Discussion.

Sir CLEMENT HINDLEY observed that the Paper was a particularly well-arranged presentation of a useful piece of work, and he commended the completeness with which the details of the experimental work were given and the fullness of the summarized information contained in the Tables. It was often the case, unfortunately, that the bare record of facts of that kind might appear to an investigator to be so uninteresting as hardly to justify its presentation as a Paper, and it was sometimes assumed that some brilliant piece of deduction was necessary in order to bring a Paper up to the usual standard of those discussed by The Institution. The detailed presentation of experimental work of that kind was, however, of much greater importance to careful students than were the deductions that an Author might be tempted to make; and while he could not sufficiently praise this Paper for its admirable arrangement and for the record of the experiments carried out, its value would, in his opinion, have been enhanced if the Author had resisted the temptation to deduce a working-practice from his experiments, and had recognized that his series of experiments formed only a part of a very large subject. The subject of welding was important to all engineers, and to constructional engineers particularly so. The Steel Structures Research Committee of the Department of Scientific and Industrial Research had appointed a panel to work on it, and some time ago the Author, who was a member of that panel, had been good enough to place all the data contained in the Paper at its disposal. Dr. C. H. Lobban, Assoc. M. Inst. C.E., was the technical officer of the Committee and, he hoped, would take part in the discussion. It might be interesting to say something about the origin and direction of the work done by the Committee on the subject. The Committee had received considerable assistance from information, passed on to it from the Institution, which had been collected by Sir George Humphreys and Sir Cyril Kirkpatrick. It had not been possible to begin active work on welding immediately, because the Committee's funds had had to be used for more urgent work in other directions; but the Committee had been fortunate in securing the co-operation of certain firms interested in processes of welding, and funds and technical resources had been placed at its disposal, including a Government grant, which had enabled it

Sir Clement Hindley.

Sir Clement Hindley. to pursue the subject. Welding had been used to a considerable extent in structural work and in shipbuilding, although he believed that in shipbuilding it was used mostly in small details and in plate work, and not in the main structure. It was commonly believed that one process or another had been largely adopted in steel structural work in America. It was true that there were notable examples of its use in that country, and a few examples in this country, but, having regard to the advantages which, prima facie, might have been attributed to it, it was surprising to some people that it had not found more extended use. Amongst the advantages claimed for the process, generally speaking, as compared with the ordinary process of riveting, were rigidity of connections, continuity of welded girders, reduction in size of members—with consequent reduction in weight of steel—less liability to corrosion, and the absence of noise during construction. One of the reasons given for its non-use was the opposition of public authorities and the prohibitions or restrictions in building regulations. In point of fact, the reason for the limited use of the process was much simpler. There was little or no scientific knowledge to guide engineers in adopting the process when designing and constructing steel structures, and most of the work hitherto carried out had been based largely on empirical assumptions. That had now been recognized in America, and an important programme of research was being carried out in that country at the instance of the welding industry. The main difficulty lay in the lack of any satisfactory non-destructive field test of a welded joint, and the unfortunate fact that the only true way of telling whether a weld was sound was by cutting it open. The Committee, having placed this testing problem at the top of its programme of welding research, had had the advantage of studying the various methods that had been experimented with for devising a suitable non-destructive field test. Those processes consisted of X-ray examination, examination of variations in the electrical field, examination of variations in the magnetic field, a method in which a stethoscope was used, and a method which employed a tuning-fork and a microphone. So far, none of those methods had afforded much hope of producing a satisfactory field test, although research on the problem was being continued at the National Physical Laboratory. Therefore, whatever might be learned from destructive experiments about the strength of welded joints, engineers would for the time being have to rely on the use of standard materials, the skill of the individual worker, and visual inspection of the joint. The second part of the Committee's programme related to statistical examination of tests on welded joints, and

Sir Clement Hindley.

would be explained by Dr. Lobban. All he desired to say on that subject was that he advised very great caution in adopting the deductions and recommendations made by the Author. The conclusions set out under the heading of "factor of safety" might appear to be supported by the results of the Author's experiments; but it must be recognized that those experiments had a relatively narrow range, and, further, that some phenomena in his results were so far unexplained. He would not feel inclined to adopt the Author's rules for design and working-stresses, as given in the last pages of the Paper, in so far as they depended on his assumptions as to the factor of safety. He thought that caution was doubly necessary for the following reason, apart from the fact that the data were at present incomplete and the experiments to some extent inconclusive. The third part of the Committee's work consisted of research, firstly, on the design of welded joints, and secondly, into the effect of rigidity of joints on stress-distribution. He supposed that one of the first results of research was to bring about a confession of ignorance, even if that had not preceded the work of research; and it had to be confessed that, although enough knowledge of the behaviour of riveted joints was available to enable, by the adoption of certain factors of safety, steel structures to be put up with reasonable security, and although it had been assumed that the most perfect riveted joint was one that approached most nearly to the ideal of rigidity, the knowledge available of the effect of really rigid joints on the stress-distribution in a steel structure was very incomplete. With riveted joints, by analysis with extensometers and other methods, it had been possible to show generally that the methods of stress-analysis gave results approximating to the true facts; but with really rigid joints such as were obtained by welding, the conditions were entirely different. Some of the results of research in that direction had caused profound distrust of rigid joints used in buildings designed in accordance with theories that had so far been accepted. At the present stage of the Committee's work it would not be possible or reasonable to say more. But, at any rate, he thought they had got far enough to be able to deprecate strongly any large adoption of welding methods in steel structures until more knowledge was available. The work done by the Author, however, marked an important step in enlightenment on this difficult subject, and all engineers would be grateful to him for publishing his results; while the Research Committee owed a special debt of gratitude to him for his work and assistance. But they were still some way from complete illumination, and he was not sure that the Author himself would not be the first to admit it.

Professor Coker.

Professor E. G. Coker remarked that the Paper would prove of great value to engineers as a record of tests of joints carried out to destruction in order to ascertain their properties under load, and the Author was to be congratulated on having made full use of the facilities available to him. One of the chief features brought out in the Paper was the comparatively low strength of the welded joints as compared with riveted joints. That was not difficult to understand when it was borne in mind that the strength of such joints depended on the way in which the added triangular sections of metal were loaded by the forces impressed upon them. The experiments had been made on plates connected together either by joints in which there was longitudinal or transverse shear or else by butt welds. In whatever way the joint was made, oblique forces resulted, and generally the oblique forces caused very high stress-concentrations at one or more places in the joint. A typical case was exemplified by the double tension fillet joint shown in *Figs. 2* (p. 284). The load in the plates was uniformly distributed in sections remote from the joint; but at the joint the lines of stress passed obliquely out of the side of the central plate, through the weld-metal, and obliquely into the ends of the outer plates. The lines of stress were thus deviated sharply, and that produced high stresses at the apex of the angle of 135 degrees between the surface of a main plate and the fillet. But the chief difficulty was the very high concentration of stress at the inner corner of the fillet, which was virtually the end of a long crack. A mathematical analysis of the stress-distribution in a joint of that kind was obviously a complex problem, owing to the sudden changes of contour and cross section; and even in the improbable event of a solution being found, the stress equations would probably be so complicated as to make their usefulness negligible. Hence experiment must be looked to for further knowledge leading to the improvement of that form of construction. Arc welding had clearly come to stay, although probably its chief use, so far as could be seen from the state of the art at the moment, would be where gastight and watertight joints were required, as in large condensers and water-mains, and not in cases where heavy loads were carried. There was room for improvement in such a new method of construction, and possibly in the course of time a joint of an improved design would be evolved so as to reduce the high stresses indicated, and thus higher efficiency would be attained. The Author mentioned Professor Coker's work on the examination of transparent models of fusion joints by polarized light, and it might be of interest to say that he had been trying to get a general idea of the nature and distribution

Professor Coker.

of the stresses in joints of that type, as a preliminary to more refined experiments to be carried out under conditions approximating more closely to the actual ones. Simplicity in the experiments had been aimed at, and therefore the conditions assumed were somewhat more elementary than those which could be inferred from general working-conditions, and were not at present quite so complicated as those actually obtaining in any joint. [The speaker then described with the aid of drawings a number of welded joints and the corresponding transparent models used in the preliminary investigation of stress-distribution.] In the case of a single butt-strap joint, the two plates were connected by means of a central strap attached by triangular fillets of weld-metal. The simplest way of making the corresponding transparent model was to fashion a sheet to the required contour, slit it to represent the plates, as they occurred in the joints, and fill the slits with packing-strips so that the contact pressures corresponded to the way in which the actual plates bore on one another. The actual joint virtually contained a very long crack, with an extremely sharp curvature at the ends. He had not attempted to imitate that too closely, because it was an advantage to have the end of the crack with a sufficient radius to enable the stresses round its edge to be determined. At the end of an actual crack the stress would exceed the elastic limit for very small loads. The lines of principal stress in that model tended to concentrate on the inner corners of the weld. The diagram of contour stresses showed the stress around the end of the crack. It was extremely high, but not as high as would occur in a fusion joint, because the radius at the end in the transparent model was $\frac{1}{32}$ inch. The diagrams of the two principal stresses showed that the stress was very high at the end of the crack, but at the outer angle there was practically no stress. Another interesting feature of his experiments was the comparatively high boundary stress observed near the centre of the outer contour of the triangular fillet. This point coincided with the end of a line of fracture of one of the experiments shown in Table I, Series 1, of the Paper. Another joint with which he had experimented was a joggled lap joint, which was a much stronger form, because most of the lines of stress passed practically straight through without deviation, and only a very little stress developed in the joggled part of the plate. That was proved by the experimental curves shown, and by the stress measurements in the joint. It was also evident, again, that the comparatively easy path of the stress through the joggled fillet was much more conducive to low stress than was the case at the other end of the joint, where the passage of the stress lines was much more restricted,

Professor Coker.

because the shape of the triangular fillet was not conducive to good distribution. [The speaker exhibited lantern-slides showing the colour bands or isochromatics observed when the loaded specimens were examined under polarized light, from which the information he had just given was derived.] With regard to the possibilities of the method of photo-elastic investigation, there was no great difficulty in making models of welded joints in transparent materials of different hardness, as a very wide variety of materials was available. One of the next steps ought to be the examination of joints made of materials in which different physical properties of hardness, Poisson's ratio, and Young's Modulus, were imitated as nearly as possible. Another very necessary line of investigation was a search for new forms of joint giving better stress-distribution. The present forms were obviously very far from ideal. The third and the most difficult thing was to carry on the optical side of the research work so that the stresses in joints, which might be partly plastic and partly elastic, or all plastic, could be pursued. This was a relatively unknown region at present. A certain amount of experimental investigation had been accomplished on plastic tension, but physicists were not agreed as to the interpretation of the photo-elastic results. It was worth remarking that the stress pictures obtained for plastic material were very much more vivid than those given by elastic bodies. A systematic attempt was now required to discover the laws of behaviour of those colour effects. Once that initial difficulty in the plastic region was surmounted there should be no real remaining difficulty in taking the last step that he had mentioned in this field. The exact physical meaning of photo-plasticity effects would moreover help in every field of work into which the plasticity of materials entered.

Dr. Rosenhain.

Dr. Walter Rosenhain observed that the Paper dealt with a subject of extreme interest and importance. In the course of his official duties he had frequently to examine cases of failure. So far, these had usually been failures of riveted joints, and not welded joints, simply because riveting was so much more commonly employed. Nevertheless, the experience gained by frequent examination of cases of that kind suggested that the riveted joint was far from being an ideal connection. The difficulties arising from it were not merely those that the designer took into account; they were rather of a more subtle kind, owing to actions that were apt to occur, particularly in the presence of certain corrosive media, in heavily-stressed riveted joints. Not only from the points of view of economy and convenience of construction was welding to be welcomed, but also from the point of view of eliminating certain actions that were highly undesirable in riveted joints. Consequently it was not surprising to find that welding was making great strides, and he wondered whether the objections to it that existed were well founded. When data such as those contained in the Paper were examined, it was natural to look at them critically, and on the whole, although convincing enough, he thought that they were still open to a certain amount of criticism. Needless to say, he offered the criticism in no mere cavilling spirit. One criticism was that the Author appeared to base his values upon the average results of the welds. Was that justifiable? After all, the weakest weld would cause disaster if disaster was going to occur, and the lower limit should really govern practice, unless means could be found of eliminating those weak cases. The breaking-stresses in the butt welds described by the Author ranged from 14·9 to 22·6 tons per square inch. That was a very wide variation (50 per cent.), far wider than would be permitted in riveted work. Further, those welds had been made under workshop conditions: he was inclined to think that they were better for not having been made under laboratory conditions; but welds had to be made under field conditions, which differed very markedly from those in the workshop. The real trouble with all welding was the question of uniformity and reliability, which depended so much upon the skill and conscientious work of the individual welder. It had been said that, under modern conditions, the art of welding was almost the only one in which the skill of the workman was a governing factor. That had its advantages, but also its disadvantages, because control, supervision, and testing, became important. One method of testing suggested had been to arrange for the welder to weld samples as he went along, using for the samples the same materials and methods as for the larger work. That, however, would be unsatisfactory, because of the influence of dimensions upon the strength of the joints, which had been brought out by the Author. The Author mentioned that the appearance of a weld was a good guide to its quality, but the figures given in the Paper rather suggested that he must have been misled by appearances in some of his own tests. It was extraordinarily difficult to determine from the appearance of the surface whether a weld was sound or not. The Author used the analogy of concrete, but in the making of concrete it was possible to exercise, owing to the larger scale of the operations, a degree of supervision which was practically impossible in welding. As to non-destructive tests, X-ray examination offered certain possibilities, but it was almost impracticable to apply it to structures in situ. An X-ray examination of a welded joint could be made if it could be Dr. Rosenhain.

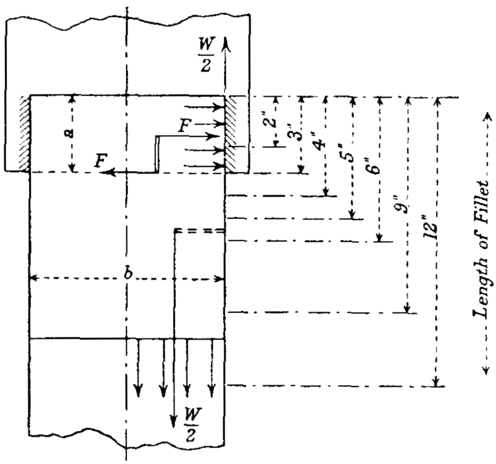
Dr. Rosenhain. handled in the laboratory or in reasonably accessible positions elsewhere, but in comparatively inaccessible positions the method was quite impossible of application. It suffered also from the disability that it was not always easy, unless the examination was made in several directions, to be sure that minor flaws or thin cracks were detected. Then, again, there was the difficulty about surface irregularities, although those could be avoided by packing the joint with some material of the same density as iron. The magnetic and electrical methods were rather more promising. It was true that they required rather careful use, and they suffered from the disadvantage of being unduly delicate for field manipulation. Nevertheless, it was possible by those methods to detect irregularities in the texture of a material which were quite small, although important from the point of view of strength. Great claims had been made in America for the acoustical method. He had not had any experience of it; but what he had heard from those who had tried it was not in its favour. With regard to the question of the rigidity of the joints, the effect of real continuity of material through the joints of a structure must be very important; but the Author showed, or at any rate claimed to show, that there were comparatively large differences in elastic modulus between the weld metal and the plate. With all deference to his experimental skill, Dr. Rosenhain was a little surprised at those figures, because they showed values having a ratio of 12 : 13 for the elastic moduli of two metals differing only slightly in chemical composition. A lengthy experience covering a very large number of tests had satisfied him that the elastic moduli of different kinds of steel never differed very much, even where the differences in composition were large. He did not think the difference between the highest and the lowest modulus was anything of the order of one-twelfth. The inference was that a good part of the specimens tested by the Author was not metal at all, and the stress was being calculated on an area much larger than the real area of the metal. If the differences of elastic modulus shown by the Author's figures did exist, that fact would have to be taken seriously into account by designers and others, and also by Professor Coker in his beautiful method of examining transparent models by polarized light. Professor Coker's remarks had suggested the question, how far it was justifiable to regard the space in welded joints of that kind, where the plates lay against each other, as equivalent to a crack. A crack, Dr. Rosenhain would suggest, was a fissure or discontinuity where there was stress on both sides. Actually, of course, the stress in some parts of those plates was zero. Professor Coker's results confirmed his own strong preference for a butt weld. It was, to his mind, unsatisfactory to stick two plates together by small fillets at the ends or sides of the areas where they overlapped. Dr. Rosenhain. If one had to trust a weld, one should trust it thoroughly, and carry the stresses right through it in direct tension, and not in shear. He imagined that the conditions under which the weld-metal had to do its work in actual practice were better in a butt weld than in fillet welds. Even the suggestion of putting a strap over a butt weld was not entirely satisfactory, if only because it hid the actual weld upon which reliance was placed. If a weld was to be relied upon, it was desirable to ensure that it was a good weld, and then to rely upon it entirely, and not to try to bolster it up with subsidiary arrangements. The Paper contained many data which would be of very great use, but, as Sir Clement Hindley had said, there were some little difficulties about interpretation. The Author had referred to the tendency of the plates, particularly when longitudinal joints were used, to tear near the joints, and had ascribed it to stress-concentration. No doubt he was right up to a certain point, but in several instances fracture had occurred just a little distance from the weld and not at the weld. Heating steel to the fusion point for welding was bound, at some point before the fusion temperature was reached, to injure it to some extent. The extent of the injury would depend upon the time for which the high temperature was maintained, and with an intense source of heat like the electric arc the time would be shorter than with other processes. Nevertheless, there was a tendency towards the spheroidization of the cementite, which was shown clearly in Fig. 8, Plate 5, as a sort of blurring of the microstructure. That meant that every little plate of cementite had begun to draw up into a globule. In that condition steel reached its weakest state, as regarded both tension and fatigue. When the joint was strained to fracture, in many cases, particularly when the weld was good, the fracture would occur in that region of the steel, unless the steel could be subjected afterwards to some normalizing treatment. That local weakening of the steel by the process of welding was one of the inherent disadvantages which designers would have to allow for when they came to calculate the strength of such joints.

Dr. W. H. Hatfield remarked that he considered that the Dr. Hatfield. Author's method of approach to the subject was perfectly satisfactory. The method of welding that he had chosen was quite as good as any that was likely to be in existence at the moment, and it was particularly interesting that he should have used a welding material the composition of which was modified in order to mitigate the effect of the oxidation that inevitably took place during welding. He referred to the particular welding material with a high manganese content; the manganese oxidized out to a normal

Dr. Hatfield. content, and thus protected the steel. The subject of welding should be regarded from two angles; in the first place, the operation of welding itself, and in the second place, the use that engineers proposed to make of welding. Bearing on that, he wished to draw attention to one point. Presumably the joints that the Author had tested had been loose ends. If two loose pieces of steel were welded together, they could contract as the welded material cooled ; but if members were welded into a structure in a position where, when the heat was dissipated, considerable internal stress was left in the weld, as might readily be the case, other considerations entered. In that event, surely, independent tests performed upon prepared samples of welds for testing purposes did not give any true indication of the value of the weld as an integral portion of a structure in cases where contraction could not be permitted. He had been very interested to hear Professor Coker, and it had occurred to him that, complex as the orientation of the lines of stress was in the simple cases he had described, he would surely be amazed if he could weld the transparent material with which he worked into a structure, and photograph the residual stresses. He did not think that too much attention could be given to the rate at which heat was applied for a given time. Under present conditions the maximum amount of work must be done in the minimum of time, and that meant that in welding a very large amount of heat was concentrated both in the weld and in the adjoining metal. Of course, welding was an art, and the excellence of a weld depended entirely upon the excellence of a craftsman. He was not deprecating the value of welding : he believed that it was of the utmost value, and that it was capable of wide application, particularly to structures. A short time ago he had discussed this question with engineers in New York. He asked them what means they had of really judging the excellence of a weld. They could only answer that they trained the welder, the welder was then observed in his work, and on the merits of that inspection the welder was permitted—or was not permitted, as the case might be—to weld important structures. If he was permitted to do so it was assumed that he welded them properly. He quite agreed with Dr. Rosenhain about the modulus of elasticity of the weld-metal, but he would place any possible variation in that modulus, within the limits of composition suggested in the Paper, at no more than 2 per cent. In building structures heat-treatment after welding could not be contemplated, but it was a fact that welding almost necessarily left some residual stresses in the weld. In certain types of weld those residual stresses would be abnormal. They were simply due to plastic deformation occurring when the heat was applied in the first place, which prevented the welded material from shrinking to its normal dimensions on cooling. Thus, governed by the coefficient of expansion and the modulus of elasticity, residual stresses would be present. If structures or parts that were welded could be heated after welding, say to a dull red heat, at which temperature the steel readily deformed, dangerous residual stresses would be dissipated by plastic deformation of the material. Dr. Hatfield.

Mr. HARRY JACKSON remarked that he approached the subject not as a physicist but as engineer who had read something of the work done in America and was anxious to design and carry out more economical structures, if possible. He thought the Author was misdirecting his efforts in endeavouring to set out rules of Mr. Jackson.

Fig. 23.



Scale: 1½ Inch = 1 Foot.

a denotes length of weld fillet.
b ,, breadth of strip.
f ,, shear stress on fillet per unit of length.
W denotes load on strip.
w ,, longitudinal stress on plate.
f_t ,, transverse tension in plate.

guidance for design. He liked to feel, if he cut a piece of material of known quality into pieces, that unless there was some inherent defect in that material he might rely upon all the pieces for particular properties such as tensile strength, etc. Three figures had impressed him in reading the Paper. The first was the tensile strength of the small pieces ; the next the strength of a ⅜-inch weld ; and the third, and intermediate, figure was the tensile breaking-strength of the Author's specimens, namely 20 tons per square inch. If there was any condition causing the recognized tensile strength of the material to be reduced from 28 or 33 tons per square inch to 20 tons per square inch, some factor was involved which he did not understand.

Mr. Jackson. An investigation of one set of tests by the Author would probably clear the matter. Taking the case of the longitudinal welds in Series 3, Table II, the stress might be assumed as being evenly distributed in the $\frac{3}{8}$-inch plates (*Fig. 23*), and there would be symmetry about the longitudinal centre line. The only force that was transferred from outside to the $\frac{3}{8}$-inch plate other than the evenly-distributed load was some force transmitted through the fillet. If W denoted the total load in each $\frac{3}{8}$-inch plate, the longitudinal pull in each fillet would be $W/2$. Those two equal and opposite forces formed a couple. If the joint was in equilibrium that couple must be balanced. Such balancing couple would be produced by a force F, perpendicular to the direction of the fillet, and an equal force F induced by lateral compression in the $\frac{3}{8}$-inch plate at or near the end of the fillet. Then

$$m = \frac{W}{2} \times \frac{b}{4} = \frac{Wb}{8}; \text{ or } m = F \times \frac{a}{2}.$$

Hence
$$F \times \frac{a}{2} = \frac{Wb}{8}, \text{ and } F = \frac{W}{4} \times \frac{b}{a}.$$

The result of applying that investigation to the Author's tests in Table II, Series 3 was given in the Table below. If a straight-line stress-variation across the section of the $\frac{3}{8}$-plate were assumed, the maximum stress would be double the average stress. Combining the transverse and the longitudinal stresses the resulting principal stresses, given in column 6 of the Table below, were very much nearer to the ultimate strength of the material, namely, 28 to 30 tons per square inch. A ready way of proving whether that method of dealing with the problem was correct was to make a

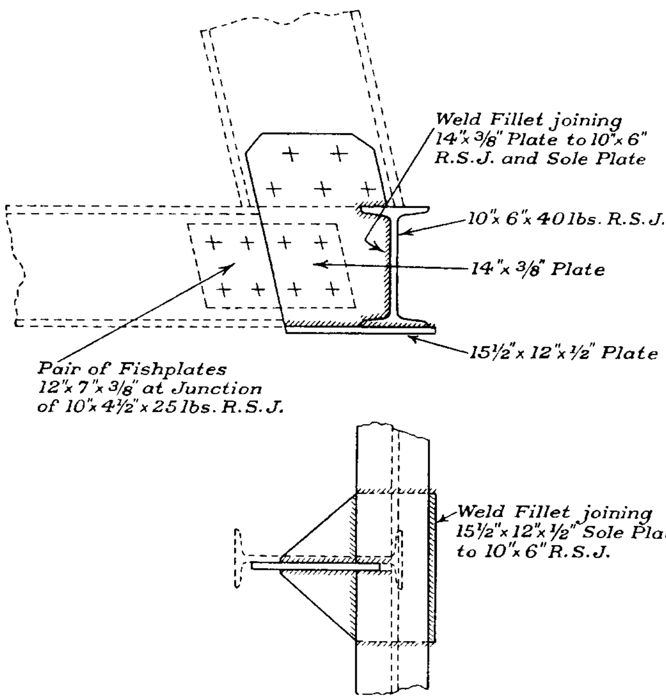
INVESTIGATION OF TEST SERIES 3, TABLE II.

No. of Test.	Ratio $\frac{b}{a}$	W	F Averaged over $\frac{b}{2}$	Maximum Stress on Assumption that Maximum = 2 × Average.	Combined Longitudinal and Tranverse Stress.	Remarks.
		Tons.	Tons.	Tons per Sq. In.	Tons per Sq. In.	
3 A .	4·0	20·4	20·4	27·2	28·0	Broke at welds.
B .	2·67	30·7	20·5	27·3	29·1	Broke at welds.
C .	2·0	42·2	21·1	28·1	31·4	Broke at welds.
D .	1·60	51·0	20·4	27·2	32·0	
E .	1·34	59·0	19·8	26·4	32·8	Plate torn at end of weld.
F .	0·89	62·0	13·8	18·4	27·6	
G .	0·67	65·5	11·0	14·7	26·3	Special fracture at end of weld.

series of tests with a fixed length of weld, the cross section of the plate being kept constant, and the ratio b/a being varied. It seemed very probable that the stresses obtained would approximate to the ultimate stress of the material found from small specimens. Such variations as occurred would probably be due to the points of application of the forces F not being exactly as assumed in the calculations. It seemed probable that all the results of the Author's

Mr. Jackson.

Figs. 24.



tests could be explained by such an investigation without reference to special stress distribution of the kind suggested on p. 292. The members might like to have particulars of a case where he had used electric welding in constructional steelwork. In adding two additional storeys to a building, he had not considered it wise to put concentrated loads on existing brickwork. The arrangement depicted consisted of a rolled steel joist laid on the existing brick wall. From that joist raking stancheons were carried, which in turn carried the beams of the additional floor and roof. The rakers

Mr. Jackson. were manifestly subject to bending as well as direct stress, and it was possible to introduce the resisting moment to the bending at each or all of the three connections. The middle connection was used. A piece of joist cut to splay was again cut along the web, the pieces were turned over and the web was butt welded. That formed the angle piece through which the resisting torque was transferred to to the raker, and was made without any waste of material whatever. The foot of the raker was joined to the supporting joist, acting as a wall plate, in the following manner (*Figs. 24*). A sole plate was welded to the bottom flange of the joist by fillets along the edges of the flange. A cut of sufficient width to receive a $\frac{3}{8}$-inch plate was made in the top flange, the plate was inserted and fillets were run where the plate touched the joist or sole plate. That plate served for bolting the web plates of the raker and transverse joists. A trial weld of the foot joint had been made, which he exhibited. The remarks of some of the speakers had a rather discouraging tone. It would almost seem from them that engineers must wait until electric welding had reached perfection before they could make any use of welding at all. Absolute perfection would never be got. Expansion and contraction must be met by design; the personal factor of workmanship could be allowed for. If welding was a cheap and safe way of working, why not adopt it? And if there were the difficulties mentioned, the adoption of a factor of safety that would cover the points raised by the two speakers who had spoken from the metallurgical point of view would allow very sound structures to be built.

Mr. Bruff. Mr. H. Bruff stated that he had been privileged to conduct, on behalf of his chief, Mr. John Miller, M. Inst. C.E., Engineer of North-Eastern Area of the London and North Eastern Railway Company, a series of experiments and tests in connection with electric welding as applied mainly to underbridges. He did not think the Author had sufficiently stressed the importance of the internal stresses set up by contraction. The tests Mr. Bruff had carried out showed that such internal stresses were likely to affect the strength of the sections. Mr. Miller had recently partly renewed two 70-foot-span skew plate-girder underbridges at South Shields. The whole of the flooring had been renewed by replacing the old floor with cross girders of joists having plates welded top and bottom, joist rail-bearers, and plate decking electrically welded by the railway company's men. Before beginning work, it had appeared advisable to obtain definite knowledge whether any deformation of the beams that were to be welded was likely to take place. For that purpose plates were welded on to the top and bottom flanges of three beams about 9 feet long. From previous experiments and experience it was considered desirable first to weld long fillets in short lengths of 1 inch; 12 inches apart, along the joints, so as to avoid undue heat-concentration, and then to complete the welding in 6-inch lengths. But in two of the tests no such precautions were taken to avoid heat stresses; on the contrary, two welders started at one end and worked continuously to the other, working as hard as they could for continuous lengths of 6 inches, so as to keep pace with each other. One beam was welded with the web vertical, the two welds on one flange having been made simultaneously, and when the beam had cooled off it was turned and the process was repeated for the other side. The second beam was welded with the web horizontal, both welds being laid down simultaneously, the beam turned, and the other side done. The third beam was welded gently as described with tack welds, in the same position as the first beam. Although in every case the beam was appreciably distorted after the first two welds had been completed, when after cooling off the welding of the remaining two fillets was finished the beams were all perfectly straight; but they had contracted, the maximum contraction being $\frac{3}{32}$ inch along the flanges and $\frac{1}{16}$ inch along the neutral axis. Assuming that the weld metal of the four fillets had contracted and caused the shortening, the stress in the weld metal would require to be 314 tons per square inch, which obviously could not be right. They were still engaged on that problem, not having so far altogether solved it. The experiments and the preparation of the test-pieces were carried out at Park Lane shops, Gateshead, and the testing was done at Armstrong College, Newcastle, by Professor C. J. Hawkes, Assoc. M. Inst. C.E. One of the beams used for the welding experiments had been tested together with a beam that had not been welded, and the yield-points of the two beams appeared to be identical. That seemed to be inconsistent with the existence of high internal stresses, and he would be glad to hear an explanation of it. Mr. Bruff.

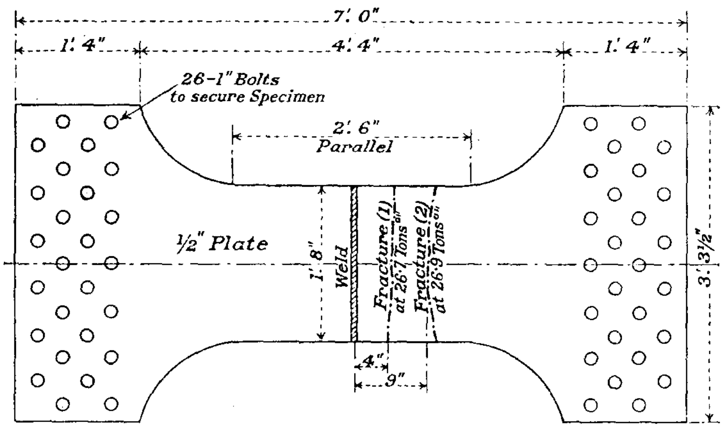
Mr. R. S. KENNEDY desired to emphasize the remark of a previous speaker, that the Author's tests had been carried out under workshop conditions and not under field conditions. In particular, the welds had been run on the flat, that was, with the electrode above the work. Those conditions must be borne in mind in estimating the value of the tests. Reference had been made to welding in ship-construction, and it had been stated, quite rightly, that, generally speaking, welding was not employed in parts directly subject to structural stress. His own experience in welding on a large scale had been with ships. Although in new construction welding was Mr. Kennedy

Mr. Kennedy. not generally employed on parts subject to structural stress, it entered very much into conversion work. He had in mind particularly the conversion of vessels to burning oil instead of coal, in which case structural water-ballast tanks were converted wholly or partially to carry oil. In order to get the seams tight the usual practice was to weld a large number of the laps. The subject of the Paper was, he thought, interesting in that connection, because undoubtedly the cause of any leakage in a seam would be movement. A welded seam would have to take the stress for which the riveted seam was designed. His firm had found in practice that with what the Author called a fillet weld (also known as a lap weld) it was necessary to weld the full depth of the plate; that was to say, a full lap was given, and in that case the joints stood. In that class of work thousands of linear feet of welding had been done, and the slightest failure would have very serious results—not so much on account of loss of oil as because any leakage of oil into the cargo-space might damage cargo worth thousands of pounds. The practice he had just mentioned had been found to give good results under heavy straining of the structure at sea, which fact was quite encouraging for the end the Author had in view. On p. 291 the Author referred to the allowable load per unit depth of fillet, and suggested a reduction as the depth increased. Whilst Mr. Kennedy had no precise data on the subject, his firm had always based their calculations more on the idea of starting with a constant due to initial penetration of the plates, after which the strength of the fillet was dependent on its section. As far as $\frac{1}{2}$-inch plates went the result would be the same, but the point became rather more important in bigger sections. Also, generally speaking, his firm considered it good practice for a weld to be in direct tension or compression, and to avoid bending or cross strain. From that point of view, as regarded fillet welds, he was in favour of the transverse fillet weld with the returned ends advocated by the Author. A rather important matter from his firm's point of view was the butt weld (Table V). In clause (2) on p. 302 the Author referred to the precautions to be taken against creep of the plates in butt welding. Presumably, therefore, they had been taken in the test specimens. He was referring particularly to the wide test specimens—those 8 inches wide. The Author had obtained about 20 tons per square inch or less in his tests, which Mr. Kennedy thought very disappointing. His own firm had had experience of butt welds for many years, and butt welds 3 to 4 feet long were quite ordinary practice. They had never had any trouble with them, but of course the classification societies had desired to be convinced that the

Mr. Kennedy.

strength was satisfactory, and about 12 years ago his firm had had to make some tests for Lloyd's Register. The width of those plates at the weld had been 20 inches, and they had been ½ inch thick (*Fig.* 25). They had been rather difficult things to test, but it had been done on Kirkaldy's machine, and they had broken in the solid. They had been rather low-tensile plates, and had broken at 26·7 tons per square inch, and 26·9 tons per square inch, respectively. That was the kind of result that ought to be got. His firm relied on butt welds in their work, and if they did not get such figures they would be very disappointed. In that connection he though the Paper was very valuable, because a more highly skilled man was required to make a good butt weld than to make a fillet weld. That was his personal experience, and he thought it was

Fig. 25.

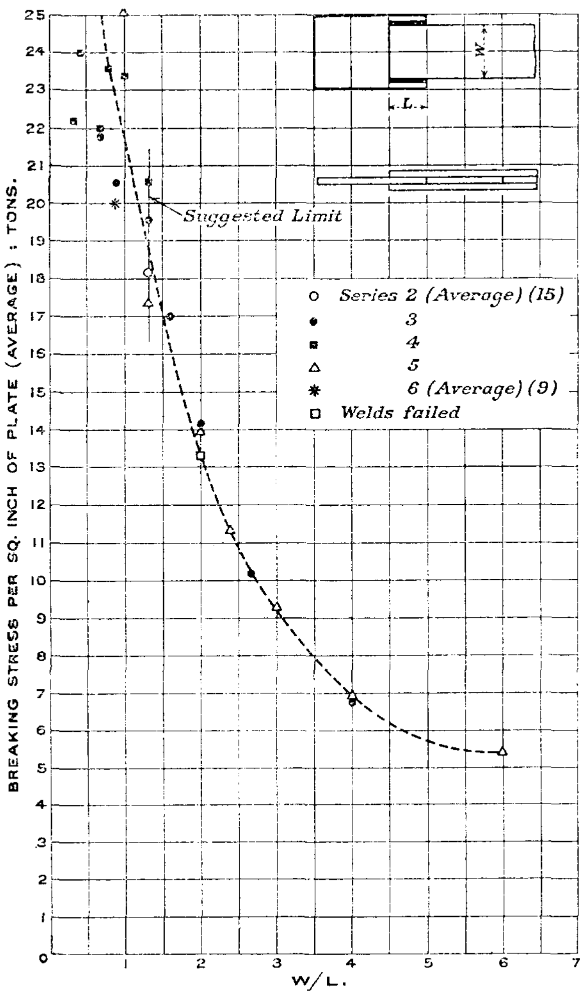


indisputable. The Author, however, was apparently rather looking to mass production. Probably from that point of view he was right in advocating fillet welds, although from Mr. Kennedy's point of view (perhaps rather a theoretical one) it was always preferable to avoid those transverse strains which had been referred to by Professor Coker and Dr. Rosenhain. On p. 304 the Author stated that butt welds should preferably be reinforced. That, of course, was quite right, and in one of his sections the Author showed the reinforcing pad. It might be just a slip in the print, but that pad should not be stopped at the edge of the **V**. It should be carried across ¼ inch or so, and be gradually tapered away on to the solid plate. In practice Mr. Kennedy's firm had found that a reinforcing pad of something like 20 per cent. of the thickness of the

Mr. Kennedy. plate was quite sufficient. If it was put on both sides of the plate the 20 per cent. was divided between the two. The last speaker had described a very interesting test which he had made on a 9-foot girder by welding on plates, working as rapidly as possible, and how the whole girder had at first distorted and afterwards returned to its original form, but with the flanges $\frac{3}{32}$ inch shorter in 9 feet than at first. Very extraordinary things did happen, and it was interesting to investigate such possibilities. Some years ago his firm riveted two $\frac{7}{8}$-inch plates, about 3 feet wide, on to as solid a structure as they could devise, made of beam sections such as were used for ships' decks. Those plates were rigidly held in position for welding, and then butt-welded right across. They were trammelled very carefully before and after, and it was not possible to detect any difference. Pieces cut out across the weld and tested gave quite normal results. His firm had then come to the conclusion that what they had to be afraid of and to avoid in butt welding was distortion of the structure rather than any weakness in the weld itself. The Author mentioned that no fatigue tests had been undertaken with welded specimens. Mr. Kennedy thought it was fairly well known that with good welds under usual alternating stresses about three-quarters of what would be got with a solid mild-steel bar was to be expected—up to the usual 5 million revolutions. In concluson, he was heartily in accord with the Author that welding should be carried out by a competent firm and by well-trained men, under supervision. A great deal depended on the welder.

Dr. Lobban. Dr. C. H. LOBBAN remarked that, the American Welding Society having already drawn up a very large programme of experiments on the strength of arc-welded joints as applied to structural steel work, including shear and tension joints, and also a combination of those, the Steel Structures Research Committee had decided to adopt a modified form of that programme on a smaller scale, according to the money available, and to carry out similar tests. The object of the tests was to get statistical information as to the strength of the joints. They were mainly a test of the skill of the welder and the type of electrode, rather than of the strength of the joints. For that purpose he had prepared a series of drawings of standard test-pieces. The idea had been that each firm interested in the subject of welding would have a number of specimens made by each welder, and that those would then be tested, so that an average figure for the reliable strength of each joint would be obtained. Certain other tests had also been arranged, in order to investigate the effect of the shape of the joint on its strength. After that had been done he had got into touch with the Author, who had very kindly let him see his results; and naturally he had gone into them very carefully. The Author mentioned, with regard to the fillet weld in shear, that the length of the joint should be not less Dr. Lobban.

Fig. 26.



than three-fourths of the width of the plate. That had suggested to him the idea of plotting the results of the Author's figures, and he had prepared *Fig. 26*, which gave the results of forty-five tests, Dr. Lobban. and showed the proposed limit of L equal to $\frac{3}{4}$ W. The diagram suggested that fillet shear welds should not be used by themselves, but possibly in combination with tension welds. The diagram was also a tribute to the care with which the Author's experiments had been carried out. Just after preparing that diagram he had seen a Paper[1] by Messrs. Mackay and Bain, who had been tackling the same question, namely, the distribution of stress in the fillets of shear welds. Those gentlemen gave a mathematical analysis which was very complicated; they had also prepared two specimens which they had carefully tested with extensometers. *Fig. 27*, which had been copied from their Paper, was a geometrical view showing one plate on top of the other. The fillet weld was along the sides and the

Fig. 27.

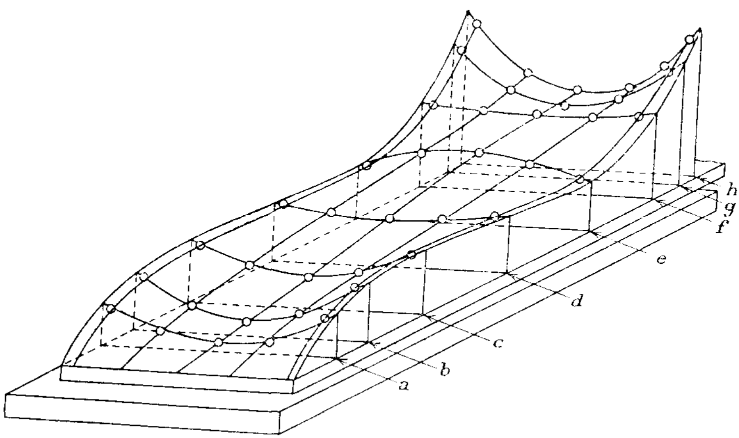
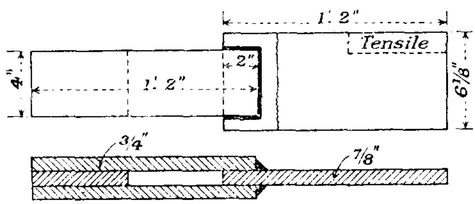


diagram was their stress diagram, as obtained from the extensometer readings. At the end it showed the distribution of stress across the top plate. The two high peaks at that end indicated the reason why the Author's specimens had failed. In the case illustrated the length of the weld was two or three times the width, and naturally that action would become more acute as the length became relatively shorter. There was such a high concentration of stress at the corner that the plate tore right across. With those figures in mind he had looked into the programme which had been drawn up for the steel structures research, and had decided that probably the bulk of the specimens would fail in that same way, and therefore no

[1] Canadian Journal of Research, September, 1930.

figures would be obtained. The programme had been modified Dr. Lobban. accordingly by thickening the plates so that a ⅜-inch weld would not be used on less than a ¾-inch plate—or something of that order; that was to say, the plates would be of sufficient strength to withstand the eccentric loading. A set of the new specimens had been got ready. It was composed of a complete set of ten, and about 3 weeks ago there had been a preliminary trial with them to see whether they would satisfy the conditions. They had done so with one exception (*Fig. 28*). Unfortunately that specimen, which weighed about 56 lbs., had been allowed to drop as it was being put in the testing-machine. Also it had sheared edges. It broke in the plate at 14 tons per square inch. It was not exactly known whether a minute crack had formed in the specimen when it was dropped, but two more specimens were to be made in order to settle that question. It might be that the result was due to the shape of the joint rather than to any other cause. With regard to

Fig. 28.



Mr. Kennedy's remarks as to the tests for Lloyd's Register made by Messrs. Kirkaldy, he would point out that the original weld was made on a plate 40 inches wide, which was then machined as shown in *Fig. 25*, reducing the original 40-inch width to 20 inches.

Mr. Conrad Gribble remarked that in the very interesting Mr. Gribble. discussion that had taken place most of the speakers had addressed themselves to the particular subject of the Paper, and, until the last speaker or two, they had not drawn attention to one or two features in arc welding which had exercised railway-engineers perhaps more than the question of the absolute strength of joints. About 3 years ago, when he first had his attention drawn to the great advantages which arc welding showed in connection with railway-bridges—particularly in connection with their strengthening—the accounts given to him and his colleagues of what had been done in arc welding abroad seemed to open out a vista of economy and of possibilities in connection with the strengthening of bridges such as they had never dreamed of previously. To some of them, at

Mr. Gribble. all events, it had seemed that they had, or were likely to have, available a means of strengthening bridges that would reduce the cost to such a degree that there seemed to be almost no limit to the advantages that would accrue. Since that time a good many bridges had been strengthened—and, so far as was known, successfully strengthened—by arc welding; but there was another side to the problem besides merely the strength of the joint. It was a comparatively easy thing to ascertain that joints which were to be made by welding would at all events be strong enough, and sufficiently free from defects, to enable the added metal to take its fair share of the load without any serious detriment to the girder. Of course, in railway work there were, in addition to bridgework, a large number of uses for arc welding in which the question of initial stresses was not of any serious importance. In another and quite different field, namely, reinforcing worn permanent way, the use of arc welding had now reached a stage where great economies were being realized. But the bridge problem had taken on rather a different aspect. There was the factor of contraction. So far as he could ascertain, no experiments made in this country on bridges which had been strengthened by arc welding had thrown any light on the question of the stresses in the girder after the metal had been added. He knew that girders which had been strengthened had been tested for deflection and for live-load stresses by deflectometers and stress-recorders, both before the strengthening had been carried out and after; but it did not seem to him that that really dealt with the question. It was quite clear that if a considerable amount of flange-area was added to a girder, the deflection of that girder under live load would be reduced, and the recorded stresses under live load would be lower; but those tests did not indicate what were the stresses induced in the girder during the process of adding the metal; and until that problem had been solved railway-engineers would, in his opinion, have to proceed rather cautiously in any work of strengthening bridges by adding metal on a large scale. Some research had been carried out in Germany, and other researches were now in progress, he believed, on the problem of contraction stresses. The German investigators had shown very clearly that two flat plates could not be laid on the floor and welded together without stresses being caused in them. Supposing that one started to run a seam between two plates which were quite free to move and in which there was no initial stress whatever. Beginning with a certain gap between the plates—which must be barely sufficient to enable a proper joint to be made—and working across, by the time the middle of the plates was reached the gap at the far end would probably have closed.

Mr. Gribble.

If, on the other hand, the plates were spot-welded together and then the spaces between were filled, local stresses between the plates would be caused by the contraction of the metal that had been added; and the investigators in Germany saw no means at present of performing even such a simple operation as the welding of two flat plates together on the floor without introducing stresses into them. The problem was, of course, a difficult one to solve. His own view was that a better idea would be obtained of what had happened to a girder as the result of adding metal to it if full-size tests of girders could be made, testing them to destruction, rather than by relying on any records taken by deflections or stress-recorders. If, during the process of welding, a stress-recorder could be applied to the girder, and all variations of stress which took place during the working process could be recorded, it would be possible to control the operation and to know what was being done; but that was hardly practicable, and he thought there was very grave risk that, in adding the metal, stresses of which nothing at all was known were being induced in the girder. One speaker had ended his very interesting and valuable remarks by urging engineers to cease worrying so much about the welding, and to get on with the work. Nothing would give engineers greater pleasure than to be able to do that, because the economies which might be obtained by strengthening bridges by welding, rather than by the comparatively crude method of cutting holes in them and riveting pieces on, were so substantial; but he would like to suggest to the meeting that, in addition to the very valuable research which had been carried out by the Author on the actual strength of joints, the next step which had to be taken before engineers could feel complete confidence in that method for railway work was to know what really happened to the girder when the new metal had been added. If the Steel Structures Research Committee could continue their researches into the induced stresses, it would be of the greatest possible benefit to railway-engineers.

Mr. Newlands.

Mr. ALEXANDER NEWLANDS remarked that the Author had brought forward a very useful Paper on an interesting recent development in engineering practice. His own interest in the subject arose largely on the question of the possibilities of arc welding in the strengthening of existing railway-bridges; and from that point of view he had looked through the Paper to see what he could find in the way of tests made on welded joints. He noticed that, as had been pointed out by a previous speaker, tests had been made on specimens which had been welded on the flat, under what were called in the Paper "workshop conditions." Those tests were doubtless interesting, but in railway maintenance it was rarely possible to do

Mr. Newlands. welding on the flat. A great deal of it had to be done from below, the electrode having to be held up to its work instead of being held above it. He wondered if the Author could give any results of tests of welds made with the electrodes operated from underneath. No data were given in the Paper as to the behaviour of welding metal or of welded joints under fatigue tests or impact tests. Those, he thought, would be almost essential if railway-engineers were to go forward with confidence in adopting welding for bridge-strengthening work. Such data might be in existence, but he could not find any reference to them in the Paper. If his criticism was founded on any basis of fact, it would probably be necessary for the working-loads mentioned in clause (14) on p. 303 to be substantially reduced in case of repair work from below on existing structures, and on structures subject to impact. He wondered whether scamped work done under the difficult conditions in which repair work had to be done on railway-bridges could easily be detected in arc welding. Scamped work had to be guarded against, and looked for, and detected, under all conditions of repair work. It was relatively easy to detect it in riveted work. Would not arc-welding work, done as it would have to be done in the field, be largely work in regard to which the engineer would be entirely in the hands of the welder, and dependent on the honest intention of the welder to make a good job? If he did not make such a job, would it be possible to detect that by inspection afterwards?

Mr. Brown. Mr. C. J. Brown considered that the Author had added considerably to the value of his Paper by including draft rules at the end, which might form the basis of any future standardization of welding work. He noticed that the Author laid down working-stresses based on the tests which he had made in the shops. As Mr. Newlands had pointed out, in field work the skill of the workman was a very important factor. The factor of safety which the Author had adopted appeared to be about 3·2. In ordinary bridgework the standard factor of safety for steel in tension was $3\frac{1}{2}$. For rivet steel it was just over 4. He doubted whether it was advisable to work to such a low limit as 3·2 for welded work. He had always understood that the strength of a $\frac{3}{8}$-inch fillet weld was about 3,000 lbs. per linear inch, or 1,000 lbs. for each $\frac{1}{8}$-inch of weld, which was rather less than what the Author put forward. He thought railway-engineers, on the present knowledge of welding for structural work, would be inclined to require rather higher factors of safety. There was another point—perhaps a little outside the scope of the Paper—which he would like to put to the Author. It related to the question of corrosion in bridge structures with welded joints. In riveted work the plates and sections were held together under considerable pressure due to the contraction of the rivets in cooling. Consequently rust could not get between the plates. Railway-engineers knew from bitter experience that if those rivets were placed too wide apart, corrosion did occur between the plates, forcing them asunder. No such initial compression existed in welded plates, though, welds being continuous, perhaps such corrosion would not occur unless a weld failed. Perhaps the Author would be good enough to give his opinion as to what might happen in that respect as the life of the structure extended. Mr. Brown.

Professor S. M. Dixon remarked that no more important and interesting subject could be brought forward than the consideration of the strength of arc-welded joints. In every country structural engineers were looking for guidance in that field, and everyone would welcome the Author's account of his researches. The Paper described so completely the methods studied by the Author that there was little room for criticism. He would limit himself, therefore, to one or two questions on perhaps minor points. On p. 286 analyses of the "Quasi-Arc" mild-steel and uranium steel electrodes were given, and it was stated that the slag prevented the oxidation of the weld metal. What happened to the manganese? The analyses of the weld metals on p. 298 showed that it was reduced from 0·5 and 3·1 per cent. to 0·1 and 0·5 per cent. respectively. What arguments could be brought forward for using transverse welds instead of butt welds, or for using longitudinal fillets? It might appear from the Paper that the arc-welded joint (other than the butt joint) was a joint in which strips of metal were substituted for rivets. The method of making the joint was so different that it seemed as if the whole design should be modified. That was especially true of the joint with longitudinal fillets, as demonstrated by the failures shown in Table II, and had been confirmed by the figures given by Dr. Lobban. He did not agree with the Author's explanation of some of the irregular results obtained. On p. 292 the Author said that inequality in the distribution of stress across the section necessarily existed; but Professor Dixon thought it was for the experimenter to design the specimen and to arrange the method of gripping it so that, in tests at least, that inequality would be insignificant. Arc welders had already demonstrated that the strength of a weld could be made equal to that of the bar or plate itself; and was not that really the information for which structural engineers were looking? In the design of a riveted joint the engineer sought to obtain the full strength of a bar, and the arc welder must show the correct form of weld in each case so as to give that. Great success Professor Dixon.

Professor Dixon. had been obtained with butt welds. In 1927 he himself had tested three bars, 1·4 inch in diameter, electrically welded. Two of those broke clear of the weld, at 55,000 lbs. per square inch, and one broke in the weld at 50,000 lbs. per square inch. In June, 1930, he tested some bars 2 inches by 1 inch, 2 inches by $\frac{3}{4}$ inch, and 2 inches by $\frac{1}{2}$ inch, butt welded by the Quasi-Arc process, all of which broke outside the weld, the strengths of welded and unwelded bars being 25 tons per square inch. The elongation on 8 inches over the weld was 26 per cent., while that of the plain bars was 31 per cent. Similar results had been obtained in more recent tests. In other countries engineers were seeking to develop the best means of welding in structures. Very elaborate tests on welded structures had been carried out for the Swiss Federal Railways. Recently a new reinforced-concrete highway bridge had been built in Australia at Pyke's Creek, near Melbourne. The total length of 246 feet was made up of five continuous spans. The erection of that bridge had been much simplified by two welded steel-framed structures which had been incorporated in the bridge as part of the reinforcement. That appeared to have been very successful. Measurement of the elastic limit by dividers (p. 297) seemed hardly good enough, particularly in experiments intended to show the unreliability of small tests as determining the strength of the material in larger plates. Other reasons for the variations in the results could, he thought, be found. The Author noted the importance of fatigue tests, and Professor Dixon believed that very interesting results had already been obtained by some arc welders. That arc-welded joints resisted vibration to a remarkable degree was shown by the successful welding of locomotive parts on the Paris–Lyons–Mediterranean Railway. Most of the speakers seemed to have been rather critical. Certainly caution in applying new methods in design was very necessary, but equally necessary was enthusiasm in research into new methods in order to determine their value as soon possible. If that enthusiasm were absent, this country would be left behind. What was needed was the best method (which meant the cheapest method) available for making structures —one conducing to economy of material and speed of construction. He was not greatly interested in the repair of old structures; he would like to see them pulled down and replaced by new ones. No doubt the Author would carry out many more experiments, not only on large plates but also on girders. He had available a magnificent machine at Middlesbrough, and Professor Dixon hoped that with its aid much more information of the kind recorded in the Paper would be obtained.

Mr. F. L. Pawley thought the members should be very grateful to the Author for having brought forward such a Paper, which had certainly led to a useful discussion. He was concerned with the use of electric welding in the repair of dredgers and railway-bridges. The question of internal stress left after welding seemed to cause some apprehension. He thought that the Paper and what had been said in the discussion afforded enough data to justify proceeding with the repair of old bridges and the construction of new by welding. As a previous speaker had pointed out, if the matter were not taken up energetically this country would be left behind in a new sphere of engineering work. Judging by what was going on in America, Germany, France and Switzerland, it was impossible to avoid the conclusion that British engineers were lagging behind. The remarks he wished to make were criticisms of the Paper from a practical point of view. In addition to the work which had been done in the North-Eastern Area on the London and North Eastern Railway by Mr. J. Miller, M. Inst. C.E., in the repair and strengthening of bridges, a large number of experiments had been carried out. Those experiments were not yet completed, and therefore it was too early to say more than that the results followed very closely on the lines of the Author's. On p. 287 the Author, dealing with ½-inch plates, mentioned one run of No. 10 and three runs of No. 8. Mr. Pawley had tried with a ½-inch plate to get results on the same lines as the Author's, and he had not been able to get the theoretical fillet which was shown on p. 288 for the ½-inch plate. He had found that one run of No. 10 and one run of No. 8 was ample. Attempts to put on more resulted in the edge of the plate being burnt, so that it was blown away and served no practical purpose. He could not see how three runs of No. 8 could be superposed on No. 10 on a ½-inch plate. Again, if it were done, from a practical point of view it meant putting in too much electrode; in other words, the job was costing too much. Dealing again with the ½-inch plate, the Author had put in nearly 110 linear feet of electrode per foot of weld. In his own company's case he thought he was right in saying, from the figures he had taken out, that one run of No. 8 and one run of No. 10, using two electrodes 18 inches long, gave a complete fillet weld ⅜ inch by 10 inches, which could be carried out in just over 2 minutes by a skilled operator. The other criticism he had to offer was on the summary at the end of the Paper, which was, he presumed, intended to be more or less a specification for welding work. He had already referred to the one run of No. 10 and the three runs of No. 8. In the general summary the Author mentioned that in the ½-inch plate he was using one run of

Mr. Pawley. No. 10 and two runs of No. 8. Why that difference? In clause (2) the Author said (p. 302): "In long welds, and especially in butt welds, special care is to be taken, by applying the weld in intermittent stretches, to avoid high contraction stresses." That was a very important point. His own practice was just to tack the additional plate with a dab of electrode at intervals of about 12 or 15 inches, and then connect those dabs with No. 10 and put No. 8 on top of that. Any stresses left in the metal by the run of No. 10 would be relieved by the remelting due to No. 8, and he thought there need be no fear as to a really good job being left at the end. In clause (3) the Author said: "The greatest importance attaches to the choice of a suitable electrode." That, to Mr. Pawley's mind, was the most important point. Experiments and actual work had been carried out in America in which bare wire had been used. There were available in this country various electrodes covered with all sorts of materials. One which he had tested, with no great results, was covered with silicate of soda, which might be useful for preserving eggs but left much to be desired as a cover for electrodes. In clause (5) the Author said: "Edges to be welded may be sheared or burned with an oxy-coal-gas flame." Mr. Pawley was strongly opposed to the use of any oxy-acetylene or oxy-coal-gas flame. A very useful appliance for the purpose was a grinding machine, working off an ordinary 50-volt circuit. He was not in agreement with clause (19) and *Figs. 22*. Both in overhead and in ordinary welding his company made the angle about 80 degrees, bevelling to about $\frac{1}{4}$ inch, and leaving $\frac{1}{4}$ inch space at the bottom. If the space were $\frac{1}{16}$ inch, as shown by the Author, even with a No. 10 electrode the arching of that would burn the $\frac{1}{16}$ away. Again, with $\frac{1}{16}$ inch space between the plates, he did not think a really good weld between the original plate and the two edges of the new plate could be made, even with a No. 10 electrode. Mention had been made of supervision. Welding did require careful and proper supervision by experts. After a weld had been made there was very little possibility of seeing what it was like inside. There were microscopical and other means of examining the weld, but the outward appearance gave a good indication of what was inside.

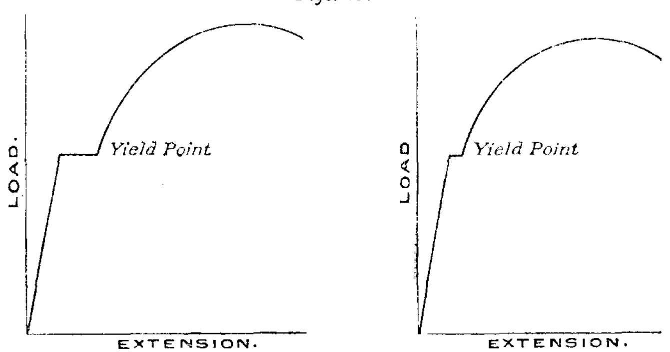
Dr. Hankins. Dr. G. A. HANKINS remarked that previous speakers had referred to fatigue tests on welds, and one of them had seemed to be of the opinion that electric welding could be applied to structures and that the fatigue resistance of the welds would be quite good. Another speaker, however, had seemed to be rather doubtful on that point. It so happened that a few weeks ago he had had occasion to look up, as far as he could, all the available results of fatigue tests on

Dr. Hankins.

welds. Apparently very little had been done in that direction in England, but a good deal had been done in America. The method used in nearly all the investigations was the well-known rotating bending type of test. With an ordinary structural steel, the endurance fatigue limit in that form of test would be about ± 14 tons per square inch; in the case of welds the results which he had been able to find had been of the order of ± 5 to ± 9 tons per square inch. Accordingly it appeared that—in the case of the bare-wire welds used in America, at all events—the fatigue resistance was considerable below that of the steel itself. It might be suggested, not unreasonably, that with the electrodes used by the Author the welds would be appreciably better in regard to fatigue resistance than bare-wire welds. That was the inference which he drew from the Author's remarks on p. 300, but he would like to point out that there was very little published information on this matter. It was found in normal experience of fatigue testing of steels that inclusions and impurities had a very marked effect on the fatigue resistance. If there was a small inclusion in a piece of steel to which alternating or repeated stresses were applied, a stress concentration was set up around that inclusion; a fatigue crack started at the inclusion, and then propagated itself right through the material. He thought it would be admitted that, even with the best form of electrode, inclusions were likely to form a larger percentage of the final material than they would in normal structural mild steel. Therefore at the moment, as far as he could see, there was a possibility that the fatigue resistance, even of the best welding metal, might be below that of normal structural steel. It seemed to him that that was a subject on which information was definitely required. Two speakers had discussed at some length the Author's tests in regard to longitudinal fillet welds, in which failures had occurred at apparently low stresses. With regard to those tests, he would like to draw attention to the diagram given at the top of Table II, where the Author showed the stress lines indicated by the mill scale. It was fairly clear from that information that at the edges of that particular specimen the stress in the material had exceeded the yield stress. From that diagram alone it was clear that the stresses had been quite high at the actual points at which failures had started. There was one point with regard to the Author's suggested working-loads on which a little further information might be given. In the early part of the Paper it was pointed out that the actual fillet welds used were over size. He was not quite clear whether that held throughout the whole of the Paper. The Author had suggested, on p. 303, working-loads which seemed to be based on the experimental

Dr. Hankins. results, after a factor of safety of $3\frac{1}{4}$ had been applied; but if those loads were applied to welds of the exact sizes given, it seemed probable that the factor of safety would be appreciably less than $3\frac{1}{4}$. On p. 300 the Author drew attention to the close relationship between the breaking-strength of a welded joint and the yield-point of its components. He wondered whether that might not be explained in the case of a simple joint by differences in the stress-strain characteristics of the welding metal and the mild steel. The Author referred to the importance of the modulus of elasticity being the same in both the welding and the welded metal, but that argument could be extended. He would like to know whether the Author had any information in regard to the amount of yield. It seemed to him that if there was a comparatively big extension at

Figs. 29.



WELDED METAL (NORMAL STEEL). LARGE EXTENSION AT YIELD. WELDING METAL (FROM ELECTRODE). SMALL EXTENSION AT YIELD.

the yield-point in the welded metal and a small extension in the welding metal, as shown in *Figs. 29*, then the stress in the welding metal would be appreciably greater than that in the welded metal when the whole joint was subjected to a stress which exceeded the yield stress. The "Uranium" electrode possessed tensile strength and elongation of about the same order as those of mild steel, but if the yield were small, it seemed to him that that might account for the failures of comparatively simple welded joints at stresses not greatly above the yield stresses.

Mr. Pollard. Mr. F. E. POLLARD remarked that his experience was not so much in structural work as in containers subject to fluid pressure, and in some directions that experience had not been altogether fortunate. He must say, however, that such failures as he had seen had been failures rather of design than of welding. Good welding was now being done, and in the successful welds which he had seen there had been none of the shear fillet design. He congratulated the Author on his work and on the production of an excellent Paper. He would like to support the suggestion that a valuable supplement to the Paper would be a report on experimental work and tests carried out on complete braced structures having arc-welded joints. Mr. Pollard.

Mr. T. M. AITKEN stated that he had had 20 years' practical experience of electric arc welding, and the Paper was therefore of very great interest to him. Like Mr. Kennedy, he was somewhat disappointed at many of the results mentioned. In the first place, the Author had used in his work No. 10 and No. 8 gauge electrodes. Taking the measurements used by his own firm that would be equivalent to electrodes of 3 and 4 millimetres, and they would never think of doing such work with such small electrodes. If they had used their ordinary electrodes of 5 and 6 millimetres on the work which the Author had carried out they would have been very disappointed at obtaining such results as he had described. With regard to the different joints, he had calculated the figures in tons per square inch, and it seemed to him that they required some reconciliation. The longitudinal fillets in shear on p. 289 worked out to 12·8 tons per square inch. For the butt welds the Author recommended only 16 tons per square inch, and for the transverse fillets, which were subject both to shear and to tension, he allowed 21·5 tons per square inch. He would like to know how the Author reconciled those figures. The most important part of the Paper to welding engineers was perhaps that which dealt with the butt welds. The butt welds referred to on p. 294 and in Table V showed an ultimate tensile strength of only 15 to 22·6 tons per square inch. His own firm had made an extensive series of tests for Lloyd's Register, including two plates 20 inches wide, which had failed at 27·2 and 28 tons per square inch; that showed that good figures could be obtained from welds of considerable length. The Author stated that in $\frac{3}{4}$-inch plates he would expect better results when the work was done from both sides of the plate. That was not the experience of Mr. Aitken's company. They obtained quite as good figures when the weld was done from one side only as when it was done from both sides. The Author had also used several runs for all welded joints. Mr. Aitken's company would not generally adopt that method. Work of that sort had been done by them with one run, building up to full size at one operation. The tests of compound welds given in the Paper seemed to him probably the most useful, and, from such tests as those, figures might be ultimately agreed upon for shear and tensile strength of all fillet welds, whether longitudinal or transverse. On Mr. Aitken.

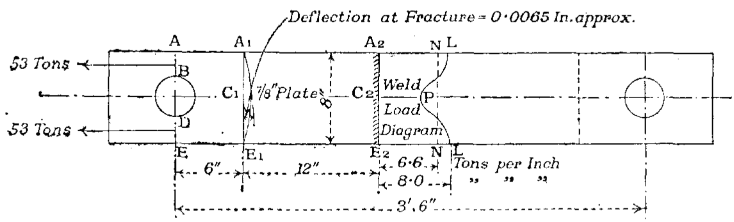
Mr. Aitken. p. 298 the Author gave the chemical analysis of the deposited metal, namely, carbon 0·06 per cent. and manganese 0·10 per cent. As the rods contained about 0·1 per cent. carbon and 0·5 per cent. manganese, there had been a considerable loss of those constituents. In some corresponding tests made for Lloyd's Register, with rods of a similar analysis but with quite a different coating, there had been found to be in the deposited metal 0·10 per cent. carbon and 0·4 per cent. manganese; which was, of course, much nearer in composition to the parent metal. The photomicrographs shown on Plate 5, were decidedly disappointing. Many of the failures had taken place at the junction of the deposited metal with the parent metal, which in Mr. Aitken's experience never occurred with really first-class welding, and was almost invariably a sign of insufficient penetration. As the elastic limit of the weld metal was nearly always higher than that of average mild steel, the latter was stiffened at the junction, and, with sound welding, failure should not take place there, but either in the plate or in the weld. The photomicrographs also showed too many slag inclusions, possibly due to the welding having been done by the multiple string system. Figs. 14, 20 and 21, Plate 5, showed very defective welds, and the top piece in Fig. 15 was particularly poor, apparently due to insufficient heat and consequent lack of penetration. Although fatigue tests were referred to on p. 300, no results were given. They were, however, of great importance, as perhaps more failures in actual practice were due to fatigue than to any other stress. The best figures he had seen published for the type of electrode used by the Author showed failure after two million reversals of a stress of 8 tons per square inch. Much better results had been obtained by welding with electrodes giving less slag, and he had known of five million reversals, without failure, of stresses exceeding 10 tons per square inch. The Author put forward twenty-two rules for the design of welded structures, but although many engineers might agree to a number of those rules, there were others to which some welding firms with long experience would take serious exception. It seemed to him that rule No. 1 was applicable only to that class of electrode which gave a thick and copious slag, and, therefore, necessitated all the welding being done by laying down a series of comparatively thin strings. That method might be quite suitable for tank work, but there was no doubt that for heavier welding of plates, from ¼ inch thick upwards, excellent work was being done with electrodes of suitable composition giving much less slag but quite sufficient to prevent contamination by the atmosphere, and that with such rods No. 6 and No. 4 S.W.G. were quite common practice and had

Mr. Aitken. been in regular use for 20 years and more. To suggest, therefore, that in the welding of steel structures electrodes should be confined to sizes Nos. 8 and 10 seemed to be a retrograde step. He could confidently say that of the total amount of electric arc welding carried out in France, Germany, Scandinavia, and America, only an insignificant fraction was done by means of electrodes giving so much slag as those used by the Author. With properly covered electrodes of good quality, strings could be run or the weld built up to the full size for fillet or butt welds; but that could not be done with rods giving a thick slag, as the latter accumulated and caused trouble. With suitable electrodes, fillets or butts could be built up in one weld by a skilled operator on plates ranging in thickness from $\frac{1}{4}$ inch up to about 1 inch. If a number of runs were used the metal cooled quickly, causing internal stresses and giving a crystalline brittle structure. It was sometimes claimed that the next run had an annealing effect, but that was only partial and could not be relied upon. On the other hand, when a full fillet seam was welded at one operation the temperature was higher, the penetration was better, and the cooling was slower, with fewer slag inclusions. The speed of working was also higher, so that the method was more economical as well as more efficient. A good deal had been said in the course of the discussion about internal stresses due to contraction; those could often be allowed for by suitable design of the joints and in the method of procedure. Such stresses, however, were not confined to welded metal, as many rolled plates and joists showed serious initial stresses, and riveting was open to the same objection. Even in modern high-pressure boilers rivets were frequently found not only highly stressed but even fractured, due to initial stresses. The Author's rule No. 3 was very important, and Mr. Aitken was in complete agreement with it. Skill and experience were absolutely essential in the present state of the industry, and if first-class work was desired welders must be well selected and thoroughly trained. A first-class welder would do better work with a bare rod than would a duffer using the best electrode on the market, and he had known good electrodes to be discarded simply because the operator did not know how to use them. It was also his experience that many firms in this country had installed plant of insufficient capacity for the best and most efficient welding of mild-steel plates of any thickness. Plants for that purpose should have an output of 200 to 250 amperes, and it was significant to note that for the latest all-welded ship built abroad the welding sets were of the 300-ampere size per arc. In conclusion he thanked the Author for giving so much information, as he knew that a great deal of time

Mr. Aitken. and trouble was involved in carrying out so extensive a series of welding tests.

Mr. Vivian. Mr. A. C. VIVIAN observed that he held no brief for welding, but, for the following reasons, he believed the results described in the Paper were even more consistent than the Author suggested. If a short thick member were put under tension the stress would be highest at the skin. The centre fibres stretched by reason of the shear stress through the material; therefore the centre fibres would be shorter than the outside fibres. Applying that to the pin-point specimen (Table III), which failed at 106 tons, there was at best 53 tons over AB and 53 tons over DE (*Fig. 30*). He suggested

Fig. 30.



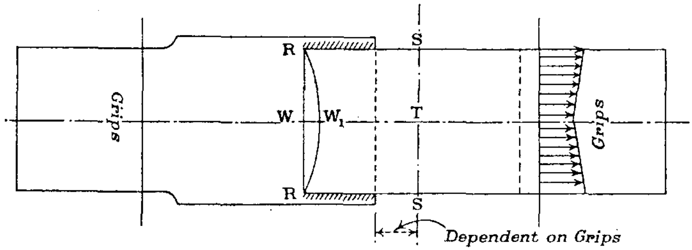
NOTES.—The weld load diagram shown in the Figure also constitutes (to a different but determinable scale) the diagram of longitudinal stress in the plate $A_1A_2E_2E_1$. When weld load = 3·6 tons per inch, longitudinal stress in $\frac{7}{8}$-inch plate = 15·1 tons per square inch.

From diagram, longitudinal stress along A_1A_2 = 18 tons per square inch, and along C_1C_2 = 11 tons per square inch. A deflection at C_1 of 0·0065 inch would correspond with a longitudinal stress difference between A_1A_2 and C_1C_2 of 7 tons per square inch. (E = 13,030.) If length A_1A_2 were doubled and deflection at C_1 remained the same, the stress difference would be halved.

that the beam AA_1E_1E in spite of its reinforcement, deflected about $\frac{6}{1000}$ inch, and that the weld load at L instead of being 6·6 tons per inch, agreed exactly with the 101B series, in which it was about 8 tons per inch. He also suggested that the actual load distribution at the test weld A_2E_2 was represented by the curved line LPL. Applying the same principle to the longitudinal welds, the common assumption was that the stress was uniform across SS (*Fig. 31*). How could it be? There was nothing pulling on the point W; consequently RWR must deflect, and if that principle were applied to the Author's results he thought it would be seen that there must be a high stress at S. It could not be otherwise, because the stress

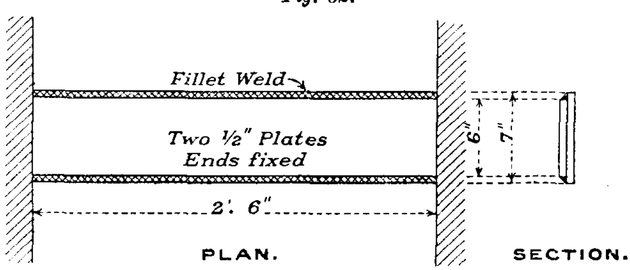
difference between RS and WT, roughly speaking, was measured Mr. Vivian.
by the deflection WW_1 divided by the length RS. If that length were doubled, the stress difference was halved, approximately.

Fig. 31.



Mr. A. H. Cantrell stated that he had a few figures to support Mr. Cantrell.
what other speakers had said as regarded temperature effects. A short time ago he had assisted in the carrying-out of a series of such tests. A simple butt weld was made, and on one bar a stress-recorder was placed; and as the welding was done the stress-recorder indicated results due directly to the change of temperature of the bar. When the metal cooled there was a residual stress of 1·3 ton per square inch in the bar. The ends of the bar were fixed: it was not a loose-ended test like the Author's. Later on another test was carried out, again with fixed ends. One plate was welded over another by means of fillet welds (*Fig. 32*). One recording instrument was placed on the top and one underneath. The next day, when everything had cooled down, the instruments having

Fig. 32.



been on the whole time, a stress of 6·4 tons per square inch was obtained from the instrument on the top, and 4·5 tons per square inch from that on the bottom. The difference in stress was caused by the bending of the test-pieces. That was due to the weld metal being on

Mr. Cantrell. the edges of the top plate, so that its contraction affected the whole thickness of that plate; but the effect was only on the top skin of the lower plate. The combined result was a downward deflection, which increased the compressive stress in the top face, and reduced that in the bottom. He thought the temperature effects also explained the failure of the test-pieces in the parent metal of the weld just away from the end, for a reason additional to that which the last speaker had given, although he agreed that that also came into the question. When the weld metal cooled, it contracted the fibres along the sides but not in the middle, with the result that at the sides there was considerable stress before the bar was ever put to test; but the stress in the middle was much less. That resulted in tensile stress along the edges just beyond the welds. Therefore when additional tensile stress was put on, the bar naturally failed at the corner. He thought also that the temperature effects explained Mr. Bruff's reference to a calculated stress of 314 tons per square inch. The weld was the whole length of the test-piece, and the compressive stress was taken up by the section of the bar; but he did not think it was tension in the weld metal; he thought it should be considered as shear stress throughout the whole of the area which was adjoining the metal, in which case a much lower figure would be obtained.

The Author. The AUTHOR, in reply, stated that in putting forward the results of his experiments, he had been faced by the alternatives of merely putting on record the results of the tests or of drawing also such conclusions from them as appeared to be justified. It had appeared that the latter course would be the more useful, as although the number of experiments was naturally limited, they did appear to to be sufficiently extensive to permit preliminary conclusions to be drawn up; and those conclusions seemed to be of such interest as to be worth recording, as being likely to provoke criticism which would be of great value in connection with the study of a new process. He would like to assure Sir Clement Hindley, however, that in drawing up the set of rules he had intended them to be merely a summary of his conclusions and not, of course, to constitute working practice recommended for general acceptance without confirmation by more extensive experiments. As Sir Clement had remarked, it would materially assist in the more general adoption of welding for structural work if a reliable and practical field test for welds could be discovered; but although considerable experimenting had been done in that direction, no satisfactory test had been discovered, so far as he was aware. In any case, he believed that when welding became a more familiar method of fabricating steelwork, the feeling that such a test was necessary would not be quite so strong. Firms who were carrying out welding on a large scale could, and did, obtain welds of as high a degree of reliability and uniformity as could be obtained in other processes of construction; and bad welding would, he believed, be found to be always due to lack of knowledge or lack of care, or the use of unsatisfactory materials and equipment. There was no mystery or difficulty about obtaining the necessary conditions for uniformly good welding, and although care and a reasonable amount of skill on the part of the workman were required, there was no difficulty in giving him a suitable training for the work in a few weeks. There was still a considerable field for the study of the proper types of joints to be used with arc welding, and Professor Coker's remarks on the nature of the stress-distribution in those dealt with in the Paper were extremely interesting. It would not always be possible, however, to adopt shapes of joint which were theoretically the best, owing to practical considerations; for instance, although the joggle type of joint Professor Coker had suggested no doubt avoided the high stress-intensities developed in fillet joints, it would involve great expense in fabrication, and would nullify one of the chief advantages of welding steelwork, namely the possibility of putting into a structure without further preparation material cut approximately to length. As Dr. Rosenhain had suggested, the stress-distribution in a riveted joint also was far from ideal, and it was only familiarity with that form of connection which enabled it to be regarded with such confidence. Dr. Rosenhain had inferred that average values had been taken as a basis for the deductions drawn, but that was not the case. Minimum values had been taken in all cases, and no doubt as the use of welding spread and the quality of work improved, strengths exceeding the minimum values found in the Author's tests could uniformly be obtained. It had also been suggested that the strengths adopted might not be suitable for welds carried out in the field. If that were the case, it would be necessary to make a percentage deduction from the strengths suggested for shop welds, as was done in the case of riveting in many specifications; but in fact field conditions did not appear to the Author to be likely to have much influence on the strength of welds, except in so far as the workman could not be given comfortable conditions to work in. Dr. Rosenhain had also suggested that the quality of the welds in different specimens showed considerable variation. That, however, was not quite true. As pointed out in the Paper, the variation was due to the shape of the joint, and it would be found that the strength of the welds, where specimens were of identical shape,

The Author.

The Author. etc., was remarkably consistent; and when the various factors influencing the strength of the joint, other than the strength of the weld itself, were taken into account—as for instance in Dr. Lobban's very interesting graph—remarkable uniformity in the strength of the weld itself was evident. With regard to the elastic modulus of the weld metal, the experiment on the modulus had been repeated on another specimen of all welded metal, and almost exact confirmation of the result given in the Paper had been obtained. The low value for the modulus might be due to the inclusion of microscopic non-metallic elements, but that would argue a higher proportion of such material than would appear to be at all likely. It should be pointed out in connection with that matter that the modulus for a piece of metal made up of all welded material was not necessarily the same as that in a fillet, where the metal was deposited in comparatively small volume and fused to the parent metal. As Dr. Hatfield and other speakers had remarked, there was undoubtedly room for experiment on the influence of contraction stresses and on the closely related problem of the internal stress set up by the welding itself. No experiments had been made by the Author on that subject, but it was undoubtedly of very great importance, especially in connection with one of the most useful applications of electric welding, namely, the addition of new material to an existing structure. In pieces of reasonable length, however, and where the ends were allowed freedom of movement, the stresses set up did not seem, so far as the Author's experience pointed, to be of any serious moment. Mr. Pawley had referred to the apparent discrepancy between the number of runs recommended on p. 302, and those used in the tests. It had been found that the number of runs could be reduced by one without noticeable reduction in strength. Tests on fillets in tension made up with the number of runs shown on p. 302 had given the following results :—

$\frac{1}{4}$ inch (one run No. 10) 5·9 tons per inch. (Average of three.)
$\frac{3}{8}$,, (one ,, ,, 10, one run No. 8) 8·3 tons per inch. (Average of three.)
$\frac{1}{2}$,, (one ,, ,, 10, two runs No. 8) 12·1 ,, ,, ,,

which should be compared with the "reliable breaking strengths" of 5·4, 8·0, and 10·7 tons given on p. 294. Another point, raised by Mr. Kennedy and others, which had not been dealt with in the series of experiments carried out, was the strength of overhead welds compared with those run on the flat. Here also he had not made any experiments, but it was commonly accepted that overhead welds did not suffer in strength, although they were more difficult and more expensive to make. The comparatively low strength of the wide butt-welded plates was, in his opinion, due solely to the fact that the edges of the plates had not been machined after welding, in the manner described by Mr. Kennedy. At the end of an unmachined butt weld there was inevitably a slight deviation from the smooth contour of the plate-edge, causing a crack to start at that point and spread across the weld. The butt welds that had been machined (Series 102) had broken at approximately the full strength of the plate, and they confirmed the results obtained by Mr. Kennedy which seemed to indicate that the low results were due not in any way to faulty welding, but to the inherent weakness in tension of that type of joint when the edges were left rough and not reinforced by edge straps or other means. It would obviously be impracticable to make the members of a structure of the shape illustrated by Mr. Kennedy, and the Author suggested that alternatively the edges of butt-welded plates should be reinforced by the addition of small covers. Several speakers had mentioned problems arising out of lines of investigation not covered by the scope of the Paper. He hoped to make some further experiments in certain of those fields and to have the privilege of communicating the results in due course. The Author.

Correspondence.

Mr. JAMES ANDERSON remarked that he had read the Paper with considerable interest, in view of the fact that during the past few years he had been mainly engaged upon the design of tall steel buildings carrying heavy dead loads; but even with the information afforded by the Author's experiments on welded joints, he would not feel justified in applying the working-rules laid down in the Paper to any great extent to a structure of any magnitude. Certain classes of structure (for example chemical plant) were liable to considerable corrosion; it was obvious that very serious difficulties could quite easily arise from that cause, and, so far as he knew, knowledge of such effects was essential before practical application of welding on a commercial scale. Mr. Anderson.

Mr. C. L. BOUCHER remarked that he could have wished that some reference had been made to the following points, and he considered that it would be of great interest if the Author would deal with them. No reference was made throughout the Paper to the polarity of the welding circuit employed. He presumed, however, Mr. Boucher.