Discussion.

The Chairman. The CHAIRMAN remarked that it was not often that an engineer had an opportunity of making a full-scale analysis of the magnitude of that referred to in the Paper, and the Author was to be congratulated on the use which he had made of that opportunity. An enormous amount of work had been carried out, both in the taking of the forty thousand readings and in their analysis, which most employers and most contractors would not have been willing to undertake. For that reason, The Institution welcomed the opportunity of publishing the Paper; the investigations which it recorded were undoubtedly in many respects unique.

Mr. Gribble. Mr. CONRAD GRIBBLE said that he had not gathered from the Paper itself to what extent the experiments and the method of erection had been adopted throughout the structure, which was one of fifteen spans; he now understood from the Author that all the spans were erected in precisely the same manner but that stress-readings were taken on only one truss. Was it reasonable to suppose that the pre-stressing had given similar results on the others? As the Author had pointed out, the fact that there were fifteen spans in the bridge had enabled the method to be used without undue expense, as the extra cost of manufacturing the bridge by jig-drilling (which was essential for the method) was small when spread over the whole structure. In England, on the other hand, single-span bridges were usual, and it might be that the extra cost of erection in the manner described, to say nothing of the cost of the records which were taken for a check, would far outweigh any economy in the design of the structure which might be obtained owing to the more accurate knowledge of the stresses.

In the early days of lattice-girders it had been the general practice to use pin-jointed trusses, because engineers in those days had been of opinion that they were more easily understood; but on account of cost and to obtain greater rigidity their use had diminished, and for bridges of the type in question they had been abandoned altogether. Engineers had had an uneasy feeling, therefore, that their ordinary stress-calculations gave only a very rough idea of the actual stresses in a truss. He had heard it said by those whose opinions were worthy of respect that in a particular member the value of the extreme stress on one corner of the member was of less importance than the average stress on the member. Although that view might be fairly sound for tension members, he felt that it was a little optimistic in regard to struts, because if an initial bending stress were put into a strut its power of resistance would certainly be diminished. Mr. Gribble.

Secondary stresses had for many years been a bugbear to engineers designing bridges, and some most laborious calculations had been made. Ten or twelve years ago he had seen a paper, published in America, the author of which had made a colossal calculation of the secondary stresses in a truss, using about seven different methods, and some of the simultaneous equations were most remarkable, because one term might have a coefficient in which there were five figures to the left of the decimal point while the next term would have five noughts before arriving at a significant figure. With simultaneous equations of that order the prospect of arriving at accurate results seemed to be remote. The author in question, however, had obtained results, but the calculation was so lengthy a nature that such methods could never be adopted for general use. Because the matter was so difficult, engineers had been inclined to put it aside as not being vitally important, and they had been confirmed in their view by the fact that, after all, the many thousands of truss bridges which had been erected without any particular calculation of secondary stresses had served their purpose without any sign of failure, and many of them were actually bearing loads far heavier than those for which they had been designed.

The Author's experiment had been made with the object not so much of ascertaining the secondary stresses as of avoiding them, and had of necessity to be somewhat limited. The Author had had to concentrate on deformations in the plane of the truss, and explained that there was no way of eliminating the secondary stresses due to floor and top-lateral systems, suggesting that that part of the problem should be dealt with by omitting the usual type of floor altogether and using a transverse-trough floor, which would not produce any bending moments in the truss-members. That suggestion was presumably made because the Author recognized that it was impossible, in dealing with a bridge of the kind in question, to eliminate the secondary stresses in a plane at right angles to the truss.

On p. 91 the Author mentioned that the Indian authorities had agreed to the working stress in the girders being raised from 8 to 9 tons per square inch provided that steps were taken to limit, if not to eliminate, the deformation-stresses. Since that date, a British Standard Specification had been issued which permitted a stress of 9 tons per square inch without any such proviso. If trusses could safely be built with that working stress without any special provision in regard to secondary stresses, it appeared that a still higher working stress might be used if with confidence it was possible to build them

Mr. Gribble. applying the Author's methods in such a manner as to minimize those stresses.

On p. 117 the Author, in describing a method of pre-stressing web members, suggested that "the riveting of the lacing-systems could be left until a full design load had been applied to the span." That seemed to be quite feasible for tension members, but it was difficult to see how the riveting-up of a strut member could be deferred until the full load was on it; the unriveted strut would appear to be in a very unsatisfactory state to resist that full load. It had occurred to Mr. Gribble that if the method in question could be applied to spans much larger than those under consideration, it might be quite sufficient to pre-stress them for dead loads only and to ignore the live loads, as for longer spans the live load would become very much less in proportion to the dead load.

The Author had described his most interesting and valuable experiment without giving any very definite lead as to how far he considered that the procedure could economically be applied to bridge-work in general, or to what kinds of spans it might best be applied. It would be of interest if the Author would give his views on that point, showing what economy had been attained, and how far the saving on account of the increase of stress permitted had been offset by what must have been a very big increase in the cost of erection of the structure.

Mr. Fereday. Mr. H. J. Fereday observed that the first recorded measurements of the actual stresses in trusses had been made by Professor Frankel [1] in 1883, a few years after the publication of H. Manderla's celebrated thesis on secondary stresses.[2] In 1905 W. Gehler had conducted a series of tests and measurements on a Pratt-truss skew railway-bridge. Dr. D. B. Steinman, commenting [3] on these results, said: "The results, on the whole, afforded a valuable proof of the remarkably close agreement attainable between theoretical computation and actual conditions in a bridge structure." In 1917, Dr. Steinman had written a paper [4] entitled "Stress Measurements on the Hell Gate Arch Bridge." That huge structure had been used as an instrument of scientific research, the cost of which had been personally defrayed by the late Dr. Gustav Lindenthal as a contribution to engineering science. Notwithstanding those outstanding contributions, there was a paucity of experiment aiding the mathematical analysis of secondary stresses.

Whatever merit might be attributed to pre-stressing, Mr. Fereday

[1] *Der Civilingenieur*, 1883.
[2] *Allgemeine Bauzeitung*, 1880.
[3] Trans. Am. Soc. C.E., vol. lxxxii (1918), p. 1043.
[4] Trans. Am. Soc. C.E., vol. lxxxii (1918), p. 1040.

Mr. Fereday. himself had been personally responsible for the introduction of its application to bridges in India. The subject of the present discussion should be the application, as distinct from the principle, of the pre-stressing of framed structures, as the principle was well understood. The Author had nowhere in his Paper given a definition of pre-stressing for framed structures. Stated broadly, it comprised the imposition of secondary stresses during erection equal and opposite in sign to those which would be induced in the structure by the loading. It should be mentioned that, for pre-stressing, the secondary stresses were not calculated. They were imposed mechanically during erection and eliminated by the loading for which the structure was pre-stressed. If success were to be achieved in the application of the principle, the fabrication, erection, drifting and riveting would have to be carefully done. If the drifting were done in such a way that rivet-holes were distorted, much of the value of pre-stressing would be lost. If the degree of success fell below the expected standard, that could only be due to non-compliance with the necessary conditions, since no fault could be attributed to the principle itself.

The success of the erection of the Nerbudda bridge could have been tested by comparing readings on corresponding members of the two trusses forming one span. It seemed a pity that, out of the 40,000 readings taken on the bridge, none, apparently, had been taken on the opposite truss. The Author attributed the failure to realize a higher degree of secondary-stress elimination than that attained partly to the form of the truss and its necessary sub-members. Nevertheless, any form of truss, whether statically determinate or not, could be effectively pre-stressed, and that statement was fully substantiated by information published regarding the Sciotoville bridge in America.[1] Not only had that structure the same form of truss, but it was a continuous structure which added to the complexity of the problem. Again, pre-stressing had been adopted on the Willingdon bridge[2] under the direction of Mr. L. H. Swain, M. Inst. C.E., and other examples of its successful application could be cited.

Dealing with specific points in the Paper, on p. 91 it was implied that the first proposal to raise the working stress in mild steel to 9 tons per square inch, on condition that steps were taken to eliminate or reduce secondary stresses, had been made by the Consulting Engineers in respect of the Nerbudda bridge in 1931; actually, in 1925 the Consulting Engineers had suggested to the Railway Board that those conditions should be allowed.

---

[1] Trans. Am. Soc. C.E., vol. lxxxv (1922), p. 910.

[2] Minutes of Proceedings Inst. C.E., vol. 235 (1932–33, Part I), p. 36.

Mr. Fereday. On p. 94, it was stated: "It was specified that . . . the top chord was to be erected . . . each panel-length being cambered in turn as soon as the splices were made." It was in fact specified in the contract documents for the Nerbudda bridge that each top chord was to be erected horizontally, with the joints drifted and bolted, and then allowed to fall into its proper cambered shape. Steel brackets were provided to support those chords, and an examination of the top-chord details revealed that sufficient riveting could have been done to pre-stress those members properly. Incidentally, *Fig. 3*, p. 95, showed the top-chord distortions incorrectly.

There were many important statements in the Paper which were not clear to Mr. Fereday. For example, on p. 100, it was stated that "the zero differences were checked by three completely independent sets of readings made by different observers." That was a necessary precaution, but it did not appear to be stated whether the stress-readings on which the Paper was largely based had also been taken by independent observers, nor, if so, how the mean results had been derived.

Reference was made by the Author to the use of the Fereday-Palmer stress-recorder. Mr. Fereday did not suggest that it was an ideal instrument for the investigation in question, but he felt bound to say that the manner in which it was used in punched holes could not be expected to give satisfaction and would result in exasperation and tiredness for the operators. Accessories had now been designed and extensively used to enable the instrument to be used with confidence for the type of stress-recording described, which was of a difficult and exacting nature. He was afraid that to avoid their use on account of time or cost could result only in uncertainty and perhaps unfair criticism of the instrument, and in such circumstances some other type of instrument should be adopted.

On p. 102 it was stated that camber was lost by oscillating the structure. The loss of camber was partly explained by reference to p. 120, where it was stated that rivet-holes were damaged.

On pp. 103 and 104 it was stated: "These results pointed to a substantial permanent set in all members except the top chord . . ." That would means, from the usually understood meaning of "permanent set," that the stresses in all those members had exceeded the yield-stress, whereas elsewhere in the Paper it was stated, as would be expected, that the maximum stresses were everywhere well below that value and within the permissible limits.

Various values were given to the elastic modulus of the steel. The elastic modulus, however, was a definite mechanical property which was independent of the geometry of the frame.

On p. 112 the Author stated that "the pre-stressing was also

Mr. Fereday.

largely neutralized by the application of the dead load." If that statement meant that the secondary stresses were largely neutralized by the dead load, then the application of the live load, which was approximately twice the dead load, should have produced, if the riveting were good, secondary stresses about twice the magnitude of those originally induced by pre-stressing, and of opposite sign. Possibly either the wording of the Paper, or else the readings or their interpretation, was at fault.

On p. 113 the following statement was made: "In the case of the member symmetrical in section, the same stresses, but of opposite sign, apply to the other flanges." That statement was contradicted on p. 123, where the Author remarked that "even in the simplest sections, the distribution of stress is far from uniform." Mr. Fereday assumed that "simplest" meant symmetrical, and that "uniform" meant linear. On p. 113, however, the Author assumed a linear relation for stress in the end raker, which was not a symmetrical section.

In conclusion, the failure to realize the desired amount of pre-stressing in the structure was not due to the type of truss but to the state of affairs mentioned on p. 120, where it was remarked that the imposition of stresses "damaged the rivet-holes and destroyed the accuracy of the riveted attachments." It was pertinent to ask whether the erection-staff were given, and made to follow, full instructions for the procedure to be observed. Unless he had been instructed, the first impression that a riveter would get on seeing a series of partially-matched rivet-holes would be that the fabrication was at fault, but that somehow he had to get the rivets in position; he would then proceed accordingly. It appeared that the rivet-holes were damaged because the drifting had not been done with sufficient care. If the drifting and riveting were faulty, much of the loss of pre-stressing in the Nerbudda bridge could be attributed to those causes. The specification provided for drifts to the extent of at least 40 per cent. of the number of field rivets to be driven.

Mr. Durant.

Mr. N. J. Durant remarked that the Author, having had difficulty in reconciling his calculated secondary stresses with those derived from his experiments, appeared to consider that the only satisfactory way to determine those stresses was either to experiment on the full-scale structure or to use a model constructed to satisfy the law of similitude. The first method was costly and the second method, involving similitude, was not very generally understood. If the Author's contentions were correct, the determination of secondary stresses would be a rather hopeless task. Properly applied, however, the analytical method was capable of yielding accurate results with greater rapidity than either method recommended by the Author.

Mr. Durant. The analytical method given by Johnson, Bryan, and Turneaure, and referred to by the Author,[1] was suitable for didactic purposes, but it needed to be used with circumspection. Those interested in secondary-stress analyses might be referred to two excellent recent papers, one by Professor Hardy Cross [2] and the other by Professor R. V. Southwell.[3] Both those methods were independent of simultaneous equations.

The Author implied on p. 95 that secondary stresses could be eliminated for all conditions of loading, but it could hardly be believed that that was his intended meaning, as it was impossible.

On p. 125 the Author, referring to redundant frames, said: "Primary stresses cannot be obtained with any certainty from rigid-body statics." "Statics" would appear to be a better term. There would be general agreement with the literal interpretation of that statement, but if the Author meant, as the context seemed to show, that redundant frames could not be correctly analysed, Mr. Durant did not agree. Every rigid airship was a space frame whose order of redundancy was far in excess of that of any bridge structure. The conditions of loading presented a problem of analysis more difficult than that likely to confront the bridge engineer, and yet the factor of safety required was much lower than that demanded in bridge design specifications. Professor Southwell had evolved an elegant method of analysing such structures. Probably one of the finest examples of bridge engineering was the design of the towers of the George Washington bridge. Those towers were necessarily highly redundant frames, but Mr. L. S. Moisseiff had designed them so that the stresses in the legs were equal, notwithstanding the fact that the two applied loads of 112,000 tons were each eccentric by many feet. The designer's success might be gauged from the published description of the work.[4] Incidentally, Professor A. J. S. Pippard had given a method for the direct design of redundant frames.[5]

The Author's construction and use of a model were open to criticism. The Author recommended the use of models apparently for two purposes: firstly, to modify the prototype of a proposed design;

[1] Footnote 1, p. 92.

[2] Prof. Hardy Cross, "Analysis of Continuous Frames by Distributing Fixed-End Moments." Trans. Am. Soc. C.E., vol. 96 (1932), p. 1.

[3] Prof. R. V. Southwell, "Stress Calculation in Frameworks by the Method of Systematic Relaxation of Constraints." Proc. Roy. Soc. (A), vols. 151 and 153 (1935, 1936), pp. 56, 41.

[4] L. S. Moisseiff, "George Washington Bridge: Design of the Towers." Trans. Am. Soc. C.E., vol. 97 (1933), p. 164.

[5] Reports and Memoranda No. 793. Aeronautical Research Committee, London.

and secondly, to evaluate the secondary stresses in the full-scale structure. The first recommendation was perhaps a good one, and, under the right conditions and with the right technique, a model might lead to results of considerable value when applied to a complicated structure. In a simple structure such as the Nerbudda bridge truss the use of a model did not appear to be necessary. The second recommendation could also be made effective, but with much greater difficulty, because in order to evaluate secondary stresses in that way the model had to satisfy the law of similitude more exactly than for the previous, or indeed any other, purpose. No single method was known by which all the factors influencing the problem could be simulated, and considerable judgment had to be exercised to give proper weight to the different similarity requirements. **Mr. Durant.**

Valuable results, however, could be and were obtained not by insistence on theoretical rigour but by the acceptance of a procedure that was partly based on similitude, justified by the fact that it led to the desired results. Probably the best-known method was that due to Professor G. E. Beggs. The Author, like Professor Beggs, made the width of a member in the model proportional to the cube root of the moment of inertia of the corresponding member of the prototype. That violated the law of similitude, but as absolute obedience to the law was not to be expected, it was necessary to violate some of the similarity-requirements in order to satisfy others having a greater bearing on the results. Professor Beggs used the model in quite a different way; briefly, his object was to find the effect at a point on the structure due to a measured displacement at a second point.

In the Appendix to the Paper there were certain relationships among various quantities, but, as the analysis proceeded, those values were arbitrarily changed. Again, as the Author was aware, the magnifying device was a source of error. The value given by Professors Coker and Filon for the elastic modulus of celluloid was 300,000 lbs. per square inch, whereas the Author's value was 500,000 lbs. per square inch, an increase of 66 per cent. Assuming the Author's analysis to be correct and his value of the elastic modulus to be in excess by the amount stated, then 1 lb. on the model was equivalent to 23 tons, and not 34 tons as stated. Professor J. F. Baker, in his researches for the Steel Structures Research Committee, had discarded celluloid because of its liability to creep.

As so many of the Author's conclusions were based on the results of experiments on his model, Mr. Durant had felt constrained to make the foregoing observations. In studying the Paper, he had tried to attain some sympathetic understanding of the Author's difficulties,

10

Mr. Durant. and had confined his remarks to what appeared to him to be major issues. It appeared to him that the greatest value of the Paper was that it directed attention to the necessity of a more general acceptance of modern research. The fault was not in the designer's command of analysis or of practical requirements but in the assumptions that he was often compelled to make. The need for a specification more general in character had been suggested by the Author.

The series of tests for static loading was one of the most comprehensive on record, and the engineering profession was indebted to the Author for carrying them out and to the Bombay, Baroda, and Central India Railway for making them possible.

Mr. Bateson. Mr. Ernest Bateson remarked that in *Fig. 3* the Author gave a diagram of the distorted structure. That diagram was of course exaggerated, but Mr. Bateson could not quite understand the severe kinks which occurred at the top-chord panel-points. He would have thought that the upper chord would be the most easily bent, as the top-flange covers could first be placed in position; then, as the chord was in compression with butt-joints, it could be allowed to fall into place, and the kinks shown in *Fig. 3* would have been avoided.

On pp. 96–100 the Author described his difficulties with regard to the determination of the zero readings on the various members. Mr. Bateson could well appreciate those difficulties; in his own experience in connexion with temperature-stress measurements on a structure it had been found that even when a drilling jig was used there were appreciable variations between different sets of plugs inserted by means of the same jig. Those variations, however, did not matter, so long as they were within the range of the instrument, but to obtain consistent results it had been found necessary to use properly-designed plugs with properly-shaped holes and to see that they were perfectly clean when the instrument was placed in position. He would imagine that it would be extremely difficult to obtain accurate readings with punch-marks such as the Author referred to, but at the same time he realized that the expense of inserting special points for a large number of positions would have been heavy.

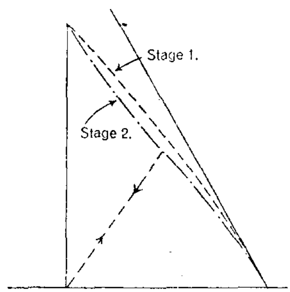
On p. 103 the Author referred to relief of stress due to the upper lateral system. As, in the truss in question, the upper lateral bracing was a K-system, it was difficult to see how such relief could occur except in the two middle bays to which the Author referred, where the bracing became a cross-brace system. At the bottom of the same page, the Author referred to a permanent increase in length of the members. He could not understand that statement, as the stresses in the members were certainly below the elastic limit of the

Mr. Bateson.

material, and, as the stress-recorders were placed outside the connexions, the question of slip did not arise. He would be glad if the Author could give some more information on that point.

The Author on p. 113 mentioned the difficulty of avoiding damage due to drifting in the fairing of holes. It seemed hardly reasonable to expect to be able to fair holes in such a case as that described without the use of some kind of straining device. In the case shown in the sketch (*Fig. 29*), it was arranged that two members did not come together at the top panel-point. It would therefore be necessary to deflect one member to bring the ends together (stage 1), but that would only result in fairing-up a few of the rivet-holes, and it would then be necessary further to deflect the member (stage 2) to fair up the remaining holes. That could be done by some very simple form of straining tackle.

Fig. 29.



On p. 114 the Author, referring to a member 40 feet long, stated that an error of 0·01 inch in its length would cause an undesired axial stress of 0·28 ton per square inch. That would be true if each end of the member were rigidly attached to some immovable point, but in actual fact the whole of the structure was elastic ; further some of the members might be an equivalent amount longer and some an equivalent amount shorter than their designed lengths, so that the stress set up would probably not be so great as that referred to.

On p. 117, discussing the action of lacing-bars, the Author suggested that the members should be turned through 90 degrees to bring the lacing-bars into the plane of the truss instead of at right angles to it. In many practical cases, however, lacing-bars were attached by one rivet only, and it would be very difficult indeed to distort the member accurately by a single-riveted connexion.

With regard to the Author's remark on p. 125 that " Pre-stressing should provide for dead plus live plus impact loads," Mr. Bateson felt that possibly a certain amount of impact should be provided for, Mr. Bateson. but the present formula seemed to make an excessive allowance. Moreover, impact was a vibratory stress, the stresses in the members oscillating about a mean value due to dead plus live load and the amplitude of oscillation on each side of that value being the amount due to impact. Before accepting the Author's statement as it stood, it would be desirable to have more information about the effects of impact upon secondary stresses.

With regard to the possible use of welded joints, referred to by the Author on p. 127, Mr. Bateson imagined that it would be extremely difficult to pre-stress a welded structure which was erected piecemeal in the same way as a bridge, and which could not be completely welded in the shops or on the ground. Further, he could not see the possible advantage of welding in the case in question; the Author found that there was no rotation of the riveted joints, so that presumably they gave the required degree of fixity.

The suggestion that the ordinary type of bridge floor should be eliminated and that a transverse-trough floor should be used would seem to involve many practical difficulties, apart from the question of design. As a suggestion for eliminating secondary stresses in chords it seemed to be unsatisfactory, as the chord itself would have to function both as a chord and a stringer, and would be subjected to considerable bending stresses while carrying a live load. A practical difficulty would be the question of camber of the truss as affecting the track.

With regard to the cost of erection, Mr. Bateson did not think that the method of pre-stressing outlined by the Author need add very much to the cost of the bridge, but he thought that it would be essential to develop a technique in erection. It would not be good enough simply to send the steelwork along to the job and leave it to the ordinary kind of bridge-erector, who, if he saw holes which did not fair up, would simply blame the works and proceed to drift them into position by brute force. With the method of manufacture in question, the holes had to be drawn into place by proper methods.

Mr. Greet. Mr. E. H. Greet said, with regard to the relative merits of eliminating deformation-stresses by pre-stressing or of calculating them and including them in the total stresses, that there was one point which stood out clearly. The British Standard Specification for girder bridges had always, rightly or wrongly, specified that for spans below 200 feet and of normal proportions deformation-stresses might be ignored. Fortunately for the average bridge-designer, by far the greater number of bridges produced fell within that limit, so that a designer might go on for years in blissful indifference to, or ignorance of, the significance of deformation-stresses; but, when once he had been compelled to calculate them, even approximately, he was inclined for ever after to shun them like the plague. To say that **Mr. Greet.** they trebled the amount of work was to state the case very mildly, and, in spite of the exhilaration which often accompanied the academic solution of a different problem, engineers would welcome any simpler alternative if they were convinced of its effectiveness. The present Paper showed an effort to provide such an alternative, with the added advantage of a reduction in the weight of steel; and, in the main, the evidence of the experiments vindicated the theoretical investigation.

There were, however, a number of disturbing factors to which he would like briefly to call attention. In the first place, in spite of the obviously careful supervision and the somewhat stringent tolerances permitted in the positioning and fairing-up of the holes, as shown in Table III, the net result had been that rather less than half of the very high deformation-stresses had finally been eliminated by pre-stressing. That was shown in Table V; the deformation-stresses remaining in various members ranged from 11 to 39 per cent. of the primary stress, and in the sub-diagonal the residual stress was stated to be as high as 153 per cent. of the primary stress.

On p. 95, the Author said: " The contract for the manufacture of the steelwork was in due course let to a firm of contractors in India who have specialized in the use of hard steel bushed jigs and who have developed this method of interchangeable manufacture to a high state of excellence. Since there was every assurance that the accuracy of manufacture would represent the highest standard that was likely to be obtained at present. . . ." It was obvious that the value of pre-stressing would be governed very largely by the skill and workmanship shown, and that the work would have to be most carefully inspected and supervised. Therefore, without deprecating the skill of contractors, he was inclined to wonder whether the method did not require a higher standard than that to which most contractors were able to conform. It had also to be borne in mind that the higher the degree of accuracy demanded the fewer became the firms to which the engineer could go for tenders for supply, and the less competitive became the tendering, with consequent increase of cost. Adding that to the increased cost of the extra work, he imagined that the saving of 9 per cent. of material indicated on p. 92 might be more than counterbalanced by the extra price per ton.

Could the Author state whether any bridges had been erected by the pre-stressing method without stress-readings being taken, and, if so, whether he had the same faith in those bridges, or whether he had any knowledge of the magnitude of the residual deformation-stresses? Before reading the Paper Mr. Greet had been puzzled to know how it was possible to avoid damage to rivet-holes consequent

Mr. Greet. on forcing members to bend by driving drifts into the compact groups of holes at the ends. That was made clear on pp. 117 to 120, but it seemed that damage did actually occur, in the sub-verticals and sub-diagonals at least, owing to the light members being compelled to conform to the requirements of the heavy members. The Author's suggestion that web-members should be of box section with the plane of the lacing parallel to the longitudinal axis of the girder seemed useful, though hardly adding to the appearance of the bridge.

In view of the very high deformation-stresses of the Nerbudda bridge, it would be of interest if the Author would say whether it was his opinion that a sub-divided truss of similar proportions but a much smaller span, say 150 feet, designed to the same working stress, would develop deformations of the same magnitude as the 282-foot span. It was a disturbing thought that the British Standard Specification might be at fault in permitting deformation-stresses to be ignored in short spans. If the magnitude of the deformation-stresses depended only on the type of truss and not at all on its length, the whole problem would require thorough investigation. Those designers who were not prepared to accept pre-stressing would then have two alternatives only; either the deformation-stresses would have to be calculated for each bridge, or a series of computations would have to be made for a number of types of truss to assess an all-round allowance for deformation-stresses for each one.

Mr. Needham. Mr. E. S. NEEDHAM said that the subject of the so-called secondary stresses had received much attention from the American Society of Civil Engineers, and some extracts from its publications were pertinent to the present discussion. Mr. O. H. Ammann had stated [1] that it remained an open question whether rigid gussets, producing large secondary stresses, were not a source of strength rather than of weakness, and had inclined to the view that they were a source of strength. Dr. D. B. Steinman had stated [2] that the actual secondary stresses would generally be lower than the calculated values, because there was an automatic readjustment of strains within a structure in such directions as to relieve the secondary stresses.

In a more recent paper Messrs. J. I. Parcel and E. B. Murer had stated,[3] from tests closely simulating the conditions for bridge-members, that "the ultimate strength is practically unaffected, even by high secondary stresses," and that "when the extreme fiber stresses reach the neighbourhood of the yield point, a radical

[1] Trans. Am. Soc. C.E., vol. 89 (1926), p. 151.
[2] *Ibid.*, vol. 82 (1918), p. 1071.
[3] *Ibid.*, vol. 101 (1936), pp. 289, 323.

readjustment takes place, greatly relieving the flexural stresses." Mr. Needham. As a result of the readjustment in stress—strain relations, they had drawn the following inferences :—(*a*) Any tension-member tended, as the load increased, to a condition of uniformity of stress over the cross-section, except for a local over-strain in a short section near each end. (*b*) Compression-members bent in single curvature might be seriously affected with regard to failure by buckling if the deflection due to secondary bending became large. That could occur, however, only in the case of large secondary moments and flexible members, a combination which was not ordinarily realizable. It had been noted that for values of $L/r \leqq 70$, the effect of secondary action in inducing failure by buckling was small. In such a case the rigid-joint action which gave rise to secondary stress acted as a brake on the deflection of the long column before the point of ultimate column-strength was reached. (*c*) For a column bent in double curvature, the secondary action, by forcing the curvature into two "waves," might actually have a beneficial effect with regard to failure by buckling. (*d*) For "stocky" columns, with values of $L/r \leqq$ about 40, which were ordinarily the only members that developed high secondary stresses, failure was nearly always due to local overstrain. Until the average stress approached the yield-point the transverse deflection was negligible, and column-failure in the ordinary sense could not occur. For such columns the secondary stresses, whether resulting in single or double curvature (the latter would almost invariably be the case for high secondary stresses), would merely result in high stresses on the compressive face, which were rapidly relieved by plastic flow of the material as the yield-point was approached. In conclusion, Messrs. Parcel and Murer made the following statements :—(i) The ultimate practical utilizable strength of a tension-member was not affected by any secondary-stress action within reasonable limits. (ii) Compression-members sufficiently flexible to develop failure by buckling before the yield-point was reached would usually exhibit too low a value for the secondary bending to reduce the ultimate carrying-capacity of the member materially. (iii) The rigid type of built column, in which alone high secondary stresses were to be expected, almost invariably failed by local over-stress, usually at a point at which local buckling could readily take place. High secondary stresses had no effect in hastening such local failure, and therefore had no effect in reducing the actual ultimate strength of such compression-members, provided that the proportions of the section were governed by the limits of present standard specifications.

Professor Hardy Cross wrote: [1] " At present secondary stresses are

[1] Trans. Am. Soc. C.E., vol. 101 (1936), p. 1365.

Mr. Needham. accepted as a necessary evil. Try to keep them within a reasonable figure, and otherwise forget about them. The idea of discounting secondary stresses is not new in America, but accurate data as to the amount that may be discounted are much needed."

Reverting to the Paper under discussion, which supplied the data needed, Table V was very instructive, but left some doubt as to whether pre-stressing was worth while, especially if the view were taken that the factor of safety on the utilizable strength of the bridge was not greatly improved thereby. Mr. Needham was fully in agreement with the necessity of investigating a design for probable deformation-stresses, especially to find out those compression-members which were distorted in single curvature. Compression-members bent in double curvature were affected only at the ends, where the area of section was usually sufficient, because of the low working stress used for the member as a column.

The Author stated that the displacement of the neutral axis in the top and bottom chords by the U-shaped section in both cases operated to reduce the effective deformation-stress. It was not clear why that was so. Referring to *Fig. 3*, p. 95, most of the top-chord members were bent in single curvature, and it would appear from the direction of bending that the bottom flange would suffer, being farthest from the neutral axis. A symmetrical chord-section without cover-plate would appear to be an improvement in the design, not only in regard to deformation-stresses but also, from his own experience, because of the difficulty of getting good rivets in the splices of the cover-plate.

One of the most interesting features of the Paper was the description of the model, which deserved a Paper to itself. Mechanical analysis was becoming more and more important as a time-saver in solving stresses due to joint-rotations, and was to be recommended when the technique was available. The analytical determination of deformation-stresses, however, could be made in a practical form by the method of moment-distribution in conjunction with the Williot diagram.

Mr. Moon. Mr. RAMSAY MOON observed that on p. 127 the Author referred to the question of the use of welding in association with pre-stressing, or, perhaps it would be more correct to say, to the use of pre-stressing in a welded structure, suggesting that there might be some difficulty in combining the two techniques. He had discussed the matter at some length with the Author, and his remarks would be confined to that question. So far, there had been no attempt to combine the two techniques, and it was therefore not possible to discuss the matter on the basis of experience, but the known behaviour of welds and of welded joints suggested not only that the use of welding was possible but also that it might provide a means by which the technique of pre-stressing might be used with a precision and certainty as to the distribution of stress which the use of riveting did not seem to afford. **Mr. Moon.**

In view of the fact that both welding and pre-stressing were still in the development stage and presented some degree of indeterminacy, it might be correct to suggest that to combine them at present would result in a second degree of indeterminacy which would be undesirable. Thus, until they were both developed more completely, it might be as well to regard the two techniques as rivals rather than as complementary. As the aim in each case was to effect reduction in weight, it might be suggested that more advantage was likely to accrue from the use of welding rather than of pre-stressing, since for pre-stressing a claim for a saving of about 10 per cent. was made, whereas a saving of from 15 to 20 per cent. might have been expected had the truss been designed for welded construction.

When a satisfactory arrangement was evolved for the joints of the welded truss, the application of pre-stressing would present very interesting possibilities. As pointed out in the Paper, there were two inherent difficulties in the riveting technique. Firstly, owing to the slip of the rivets which took place on the first application of the load, the distribution of the initial stresses was altered in a way which was variable and which could not be calculated. Secondly, it was difficult to put the stress into the members, particularly if they were stiff, though the Author made a useful suggestion in that respect. There was some danger of damaging the metal in the neighbourhood of the hole, and always considerable uncertainty as to the magnitude and the distribution of the stresses induced. The use of welding might provide a solution of both those difficulties. Firstly, a welded joint was essentially rigid, and no appreciable slip or change of stress-distribution was likely to take place on the first loading, so that once the stress was put in it remained. Secondly, the shrinkage of the weld-metal on cooling, to which the Author referred as a possible source of difficulty, might in fact provide the means for introducing stresses of any required magnitude into the members. Engineers who had had trouble with shrinkage and distortion of welded members might be inclined to laugh at that suggestion, but the technique of welded construction was gradually approaching a stage where accurate control would become possible. If the joints were of the type which was normal in riveted construction, with plates lapped, he saw no way in which welding could be satisfactorily applied or in which stresses could be effectively induced thereby; but it was certain that when trusses of the size and type in question were built by welding they would be built with butt-joints. Experiments had shown that the shrinkage across a weld was proportional to the sectional area of the weld. Thus the stress induced in a particular flange of a member could be controlled by

Mr. Moon. the design of the weld. If it were shown that too much stress or too little stress had been induced by a particular weld, the stress could be adjusted by adding additional runs of metal or by removing a certain amount of metal and re-welding. He would suggest, therefore, that when welding technique had been developed sufficiently, which was not the case at present, it might prove to be a valuable means of applying pre-stressing.

Mr. Thornton. Mr. D. LAUGHARNE THORNTON remarked that the Paper touched an important aspect of structural engineering, particularly in relation to the construction and design of large suspension-bridges with unstiffened trusses. Moreover, the process of pre-stressing described by the Author afforded a measure of control over the natural frequency of vibration of a structure, and was therefore of interest in connexion with bridges designed to withstand the consequences of prescribed hammer-blows produced by locomotives, since one effect of a partial constraint was that of raising the gravest frequency in particular.

In various parts of the Paper reference was made to apparent variations in the value for the direct modulus of elasticity concerned, which were enumerated in Table II. Those observations lent support to the view that the variations in question might advantageously be regarded as a measure of the " mechanical efficiency " of the bridge considered as a machine which was describing small displacements about a mean position. In view of the different sizes and types of joints associated with the structural system, the magnitude of that efficiency would be different for different members, and for certain parts of the bridge the value of the modulus was as likely to be slightly greater than 100 per cent. of E as it was to be less than that figure, since for structural members connected to a common joint the relative motion was probably executed in a series of jerks. That might also partly account for the observed loss of camber.

With reference to dynamic loading alone, the transverse beams mentioned on p. 108 and indicated in *Fig. 15* would act as " condensers " with respect to the stress-waves associated with the resulting oscillations of the model. Consequently, were it possible to take autographic records of the stresses acting on various parts of the model, the graphs would in general not be of the same shape or form as those taken on corresponding members of the actual bridge, which did not include such transverse beams. For corresponding parts of the model and bridge the difference in shape of the two graphs would in some respects resemble the effect of the phenomenon of interference in the case of complicated structures, in so far as some of the harmonic components of the stresses would on that account be " masked," and therefore extremely difficult to evaluate from records taken with the aid of the model.

The process of pre-stressing might involve the reversal of some or

Mr. Thornton.

all of the forces acting on the members of the bridge compared with the direction of the forces imposed on similar systems which were not constructed in accordance with the pre-stressing procedure. In those circumstances due consideration should be given to the disturbed motion of systems with reversed forces of a dynamical character. That arose from the fact that, in any dynamical system acted on by constraints independent of the time t and by forces that depended only on the configuration of the system, the integrals of the equations of motion remained real if $\sqrt{-1}t$ were substituted for the original symbol t and $-\sqrt{-1}v_1, -\sqrt{-1}v_2, \ldots -\sqrt{-1}v_n$ in turn substituted for the initial velocities $v_1, v_2, \ldots v_n$ of the n points on the system under examination. The expressions thus obtained represented the motion which the same system would have if, with the same initial conditions, it were acted on by the same forces reversed in direction. It was therefore in general advisable to utilize a complex variable in the process of comparing the disturbed motions of two similar systems, one of which was acted on by reversed forces.

The Author.

The AUTHOR, in reply, wished to express his deep appreciation of the encouraging reception accorded to the Paper and of the very interesting and instructive discussion which it had provoked, even though its subject was not a "popular" one. There seemed to be little doubt that a fuller knowledge of the internal stresses in the members of a truss when carrying its design load would make possible economies of materials which could not be ignored.

The elimination of secondary and deformation stresses, as Mr. Gribble had suggested, would undoubtedly in time lead to the general acceptance of higher working-stresses for design purposes, but before that could come about it would be necessary that a method of design and erection should become general that would ensure, even under ordinary conditions of moderately careful erection, that the desired results would always be attained. The method put forward, namely, that of rotating the web-members to bring the lacing-bars into the plane of the truss and riveting them up when under load, was one which did ensure that effectiveness, and the operations involved could be under the complete control of supervising staff. Further, a truss erected in that way and free from serious deformation-stress could always be recognized as such, a matter of some importance in ascertaining the carrying-capacity of girders after they had been in use for some years. In the case of trusses composed of light members the mere rotation of the members without leaving the lacing-bars loose should be all that was necessary to ensure effective pre-stressing. With such members a double advantage was obtained, in that the members were made more flexible in the plane of the truss and easier to deform by drifting at the ends, and also that, should

The Author. the resulting pre-stressing be in defect, subsequent distortion in the same plane would produce smaller stresses than would occur if the webs were solid. Mr. Bateson's objection to dealing with single-riveted lacing-bars would not arise in such cases.

Mr. Greet had asked whether there was any justification in distingishing between spans of less than or more than 200 feet. It was clear that, provided the same working-stresses were used, joint-rotations of the same magnitude would occur in spans of similar geometrical outline regardless of the span. The intensity of deformation-stresses would then be directly dependent on the cross-sectional dimensions of the members. In short spans those dimensions would normally be less than in longer spans, but the intensities of loading for which they were designed might counteract that effect. Those factors, together with numberless variations in details, would make any attempt to standardize allowances for particular types of truss little more than guess-work.

Mr. Bateson's question with regard to impact would appear to be answered at the top of p. 107. If the span were to be pre-stressed to eliminate deformation-stresses for a load covering the whole span, then the object sought would be to reduce to zero the deformation-stresses when the span was deflected by a load equivalent to the total of the dead, live, and impact loads.

The important feature of the proposed method of eliminating deformation-stresses outlined in the Paper was the fact that by loading the span it would be ensured that all members were being effectively dealt with; as observed by Mr. Greet, that could not be guaranteed by methods based on the use of drifts or on straining devices, and Mr. Fereday's statement that "other examples of the successful application of pre-stressing could be cited" could not carry great conviction in the absence of test results. So far as the Author was aware, no strain-measurements had been carried out on the bridges referred to by Mr. Fereday, with the exception of the Willingdon bridge, in which case, owing to special circumstances obtaining at the site, a series of readings at one cross-section only of a bottom chord had been obtained. Further, he understood that on the Willingdon bridge the chords only had been dealt with, by being provided with square butt-joints and cambered after riveting.

The cambering of the top chords required some consideration, since if the splices occurred at the panel-points the chords could not be fully riveted in the "straight" position, as it was necessary to drop each splice on to the top of the post before all rivet-holes could be filled. That point was mentioned on p. 125, and he could assure Mr. Fereday that nothing short of a completely-riveted splice was good enough before cambering, if stress were not to be lost.

The specification as quoted on p. 94 was worked to in the erection, **The Author.** for the reason given above.

The shape of the top chord in its distorted shape, which had been queried by Mr. Fereday and others, was correctly shown in *Fig. 3*, p. 95. That diagram, with the conventional exaggerations, showed the top chord when correctly pre-stressed to be in tension throughout on the top flange except near the hip joint, and that distribution was confirmed by calculation, by actual measurement, and by the model.

Mr. Needham had asked why the U-shape of the top and bottom chords operated to reduce the effective deformation-stresses. The reason was that the neutral axis was nearer the bottom flange in the case of the tension (bottom) chord, and deflexion causing a sag at the centre of the span would cause a reduced tension in the bottom flange but a larger compression in the top flange. The compressive stress was, however, of little importance, since it was neutralized by the primary tensile stress. The reverse occurred in the case of the top (compression) chord, where the U-section was inverted.

He appreciated Mr. Fereday's remarks as to the value of taking stress-readings on more than one truss, but had the time, money, and energy been available for more readings he would have been more inclined to double the number of readings on the test-truss, before taking spot readings on other trusses, since in his opinion more valuable information would thereby have been obtained.

All stress-readings and all work connected with the tests had been carried out by the Author's staff and not, as Sir Clement Hindley had suggested, by the Contractors' staff. Extra work required of the Contractor, such as the erection of a truss in the prone position, had been paid for as an extra.

He did not contend, as Mr. Fereday appeared to believe he did, that statically-indeterminate structures could not be effectively pre-stressed. The Paper pointed out certain difficulties which had been met with in practice on the Nerbudda trusses: on p. 125 proposals were put forward to assist in overcoming those difficulties in the case of redundant frames. Again, Mr. Fereday took exception to the statement made on p. 112 that "the pre-stressing was also largely neutralized by the application of the dead load." That statement was intended to be taken literally, and an examination of Figs. 5 to 10 (Plate 1) would substantiate it; Mr. Greet, in his remarks, appeared to have interpreted it correctly.

The statement on p. 123 should also be taken literally, and not necessarily as interpreted by Mr. Fereday. By a "simple" section the Author meant a section composed of few scantlings and not necessarily symmetrical; and by "uniform" the Author meant a uniform distribution across a section of a member subjected to an axial load.

The Author. Mr. Gribble raised the interesting question of eliminating secondary stresses resulting from the floor-system as usually designed. There was no doubt that substantial relief could be obtained from lateral distortions of the bottom chords (in a through truss) and cross girders by the expedient of putting a full design load on a span before finally riveting the floor-members and bottom lateral bracing. Relief to the web verticals might be obtained as stated on p. 123 by inclining slightly the ends of the cross girders, but if those steps were to be put into effect there would still remain a duplication of metal in the stringers and bottom chords as referred to on p. 128. A more satisfactory and more economical solution would evidently be to combine the duties of stringer and bottom chord in the same member; Mr. Bateson's objection to doing so could not easily be understood. It had to be remembered that, whatever undesirable stresses might remain after making that change, under conditions of partial or concentrated loading, the same stresses would also be present though probably to a larger degree in a truss as designed at present. The Author was satisfied that if Mr. Bateson cared to examine the camber of a truss as a specific case he would find that the shape of the bottom chord was improved rather than otherwise. The Indian authorities had already accepted a cambered track on an unloaded truss as satisfactory, provided the camber did not exceed 1 inch per 100 feet of span.

With regard to the top-chord lateral system, if a K-system were used the secondary stresses induced (as noted on pp. 105 and 121) should always be small, except perhaps at the centre of the truss, where continuity over the two centre panels would occur unless the Ks pointed away from the centre.

He was at a loss to understand how Mr. Durant could have so misread the statements made on p. 95 as to have deduced that it was the Author's contention that a span could be free from secondary stresses under all conditions of loading. The further reference to this question made on p. 126 discussed the difficulties to be dealt with and the alternatives available. It would appear to the Author that the methods of obtaining "automatic" pre-stressing put forward in the Paper could with advantage be applied to all types and dimensions of truss spans, and, in fact, wherever pre-stressing was contemplated. Even where no pre-stressing was desired, the suggested rotation of web-members would itself be of assistance in bridge spans of all types, including open-spandrel arches, since that rotation would produce extra stiffness in the transverse plane where it was usually very desirable.

Although the day was probably rather distant when an all-welded pre-stressed truss would be produced, yet the possibility of a reduction in weight and cost made the prospect an attractive one, and Mr.

The Author.

Moon's remarks as to possible directions of development were most interesting. It appeared that if Mr. Moon's forecast became a reality it would be possible to induce deformation-stress in a member by adding repeated runs of weld-metal until strain-meters indicated that exactly the required stress had been obtained.

There appeared to be a widespread opinion that the introduction of the use of hard steel jigs increased the cost of manufacture of steelwork unless quite a large number of similar spans were to be fabricated, and that view appeared to be held by many British manufacturers. The Author, however, was convinced that a works thoroughly organized for the use of jigs would be able enormously to increase its output, with savings in both labour and plant. Such reorganization had been carried out in the case of at least one firm of bridge manufacturers, involving some initial outlay, but he understood from them that their capacity had thereby been greatly increased and their working expenses so far reduced that even for twin or single girders the use of steel-bushed jigs was more economical than the usual method of manufacture. It was interesting to record that for reasons of economy steel-bushed jigs were being used for every joint in the Howrah cantilever bridge, which was in fact a single span. Even in that case there would be four or eight members of each type to manufacture.

The extra cost feared by Mr. Gribble of erecting spans in the manner proposed in the Paper would be confined to the cost of the extra field riveting—a small item, as noted by Mr. Bateson. The certainty of removing deformation-stresses should be sufficient to permit the use of higher working stresses, as Mr. Gribble had remarked, perhaps 10 tons per square inch, and that point merited serious consideration. Even with such a very moderate increase in working stress, an overall economy in cost of from 5 to 10 per cent. could be anticipated, apart from the considerable saving which in India would result from the elimination of timber sleepers.

Mr. Gribble had questioned the possibility of putting compression-members under full load before the lacing-bars (or batten-plates) were riveted to them. That point, however, offered no difficulty when the details of the unriveted member were considered more closely. The lacing-bars would in any case be attached by small bolts allowing a movement of $\frac{1}{16}$ inch to $\frac{1}{8}$ inch, which would be sufficient to permit the deflexions required by the pre-stressing method but quite insufficient seriously to affect the capacity of the member as a strut. In fact, many erecting-crane jibs worked constantly with bolted connexions and lacing-bars, and were virtually under the conditions visualized for pre-stressing compression-members.

With regard to the permanent set which appeared to have occurred, The Author. particularly in tension-members (pp. 103 and 104), stresses measured on the 10-inch bases were in general well below the yield-point of the metal (though at three or four points stresses up to about 12·5 tons per square inch were measured), but it was possible that members might have behaved much as a wire rope did under its first loading. Minor wrinkles were bound to exist in rolled sections and plates, and a tensile load well below the yield-point of the metal could operate to draw them out and pack the scantlings more closely together, the resulting strain resembling a yield in the members as a whole and reducing the effective modulus of elasticity. Mr. Thornton's theory to account for departures both above and below the recognized value of E was most interesting, and the Author would be glad to hear more about it.

The model had been primarily intended to be used as a more convenient and rapid method of analysing complicated frames, as noted by Mr. Needham, but the Author was not aware that he had implied, as stated by Mr. Durant, that more cumbersome mathematical methods were impossible. He was also not aware of any departure in the Appendix from a true-scale relationship, as Mr. Durant seemed to fear. The material used in the construction of the model, though sold commercially as celluloid, was in fact some other synthetic composition, and its elastic modulus was not the same as that for celluloid quoted by Mr. Durant. He was of the opinion that it was easier to represent the many structural details of a truss in a model, with some approach to accuracy, than to make proper provision for them in a purely mathematical calculation. Gusset details at a joint were a typical example.

The condenser effect under dynamic loading of the magnifying beams used on the model could be understood, but it should be emphasized that the model had not been intended in any way to examine the effects of dynamic loads.

Mr. Needham's précis of current American opinion on secondary stresses was of great interest, and indicated that the question, which had been largely neglected in Britain, had received careful study in America. It should be remembered, however, that Amercan bridge practice differed considerably from British practice. The effective depth of American trusses was considerably greater than that of those built in Britain, so that the deflexions and distortions were smaller, and, further, the members were in general more slender and suffered less from a given distortion. Moreover, a dead-load plus live-load plus secondary stress might be just below the yield-point and be perfectly safe for a single application but disastrous when occurring as a repeated stress.

*** The Correspondence on the foregoing Paper will be published in the Institution Journal for October, 1937.—Sec. Inst. C.E.