The Author. soft irons and mild steels did mend themselves and recover their elasticity by mere lapse of time[1]. A comparatively slight change of temperature in a metal, due, for instance, to boiling in water, accelerated the process of mending[2]. In the Paper by the Author referred to, examples were given of two test-pieces; one of them recovered its elasticity at ordinary temperatures in a night, and the other, when tested after a year's interval, was found to give exactly the same "hysteresis" loop as when tested 12 months previously. With regard to Dr. Rosenhain's further remarks, when a load was applied to a test-piece and its removal and re-application formed a loop, even although the yield-point had not been passed, the true elastic limit had been passed, and unless the space lattice mended itself or was subjected to some heat treatment, the Author did not think it safe to say that it would be able to withstand reapplication and removal of the load indefinitely. It should always be remembered that a fatigue test was not so much a test as a process; and the results obtained by this process required careful interpretation before they could be accepted in relation to the use to which the metal was to be put. In conclusion, he would like to emphasize the fact that the recorders mentioned in the Paper were not designed primarily for purely scientific research but for the purpose of giving engineers quickly and accurately information about the elastic and plastic properties of the metals which they used.

Correspondence.

Mr. Batson. Mr. R. G. C. Batson remarked that the Author had expended considerable care and thought upon the design of his optical instruments and had given numerous interesting records in the various Papers which he had written on the subject. He was interested to find that the Author advocated the application of positive and negative strains in order to obtain a true estimate of the elastic properties of materials. Work in this direction had been carried out, in alternate tension and compression, by Professor L. Bairstow[3] and in alternate torsion by Mr. Batson[4]. By twisting the bar in one direction beyond its limit of proportionality (stress x) and leaving it in boiling water for 30 minutes, Mr. Batson had found that the limit of proportionality was raised to (or "fixed" at) the maximum stress x_1 to which the bar had been subjected. The bar

[1] Proc. Roy. Soc. vol. lviii (1895), p. 134.
[2] *Ibid.* vol. lxiv (1899), p. 337.
[3] Phil. Trans. Roy. Soc. Series A, vol. ccx (1909), p. 35.
[4] Proc. Inst. Mech. E., 1917, p. 182.

Mr. Batson.

was then twisted in the opposite direction, and the limit of proportionality, y, was obtained in that direction. The process was repeated a large number of times, and in all cases the range $(x_1 + y)$ was found to be constant. That was to say, if the "fixed" limit of proportionality was high in one direction, the limit of proportionality, found when the twisting was reversed, was low. Further, basing his conclusions on the work done by Professor Bairstow in alternate tension and compression, it was assumed that the range $(x_1 + y)$ corresponded to the limiting range of stress in repeated alternating shear. This assumption had been verified subsequently by Mr. H. J. Gough,[1] by actual fatigue tests (in shear) on the materials used for the work cited above. The actual estimation of the limits of proportionality in the work carried out by Professor Bairstow and himself had necessitated the measurement of very small strains with a high degree of accuracy. This requirement was indeed essential in all work dealing with the proportional or elastic limits, and Mr. Batson was doubtful whether, owing to the fundamental characteristic of the optical instruments, to which the Author referred (namely, a spherical error due to the spot tracing a curve on the photographic plate), the optical elastic recorder could give results accurate enough for research purposes. The observations were, however, probably sufficiently near for commercial work.

Fig. 15, Plate 1, showed that the yield load was not quite the same in tension as in compression. On comparing that diagram with Figs. 12 and 13, Mr. Batson had been unable to decide which kind of strain the Author found to give the higher value, as the latter diagrams appeared to give identical values of the yield. Theoretically the yield should be higher in compression than in tension, and some careful extensometer tests by Mr. Batson had shown such to be the case. These tests were given in the following Table, and represented average values from three tests (in close agreement) in each case.

COMPRESSION.

Material.	Limit of Proportionality.	Yield Stress.	Modulus of Elasticity.
	Tons per Sq. In.	Tons per Sq. In.	Tons per Sq. In.
Forged Steel A	17·2	19·6	13,030
Forged Steel B	20·3	20·6	13,120
Pure Nickel Bar	6·6	8·6	12,940

[1] "Fatigue of Metals." London, 1924.

Mr. Batson.

TENSION.

Material.	Limit of Proportionality.	Yield Stress.	Ultimate Stress.	Modulus of Elasticity.
	Tons per Sq. In.	Tons per Sq. In.	Tons per Sq. In.	Tons per Sq. In.
Forged Steel . . A	16·9	18·5	33·3	13,300
Forged Steel . . B	16·8	19·9	34·8	13,390
Pure Nickel Bar .	5·6	8·4	32·4	13,210

Professor Cook.

Professor GILBERT COOK considered that it was remarkable, in view of the amount of research and study that had been devoted to the mechanical properties of mild steel, that a state of considerable uncertainty still existed regarding the process of failure under stress, and the laws which governed it. It was, perhaps, not difficult to visualize what took place at rupture. Whatever might be imagined to be the ultimate molecular arrangement, it was possible at all events to conceive of the molecules being torn apart by the application of sufficient force. Nor was it difficult to conceive of the process of elastic deformation. Any form of space lattice in which the molecules were held together by mutual attractions must deform in some way under the application of external forces, and it was readily conceivable that such deformations might be proportional to the applied forces up to a certain limit. The difficulty lay in trying to get some idea of what was happening between the elastic limit and the point of rupture, and particularly what happened at the yield-point. In this connection Figs. 8 and 10, Plate 1, were of special interest. Fig. 8 was of the familiar type obtained with any kind of autographic recorder, but it exhibited very well the feature which, among all materials, appeared to be possessed only by mild steel and wrought iron in the annealed state ; that was, the sudden reduction in the stress immediately after the yield-point. In collaboration with Professor Andrew Robertson, Assoc. M. Inst. C.E., he had described[1] experiments which had been carried out in order to explore this region. A diagram very similar to Fig. 10 was obtained, but not by autographic means. It was found, however, that under certain conditions a much greater reduction occurred than was indicated in the Author's diagram, the amount of the reduction ranging chiefly between 24 and 30 per cent. of the yield stress, but being as high as 35 per cent. in two specimens

[1] Proc. Roy. Soc., Series A., vol. 88 (1913), p. 462.

The material used was ordinary commercial mild steel, but it was carefully annealed, and special precautions were taken to secure a uniform distribution of stress during loading; also the extension was increased at a very slow rate. The rate at which the extension was increased appeared to affect the amount of the drop in stress, a fact which was important in considering any theory of failure. The results had been confirmed by a torsion test of the material. A torque-twist diagram almost identical with that shown in Fig. 7 was obtained. As the torque was increased, the angle of twist was proportional to it up to a point at which elastic breakdown occurred in the outer layers, and it was found then that the twisting continued without any further increase in the torque; in other words, the torque-twist curve became a horizontal line starting from the yield-point. If x denoted the ratio of the stress immediately after, to that immediately before, the yield-point T, T' the torques at, and immediately after, the yield-point, and θ and θ' the corresponding angles of twist, it could be shown that Professor Cook.

$$T' = T\left(\frac{\theta}{\theta'}\right)^3 + \frac{4x}{3}T\left\{1 - \left(\frac{\theta}{\theta'}\right)^3\right\},$$

from which it would be seen that T' would be equal to T if x were equal to 0·75; that was, the twist would continue without increase of torque only if the drop in stress in the layers which had yielded was equal to, or greater than, 25 per cent. There was evidence, therefore, once the process had started, of a comparatively low resistance to sliding, which appeared to diminish when the rate of sliding was reduced. This fact lent support to the theory put forward by the late Sir George Beilby, whereby it was sought to explain the process of sliding by the occurrence of a change in the physical state of the material adjacent to the planes of sliding—a transformation from the crystalline state to an amorphous state. One of the properties of this amorphous material was assumed to be that of a highly viscous liquid while sliding was taking place, and of considerable hardness when the sliding stopped. In that connection Fig. 10, Plate 1, was extremely interesting. When the load was removed and reapplied, the extension continued at approximately the same stress as that which existed just prior to the removal of the load, although that was considerably less than the yield stress. It followed that the sliding must continue over the same crystal planes, and not over new ones, otherwise the original yield stress at least would be required to re-start the movement. It would appear, therefore, that the hardened amorphous state could be reconverted to the viscous state, and that in the period occupied in the removal and reapplication

Professor Cook. of the load no appreciable recrystallization of the amorphous material could have taken place. It would be interesting to know whether the same effect would be produced if, while still in the region of the "drop," the load were removed, and then reapplied after a prolonged interval of rest, and possibly heat treatment at 100° C. Would the new yield-point have the primitive value or the reduced value? The answer to that question would undoubtedly throw additional light on the properties of the material adjacent to the planes of sliding. On the hypothesis of an amorphous material having the properties described above, there did not appear to be any serious difficulty to encounter in interpreting the Author's diagrams. The quasi-elastic response to reduction of load was what would be expected as a necessary consequence of the cessation of sliding and the production of the hardened state. In those parts of the material remote from the planes of sliding the distortion was purely elastic, and the removal of the load would be an elastic process modified to some slight extent by readjustments in the amorphous layers. The removal of the load would not, however, relieve the material of internal stresses. The process of sliding must produce considerable complication in the stress-distribution in the material, and the mere process of removing the load would be to generate compressive stresses in those regions which had sustained a comparatively low tensile stress when under tensile load, as, for example, in the amorphous material itself. A reversal to compressive load would increase such compressive stresses, and it was readily conceivable that a very small compressive load would bring such material again to the conversion point, and slipping would occur again over the same planes, but in the reversed direction; and a diagram like Fig. 16, Plate 1, would be obtained. The amorphous state was as yet little more than a hypothesis. But it was one that was consistent with a great deal of external evidence, which was very largely contributed to by the Author's results.

Mr. Doak. Mr. W. J. DOAK observed that the Author's picture of the internal structure of iron and steel and of the difference between the fractures of single-crystal and multi-crystal test-pieces was very interesting, and it occurred to him that it might be worth while to investigate the effect upon the fracture if a test were carried out on soft iron in a strong magnetic field. The well-known effect which hardening had upon retention of magnetism seemed to suggest that the arrangement of space lattices might be responsible for both strength and magnetic properties, and that variation of one would affect the other. The weakness of magnetic force as compared with tensile strength indicated that the effect would be slight: still, with the Author's delicate apparatus some interesting results might be hoped for. Mr. Doak did not notice any reference to "yield-point" or to "limit of proportionality"—terms which had been considered to be of serious importance by the engineers of Sir William Arrol and Company in connection with their tender for Sydney Harbour bridge. In fact, although on p. 25 proportionality was stated to cease at 20·4 tons, there was exceedingly little visible bending in the straight line of Fig. 6, Plate 1, till the peak load was reached at 23·8 tons per square inch. In this connection it would have been interesting to stop the test at, say, 0·009 inch extension and reapply the load; probably then the diagram would have been quite straight, with a permanent set at no load, and the limit of proportionality would coincide with the yield-point. In regard to practical affairs, every bridge-engineer was familiar with cold bending of parts of bridges which had been damaged in transit: there could be no question that such parts were strained beyond the elastic limit, and it then became a matter for judgment whether they should be annealed before going into the bridge. Whether the part was subject to reversal or not, the Author's tests showed that resistance was weakened for load of an opposed kind; and, as a bent plate would have been strained in both tension and compression, there must be a lessened resistance to strain of both kinds. Fortunately, however, as soon as the slack, so to speak, was taken up, the plate became again elastic material, and the only bad effect would be a certain deformation of the girder. As already said, judgment must be used so as to steer a course between taking too much risk and incurring needless expense in annealing. Railway rails afforded a practical instance of the use of permanently-strained material to carry heavy loading. Where lines had sharp curvature the rails were universally pressed cold to the precise curvature, although on flat curves it was common practice merely to spring them without permanent bending. In addition to this, the concentrated pressure of wheels caused the rail-surface to flow, so that it was quite common to see the metal rolling down the sides in the form of a bead; yet those rails suffered heavy reversals of stress under every passing train. It was not as though the stresses were low, because extended investigations in America had proved that very high stresses occurred under ordinary traffic-conditions. These facts seemed to suggest that the factors of safety and allowances for impact and dynamic effect in the design of bridges (highway bridges in particular) were probably greater than was necessary. In his experience several important bridges were carrying loads much in excess of those for which they had been designed, and it would seem more logical to cut down the impact-allowance and to include in the factor of safety definite allowances for possible increased loading and reduction by corrosion. In that way consistent rules for condemning Mr. Doak.

Mr. Doak. old structures might be evolved. At present there was a wider gap than might be expected between the loads for which bridges were designed and those they could carry, taking into consideration the better knowledge of theory and extended experience now available.

Sir Alfred Ewing. Sir J. Alfred Ewing congratulated the Author warmly on the perfection to which he had brought the photographic stress-strain recorder and on the applications to which he had put it. He could speak with some knowledge of the difficulties that were met in obtaining a satisfactory record by giving two components of angular motion to a pencil of light, for it was a problem he had attacked years ago in designing his magnetic curve-tracer. In the Author's device much finer and better diagrams were obtained, which for precision and clearness left nothing to be desired. The Author had, in fact, placed in the hands of engineers a really valuable instrument of research. It was also an instrument which might, and should, greatly simplify practical testing. In cases where large forces had to be applied, as in tension tests of chain cables or compression tests of columns, there was no longer any need to resort to testing-machines provided with a lever, or system of levers, and weights for the purpose of measuring the applied force. That could now be better done—more conveniently and at least as accurately—by using a steel master-bar or bars in tension, and taking the master-bar's extension as the measure of the force applied. This allowed the design of the testing-machine to be immensely simplified. It meant substituting, as the means of measuring the hydraulically applied force, a spring in its simplest form—that of a rod in tension—for the complication of knife-edges and fulcrum planes, levers, and movable weights. Thanks to the Author's recorder the old-fashioned type of testing-machine for large stresses would give place to a handier, less costly, and equally effective appliance: indeed, a more effective appliance, for it would not only measure the stress with at least as great precision, but would record the strain suffered by the object under test, at every stage from start to finish. There was, of course, nothing new in this idea, but the point was that the Author's device made it thoroughly practicable. He commended it to the attention of all engineers who were concerned with large-scale testing. He was struck by the close resemblance of the Author's diagrams for cyclic processes of straining in steel and other metals to another class of diagrams with which he was very familiar, namely, hysteresis curves showing the variations of magnetism produced by cyclic variations of magnetizing force. The correspondence was remarkable, not only as regarded general form, but also in detail—as, for instance, in the progressive differences which were observed between one cycle and the next when a straining, or magnetizing, process was repeated.

The curves of strain and stress were so exactly like hysteresis curves that any one accustomed to magnetic testing might, on glancing at the plates without reading the letterpress, mistake these for records of magnetic tests. Surely so close a resemblance implied some fundamental community in physical origin. It would seem that the interatomic forces which controlled the response to mechanical straining must act in an essentially similar manner to those which controlled the response of the magnetic elements in the atoms of an iron crystal to the turning influence exerted by an externally-applied magnetic field. Sir Alfred Ewing.

Mr. F. L. WATSON considered that the Paper, in continuation of the Author's previous publications, furnished a most valuable step in the solution of the urgent problem of a simple and rapid means of co-ordinating the results given by the various mechanical tests already in use, so as to arrive at the vital point in regard to the strength of material, namely, the maximum load which might be applied to the material without weakening its internal structure. Opinions on the well-known mechanical tests varied between the extreme views that simple tensile tests would give all the information required if properly interpreted, and that the shock-breaking test should supersede all others. The instruments devised and used by the Author in these researches had the advantage that the test might be carried out at any desired speed, whilst the complete record could be examined at leisure; and the absence of friction and inertia in the apparatus ensured that speed of testing could introduce no error into the record. The actual handling of the apparatus was simple, and errors due to differences of touch did not arise: although special skill and care were required in the original construction and calibration of the instrument, its actual use involved no anxiety, but only ordinary care. A comparison between the diagrams obtained by the Author with these instruments, and those obtained with the stress-strain recorder of the late Mr. J. Hartley Wicksteed, M. Inst. C.E., which was widely used in single-lever testing-machines, showed the remarkable conformity of the forms obtained with the earlier recorder with those obtained by the far more accurate instruments of the Author. The much higher magnification of strain-scale in the Author's elastic instrument rendered it peculiarly suitable for the important research work disclosed by the Paper. The Author's instruments were now put to various uses in several very important research laboratories, in addition to his own, and Mr. Watson felt that it was only a question of time before the results of these researches would find a place in the standard specifications used by engineers. Mr. Watson

The AUTHOR, in reply, remarked that the valuable results obtained by Professor Bairstow (which Mr. Batson had mentioned) might be The Author.

The Author. taken as showing that the elastic range must fall within the plastic contour of a metal. That, of course, was in accordance with the later information given by the diagrams in the Paper. He thought Mr. Batson had omitted to notice that the spherical error due to the spot tracing a curve on the photographic plate was not carried into the data derived from the records, because, as mentioned on p. 22, the diagram was read by means of a grid plate which was placed upon it, and which had been calibrated in situ, so that the spherical error was eliminated. Such an error would arise only if the records were measured with linear scales, but that was never done. The Author's experience led him to believe that the recorders, and expecially the elastic type, had greater possibilities for research than Mr. Batson would appear to believe. The standard forms of laboratory extensometer suffered from two fundamental disadvantages: the loading was not continuous, and the observations were plotted by an observer who drew the best curve he could through the dots, so that personal error was introduced. To these might be added a third disadvantage, namely, the time occupied in obtaining even one curve. When the load was put on in steps, during the pause when the strain was being observed, creep began in the region of the limit of elasticity, and therefore the stretch was strictly a function of the time. Consequently, a step-by-step observation was very liable to give anomalous results when reduced to a curve. The Author was convinced that the recorders gave more accurate results for the purpose of most researches into the strength of materials than did other instruments now in use, and they possessed the further advantage of rapidity of action, which specially suited them not only for commercial work but also for scientific investigations involving a large number of tests. He felt sure that such a diagram as Fig. 26, Plate 2, could not be obtained with any other existing instrument. That diagram had been taken and the photographic plate developed in a few minutes. He did not think that the Table given by Mr. Batson, showing the difference between yield in tension and yield in compression, could be regarded as conclusive. That difference might be due to initial stress in the metal or to other causes. From the very large number of plates which the Author had taken he was led to believe that there was now practically no difference between the yield in tension and the yield in compression, especially in the higher carbon steels and the alloy steels.

Professor Cook had directed attention to an important property of iron and mild steel, namely, the sudden reduction of stress immediately after the yield-point had been passed. When the diagrams obtained with the Author's recorders were first shown, he had to meet the criticism that the indication of this reduction was due to instru-

The Author.

mental error; but Professor Cook's own experiments confirmed the accuracy of the records and showed that this drop was a property of the material. Professor Cook pointed out that the amount of reduction ranged from 24 to 30 per cent. and that he had obtained reductions as high as 35 per cent. The Author quite agreed; but the amount of reduction, of course, was not constant; it varied even in specimens cut from the same bar. In the hundreds of diagrams which he had obtained from iron and mild steel he had never found this property absent when the metal had been annealed, but it disappeared if the metal had been overstrained. His experiments showed that this was not a general property, but was peculiar to iron and mild steel, and that the sudden reduction of stress at yield became less as the carbon content was increased, until in the alloy steels it disappeared altogether. But he did not think that any general conclusion could be drawn without making a large number of experiments; he had found, for example, that the magnitude of the drop increased when the rate of loading was increased. In a record taken by him 6 years ago from a piece of Yorkshire iron, the drop, which was normally 10 per cent. when the specimen was broken in about 30 seconds, increased to 30 per cent. when it was broken so quickly that the yield was passed in approximately 2 seconds. He had always associated a large drop with acceleration in the rate of applying the load. That record, like all other records, had not been obtained in the ordinary lever testing-machine but in an hydraulic press, specially designed for use with the recorders. The torsion results and the deductions drawn therefrom were very interesting. He presumed that the formula would have no application to alloy steels; in fact, to be capable of general application it would have to have the composition of the steel brought into it.

Concerning Mr. Doak's remarks on the yield-point and the limit of proportionality, the Author had not tried to define these points, or indeed any points at all. It would be very difficult to do so satisfactorily, because the same terms were used in different senses by different workers. He found that it was much better to present the curve showing the elastic properties and to leave nomenclature alone. A definition of the yield-point which would apply to iron and mild steel would not apply to alloy steels. So far as the elastic property was concerned it was more satisfactory to say that, if the load could be removed and reapplied without producing a loop in the diagram, then the limit of elasticity of the material had not been exceeded; and that, if a loop appeared as a result of this process, then the limit of elasticity had been passed. The change from the elastic to the non-elastic condition was usually so gradual and subtle that it was difficult to identify it. As Mr. Doak had stated with

The Author. reference to Fig. 6, Plate 1, it was difficult to say where yield began, but it was quite certain that the limit of elasticity had been passed before the yield load. This could be shown by leaving a load less than 23·8 tons per square inch hanging on the test-piece; it would be found to creep with any load between 20 and 23 tons per square inch. Reference might be made here to the Author's Paper[1] on "Research on the Elastic Properties and the Plastic Extension of Metals."

Mr. Doak had referred to the cold bending of parts of bridges, which occurred in transit. The material used in making bridges was low-carbon steel which possessed the power of elastic recovery with mere lapse of time. Annealing was therefore unnecessary. But any parts made of high-carbon or alloy steel should be annealed, as those materials had not the power of elastic recovery with lapse of time. Reference might again be made to the Paper mentioned above[2]. The object of the extended research which the Author had undertaken was to correlate the mechanical tests now in use. He did not doubt that a little experience in the use of his recorders would convince anyone that supplementary tests were in most instances unnecessary.

He was particularly glad to read the remarks of Sir Alfred Ewing, and he would like to acknowledge the inspiration and encouragement which he received from Sir Alfred when he assisted him in the development of the Engineering School at Cambridge University.

As both Sir Alfred and Mr. Doak had pointed out, the resemblance between the stress-strain diagrams for cyclic processes of straining in steel and other metals and hysteresis curves showing the variations of magnetism produced by cyclic variations of magnetizing force were remarkable. The tracing of the fundamental community in physical origin of these curves did indeed open up a field of research which had still to be explored, but the Author had not yet been able to undertake this work.

He was gratified that his conviction concerning the possibilities of the recorders was shared by such an eminent investigator in this subject as Sir Alfred Ewing, whose statement that the Author had, in fact, placed in the hands of engineers a really valuable instrument of research showed that not only did the rapidity of action of the recorders suit them for commercial work, but they were equally useful for research.

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[1] Phil. Trans. Roy. Soc., Series A, vol. ccxxi (1920), p. 134, Fig. 24.
[2] *Loc. cit.*, p. 121, Fig. 4.