrous, would exhibit a crystalline fracture. He therefore inferred that, in iron as it came from the rolls under ordinary circumstances, there was no such thing as fibre. There was a rough mode of testing iron in rolling-mills by nicking and bending it; but it could not be classed with such experiments as had been described, and the question was one that could only be satisfactorily settled by elaborate and careful experiments. Every practical man knew that iron, when it was warmed, showed under those circumstances more fibre than the same iron did in its cool state, and no man who wished to exhibit iron would think of cooling it to 5° Fahrenheit before he asked his customer to inspect the fracture. The fact was, that the molecules of iron acquired a certain amount of mobility as they approached a high temperature, until ultimately they were so mobile as to enter into fusion. With a warm bar of iron the

Mr. Bell.

Mr. Bell. wire-drawing process was effected with greater ease than it would be if the iron were cool. He believed some fractures might be accounted for by coldness in the weather, when the iron was less liable to be drawn; but he agreed with the Author that, unless the force were applied in the direction to which he had alluded, frost would probably not make much difference in good iron. Of course the matter was very different when frost was applied to a railway tire, shrunk on to a somewhat rigid centre, where the mere contraction of the iron greatly increased the strain upon its parts.

Mr. Williams. Mr. EDWARD WILLIAMS said he was a maker of iron and steel rather than an engineer using them. He thought the views of Mr. Bell were very much those of practical iron and steel makers. Long experience in making and testing rails had convinced him that they broke more readily when very cold, than when above freezing point. The results arrived at by the Author were such as might have been expected, namely, that bars in a frozen state would stand a high tensile strain without elongation, and that they would break readily under impact. He agreed with Mr. Bell in reference to fibre and crystals. He believed that there was no fibre in iron until it was produced by bending. This he had proved, at least to his own satisfaction. At Dowlais in his time there was a most efficient testing machine in the shape of a very steep inclined plane, on which there were occasional wild runs that smashed up into small bits all the wagons let loose. For many years he made the axles, tires, and ironwork generally of those wagons as good and as strong as possible, and samples of the several parts were doubled up cold with scarcely a sign of cracking; but the inclined plane accidents broke all up short as though it had been of the commonest quality. The fact he believed to be that, under a force enormously beyond that which iron could bear, it always broke off short; and, on the contrary, where the iron began by bending, there was fibre. Reference had been made in the Paper to two samples of tool steel that gave very different results, and it was stated that the steel was rolled. He knew nothing more difficult to roll than hard steel, because of the delicate heating required, and he was inclined to think that this was the cause of the widely different behaviour of the samples.

Colonel Greck. Colonel P. GRECK said his experience quite confirmed the results obtained by Mr. Webster. The effect of cold on iron and steel, especially on iron, was very great, and he had had ample proof of it in his own country (Russia), where the breakages had been so numerous that it was necessary to use particularly soft steel.

Of late years the Government had required that all rails should be tested at a temperature of about 34° Fahrenheit below freezing point, or 2° below zero. He had had occasion to make many experiments, both in England and in Germany, and he had always found that the deflections under blows were smaller when the rail was subjected to a temperature of zero or below, than under the temperature of 55° or 65° above. He had a record of some tests conducted last year at the Osnabrück Steel Manufactory, with a rather light rail, 35 lbs. to the yard, from a narrow-gauge railway, as shown in the accompanying Table. The rails were cut in 6-feet

RECORD of TESTS MADE at OSNABRÜCK on the 23rd of JULY, 1879, on STEEL RAILS at ORDINARY and at LOW TEMPERATURES.

The height of fall was 1 mètre, and the deflection is in millimètres.

Num- ber of Rails.	Temperature of Rails Tested + $60\frac{3}{4}^{\circ}$ Fahrenheit.				Temperature of Rails Tested - 84° Fahrenheit.				Observations.
	First Blow.	Second Blow.	Third Blow.	Fourth Blow.	First Blow.	Second Blow.	Third Blow.	Fourth Blow.	
	Milli- mètres.	Milli- mètres.	Milli- mètres.	Milli- mètres.	Milli- mètres.	Milli- mètres.	Milli- mètres.	Milli- mètres.	
1	52	107	154	200	44	88	132	..	Weight of rails, 35 lbs. per yard. Length of pieces tested, 6 feet. Distance of supports, 3 feet 6 inches. Weight of monkey, 935 kilogrammes = 18.4 cwt. From each rail two pieces 6 feet long were cut, and both pieces were tested by impact, one at ordi- nary temperature, and the other at a tempera- ture artificially lowered, by keeping the piece in a mixture of ice and salt.
2	48	98	144	191	44	87	127	177	
3	52	105	153	..	46	89	133	..	
4	54	107	155	..	45	91	134	..	
5	52	103	150	201	43	84	128	174	
6	52	102	150	201	45	87	134	..	

pieces. One piece was tested at the ordinary temperature of $60\frac{3}{4}^{\circ}$ Fahrenheit, and another piece of the same rail was frozen in a mixture of snow and ice at a temperature of $40\frac{1}{4}^{\circ}$ Fahrenheit below freezing point. They were then subjected to blows from a weight of 935 kilogrammes, or nearly $19\frac{1}{2}$ cwt., falling from a small height. The first blow on the iron at the ordinary temperature produced a deflection of 52, and the same blow on the other piece a deflection of 44, showing a difference of 16 per cent. A second experiment showed a diminution of from 16 to 20 per cent. in the deflection when the rail was warm. He found that there was

Colonel Greck. no appreciable difference in the results from tensile strain under a cold or a warm temperature. The breakages of steel tires in Russia were 50 per cent. more in winter than in summer; but the greatest breakage did not occur on the day of a severe frost, but usually a day or two after, when there was a change of temperature. That, he thought, must be ascribed partly to the overheating of the tire when it was set on the wheel, and partly to the fact that the permanent way in cold countries was always much worse at the moment of a change of temperature.

Mr. E. F. BAMBER observed that, setting aside the chemical consideration of the subject, and treating it only from a physical point of view, it would appear that certain statements might be made *à priori* with regard to the influence of heat upon metals. Such and such results might be predicated, which experiment should afterwards prove correct. Of this class were the following:— (1.) There was a certain temperature of a material body, which might be called the temperature of normal strength, and this was the temperature at which the molecules arranged themselves during manufacture; it would be under ordinary circumstances that temperature of the atmosphere of the factory at which the material cooled in settling into the solid state. The value of the consideration of such a temperature was that, if the material had little range of strength, it would be most advantageous to employ it in a climate where the average temperature of the atmosphere did not differ widely from the temperature of normal strength. In this connection, it might be added, that if pieces of a structure which had been manufactured at a temperature varying much from the temperature at which they were to be employed, *e.g.* rails made in winter in Great Britain to be used in a tropical climate, the best guarantee of strength would probably be, to anneal the pieces in the climate in which they were to be used, as the action of annealing appeared to be equivalent to re-manufacture. It would probably conduce to economical wear (if market exigencies would allow it) to send winter-made stuff to a cold climate, and summer-made to a warm, when it was inconvenient to follow the action of annealing. (2.) Homogeneous material which had to bear a strain of extension would diminish in strength with a rise of temperature above its temperature of normal strength, in a certain, probably in an arithmetical, proportion, for every degree; while it would increase in strength with a fall of temperature through its range of strength in both directions. That this was so might be made evident by the consideration that the mode of action of heat and of a strain of extension was in both cases the same,

i.e. to cause, or tend to cause, a separation of the ultimate Mr. Bamber. molecules of the substance. (3.) Homogeneous material, which had to bear a strain of compression, would increase in strength with a rise of temperature above its temperature of normal strength, while it would diminish in strength with a fall of temperature through its range of strength in both directions. In this instance the physical and mechanical actions were in opposition; hence the strength was increased when they were thus opposed, and diminished when they worked together. (4.) It was not easy to predicate the effect of variation of temperature upon homogeneous matter under transverse strain, as it would be a function of the two preceding cases. If the figure of the piece had been designed for greatest strength, then, where most material had been thrown into the lowest member, the piece would increase in strength with a fall in temperature, and would diminish in strength with a rise of temperature above its temperature of normal strength within its range of strength. But when the material of the piece had not been so arranged, and remembering always that the reliable strength of a piece was that of its weakest part, a fall of temperature would certainly diminish, and a rise of temperature possibly increase its strength. This might be considered to be the case of reversible rails, made with the upper and lower flanges of the same size and section. Thus, if, in the case of a rail, it was assumed that the upper flange and web together were to the lower flange in the proportion of 5 to 4, which was about the proportion of the resistance of its material to extension and compression, then the neutral axis would be at the junction of the lower flange with the web, a fall of temperature would diminish the strength of $\frac{5}{9}$ of the material under compression, and increase the strength of $\frac{4}{9}$ of it under extension. If the rail should be considerably worn before reversal, the neutral axis might rise so high as either to modify the effect of fall of temperature below that of normal strength, or even to produce the effect of increasing the strength of the rail, by bringing a larger portion of the material under extension than under compression. (5.) Heterogeneous material, or homogeneous material, which had only a small range of strength, would be affected considerably by variation in temperature, and its ultimate effect could not be predicated, as the molecules would be easily removed from their original position, and it was not possible to indicate what new position they might take up. For instance, in a piece of cindery iron the effect might be to move the molecules of cinder out of the line of direct strain, substituting in their place metal, and thus to increase the strength of the piece, or by

Mr. Bamber.

producing the opposite effect, to diminish it. (6.) With regard to the influence of variation of temperature upon resilience, or that property of matter which resisted vibration, &c., it might be stated that, although pieces were made of double strength to resist dynamic as compared with static strain, temperature variations would influence both proportionally. For pieces under vibration were under a certain amount of play, and as variation in temperature increased or diminished this amount of play, setting a physical as well as a mechanical influence at work, and thus bringing the material more quickly within the limits of its proof-resistance to vibration, it would appear that the same allowances must be made for the influence of temperature for dynamic strain as had already been indicated in the case of static strain, even after the material of the piece had been doubled. (7.) All the existing moduli of resistance to extension, compression, and bending, and the moduli of elasticity and resilience in regard to these tests of strength, could only be considered as of mean or average value, as the temperatures at which the experiments had been made, from which the moduli had been deduced, were not allowed for. Since, in the latter case, when the moduli of elasticity were under consideration, if the intensity of the stress were kept constant (when testing homogeneous material within its proof strength), the extension would be found to vary with the temperature, to increase as it rose, and to diminish as it fell; whilst if the proportional elongation or measure of the strain were kept relatively the same at all temperatures, the intensity of the stress would be found to vary with the temperature, and as the modulus was the ratio of the intensity of the stress to the proportional extension, it must also vary with the temperature. That the extension of a piece of material would increase with a rise of temperature, and diminish with a fall of temperature, could be made mentally evident by supposing a current of electricity to be passing through and heating a wire under extension, when it would evidently be elongated more under the same weight than another exactly similar piece kept at atmospheric temperature. (8.) The object of these remarks was to show the necessity of making the thermometer the constant companion of the testing machine, and perhaps also a necessary appendage in the works. In experimenting on ultimate or proof strength, instead of attempting to make the experiments at certain fixed temperatures, it would probably be found both more easy, satisfactory, and economical, to conduct them at atmospheric temperature, as this varied with season and climate, and thence to deduce curves from which

formulæ and moduli, at a certain fixed or standard temperature, Mr. Bamber. might be calculated.

Mr. C. DOUGLAS FOX remarked that in North America, where he Mr. Fox. had had some experience, there was a great variation of temperature between summer and winter, the temperature sometimes falling in winter to 30° or 40° below zero. His experience bore out that of Colonel Greck as to the effect of low temperature producing a large number of fractures, especially of steel tires, where the skeletons were rigid cast iron or wrought iron; but that did not apply to the cast iron used for wheels in America. The wheels made of that iron stood marvellously well, even under very severe temperatures. Similar experiments would be valuable if applied to bars and other objects of a larger size. The size in the cases mentioned by the Author was so small that he thought there was a liability to local error, so that the results would not be so satisfactory as they otherwise might be. The number of the test bars in each case was also comparatively small. Experiments with full-sized bars, and at a lower range of temperature, would be of great use to those who had to deal with structures subjected to heavy shocks under different temperatures. He thought that in the experiments there ought to be a range of from 30° below zero up to at least 120° Fahrenheit above. Even in England during the winter of 1879-80 there had been lower temperatures than those mentioned in the Paper, and in the summer the temperature occasionally rose to 120° in the sun.

Mr. W. ATKINSON said it was on record¹ that in one winter four Mr. Atkinson. thousand rails broke on the Grand Trunk railway, Canada, when the line was about 1,400 miles in length. With regard to the question of extremes of temperature, a Paper had been read at the Literary and Philosophical Society of Manchester in 1871 by Mr. Brockbank, giving the results of experiments such as Mr. Fox desired.² It was found at the Darlington works that the rails contracted for for the East Indian Railway Company in 1869, which were of a very superior quality, would not stand the tests in the colder weather; and the company naturally wished to ascertain whether it depended upon any defect in the iron or upon any peculiar conditions of the temperature. Ten rails were taken out of a batch of one thousand, and four of them were warmed to a temperature of 120° , each of which was tested with a falling weight of

¹ *Vide* Minutes of Proceedings Inst. C.E., vol. xxviii., p. 404.

² *Ibid.*, vol. x., p. 77.

Mr. Atkinson. 2,000 lbs. from a height of 5 feet, then with a second blow from a height of 5 feet, and then with a third blow from a height of 7 feet. Not one of the rails broke. Six rails were next taken at a temperature of 26° —the temperature of the atmosphere at the time. One of them stood the three tests, four of them broke with the second 5-foot blow, and one broke with the first 5-foot blow. The other experiments entirely confirmed those given by the Author. Mr. Brockbank's Paper was followed by a record of some experiments by Sir William Fairbairn on plate iron and cast iron, in which it was found that between the temperature of zero and 400° with plate iron there was very little difference in the tensile strain, but with bar iron with the higher temperatures a much higher result was obtained—in some cases as much as 39 tons per square inch. Mr. Brockbank tried the experiment of impact, because he felt satisfied that tension and torsion were extremely uncertain on account of the heat acquired during the operation.

Mr. Mosse. Mr. J. R. Mosse was for several years engineer and manager of the Nova Scotia Government railways, and during that time there had been some very severe frosts. In January there was generally a severe snowstorm with perhaps 2 feet of snow on the ground. In the middle of the month there would come a thaw with a south-west wind for a couple of days, followed by rain in torrents for ten hours; then the rain would suddenly cease, and the wind veer to the north, and the thermometer, in perhaps six or eight hours, would go down to zero. A large portion of melted snow from the slopes of the cuttings would come upon the rails, and in extreme cases ice 3 feet thick would sometimes be found on the rails on the following morning; so that the only way of getting along was to cut the ice away with an axe. During that time the rails would rise 6 inches in a night, and he had sometimes known the rails to rise as much as 1 foot in the same time. The chairs were of the ordinary size, and the rails were fitted to them by Ransome and May's compressed keys of elm or oak. The number of chairs that broke in the winter was excessive, he believed as many as six thousand per annum on a length of 90 miles. Eventually that system of road was discontinued, and the American system was adopted. The tires of the wheels used to crack, though, strangely enough, never at the time of severe frost, but soon after the thaw commenced. On one occasion there were three engines in the running shed, which it was believed would suffice for any emergencies, but one night, after a severe frost, when the weather began to moderate, a Low-moor tire of each of these engines cracked. He did not remember

a single case of the chilled cast-iron wheels having cracked. The Mr. Mosse.
chairs broke both in the lip and in the bed, chiefly in the latter;
and as they did not thus break in mild weather, it would seem
that cast iron was rendered peculiarly brittle by severe frost.

Mr. J. A. LONGRIDGE thought the Author had drawn a wrong Mr. Longridge.
inference from his experiments upon impact. In Table VI.,
with reference to the iron bar subjected to impact, he gave the
average ultimate fall as 5 feet 6 inches, and the average deflection
as 0.71 inch, taking those averages by adding together the whole
of the final falls and deflections. That, he thought, was not
the way in which such averages should be taken, particularly
when there was such an enormous discrepancy in the bars as
to the actual weight which broke them, and the deflections.
He thought it would be better to take the amount of work done
upon each bar—to take all the falls multiplied into the weight,
which would give the work done upon the bar, and then, for each
bar, to ascertain how many foot-pounds it required to give, say,
 $\frac{1}{100}$ inch deflection. He had treated the Tables in that way,
excluding from the bars in Table VI., at 5° Fahrenheit, Nos. 8
and 9, which appeared to have been abnormal in their condition,
not at all corresponding with the other bars, and he found the
following results: With iron bars at 50°, in order to get $\frac{1}{100}$ inch
deflection, it required 1,203 foot-pounds, and at 5°, 1,170 foot-
pounds, so that the work done to give a certain deflection was
less in the cold than in the hot bar, which was the reverse of
the conclusion arrived at by the Author. Again, taking steel bars,
Table VII., and excluding for the same reason Nos. 3 and 5 at
50°, and Nos. 8 and 11 at 5°, he found that in order to get $\frac{1}{100}$
inch deflection at 50° it required 4,236 foot-pounds, whereas at 5°
it only required 4,061 foot-pounds, again showing a less amount
of work to give deflection in the cold bar than in the hot. In the
case of malleable cast iron, Table VIII., he found that with
the bar at 50° it took 1,652 foot-pounds $\frac{1}{100}$ inch deflection,
and at 5°, excluding bar No. 10, 1,570 foot-pounds. So that in all
cases, measuring by the work done, the deflection was produced
with a less amount of work in the case of the cold bar than in
the case of the hot bar. The conclusion arrived at by the Author,
by taking an average, was that the reduction of flexibility was
about 18 per cent. in cast iron; 17 per cent. in steel, and 15 per
cent. in malleable cast iron, which was at variance with what
Mr. Longridge considered to be the right deduction to be drawn
from the experiments. The annexed Tables showed the work
done, and deflection in each bar.

Mr. Longridge.

IRON (TABLE VI.).

Temperature 50° Fahrenheit.				Temperature 5° Fahrenheit.			
Number of Bar.	Work Done in Foot-Pounds.	Final Deflection in $\frac{1}{100}$ Inch.	Work Done for $\frac{1}{100}$ Inch.	Number of Bar.	Work Done in Foot-Pounds.	Final Deflection in $\frac{1}{100}$ Inch.	Work Done for $\frac{1}{100}$ Inch.
1	340	28	1,213	7	520	52	1,000
2	1,020	76	1,343	8*	200	52	390
3	1,260	98	1,286	9*	340	38	890
4	540	52	1,040	10	560	44	1,275
5	1,260	110	1,145	11	800	70	1,143
6	760	64	1,188	12	1,260	92	1,261
Average work done for each $\frac{1}{100}$ inch . . .			1,203	Average work done for $\frac{1}{100}$ inch, omitting Nos. 8 & 9			1,170

STEEL (TABLE VII.).

1	1,080	25	4,320	7	1,640	34	4,822
2	1,900	46	4,134	8*	600	12	5,000
3*	560	22	2,550	9	1,700	40	4,250
4	3,240	80	4,050	10	4,400	118	3,774
5*	4,000	160	2,500	11*	1,380	20	6,900
6	800	18	4,440	12	2,440	72	3,400
Average work done for each $\frac{1}{100}$ inch deflection, omitting (3) and (5) . . .			4,236	Average work done for $\frac{1}{100}$ inch, omitting (8) and (11)			4,061

MALLEABLE CAST IRON (TABLE VIII.).

1	1,800	118	1,526	7	1,400	82	1,708
2	1,320	90	1,466	8	1,180	81	1,454
3	1,200	82	1,450	9	1,180	82	1,450
4	1,840	98	1,870	10*	2,200	76	2,900
5	1,880	96	1,950	11	1,546	92	1,670
6	..	..	..	12	..	..	..
Average work done for $\frac{1}{100}$ inch deflection . . .			1,652	Average work done for $\frac{1}{100}$ inch, omitting (10) . .			1,570

SUMMARY.

—	50°.	5°.
Iron	1,203	1,170
Steel	4,236	4,061
Malleable cast iron .	1,652	1,570

* Excluded in the calculations.

Mr. E. MATHESON remarked that the Author had made some Mr. Matheson. useful experiments in regard to plain rectangular bodies, but it was a question how far those experiments elucidated what was directly interesting to engineers—the fractures that took place at low temperatures with different structures. Bars, which were free in all directions, such as those which the Author had tested, did not represent the structures of cast iron, wrought iron, and steel, which occasionally broke in cold weather. Most engineers, experienced in the manufacture of such structures, would agree that it was owing to some self-contained or initial strain in the structure from the beginning, and the experiments which the Author had described showed that the results he obtained were not sufficient to account for those fractures. Taking the worst cases of cast iron, the Author recommended engineers to allow 25 per cent. more margin of strength for the effect of cold. But as in cast-iron structures it was the habit to have a considerably greater margin, there must be some other cause for fracture, and it was due, as most founders knew, to inequalities in cooling, a self-contained strain being left in the casting without any immediate sign of it; but which, when intensified afterwards by greater cooling, caused the casting to break. So great were the effects from this cause that he had, when in Russia, heard of columns breaking simply because one side was exposed to the rays of the sun, while the other was buried in snow, a difference in temperature perhaps not greater than those the Author had had in his experiments. There was another point that he thought worthy of some elucidation. The Author had stated that all the specimens had been planed. Mr. Matheson thought that in destroying the skin of the iron the value of the experiments was greatly reduced, because in castings the skin added to the strength, and yet often had that self-contained strain in it which caused the fracture; and in wrought iron, too, the skin, though differing less with the interior than in cast iron, also had its value. If the specimens had not been planed he thought different results might have been obtained. Mr. Lowthian Bell had stated that wrought iron had no fibre in the sense ordinarily understood. But surely if the particles had been elongated, they were elongated in the process of rolling, and were in that condition when they reached the hands of the smith. All those experiments seemed to point to this, that any iron that had been recently in a fluid condition was affected much more by cold than what had gone through intermediate processes, such as puddling. There was therefore little difference in wrought iron, but there was a great

Mr. Matheson. deal in cast iron. That led to another inquiry, how far steel made by modern processes was affected by cold? because the intermediate processes between the ingot condition and the finished bar were much less than those between cast iron and rolled iron. No doubt steel, when made into ingots 8 or 10 inches wide, had much larger crystals in the middle than towards the outside; and it would be interesting to know how far those inequalities were altered or removed, or equalised by the intermediate processes of cogging, or hammering, or rolling. It had been stated some time ago, at a meeting of the Iron and Steel Institute, that ship plates broke in a remarkable manner, from causes not thoroughly understood;¹ and it was just possible that that might have been because the inequalities in the crystals had not been removed in the intermediate processes since the metal was in a fluid condition, and because the plates had some self-contained strain which became intensified by cold. It had been mentioned that the cast-iron railway wheels used in America did not break, though railway chairs broke in large numbers. It should have been stated that the wheels were made of unusually good iron, and so cooled as to be partially annealed, and therefore to some extent decarbonised and made into malleable cast iron, and these no doubt were the reasons why they did not break.

Mr. Adamson. Mr. D. ADAMSON considered that, in carrying out a series of experiments on the strength of metals at variable temperatures, engineers ought to try and get at some fundamental law, whereby the metal had arrived at its greatest density, and was then subjected to an increase or diminution of strength as it expanded or contracted from that point by change of temperature. From the investigations carried on by the Author, it appeared to be supposed that there was a peculiar property about wrought iron and steel like that of water—that when the metal was subjected to a temperature of 32° it assumed other conditions, and altered as the temperature went downwards. He hardly thought that anything of that kind took place. If any alteration were produced by heat it must be connected with the composition of the metal itself. Was there anything in the Paper that would be a guide even with regard to mechanical action? The mechanical tests were not such as he should have preferred. A piece of iron at its normal temperature carried a certain load, and a certain strain upon it produced a permanent disorganisation in the particles called permanent set; and it might certainly be expected that, by sub-

¹ *Vide* "The Journal of the Iron and Steel Institute," 1879, p. 265.

jecting the metal to a lower temperature, the process of contraction would continue, and that the first valuable indication of which one would desire to have a record would be that of permanent set, and not of total breaking force. Besides that, accurate tests with malleable metals showed that the maximum load would be carried somewhere between $\frac{5}{8}$ and $\frac{3}{4}$ of the total elongation in moderate lengths, say 8 or 10 inches. There was no indication, however, in the Paper that any of the metal tested by tensile strain possessed that property, but the specimens simply broke down with a crash when the full load was laid upon them, as was the case with bad iron, or rather a compound of iron and dirt, but not with good malleable iron or mild ingot iron. Mr. Adamson.

The best malleable iron that could be obtained commercially in this country was composed of about 99·8 per cent. of metallic iron. It possessed great ductility and exceedingly great malleability, with high welding power; it was not subject to disaster or distress by a rapid or great change of temperature. It had only a low carrying power; it took a permanent set at from $10\frac{1}{2}$ to 11 tons per sectional inch when tested in square or round bars equal to 1 square inch in area, which was a fair guide and not likely to lead to trouble by errors in calculation. It would only carry a total load of about 19 tons; but what was its behaviour while perishing? It ultimately broke down with just about the same force as produced the original permanent set, $10\frac{1}{2}$ tons. Now it would be easily understood that a highly malleable metal of that sort, at a temperature of 32° , as recorded in the Paper, would not be subjected to any serious disturbance, either in its carrying power, or in any other of its vital properties. The common commercial irons of the country imbedded pure metal to the extent of about $98\frac{1}{2}$ per cent., the rest being alloying elements of a noxious and injurious character, phosphorus in the first place, silicon in the second, and sulphur in the third. Irons so alloyed possessed the power of carrying a greater tensile load than the exceptionally pure malleable iron; they had, at least in bars, a carrying power of from 20 to 22 tons per square inch. The permanent set was at a much higher range than in the pure metal, taking place generally at between 15 and 16 tons, while there was a diminution in the power of elongation of from 30 per cent. to at most 20 per cent in a length of 10 inches. It could hardly be believed that heat would affect alloyed iron in the same way as it affected pure iron. It might affect iron more that was mechanically mixed with a considerable amount of cinder, but it certainly did not affect ingot

Mr Adamson. irons free from cinder, and only alloyed with substances that as a rule gave strength and endurance. He referred to such metals as the Siemens-Martin and the Bessemer mild ingot irons, possessing 99 $\frac{3}{4}$ of metallic iron, having less alloy than was mentioned in the Tables. The Bessemer metal there illustrated was an ordinary rail metal of moderate good properties, but rather too hard for its purpose. The carbon, he thought, should be reduced $\frac{1}{10}$ per cent. It was represented at 0.4 per cent. If reduced to 0.3, and the manganese to 0.75, the other alloys being as low as could be got consistently with economy of manufacture, a good rail metal would be obtained. If the tests had been such as could be depended upon a very different notion of the carrying power of the metal from its permanent set would be deduced. It was, no doubt, true, as stated, that there was an elongation in 6 $\frac{1}{2}$ inches of something like 21 per cent. According to his experience, any metal that would elongate in a sectional inch over 12 per cent. in a length of 10 inches would break down, as a condition of its malleability, at a much lower stress than the maximum load that it would carry at another period of the operation; the behaviour of the rail metal was in accordance with its composition. It carried a total maximum load of about 46 tons, and at 50° Fahrenheit it also carried about 46 tons, so that in reality cold produced no injurious effect on it. But he presumed, following the views taken by Mr. Bamber, that as there was an increased crystalline tendency by the abstraction of heat, there would be when starting at lower temperatures, a greater resisting power; and as the work went on, and heat was developed in the material, the metal would arrive at nearly the same extension, and carry an ultimate load quite as large or larger than if tested at a temperature 30° higher. Hence the only influence that, in his opinion, could be produced upon either mild steel or the better class of wrought iron, was that the continued contraction from 32° down to 5° followed the same law as if the tests had been made from 32° upwards in the same proportion. It was true that one could not expect such a result to be established with a piece of cast iron or porous metal of a different composition, because in that case the composition was more seriously affected by cold, than in the case of malleable metals. Pig iron obtained in this country, containing manganese to the extent of 2 per cent., ran freely when molten; it worked well with a cutting tool, but ceased to be practically "fileable." When carrying manganese to an extent of making it Spiegeleisen, it lost its carrying power, it could not be cut, and the hardest drill produced little or no effect upon its surface. Variable compounds

could hardly be expected to possess the same properties and Mr. Adamson. endure in the same ratio, with such a difference in their natural constitution. Every matter connected with mechanical tests should include, in conjunction therewith, a full record of the composition of the metal and its behaviour in standard bars. From what had been said, it might be imagined that a bar of iron had such singular and unmanageable properties that there was neither law nor order about it. It would be a great misfortune if such were the fact. A crystalline appearance and a fibrous appearance might, indeed, partly be obtained at will; but the operations through which the metal was put developed other conditions, and it could not be expected that the same equable appearance would follow when a bar was so differently treated. Taking the case of a billet, 4 inches square rolled down to a $\frac{1}{2}$ -inch rod with its particles elongated in one direction, and with cinder interlaced and laid down by the side of those particles, metal so elongated supported a much higher strain when tested in a longitudinal direction; but he was not aware that there was any guide as to how such a bar would retain an approximate power of resistance in a lateral direction, because the batch of cinder that might appear to the eye $\frac{1}{8}$ -inch in diameter in the larger piece of metal, would probably occupy a length of 2 or 3 inches in the elongated metal. Such must exist, only the elongated metal having thrown off cinder from its surface by having probably exposed from sixty to seventy times the surface originally occupied by the 4-inch bar, a purer iron was obtained, purer from cinder, probably not purer from alloys. How, then, would cold affect a flat bar subject to vibration, when at the same time carrying a moderate tensile load? According to his experience in bursting bars with a drift, it was necessary to increase the area at the end, assuming that a hole was put through the bar, and that it was then drifted with a punch, or a series of punches, advancing 1 in 20; the length of the bar must be increased at the end, to resist the strain that the two side portions should endure, to at least three times the sectional area of the side metal. In other words, if a hole 1 inch in diameter were drilled through a bar 3 inches wide, and then steel pins driven through the hole until the bursting point was reached, both longitudinally and through the narrow section of the side, according to his experience at least three times the metal would be needed in the longitudinal direction to resist the action of the punch that would be required to resist it on the sides, the difference being that the cinder lay chiefly in one direction. He thought that frost would affect such material much more in a

Mr. Adamson. lateral than in a longitudinal direction, as the exposed cinder had power to absorb moisture. He had only once planed an iron rail, to compare it with a steel one, and in that case the streaks of cinder in the longitudinal direction were so manifest as clearly to show that a large portion of the metal was merely glued, as it were, together by the interposed cinder, and had, in large sections of its surface, no metallic contact. Hence the breaking down of the old iron rail when subject to a rolling load must have been expected, if it had been examined closely after planing its upper surface to see how it was constructed. The steel rail, on the other hand, had no cinder; it had no interlaced earthy matter. Hence it endured a heavy rolling load, and did not break down either on one side or the other. Engineers were bound, he thought, to pay great attention to the composition of the metal they desired to use for particular purposes. Mild steel boiler plate and ship plate now in common use contained $\frac{1}{10}$ per cent. of carbon, and $\frac{1}{3}$ per cent. of manganese. If it was desired to get a material of double the carrying power, raised say from 29 tons to 58 tons, it was only necessary to increase the alloying elements about 1.2 per cent., taking care that the quantity of carbon was double the manganese, or say 0.5 of carbon and 1.0 per cent. of manganese. The material would then be one with a carrying power of nearly 60 tons to the square inch, and yet one that could be elongated nearly as much as the best plate iron produced in the country. He thought that, for all practical purposes, as bridge building, steel, especially of the higher malleable class, was the proper material to carry heavy weights with security, because in its longitudinal and in its lateral directions, according to his experience after some hundreds of tests, its strength did not vary more than 2 per cent. Bar iron, on the other hand, did not possess lateral security, but it lost, he felt sure, as much as 66 per cent. of its carrying power if pulled transversely to the grain of the material over some portions of its length in exceptional cases. It was certainly impracticable to pull a $\frac{1}{2}$ -inch or a 1-inch square bar transversely through the material, but it was not impracticable to pull transversely a 4-inch or a 6-inch flat bar, and develop all the facts that he had stated as requisite to be known, as a guide in the application of metals to particular purposes at all ordinary temperatures.

Dr. Siemens. Dr. C. W. SIEMENS observed that the impression produced on his mind by the Paper was that it dealt with a limited question purely for a practical purpose, and that within those limits the results were much as might have been expected. The Author had dealt

with various metals at the limits of 5° and 50° Fahrenheit. The Dr. Siemens. limit was a narrow one, and it could not be expected that the results of the experiments could be such as would lead to a general conclusion regarding the effect of temperature within wider limits on materials of that description. There could be no doubt that tolerably pure material must be stronger at the lower temperature than at the higher, because the last particles of the material were closer in contact at the lower than at the higher temperature. For a similar reason the material would resist a blow rather less at the lower temperature than at the higher. But he agreed with the observations of previous speakers that no influence was produced by those degrees of temperature on the absolute structure of the metal. It was quite out of the question to expect to change the metal from a crystalline to a fibrous condition, and back again from a fibrous to a crystalline condition, by lowering or raising the temperature a few degrees. The Paper, though valuable of its kind, was suggestive chiefly of what had been omitted to be taken into consideration; for instance, the Author had not in the first instance given a chemical analysis of any of the materials he had employed, though that omission had, he believed, since been remedied; but at any rate he had drawn no conclusion from such analysis. Nor had he considered the elastic limit of the materials, although to an engineer the elastic limit and the condition of the material up to the elastic limit were more important than the breaking strain. Engineers did not want to break down with their materials; but they wanted to see to what extent it was safe to use them. It would have been most interesting if the Author had given the elongation due to the rate of elastic extension at different temperatures. It was Mr. Barlow, the President, who first established by experiments that steel of various tempers was equally strong up to a certain limit. In other words, that if a bar of very mild steel having a sectional area of 1 square inch pure iron, as Mr. Adamson had correctly described it, was weighted with a ton weight, the same absolute extension would ensue as in weighting a bar of very hard steel of 1-inch sectional area and of the same length, and so on, each ton additional weight producing the same amount of extension in both bars until the elastic limit of the mild metal was reached. These results which he must confess at first appeared hardly credible to him, had since been fully confirmed by experiments of his own. It was a remarkable fact for engineers to consider that in constructing a bridge it came to the same thing whether they took mild steel that would break with 28 or

Dr. Siemens. 30 tons strain to the square inch and would come to its elastic limit at about 15 tons, or whether they took a material that would find its elastic limit only at 25 or 30 tons, and would break at 50 or 60 tons; therefore the advantage that would be derived from the hard and strong material would only be met with after exceeding the elastic limit of the milder steel. He was not aware of any experiments giving the influence of heat upon the coefficient of elastic extension of those materials. If it should so happen that a bar of iron or steel at the temperature of 5° would follow a less rate of elastic extension than a warmer bar, a steel bridge that was ordinarily loaded to a strain of 8 tons per square inch, when it cooled down, would contract rather more than in the ratio of simple thermal contraction; and afterwards, if the bridge changed from the one temperature to the other, it would alter its length in all its members due to the two ratios of absolute contraction and a contraction due to a different rate of extensibility. It would be interesting to know how the modulus of elasticity was influenced by temperature. It was often thought that metals were fatigued by strain; and in the case of inferior classes of iron, it might be true that, if a bar were strained a little beyond its elastic limit, its fibres would be ruptured to some extent, and the material would receive an injury; but in the case of high-class material, such as mild steel, there was an absolute advantage in straining it beyond the limit of elasticity. It was possible, by careful manipulation, to raise the breaking strain of a bar of a given sectional area to a remarkable extent by gradually accumulating it to the strain. By taking a bar of mild steel of 1 square inch sectional area and loading with a weight of, say, 15 tons, and leaving the weight on twenty-four hours, it would be found that both the elastic limit and the breaking strength of the bar were materially increased. That was a quality of high-class material which was, perhaps, not yet thoroughly understood, and which certainly gave a great factor of safety in favour of its employment. The chemical analysis, which had not been dealt with in the Paper, was of great importance in reference to the object the Author had in view. The Author had spoken of certain brands of material—of certain makes, for instance by a firm like Krupp's. Now, he happened to know that Krupp made steel by at least three different processes, and it was difficult to say by what process the particular bar was produced that had been referred to in the Paper, nor, if the process were known, could it be affirmed what were the materials used in its manufacture. If it was a cheap material, probably Mr. Krupp would have put in

some cheap iron, and the result might be a bar that would give a very fair absolute strength, but would give way to a bending and breaking test before its time; whereas if he knew that the metal would be tested very severely, he would produce something that would be capable of resisting very high impacts. Some years ago Dr. Siemens made a series of experiments on the power of materials of particular composition to resist blows at different temperatures. His object was to see what effect phosphorus had on mild steel with regard to that quality. From those experiments he had arrived at the conclusion that the power to resist blows in mild steel, at any rate, was dependent almost entirely on the presence or absence of phosphorus. It was well known that phosphorus might be counteracted in the steel, to a great extent, by the addition of manganese; but let the steel be cooled down to freezing point, or below, and the metal would break short: whereas true mild steel, containing only traces of phosphorus and of manganese, would be affected to a very slight extent by any depression of temperature to which it might be subjected. He believed that pure metal was influenced very little by changes of temperature, whereas metal containing foreign matters, and particularly phosphorus, would be influenced to a great extent. It would, therefore, be erroneous to draw general conclusions from such experiments as were given in the Paper. The Author seemed to have attempted to find out results without the chemist; but the chemist now-a-days would put his finger into everything. If they called a thing iron, he would call it a mixture of iron, phosphorus, sulphur, silicon, carbon, and other earthy materials, and it was useless to try to do without him. They must work hand in hand with him in order to arrive at such results as would guide them safely in their practice.

Prof. A. B. W. KENNEDY desired to bear witness to the great trouble which the Author must have taken, not only in making the experiments, but in preparing the means for making them; and he was the more sorry on that account that the results obtained did not appear to be, qualitatively at any rate, of any great value. The small percentage of difference with reference to tensile strength (page 176), Prof. Kennedy thought should be discarded, as being quite within the irregularities of such experiments, so that the results might be regarded as practically identical. But in the matter of impact especially, where the Author found the greatest difference, he agreed with Mr. Longridge that the results were hardly given in a form that was fair. In regard to the five

Prof. Kennedy. experiments made on impact with wrought iron, it might be observed in the first place that the experiments were begun at a great number of different falls, 4 feet, 3 feet, 5 feet, &c., and then the average deflections were measured in the way Mr. Longridge had mentioned, simply by taking an arithmetical mean of the deflections to the last fall before fracture. It was quite a different thing, however, to let a weight fall from a height of 4 feet 6 inches on a bar and break it off, and to let the weight first fall from a height of 1 foot, then from 2 feet, then from 3 feet, and so on. The results in such a case could hardly be compared. It was impossible, for instance, to compare bar No. 1, which had received a blow with a fall from 4 feet, and then a fall from 4 feet 6 inches, with bar No. 3, which began with the weight falling through 3 feet, and went up by 6 inches at a time to 6 feet. In some other cases, the difference was even more marked. In one case of malleable cast iron, the fall began at 1 foot, and in a number of other cases it began at 4 feet. He had worked out the following results:—in the wrought-iron bars, the average deflection at 5 feet, if the first fall was 5 feet or 4 feet, was 0·49 inch; the average deflection at 5 feet, if the fall began at 1 or 2 feet, was 0·62 inch. With steel bars (Table VII.), the average deflection at 4 feet, where 4 feet was the first fall, was 0·07 inch; the average deflection at 4 feet, when the experiments began at 1 foot or 2 feet, was 0·13 inch. He did not think that those quantities were comparable, or that an average could fairly be taken from them. To show how discrepant the results were, it was only necessary to refer to Table VIII. If here the Author, instead of having six specimens at each temperature, had had only five, and if in one group No. 5 had been left out, and in the other No. 11, the result calculated in the same manner as in the Paper would be—average ultimate fall warm, 6 feet 6 inches; cold, 7 feet 10½ inches; and average ultimate deflection warm, 0·38 inch; cold, 0·55 inch; results entirely opposed to those given in the Table. It was really very much a matter of chance in what way the average came out, so that he feared the experiments could not show much as to actual results. As to the question of heat during testing, he had not personally experimented with reference to the point mentioned by Dr. Siemens, but he should be happy to bring forward one or two results as to the difference made by temperatures in the modulus of elasticity. There was one point, however, with regard to heat during testing which was hardly sufficiently remembered—that in its first or elastic stage, when a bar of iron was stretched, it cooled if left to itself. The converse proposition was tolerably

well known, that if a piece of india-rubber was stretched, and put to any sensitive part of the skin, as it was extended a rise of temperature would be distinctly felt, india-rubber being a material that contracted when heated. The converse action took place, as explained in books on physics, with a piece of iron, or any material which expanded when its temperature was raised. He had recently made a number of experiments with a view of examining this action. He had placed a small thermopile in front of the bar tested, and then read off the alteration of temperature by the aid of a reflecting galvanometer, the result being that up to the limit of elasticity there was always a small fall of temperature when the load was put on, and a small rise when the load was removed. He had done the same thing with an air-thermometer, but the results were so small that they could not be measured with the means at his disposal.

He had made some experiments as to whether moderate changes of temperature measurably affected the modulus of elasticity, and if so to what extent. He used for the purpose a round bar of Landore "S" steel (carbon 0.18 per cent.), turned down to a sectional area of 0.2 square inch, and ascertained the extensions between two points on the bar 54 inches apart. For measuring the extensions he had used the mirror apparatus alluded to by Mr. Webster, which had been described in "Engineering" some months ago, and which measured with accuracy quantities as small as $\frac{1}{100000}$ inch. Between the 54-inch points the bar was enclosed in a piece of 1-inch brass tube, made watertight at each end by india-rubber packing. This tube was encased in wood to reduce radiation as much as possible, and means were provided for running a current of hot or cold water or brine through it. The temperature of the water was measured by a thermometer placed in a short branch at the centre of the tube. Two sets of experiments were made, as given in Tables I. and II. (see next page). In the first experiments (Table I.) the piece was loaded successively with weights amounting to 5000, 10,000, 15,000, 20,000, 25,000, and 30,000 lbs. per square inch, the extension being noted at each load, and the temperature as often as was necessary during the experiment. From the observed extensions at these loads the modulus of elasticity (E.) was calculated in each case, and it would be seen from the Table that a distinct, although small, decrease occurred in it with the rise of temperature. The only special difficulty in making the determinations arose from the necessity of keeping the temperature constant during each. Even a small change of temperature during

Prof. Kennedy.

TABLE I.

Series.	Number of Experiments in each Series.	Highest and Lowest Temperatures, Fahrenheit, used in the Series.	Mean Temperature, Fahrenheit.	Mean Change of Temperature, Fahrenheit, during each Experiment.	Mean Extension per 1,000 lbs. per Square Inch.		Modulus of Elasticity. E.		Increase or Decrease of Extensibility as Compared with Extensibility at 60° Fahrenheit.
					On Whole Length 54 Ins.	Reduced to 10 Inches Length.	Pounds per Square Inch.	Tons per Square Inch.	
A	11	37.5—48.0	41.5	+0.04	1.610	0.298	33,550,000	14,980	{ 1.0 per cent. decrease.
B	9	57.0—64.5	60.3	..	1.625	0.301	33,220,000	14,830	
C	5	168.0—180.0	175.0	+0.25	1.653	0.306	32,680,000	14,590	{ 1.7 per cent. increase. 3.0 per cent. increase.
D	5	200.0—201.0	200.6	-0.36	1.674	0.310	32,250,000	14,400	

any experiment affected all the readings—independently of the particular temperature which was changed—and in some instances sufficiently to disguise altogether their results. Out of forty-three experiments, he had rejected all those in which the change of temperature during the experiment exceeded 1° Centigrade, and he had given in the Table the average change of temperature in the thirty remaining ones, which was slight.

The second set of experiments (Table II.) was made to compare

TABLE II.

Series.	Number of Double Experiments in each Series.	Highest and Lowest Temperatures, Fahrenheit, used in the Series.	Mean Temperature, Fahrenheit.	Mean Change of Temperature during each Experiment.	Mean Extension under a Load of 30,000 lbs. per Square Inch.	
					In 54 Inches.	Reduced to 10 Inches.
E	12	41—55	48.2	0.0	48.00	8.88
F	15	185—196	0.191	+0.32	48.94	9.06

the extension of the bar at or near its limit of elasticity at different temperatures. In each experiment readings were taken first at zero, then with a load amounting to 30,000 lbs. per square inch, and lastly again at zero. The final reading was seldom exactly the same as the first, on account of small variations of

temperature, but to eliminate any errors arising from this cause, Prof. Kennedy. the extensions given in the Table were those corresponding to a zero which represented in each case the mean between the initial and the final length of the bar. The average difference between these two lengths was only $\frac{4}{10000}$ inch (plus or minus), and amounted to as much as $\frac{1}{1000}$ inch only in two cases. Out of thirty-two experiments five were rejected in which the change of temperature during the experiment had exceeded 1° Centigrade, and the Table gave the result of the other twenty-seven. The average change of temperature during these was also given in the Table.

As a result of the whole, it would be seen that while the extension of the bar under any given load was distinctly greater at high than at low temperatures, the ratio in which the extensibility was increased was comparatively so small that it might safely be neglected for all practical purposes. The experiments had been conducted in the testing machine in the engineering laboratory at University College, and the special arrangements necessary for conducting them were made by Mr. A. E. Ashcroft, Stud. Inst. C.E.

He had no doubt that at the limit of elasticity the material ceased to change its form, even approximately, as it would change it if heated. It behaved as a viscous material, and the expenditure of energy and the change as to heat went on in a different fashion. From other experiments there seemed no doubt that the material began at this point to heat, and heated gradually up to the point where local extension commenced. The Author had pointed out that the material by no means stretched uniformly, even at the place where it did not break. That was quite true, if the final extensions were measured after it had broken, because, of course, there were a number of places where the material was unhomogeneous, and in the weakest places it would stretch a little, but if the process were stopped short of the point where local extension took place, the stretching, at any rate with mild steel or ingot iron, seemed to be practically uniform. He might be permitted to give some results bearing on this point. On a bar of Landore "S" steel, with an area of 0.9 square inch, and a limit of elasticity of 42,000 lbs. per square inch, he put on a somewhat larger load, 49,000 lbs., and on further testing he found that the limit of elasticity was raised to 49,000 lbs. He next put on a load of 58,000 lbs. per square inch, leaving the load on and letting the bar rest afterwards; the limit of elasticity was then raised to 58,000 lbs. The bar was still undergoing test, having borne a

Prof. Kennedy. load of 60,000 lbs. per square inch, and it was still perfectly elastic, that was, at its new length, which was of course much greater than its former one. Its area was reduced to 0.78 square inch, but remained sensibly uniform throughout its length. The same thing had occurred with a piece of Bessemer iron, which he had obtained from Mr. F. W. Webb. The original area was 0.77 square inch; the first limit was 35,000 lbs. per square inch, which by degrees was increased to 62,000 lbs., when the area was reduced to 0.7 square inch, and was sensibly uniform. Between 62,000 lbs. and 64,000 lbs. local extension took place, and the material broke at 65,000 lbs. per square inch. In quoting these figures he had, for simplicity, rounded them off to even thousands of pounds. He therefore thought the better class of materials, at least, remained uniform in section up to within a small distance of their breaking load, if treated in the way described. He was glad to hear the view advocated by Dr. Siemens, Mr. Adamson and others, that what they wanted to know was what the material would do when they could use it, not when they could not use it in connection with the importance of observations made at its limit of elasticity. He would take the opportunity of saying that in any investigations of this kind, where additional scientific experiments were required to be carried on, if they fell within the limit of the testing machine in the engineering laboratory at University College, he should be exceedingly happy, as far as his engagements permitted, to put himself at the service of gentlemen desiring to make experiments in connection with papers to be read before the Institution.

Mr. Wentworth Shields.

Mr. F. WENTWORTH-SHEILDS said it had always appeared to him, in testing iron for practical purposes, that any test in which the iron was planed down to a given size and the skin so removed was fallacious. He had adopted the plan of testing a bar, of whatever size it might be, and reducing the strain upon the sectional area of the bar to so much per square inch. Again, if a plate were tested for tensile strain, a piece would have to be cut out of each plate, and then there were two sides from which the skin had to be cut. In that way the imperfection of the test would of course be half of what it would be if the bar were planed on all four sides. Plates might be tested, with great advantage for practical purposes, especially when the work was in a hurry, by the hammer, by doubling up the plate and straightening it again, or by punching holes along a piece of the plate and trying how near one could punch to the edge without bursting it through; in that way a good practical test could be applied at the works without difficulty.

He had been much struck with the observations of Dr. Siemens as Mr. Wentworth-Sheilds. to the increased strength given to steel by straining it to about 15 tons per square inch or within a little of the permanent set. He had heard the statement before, but he had never acted upon it himself nor had he known of anyone else doing so. He hoped that Dr. Siemens would be able to give some reliable rule showing the extent to which this might be carried. Dr. Siemens had also stated that it was not necessary to test iron to its full breaking weight for practical purposes. That was formerly his own opinion, but it had lately been somewhat shaken. There were many cases, for instance, in which high roofs were exposed to the pressure of the wind, and in which other structures were exposed to severe and unexpected strains—sometimes by loads moving at high velocities, where the iron was strained nearly to its full extent. Many years ago, when resident engineer of the Crystal Palace, it was his duty to make a number of calculations upon the strain to which the roof of that building, especially the great transept, was subjected; and the extreme pressure of the wind was taken at 30 lbs. per square foot. Quite recently he had been called upon to calculate the overturning strain from the pressure of the wind on a high iron viaduct, in order to determine the advisability of taking precautionary measures, and he found by calculation that, with a pressure of 50 lbs. to the square foot, the viaduct would overturn. He had formerly believed that in this country the wind never got up to as high as 40 lbs. pressure per square foot, but he had ascertained that sometimes it exceeded that pressure. He was therefore inclined to dissent from the principle that the ultimate strength of iron should not be taken into account in such structures.

Mr. J. N. SHOOLBRED desired to draw attention to a series of Mr. Shoolbred's interesting experiments as to the effect of temperature on iron rails, which took place some time ago in France and Germany,¹ especially as they supplied some of the information which Mr. Douglas Fox had said was still wanting. The experiments were carried on mainly by the Paris and Mediterranean Company, the Eastern of France, and also by the large rail-making works at Creuzot. They were conducted at different degrees of temperature, ranging from 18° Fahrenheit in winter to 115° in summer. In the trials by impact, the fall, with the same weight, required to rupture the rails differed in some cases con-

¹ *Vide* "Voie, Matériel roulant, et exploitation technique des Chemins de fer," par C. Couche, 1867, tome i., chap. 2.

Mr. Shoolbred. siderably; extending from 4 feet 3 inches in winter to more than 12 feet in summer. The diverse behaviour of the rails on different occasions was so well recognised that the test required by the Paris and Mediterranean Company in the specification was altered according as it might take place in winter or in summer. Below 32° , with a weight of 4 cwt., a fall of only 4 feet 3 inches was required, but between 32° and 68° the fall was increased to 5 feet, and above 68° it was further augmented to 5 feet 7 inches. There was a great difference in the tests by falling weight required on English lines and those used on the Continent. Not only was the fall much greater in the former case, but the weight itself was considerably greater; and the bending moment produced by the fall was nearly three times what it was in the Continental requirements.¹

Mr. Cowper. Mr. E. A. COWPER could state advisedly that iron took a very slight permanent set much earlier than was generally supposed. It used to be thought that iron did not set as a rule earlier than at 10 tons pressure or tension per square inch; but by means of a little instrument he had used many years ago, he had proved that most irons would set at 8 tons tension. It was important in all structures to make them of a permanent character, and of such a strength that they would not alter their shape by any ordinary strain coming upon them. In proving to twice the weight to which they were commonly subjected they ought to be still well within the elastic limit; and having that object in view, it was necessary to ascertain what the elastic limit was. He need only refer to a common experiment. By putting one end of a piece of copper wire a couple of yards in length in a vice, it might be stretched with a pair of pincers 3 or 4 inches without being broken or injured, and it would then carry more than it would before without altering its length. It was soft, perhaps, at first; but when stretched 3 or 4 inches it became harder and capable of resisting a greater strain without altering in length. It had been shown that iron did the same thing to some extent. An experiment had been tried with a bar 7 feet 6 inches long, 5 inches wide, and 1 inch thick. When stretched $\frac{1}{8}$ inch it went back to its original length; when stretched $\frac{1}{2}$ inch it went back about $\frac{1}{8}$ inch; when stretched 1 inch it still went back nearly $\frac{1}{8}$ inch, and so throughout the whole $3\frac{5}{8}$ inches to which it was stretched, having some elasticity at the

¹ *Vide* "Permanent Way Rolling Stock," by Ch. Couche. Translated by J. N. Shoolbred, vol. i., p. 540.

last. To ascertain the point at which iron or any other material Mr. Cowper first began to stretch, a small instrument was made, 4 feet long. There was a fine vernier, the length of which was one-tenth of the total length of the bar, and it was so divided as to enable one to read the $\frac{1}{10000}$ of the total length. When the bar was strained at 7 tons it came back perfectly, and also at $7\frac{1}{2}$ tons; but just as the strain touched 8 tons it began to be visible that it did not come back. The instrument was used in testing the links employed in the construction of the Kieff bridge, and it was found very useful for that purpose. Such an Extensometer would be much more delicate than the usual means adopted. He thought it was very liberal of Professor Kennedy to offer to place his testing machine at the disposal of the members of the Institution for scientific investigations. It was capable of recording a strain of 100,000 lbs.; and it would be very valuable for scientific experiments.

Mr. J. J. WEBSTER, in reply, observed that one or two interesting Mr. Webster points in the Paper had not been referred to in the discussion. One was the value of the reduction of area of a bar when tested, as a measure of its quality; another was the question of the best gauge to adopt in measuring the extensions, he having recommended $6\frac{1}{4}$ inches; and another was the peculiar form of fracture which took place when steel bars were broken by impact. He had been asked, if, when he measured the $6\frac{1}{4}$ inches on the bars to be tested at the low temperature, the bar was at the low or the ordinary temperature. He measured all the bars at the ordinary temperature, and although by so doing the length would not be exactly $6\frac{1}{4}$ inches during the test, yet the difference was so small that it could hardly be appreciated, and could not be measured with the apparatus at his disposal. Mr. Lowthian Bell had said that it had yet to be proved that fibre existed in iron; and Mr. Edward Williams went further, and said that there was no such thing as fibre in iron. Mr. Bell had argued that the fibrous fracture which appeared when a bar was nicked on the top edge and broken in the ordinary way, was caused by the particles being drawn out. The fact, however, of the particles being drawn out, showed clearly that the ultimate particles were capable of being put into parallel lines, or what were commonly called fibres; and it was not necessary to apply a heavy tensile strain to produce this formation, as it was no doubt done in passing the iron through the rolls. If there were no such thing as fibre in iron, plates and bars would have equal strength whether tested with or against the grain; but it was known from experiments

Mr. Webster. that the results obtained were very different, in some cases being as much as 20 per cent. He had seen hundreds of such experiments, and in most specifications for bridgework the tests for the iron with and against the fibre were given. He had with him a portion of plate, one half of which had been bent across the fibre and showed no signs of fracture; but the other half, being bent with the fibre, was broken across. Mr. Bell, in referring to the statement of Mr. Brunel, quoted in the Paper, that a bar of iron could be broken alternately with a fibrous and crystalline fracture with the temperature, had said he would go further, and say that the same result could be obtained without the application of heat. Mr. Webster had broken a bar as suggested, and he had with him one of the pieces, which showed, as he expected, the alternate fibrous and crystalline fracture; but this crystalline structure did not prove that there was no fibre in iron, for the structure of the iron could be altered in many ways. Mr. Douglas Fox had mentioned that a large number of steel tires had broken in Canada during the severe frosts there, but that none of the solid cast-iron wheels had done so. If this was given as a proof that cast iron was not affected by frost, Mr. Webster could not agree with him, as it was not necessary for a thing to break in order to show that it was affected; and the comparison could not be fairly made with the steel tires, because they might have been broken from causes mentioned in the Paper—unequal and restrained contraction, or hardness of the road. Mr. Fox had stated that the bars tested were not sufficient in number, and that the range of temperature was too limited. The Paper, however, was not intended to be conclusive on all points; it only professed to give a certain amount of evidence. It would require a very large number of experiments to settle the whole question, and it could hardly be expected that the labour should devolve upon one individual. With regard to the alleged insufficiency of the range of temperature, if he had experimented on higher temperatures—which was never his intention, as elaborate investigations had been already made by Fairbairn, Styffe and others—a new series of experiments would have been required; and he considered that 5° was sufficiently low, as it fairly represented the greatest cold experienced in this country. The experiments of Styffe were carried out at a very low temperature, 30° below zero, and their conclusions to a certain extent agreed. Mr. Longridge had objected to his plan of taking the averages, and having worked out some of the averages according to a plan of his own, obtained some remarkable results, which were at variance with those

obtained by him. These results, however, were entirely fallacious, as they were founded on a misapplication of the term "foot-pound." By adding the height of falls on a certain bar, and multiplying by the weight, it was clear that the result could not in any way represent the force of impact on the bar, for the very necessary element, the velocity of the falling weight at the end of its fall, was not taken into consideration. The fallacy of this method could be shown in many ways. Take, for example, the first case in Table VI., where the fall was first 4 feet and then 4 feet 6 inches, after which the bar broke. The sum of these two falls was 8 feet 6 inches, which being multiplied by 40 gave 340 of Mr. Longridge's foot-pounds. Now it was evident, without going into the calculations, that the force of impact of a weight of 40 lbs. falling 8 feet 6 inches was far greater than that developed by the weight falling first 4 feet and then 4 feet 6 inches, even supposing the force of the first blow to be counted as accumulated work, which was scarcely a fair assumption. Again, this number 340 could be made up by having seventeen blows of 6 inches each, multiplied by 40; but it was certain that the bar would never break with such blows, unless repeated perhaps some thousands of times. Mr. Matheson had spoken of the strength of the cast-iron bars being reduced by removing the skin when they were planed. The cast-iron bars, however, were not planed; and even if they had been, it would not have affected the results, as the conditions would have been the same in both sets of experiments, the results being merely comparative. Mr. Matheson had also remarked that the experiments described in the Paper did not account for the fractures which took place at low temperatures with different structures. It was never intended that they should do so, as these fractures doubtless occurred from countless causes; the object of the Paper being to ascertain how the material itself was affected by frost when under certain strains. Professor Kennedy had remarked, that as the percentage of difference obtained in some of the experiments was so small, they ought to be discarded. Mr. Webster could not agree with this; for the experiments showed that there was a difference, and however small it was, to give a true account of the experiment, it should be recorded; and in his summary of the results he had mentioned that for practical purposes it might be neglected. Professor Kennedy also objected to the method adopted for taking the averages. Averages, no doubt, could be taken, in many ways, and so twisted as to give curious results; but he thought that the plan proposed by Professor Kennedy was a most unfair one.

Mr. Webster.

Mr. Webster. In taking averages, good results must be taken with bad ones, and the more results there were to work upon, the more likely it was that the truth would be arrived at. The plan proposed by Professor Kennedy of taking out a result here and there, because it happened to be higher or lower than the others, was obviously an unfair one, for the number of results was thus reduced, and by repressing any one or more of them, a false average was obtained. As regarded his remarks about the height of fall not being the same at the commencement in all cases, this did not affect the results so seriously as he supposed; but the number of falls on each bar varying so was a difficulty which could not be avoided. If it were possible to ascertain the exact height from which the weight should fall to break a bar, or the exact number of blows required to do so, the results would be more accurate; but as this was an impossibility, the next thing was to break the bar with as few blows as possible. The effect of the falling weight from a 1-foot to a 4-feet fall upon the steel bars was so trifling that they might be said practically to have started from the same height. With reference to the remarks of Dr. Siemens, that no deductions had been drawn from the chemical analyses of the materials, this was outside the scope of the Paper. A chemical analysis was given of all the samples, so that if any member wished to draw any conclusions from them, he had thus an opportunity of doing so. With regard also to the moduli of elasticity of the materials not being given, that was another question which was without the range of the Paper, and would have required a new set of experiments to have investigated it. Dr. Siemens, Mr. Adamson, and Professor Kennedy had remarked that engineers did not wish to know at what strains the materials would break, but how far they could be used without exceeding the limit of elasticity. This opened an important and rather complex question, and much could be said on both sides; but the majority of engineers were decidedly in favour of the breaking strain of the material being adopted as a base upon which calculations should be made when determining the sections required to withstand any given load; and it was upon this base that the requirements of the Board of Trade in bridgework were founded. It was well known that in the commonest iron, the limit of elasticity was often as high, if not higher than that of the best iron, and supposing a bridge to be designed, having its several parts apportioned according to a factor of safety based on the limit of elasticity, it was evident that as this limit might probably be near to the breaking strain of the material, the bridge would not be so strong as anticipated, and

the factor of "safety" would be a misnomer. He was strongly of Mr. Webster's opinion that the factors of safety adopted in bridgework should be based upon the breaking strains of the materials employed, and not upon their limits of elasticity; although it might be advisable under certain circumstances to specify the latter also.

Mr. W. H. BARLOW, President, thought the members of the Institution were greatly indebted to the Author for bringing forward a subject of a philosophical character, thus usefully leading their minds a little off the ordinary track which they were accustomed to pursue. It was perhaps true that the Paper did not take such a wide range as it might have done, but it could not be expected that any individual member should carry such a subject to its extreme limits. They were not only indebted to the Author for his Paper, but for the excellent discussion which had arisen from it, and the valuable information contained in the remarks of Mr. Adamson, Dr. Siemens, Mr. Longridge, and Prof. Kennedy. He desired especially to refer to Prof. Kennedy's offer to place his testing machine, when he could do so without disturbing the ordinary course of his avocations, at the disposal of the Institution. He had no doubt that the offer would be gladly accepted by the members, and that the result would be a great addition to their knowledge with reference to such subjects.

Correspondence.

Mr. W. ANDERSON regretted that the chemical composition of the metals experimented on had not been given. It was now generally agreed among metallurgists that by "iron" should be meant pure iron only, and that by "steel" should be meant a combination of iron and carbon only. When those metals contained other substances they ceased to be iron or steel in the strict sense of the terms; their properties became different, and many of the apparent anomalies which had arisen during experiments on metals might be accounted for by the fact that chemically the substances were not the same. Mr. Chernoff had illustrated the importance of this remark in the following communication, in which he showed that the presence of phosphorus made the steel with which it was allied more brittle in cold weather, while pure steel was not affected by cold. The "cast-steel," which had been tested, it was presumed, was crucible steel, forged or rolled after casting, and not steel castings. According to Chernoff, steel castings, in the form of steam-hammer heads, seemed to be affected by cold much as cast iron had been shown to be.