Dr. Stanton. nized, but it had not been thought advisable in this Paper, which professedly dealt with questions in the everyday practice of the designer, to make experiments on materials which were permanently deformed at the outset, since it was well known that the maximum stresses reached in such experiments were, in the majority of cases, well above the elastic limit of the material. Further defence of the position which the Authors had taken up in this respect was rendered unnecessary by the contribution of Sir Alexander Kennedy to the discussion, with which the Authors were in complete agreement, and which, coming from such a high authority, they felt would have much more weight than any statement of their own. The Authors were grateful to Professor Unwin for the interesting account of Bauschinger's work which he had given, and they hoped at some future time to go more completely into the investigation of the variations of the elastic limit than they had done in the preliminary work in that direction described in the present Paper. From observations which they had made since the communication of the Paper, they had strong reasons for believing that in cases in which the range of stress was greater than its limiting value, that was, those in which fracture ultimately took place, the limits of elasticity, both in tension and in compression, became smaller as the test proceeded. He thought that the results which Professor Unwin had promised to communicate[1] on the resistance of steel at 400° F. would be most valuable to all designers of heat-engines, especially as information on the effect of temperature was so rare on account of the difficulty in carrying out the experiments. He was indebted to Mr. Inglis for his offer to supply material which had been under stress for a considerable time, and he ventured to hope that, in cases where unexpected failure in structures and machines had occurred, engineers would kindly present samples of the material to the Laboratory for investigation.

Correspondence.

Mr. Breuil. Mr. P. Breuil, of Paris, congratulated the Authors on the important conclusions at which they had arrived. He would refer specially to the one pertaining to the determination of the elastic limit of test-pieces subjected to a large number of alternations of stress.

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[1] See foot-note p. 117.—Sec. Inst. C.E.

As this elastic limit was equal to the maximum stress on the test-pieces, it was possible to predict, on determination of this limit in the case of a piece broken in use after a large number of reversals, for how many such reversals it might be depended on. This conclusion, which had been arrived at by statical tests, was now confirmed by the Authors, and gave cause for congratulation. The development of cracks in steel was admirably shown by the Authors. He would advise that they should repeat their tests, not only with sound specimens, but also with palpably poor specimens, which, just on account of their poor quality, were of greater importance. He felt sure that herein would be found the explanation of a host of anomalies exhibited in practice by these metals. The accompanying microphotograph (Fig. 40, Plate 2) depicted a sample of iron, containing 0·5 per cent. of phosphorus, from a part which broke in use after 8 years of stress by bending. It would be noticed that definite flaws, springing from and connecting specks of slag, existed between the particles of iron, which tallied very well with the form of the cracks in the corners of the specimens tested by the Authors; as this microphotograph showed the centre of the specimen, it might be supposed that the study of these deformations, in the case of other faulty specimens, would prove of interest. Mr. Breuil.

Dr. A. W. BRIGHTMORE had not noticed that the following explanation of the phenomena exhibited by metal bars subjected to variations of stress had been definitely brought forward as a satisfactory method of accounting for the results obtained. It would no doubt be readily conceded that such a condition of things as a "uniform distribution" of stress over a cross section of a bar or specimen subjected to the action of a force, notwithstanding all the precautions which might be taken to ensure it, existed only in the imagination. Consequently, whenever a force, however axially it might be applied, acted upon a bar, certain particles in any cross section were stressed to a considerably greater extent than other particles in the same cross section. The excess of this maximum stress over the average stress would of course depend on the lack of homogeneity in the material, assuming that the force was applied axially. Now, if the average stress was in the neighbourhood of the elastic limit of the material, certain particles would be stressed beyond their elastic limit when the force was applied, and would therefore take a permanent set ("slip-lines"). This would cause the distribution of stress on the section to be still less uniform the next time the force was applied; consequently other particles would then come in for more than their share of the stress, and would be stressed beyond Dr. Brightmore.

Dr. Brightmore. their elastic limit, and take a permanent set, again altering the distribution, and increasing the lack of uniformity of the stress the next time the force was applied. Now, it was well known that if a bar was stressed slightly beyond its elastic limit, that limit was raised; and if this process was repeated many times the elastic limit was increased up to the breaking-weight of the material, and the bar finally snapped suddenly. Applying the same reasoning to an individual particle in a cross section of the specimen under consideration, it would be seen that in course of the oft-repeated applications of the force, the turn of such a particle to take stress beyond its increased elastic limit would come round from time to time, as the intensity of the stress over the section became less and less uniform, and it would in course of time fracture. When numbers of these particles near together had fractured, the bar or specimen developed a crack, and finally broke. This explanation received confirmation from the statement by the Authors that in many cases the first set, or sets, of slip-lines observed were not those along which ultimate fracture occurred, or which developed into cracks. From the foregoing it followed that if the force, even though applied axially to a bar, was great enough, allowing for the lack of perfect homogeneity in a section of the bar, to stress some of the particles in a cross section beyond their elastic limit, the bar would ultimately fracture under the repetition of the application of such force. The above considerations explained the superiority in resistance to reversals of stress of homogeneous fine-grained steel over more heterogeneous wrought iron.

Mr. Gillott. Mr. THOMAS GILLOTT remarked that there were few engineers who had not had experience of failures of parts of machinery subjected to alternating stresses, and such cases had often occurred with mild-steel valve-spindles on a number of locomotives formerly under his charge, the driving arrangement being as shown in *Fig. 41*. The valves were of brass, measuring $8\frac{3}{4}$ inches by $14\frac{1}{2}$ inches over the faces, the safety-valve pressure was 140 lbs. per square inch, and in average work the force required to overcome all the resistances would not exceed 1 ton. The valve-spindles were $1\frac{3}{8}$ inch in diameter, turned from rolled bars, and had a screw-thread cut on them for a length of about 7 inches where they were secured to the driving arm; and these spindles broke at the end of the screwed part with a clean smooth fracture as though they had been sheared straight across, no signs of tearing being apparent to the naked eye. In every case when the broken screwed part was bent cold and closed under a steam-hammer, no signs of fracture were apparent at the bottom of the threads. Mr. Gillott could not remember a single case in which valve-spindles for the same engines, made from best Yorkshire iron, case-hardened, had had to be replaced on account of breaking. This led him to make some remarks on the materials experimented on by the Authors, whose painstaking work he fully appreciated, although he would have preferred some tests on a larger scale. Mr. Gillott had had experience of rolled-steel rounds made at works where the practice was to cast small ingots (*e.g.*, about 4 inches square for $\frac{3}{4}$-inch and $\frac{7}{8}$-inch rods) instead of cogging down larger ingots into billets for the rods to be rolled; and the results had been so unsatisfactory that he had had to discontinue using such material, although the evidence of the testing-machine could be quoted against him. The uncertainties of such steel were small compared with those from "iron" bars made at works where scrap was purchased, and steel and iron were often mixed, although professedly kept separate: some irregularities of this kind might account for the appearance of Fig. 14, Plate 1. Mr. Gillott would be glad if the Authors would describe the appearance of the various fractures on their broken test-bars, and he also asked whether details of the processes of manufacture of the various bars were available. Mr. Gillott.

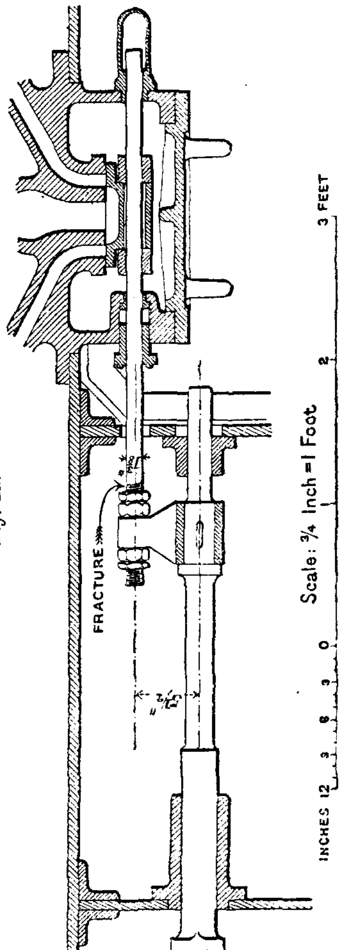

Fig. 41.

Mr. Henry Le Chatelier, of Paris, considered the Paper to be one of the most important of those to which metallurgists should at the present juncture turn their attention. For a considerable period it had been thought that in a tensile test the elastic limit and the extension sufficed to determine completely the behaviour Mr. Henry Le Chatelier

Mr. Henry Le Chatelier. of a metal under working-conditions. This supposition had now been set aside. Brittleness-tests had shown the small importance of the ultimate elongation at fracture, since specimens giving an elongation of 35 per cent. might in practice break like glass. Tests on reversal of stress probably also showed that the elastic limit had but little influence on the capability of a metal to resist variations in stress such as were always met with in practice. So far the results of the Paper itself, which at present could only be considered a preliminary one, exhibited variations in the ratio of the elastic limit to the resistance to alternations of stress between 1·1 and 1·8, which was a very wide difference. Most likely, on further varying the tests, still greater discrepancies would be noticed. It was a matter for regret that in this first set of tests the Authors had only taken metals in everyday use, whose conditions of manufacture were inadequately detailed. It was impossible from the figures obtained to draw any inferences applicable to other varieties of metals. It was reasonable to suppose, however, that this communication was merely the forerunner of a lengthy set of investigations, extending over a number of years, and carried out in a methodical manner. The study of the variation in the ratio of the elastic limit to the ultimate strength under alternating stresses, while systematically varying the thermal treatment of one and the same metal, would be of considerable practical value; as would also an investigation of the effect of the various metals now introduced in the manufacture of special brands of steel. Did these new kinds of steel really possess that superiority which was often attributed to them? Opinions differed on this point. Some engineers were of the opinion—and exceedingly plausible reasons appeared to confirm their view—that ordinary steels might, for the majority of purposes, yield the same results, but that their thermal treatment required greater accuracy and was beyond the capabilities of some manufacturers. If this view of the matter were adopted, the special brands of steel would be the products of ignorant persons. It would be of great practical importance to thrash out this matter. In any case it was impossible to adequately congratulate the National Physical Laboratory and its Director, Dr. Glazebrook, on the skilful and persevering initiative exhibited by their staff in attacking all problems of general interest to manufacturers. If the utility of the laboratory required any further vindication, the work carried out since its inauguration provided it in a brilliant manner.

Mr. Osmond. Mr. F. Osmond, of Paris, observed that Bauschinger's idea, which assumed that the elastic limit as usually measured was an artificially raised limit, and that there existed a true lower limit, which appeared under alternations of stress, was exceedingly ingenious

Mr. Osmond.

and interesting; it would give an explanation of the yield-point which generally appeared on the curve of deformations under load in the case of iron and mild steels. But, in spite of the Authors' deductions, this idea did not appear to be verified by the results of their tests. Surely, if a true elastic limit existed which was lower than that obtained from tensile tests, this true limit should be a physical constant, independent of the shape of the test-pieces. But the curves of *Fig. 5* showed that the half limiting range of stress was, roughly speaking, doubled in the case of test-pieces of type IV as against test-pieces of type I. True, in the case of specimens which had withstood a large number of reversals of stress without fracture, the Authors found that the elastic limits in both tension and compression were lowered, and practically coincided with the maximum stress imposed during the reversal tests. But, in order to make this determination on a very short test-bar, the Authors employed a specially sensitive extensometer, and it might be that its sensibility was the cause of an apparent diminution. As a matter of fact, Mr. C. Fremont's experiments,[1] which, although the account of them had been translated into English,[2] seemed to be but little known in England, showed that the elastic limit, as usually determined, did not correspond with anything definite. When a small test-bar was subjected either to a tensile or to a compressive test, the load was never distributed uniformly over the whole section, on account of lack of homogeneity or errors in adjustment, and because the line of effort was not absolutely parallel to the axis. This was proved by the fact that in a tensile test certain fibres shortened while others extended, and that small bars of hard steel often broke at the head, outside the least section; and again, the appearance of Lüders lines occurred much below the elastic limit of the test-piece. These lines, easily visible on polished specimens, consisted of mountains and valleys; their mere existence proved that the elastic limit had been exceeded at the points where they appeared, that was, locally. Mr. Osmond had had a series of tests made, which were referred to by Mr. Fremont, on some small test-bars, all of the same mild steel. One bar had been loaded to 5 kilograms per square millimetre (3·2 tons per square inch) and then taken out of the machine; another had been loaded to 8·5 kilograms per square millimetre (5·4 tons per square inch); a third to 12 kilograms per square millimetre (7·62 tons per square inch); and so on, until in the case of the last bar the yield-point had

[1] "Mesure de la limite élastique des métaux." Bulletin de la Société d'Encouragement, 1903, pt. ii, p. 350.

[2] *Nature*, vol. lxix, p. 276.

Mr. Osmond. been passed. The operations had been carried out under conditions usual in factories, except that the test-pieces were previously polished. The piece loaded to 5 kilograms per square millimetre had shown no traces of Lüders lines. These lines had, however, been plentiful in the case of the specimen loaded to 8·5 kilograms per square millimetre, and on the specimen loaded to 21·5 kilograms per square millimetre (13·65 tons per square inch) the lines had not quite covered the whole surface. The yield-point had been therefore a little greater than this load, and the elastic limit, as determined by accurate instruments, would have been found to be a little less. It followed that the elastic limit had been *locally* exceeded under a mean load about one-third as great. With sufficient accuracy, it should have been possible to find an elastic limit less than 8·5 kilograms per square millimetre, but this latter would not have been that of the metal itself, but that of the test-piece, for the conditions under which the test was made—that was, when the obliquity of the tension, the heterogeneity of the material, and errors in adjustment of the piece and the ends of the piece, were taken into account, all of these being inevitable defects. On a specimen presenting points of weakness, such as a screw-thread or a groove, the differences would have been still greater. Now, was there any connection between the statical stresses which usually determined what was known as the elastic limit and the dynamical stresses brought into play in Wöhler's experiments? Nothing could be less certain. It was thought that rupture due to reversals of stress should only occur if the elastic limit, as determined by a statical test, had been exceeded. But in a statical test, a load was applied to the metal, which was measured in units of weight; in tests with reversals of stress, work done was measured in kilogram-metres. The two units had different dimensions. Kilogram-metres were the product of two quantities, the one being the instantaneous pressure due to shock; but if the product was known, the two factors were unknown. In Mr. Osmond's opinion, it was necessary, in order to cause rupture, for the instantaneous pressure due to shock or vibration to considerably exceed the elastic limit as determined by a tensile test, and to attain at the point of greatest fatigue the breaking stress—not the maximum load divided by the initial area, but the load at the moment of rupture divided by the contracted area. According to Mr. Considère's researches,[1] this stress, for a mild steel, was 71·6 kilograms per square millimetre (45·4 tons per square inch), whereas the ordinary method of calculation gave

[1] "Mémoire sur l'emploi du fer et de l'acier dans les constructions," *Annales des Ponts et Chaussées*, 6th series, vol. ix (1885), p. 574.

40·3 kilograms per square millimetre (25·6 tons per square inch). This theory appeared to be confirmed by the Authors' tests. They found, as a matter of fact, that the resistance to reversals of stress did not depend on the maximum tensile or compressive stress, but on the range between the maximum tensile and compressive stresses. Wöhler and Bauschinger had arrived at the same conclusions. It followed that a given sample, *e.g.*, the Authors' "Wrought iron No. 2," the limiting range of which was 19·2 tons per square inch, could withstand this stress of 19·2 tons per square inch in tension for an indefinite period, provided the compressive stress was made equal to zero, and this in spite of the fact that the primitive elastic limit (Table II) was only 13·37 tons per square inch. Statical stresses caused rupture after the preliminary deformation; shock and vibration caused it without preliminary deformation. The two processes had nothing in common, although in practice they were superimposed. It followed that statical tests were quite insufficient to determine the quality of a metal intended to resist dynamical stresses, and that the use of Wöhler's or an equivalent method (such as, for example, impact tests on notched bars) was not only advisable, but also absolutely necessary. Too much dependence, however, must not be placed on the limiting range, measured under fixed conditions. It might no longer be accurate under different conditions, and the Authors themselves gave a conclusive proof of this. Under certain conditions they obtained a limiting range 9 per cent. below the normal, and sometimes considerably less than that. They attributed this to the coincidence of the period of the machines with the period of vibration of the specimen, and this explanation was quite satisfactory. But why should not this coincidence, which occurred accidentally in certain tests, occur in a bridge when a train passed over it, or in a number of other cases easy to enumerate? This seemed to be an extremely important point. Why should the high limiting range be more accurate than the low one? If the lowest limiting range had been able to occur, then it could occur also in practice; further, there was nothing to show that even this was the lowest one possible, and prudence did not permit of neglecting it. It was this latter value that must be taken as a maximum—too high a one, perhaps, but one that should never be exceeded unless the coincidence of the periods of vibration that brought it about was absent. In the matter of elementary deformations, the Authors confirmed the observations of Messrs. Ewing and Humfrey. These observations, were, moreover, perfectly accurate and practically indisputable, yet Mr. Osmond, Mr. Osmond.

Mr. Osmond. like Messrs. Fremont and Cartaud,[1] was of opinion that they should be somewhat otherwise interpreted. Messrs. Ewing and Humfrey thought the lines of fracture to be developments of the slip-bands. As a matter of fact, these lines were not of the same character. The latter corresponded to statical deformations: they were not of crystallographic origin, as was proved by the photograph reproduced in Fig. 42, Plate 2, which depicted a plane of cleavage of a crystal of iron; the point of a needle had been pressed on to this polished surface under a load of 1·5 kilogram (3·3 lbs.); this pressure had caused the formation of a cross, the branches of which, consisting of a number of lines, ran parallel to the diagonals of the square plane of cubic cleavage. A general deformation by compression therefore occurred, developing new lines called "slip-bands" by Messrs. Ewing and Humfrey. According to Mr. Rosenhain these lines were parallel to the faces of a regular octahedron, as indeed was the case with copper, gold, silver, platinum, and lead. They should, accordingly, also be parallel, on the face of the cube, to the branches of the cross. The figure showed at once that there was no sign of this, and, moreover, that these lines were not straight.[2] On the other hand, lines along which rupture due to reversals of stress took place were straight and of crystallographic origin; they were either cleavage lines of a cube or a series of maclés known as Neumann's lamellæ; they showed up when etched with acid, whereas the slip-bands did not do so unless the deformation had been carried very far. Besides, it was quite natural for these various lines to become superimposed at the same spot, which was thus shown to be the place of maximum fatigue. They were nevertheless different, and the latter only were dangerous. It was important to discriminate between them.

The Authors. The AUTHORS, in reply to the Correspondence, observed that the wide range in the values of the ratio of the elastic limit to the resistance under alternations of stress, pointed out by Mr. Le Chatelier, was no doubt due to the artificial positions of the primitive elastic limits. As an instance of this, a sample of the Swedish Bessemer Steel No. 1, which had a primitive elastic limit of 21·4 tons per square inch, was raised to 1,000° C., and then cooled slowly in air. Its elastic limit had then been found to be 11·6 tons per square inch, and its limiting range of stress for one million reversals

[1] *Revue de Métallurgie*, January, 1904.

[2] One branch of the cross is badly formed, probably because the needle was not absolutely perpendicular to the surface. The two large parallel black lines running across the mouth of the puncture are purely accidental and of no special import.—F.O.

approximately 18·5 tons, so that the heat-treatment had brought about a change in the value of the ratio from 1·25 to 1·60. Mr. Osmond appeared to be of opinion that the natural elastic limits determined by reversals of stress should, if they existed at all, be physical constants independent of the form of the specimen, and hence was unable to reconcile the differences between the limiting ranges of the stress for specimens of types I and IV. The Authors would point out, however, that agreement of the ranges of stress for different forms of the same material could only occur if the distribution of stress over the sections of the specimens were identical in the two cases. Now, in the case of a specimen with a sudden change of section, although the mean stress over the section might be the same as in a specimen of maximum resistance, the actual intensity of the stress at the sharp corners was higher than that which existed in the specimen of maximum resistance. This conclusion was, they thought, inevitable from the evidence in the photograph forming Fig. 28, Plate 2, and fully accounted for the low range of stress obtained. The existence of such a physical constant must be sought for in the results of experiments in which the distribution of stress in the specimen was approximately uniform, and there was no evidence in the present research that this constant did not exist. Mr. Osmond had apparently not appreciated the method of application of the stress to the specimens on the testing-machine described in the Paper, since he referred to the "instantaneous pressure due to shock" on the specimens. In the machine used the stress was produced by the acceleration of a definite mass attached to the specimen and caused by it to move in an approximately simple harmonic manner, so that there was no "shock" on the specimen in any way. The Authors could not agree with Dr. Brightmore's theory that a small local permanent set in a specimen under test, which was due to the line of action of the load not being strictly axial, would cause the distribution of stress to be still less uniform the next time the load was applied. Their observations led them to the conclusion that a slipping in certain crystals had the effect of distributing the stress more evenly and of temporarily preventing the deterioration of the specimen at that section. An apparent illustration of this action had been noticed in the Paper (p. 100, and Figs. 31 and 32, Plate 2). In reply to Mr. Gillott's question as to the appearance of the fractures of the various specimens, these had the same characteristics in the hard and soft materials alike. There was no permanent elongation of the specimen, the fracture being of the nature of a sharp-edged crack across the specimen. In some cases the plane of the fracture was normal to the axis of the

The Authors.

The Authors. specimen, and in other cases inclined to it—in the extreme cases, at an angle of 30° or 40°. The Authors had communicated Mr. Gillott's inquiry for details of manufacture of the bars to Mr. Hadfield, who had replied that the bars which he supplied were produced by the ordinary Swedish Bessemer method. There was nothing special about their manufacture. The Authors desired to thank Mr. Osmond and Mr. Breuil for the photomicrographs which they had sent for publication in the Correspondence, both of which afforded valuable evidence of the manner of failure of materials under stress.