Discussion.

The Author. The AUTHOR, in describing a number of lantern-slides dealing with the work of the Bridge Stress Committee, remarked that 29 years ago, under the Presidency of the late Sir Douglas Fox, Mr. W. B. Farr's Paper[1] had occupied three nights of discussion at the Institution; 14 years ago Mr. C. W. Anderson's Paper[2] on impact coefficients for railway girders, with particular reference to Indian railways, had been read, and 5 years ago, almost to the very day, Professor Inglis's Paper[3] on the transverse oscillations of girders had been discussed. During the last 5 years the acquisition of knowledge on the subject had been very much greater than in the previous 25 years, because during those 5 years there had been a co-operative attempt, by bringing together various bodies and interests, to put the matter on a sound footing. It was the hope of those who had been engaged in the research that their work would be accepted as having put the matter on a very much sounder basis than it had been before. His Paper was merely a sign-post to the subject-matter of the Report.

Sir Alfred Ewing. Sir ALFRED EWING remarked that during the 5 years or more of the Committee's efforts he had sometimes, in his more despondent moments, recalled the Latin proverb *Parturiunt montes; nascetur ridiculus mus.* He had feared that after the long labours of the Committee the engineering world might consider the output trifling and exiguous. But it was not so. If anyone took the Report in his hand he would realize that it was a weighty document; if he opened it he would discover that while here and there it bristled with repulsive-looking mathematics, most of it was quite interesting; he would also see many excellently reproduced diagrams; and, if he read it carefully, he would discern a gradual development of knowledge on the part of the Committee. The Committee became aware as they went on that they had everything to learn about impact effects. At first the problem had appeared to be simple; but, as they proceeded to investigate it, the difficulties increased, and there had been moments when it seemed as if a complete solution was not to be attained. He was glad to say, however, that at last they

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[1] "Moving Loads on Railway Underbridges," Minutes of Proceedings Inst. C.E., vol. cxli (1900), p. 2.

[2] Minutes of Proceedings Inst. C.E., vol. cc (1915), p. 178.

[3] *Loc. cit.*

Sir Alfred Ewing.

had reached full daylight—mainly through the guidance of Professor Inglis. Professor Inglis would be the first to admit that without the experimental work of the Committee he could not have given a complete solution. The experimental work of the Committee had not only taught Professor Inglis what problems had to be faced, but had also supplied him with the necessary data. Without these his mathematics would have had no interest for engineers. Besides supplying experimental data, the tests carried out by the Committee had shown what features in the behaviour of bridges had to be explained. Thus it was that Professor Inglis had been able to advance the matter to the present entirely satisfactory conclusion.

The whole work of the Committee was an example of what might be done by co-operative research. There were types of research which an individual worker might undertake in a laboratory with a few simple appliances. The research under discussion was of a very different kind: it had required the co-operation of the four great Railway Groups. He did not think it could have been carried out by them alone. It had required also another element which the Department of Scientific and Industrial Research had been careful to introduce into the Committee—a theoretical element, contributed by certain members familiar with the application of mathematics to engineering problems. Besides that they had had a representative consulting engineer, and also Sir John Pringle, who was well acquainted with the vagaries of railways and had often to sit in judgment upon them. Finally, when they began to realize, as they very soon did, that the problem before them concerned locomotives quite as much as the permanent way, they invited Sir Henry Fowler to join them, and he had immensely strengthened the Committee. To himself, as Chairman, it was a matter of much gratification that they had issued a unanimous Report. Considering the diverse elements which the Committee included, their unanimity was important as well as satisfactory. The permanent-way engineers on the Committee, who were responsible for bridge-construction, would not have been disposed to sign recommendations which were obviously wasteful. Sir John Pringle would not have been disposed to sign recommendations which were rash. He left it confidently to the judgment of engineers as to how far the recommendations of the Report should be accepted. It should be understood that the Report, which in its earlier chapters recorded the Committee's groping towards knowledge, had been finally issued with a full sense that they had got to the bottom of an extremely complicated problem. The experimental work, and, to some extent, the analysis which followed, had been essentially the work of the Author and

Sir Alfred Ewing. his staff. The Committee had been fortunate in the appointment of their Chief Engineer and of those who had assisted him. As the Report truly said, the Author's skill as a designer of bridges was only one of many qualifications which he had for such a position. The Committee soon found that the older methods used by engineers, such as the Pencoyd formula, were illogical and could not be justified as a means of coming to any rational conclusion about the safety of a bridge; but it was much more difficult to devise anything that could appropriately take their place. What the Committee had done was to give numerical results under certain assumptions about load and about hammer-blow—numerical results which would be applicable to British railways, adequately covering all the heaviest loads which railway-bridges were likely to bear under existing conditions of gauge and loading-gauge, and also the worst hammer-blow to which they were likely to be subjected. It was a great satisfaction to know that already, very largely through the influence of Sir Henry Fowler, the locomotive-engineers were looking sharply after their hammer-blows. They had already scrapped many of the worst offenders, and they had even undertaken not to exceed certain definitely limited amounts of hammer-blow in their future designs. The Committee recognized before they began their labours that in the then existing three- and four-cylinder engines there was an immense advance in the direction of minimizing hammer-blow. But among two-cylinder engines there was still a wide range in the amounts of hammer-blow, and if the worst offenders could be got rid of, the numerical figures given in the Report showed how large an advantage would result. The Committee had been able by the aid of "locomotive K" to punish certain bridges as they had perhaps never been punished before. Thanks to Sir Henry Fowler's co-operation, and that of the company, they had been able to use that locomotive all over the country as a valuable testing-appliance. They had run it at speeds which caused a fluctuation of pressure upon the rails of more than 50 tons in every revolution of the wheels. They had become much attached to "locomotive K" and hoped it would some day be given honourable burial. In conclusion, he would warn engineers not to expect too much from the Committee's Report. They would find in it some reading-matter which might not be easily intelligible, but all of it was worth trying to understand. The Author had been quite right in saying that his Paper was essentially in the nature of a sign-post. It pointed the way to things that were more fully set forth in the Report. Numerical values were given there for the allowances which the Committee considered necessary and sufficient in particular cases, but engineers must not expect to find any simple rule applicable to cases outside the range of British locomotives and British railways. On the other hand, a careful student of the Report would find in it the complete theory and an account of the method by which numerical results were arrived at, so that, if he took the pains to carry the method out, he could apply it to any problem which might present itself. A rational treatment of a very difficult subject was what the Report might fairly claim to present to the engineering world. Sir Alfred Ewing.

Mr. H. J. Fereday observed that the work described in the Paper might not be the last word on the subject, but there was no doubt that it was a great advance on any work described hitherto. In the application to Indian bridges (with which he was specially concerned) of the allowances calculated from the results of the investigation he would have to keep an open mind. He would have been pleased to see some synchronized deflection records taken at points about five-eighths of the length of the span, because that was the critical part of the web members in a bridge. Such records apparently had not been taken. That matter should not be omitted from investigations, because at that point on the length of the span there were actual reversals of stress in combination with dead-load stresses. He thought the Author had rather missed a point in not having continued the series of tests which he had taken on the web members of the Newark Dyke bridge; if he had only taken them a few bays farther he would have had some very interesting results, and more particularly so if at that point synchronized deflection tests had also been taken on the main girders. No test on the same member with the same locomotive running at the same speed would have given the same result. That question would want clearing up before one could be quite sure that the tabulated allowances for shear were sufficient, particularly where engines were running in tandem, as they did on Indian railways. Locomotives with 28-ton axle-loads were now being adopted as the standard for some of those railways, and in fact all new important bridges were being designed for those locomotives. A point of importance of which he wished particularly to speak concerned the floor-system of a span consisting of cross girders and rail-bearers. If the Author had taken stress-records on the rail-bearers and cross girders in the Newark Dyke bridge, he would have been rather surprised at some of the results, because, while he alluded in the Paper to the reduction of stress in the main girders due to the relief afforded by the floor-system, he forgot to say that whatever relief there was in the chord members was an added stress in the floor-system. It was obvious that, as the rail-bearers were riveted to the cross girders, and the cross girders Mr. Fereday.

Mr. Fereday. to the main girders, the whole of the rail-bearers were in tension, owing to the elongation of the lower chords of the span under the load, and those nearest to the centre of the span were stressed the most. The cross girders, particularly at the ends of the span, were bent laterally, sometimes to such an extent that in one of the bottom angles of the end cross girder there was compression instead of tension. Further, the maximum effect in rail-bearers and cross girders was greatest when the bridge was fully loaded, particularly with two locomotives. The effect of the vertical oscillation of the main girders was also induced in the rail-bearers and cross girders. The sum total of those induced stresses might be very great. Moreover, when a rail-bearer was shorter than the distance from the centre of the rear wheel of a locomotive to the centre of the leading wheel of a tender, there was a moment when theoretically there was no stress in that rail-bearer; but, as a matter of fact, there was always a stress in it due to tension, and there was always an instantaneous relief of stress when the wheels passed over it. Then there was a considerable amount of vibration in the rail-bearer; and it was a definite fact that during the passage of a train over a bridge there was neither a rail-bearer nor a cross girder which could possibly have the same record of stress as any other. The whole of the floor-system was in rapid vibration from end to end, and those vibrations were communicated through the attachments to the chord sections. Those vibrations of stress ought to have been recorded by stress-recorders. For many years that tension had been recognized, and, in special cases, allowances had been made for that effect. So important had that been considered, that 6 months ago an Indian Railway had been asked to select a recently-erected span of about 150 feet, broad gauge and single track, and to test the floor-system and the bottom chords, in a manner suggested to them. They were to use two locomotives at a crawling speed. While those records were being obtained in India, he had had the whole of the stresses calculated in his office, treating the span as an entirely elastic structure. That had taken many weeks to do, but it had been done before the records were received. The ninety-five records had arrived only very recently, and although he had not been able to study them fully, he found that the tension in the rail-bearers and the lateral bending in the cross girders were clearly shown. Some of the calculations were identical with the stress-records. From some of the remarks which were made in the Paper about instrumental defects or errors, he thought the Author had been rather disappointed with some of those records of vibration stresses because they were in such contrast to the beautifully smooth curves

Mr. Fereday.

calculated for comparison. Some of those records were given in Fig. 38 of the Report for cross girders, and it would be found that the stresses were quite different for the two bottom angles, and the mean had been struck between the two records and had then been compared with the calculated stress. The calculated stress did not agree with the records. Actually the maximum of those stresses should have been taken. The phrase "instrumental error" occurred so often that he felt bound to say a few words about the stress-recorder which bore the names of Mr. Palmer and himself. The instrument had been used on the bridge tests inaugurated by Colonel Sir John Pringle, and he was rather inclined to think that criticism had been levelled against it both because it was new —rather a British characteristic—and also because it did not register the calculated stresses. But stress-recorders were designed to record actual stresses. Two engineers, in giving an address on bridges at Hull at a meeting of the British Association, had compared stress-records with the calculated stresses in a loaded beam. With the maximum load on that beam they had given 8·6 tons as the recorded stress and 10 tons as the calculated. Those calculations were the ordinary calculations, such as one might use for calculating the strength of a lintel beam; but they were not refined enough to be compared with a scientific instrument. He had recalculated those stresses and found that the stress-records were correct. That had been the state of affairs when the Committee commenced their work and obtained two stress-recorders. He was sorry to say it, but he stated the fact, that for a whole year those two instruments had been used in a way for which they had not been designed; and he had called the Author's attention to it. It was hardly necessary to say that the Author had immediately taken steps to remedy that sort of work. Mr. Fereday would not accept any record that came from the stress-recorders during that period. When he knew that the Bridge Stress Committee had been about to develop an instrument of their own, he had naturally expected something new and brilliant. He had been rather amazed to find that they had actually adopted an instrument in which the magnification was by a metal lever—a method that had been discredited since 1910, when the American Railway Engineering Association issued their Report on bridge-testing for impact, in Bulletin No. 125.[1] He was also sorry to say that the Committee had not obtained his permission for copying the essential part of his stress-recorder and adopting it in the new one. If the Committee had asked his permission he

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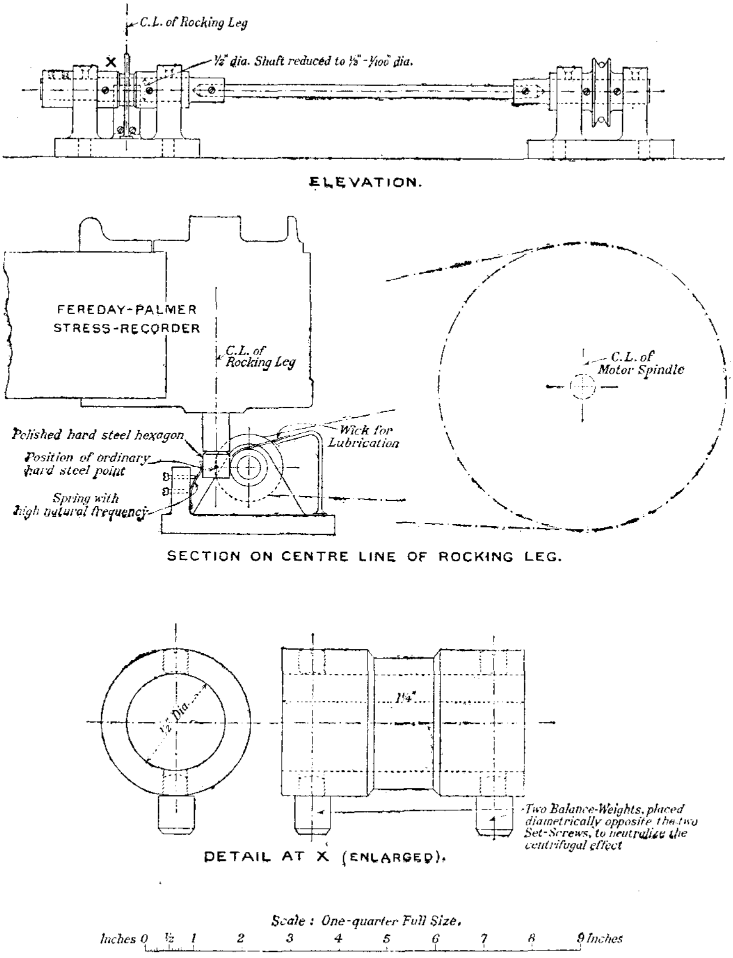
[1] Proc. Am. Ry. Eng. and Maintenance of Way Association, vol. 12 (1911), pt. 3, p. 12.

Mr. Fereday. would have given it immediately, and he thought that this was clearly proved because he had taken no action in the matter. The Author stated that there was a wide field for inventive genius in the design of such instruments. The Committee had held that

Figs. 13.



ELEVATION.

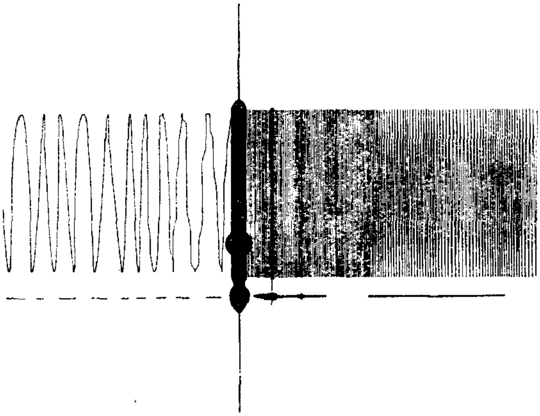
SECTION ON CENTRE LINE OF ROCKING LEG.

DETAIL AT X (ENLARGED).

field for 5 years, and he could not say that they had searched very far for that inventive genius. He would like to read an extract from the Report (p. 190) with reference to the inaccuracy of instruments. "The only conclusion which can be drawn is that such errors, which are due to the inertia of the various component parts of the instruments, Mr. Fereday.

are inherent in any mechanical device and therefore in any instrument in which the magnification of a small movement is accomplished partly or wholly by mechanical means." That was the first time he had heard of a beam of light considered terrestially as having inertia. The magnification in the instrument was by a beam of light, and the only oscillating part was a small mirror weighing a few grains. The Committee carried out some rapid-vibration tests on stress-recorders, described on page 188 of the Report, and stated that at 200 vibrations per second the magnification increased about 25 per cent. He did not admit that that was correct. The instrument they had developed for carrying out those tests (shown in Fig. F. 3 of the Report) had certain serious defects. One was that ball bearings were used, which were quite useless for such work. Another was that the crank was connected with the leg of the instrument by a metal connector, which, of course, had considerable weight; and, as far as he could see, there had been no attempt to poise that part of the apparatus. Then again, the pulley driving the apparatus was between the jaws of the apparatus itself, and the effect of a joint in the belt passing over the pulley would be registered on all the records. Before that instrument had been made he had designed an apparatus (*Figs. 13*) which was used by the makers for testing every Fereday-Palmer instrument before it left the works. To test a recorder the point on the leg of the recorder was removed and a corresponding square block was screwed tightly in its place. That block was pushed against a cam by a spring having a very high natural periodicity. The cam had a throw sufficient to register about 6¾ tons per square inch on the scale of the instrument. The

Fig. 14.



Mr. Fereday. cam was poised, and the driving-pulley was kept well away from the instrument itself, so that the effect of the belt would not be felt on the record. *Fig. 14* was one of the records taken. The open vibrations were produced by pulling the belt round. The closely-packed lines were a record of vibrations of a periodicity of 210 per second. The recorded amplitude of that high-frequency vibration exceeded that of the low-frequency vibration, not by 25 per cent., but by 7 per cent. Thousands of records were received every year from India, which were, for the most part, well produced; and he could not understand why some of the records obtained in England had been rejected as containing instrumental errors. The stress-recorder was doing the work in India for which it had been designed; and it was saving bridges marked down for complete renewal, because it showed the actual, and not the calculated, stresses. While the Committee's deliberations were proceeding, the German State Railways had been carrying out tests of instruments submitted in open competition for a prize. They had suggested that the instrument was not to be optical. Notwithstanding that, they had given his instrument the first prize of 4,000 marks. He was loth to mention that, because he did not desire self-advertisement; but he did want to draw attention to the opinion of Continental engineers with regard to stress-recorders. He had rather denounced some of the Author's work, but he did not wish his remarks to be taken as applying generally. He looked upon the Author as one of the most capable engineers in this country with regard to bridgework, and he hoped that he would be allowed to collaborate with him from time to time in discussions which might be to their mutual advantage.

Dr. Lowe-Brown. Dr. W. L. LOWE-BROWN observed that, in justification of his venturing to take part in the discussion, he could claim that he had read the Report, though he would not say that he had understood it all. He was proud to think that, as a member of The Institution and as an Englishman, he received a certain amount of reflected glory. British engineers had taught Civil Engineering to the world, and he thought that again on this occasion they showed themselves leaders of engineering thought. The Committee, although it had done its best to disguise the fact, was really very proud of the Report. The members must be grateful that the Author had given them the opportunity of discussing the subject and thereby linking the work of the Committee with the "Proceedings." A second claim he could make was that he had been familiar with this subject for some considerable time. When the Argentine Government was contemplating the revision of bridge-

Dr. Lowe-Brown.

specifications 15 years ago, he was delegated by the Chief Engineers of the four principal Argentine railways to represent them before the Government, although he was not appointed Chief Engineer of one of those railways until a year later. One of the principal points on which differences of opinion had arisen was the question of impact, and, as the American Railway Engineering Association's Report, giving a mass of information, was then available, he had gone to that for assistance in preparing a proper argument for his case. He had plotted the results of the tests given in that report; but with the information available it was impossible to draw any definite conclusion beyond a general confirmation of the assumption that considerable impact effects occurred. When the Bridge Stress Committee started work they had the advantage of having in front of them not only the American results but also the Indian Railway Board results, which, being of later date, naturally approached nearer to the heart of the question. Although neither of those reports had yielded any very satisfactory or definite conclusion, they must have been of very great value to the Committee, especially in enabling them to avoid a large amount of unproductive work. The task set to the Committee had obviously been very difficult. Would they succeed where others who had spent so much time and thought and money had failed? Their progress was really typical of research at its best. They had started practically in darkness. Flashes of light had occurred as they went on, but these had not led very far until Professor Inglis experienced not only a "brain-wave," but a series of "brain-waves," until it might almost be said that he had established a state of cerebral resonance! He had thrown such a brilliant light upon the subject that the key had been found and the Committee had been able to present their Report with a feeling of assurance in its correctness that must have been very satisfactory. The brilliant work of Professor Inglis was not placed in the forefront of the Report, but in Appendix J; which was, to him, a clear indication of its importance; for in such reports it was customary to present first a general outline of the results, with a necessary but not a tiresome amount of reiteration, then the easy parts of the argument were put in, and finally all the hard parts were put into Appendixes. The Report gave a number of tables which would in future form the basis of the calculation of railway-bridges in England. It showed in the diagrams and by implication how in the range of span between 80 and 160 feet, by choosing either a stiff or a flexible bridge, a heavy one with ballasted track or a light one with rail-bearers, a type could be found for any particular span that would avoid the worst effects of the hammer-blow at 6 revolutions per second. The

Dr. Lowe-Brown. effect of the rail-joint, which at one time had been generally considered to be the principal cause of the impact on bridges, was shown to be trifling, whereas the lurching of the locomotive, previously left out of consideration, was shown to have a very much greater effect. In a deck-span bridge, for instance, where the rail was directly over the girder, it might increase the load by 12½ per cent.; in single-track bridges with cross girders the addition might be reduced to 4 or 5 per cent.; while with double-track bridges with cross girders it was insignificant (about 1 per cent.). After the whole-hearted way in which the locomotive-engineers had given their assistance to the Committee, it would be ungenerous not to follow the Author in refraining from pointing a finger of accusation. Certainly some of the locomotives did cause very serious stresses in bridges, but the locomotive-engineers had been no more aware of that fact than had the bridge-engineers, and now Sir Henry Fowler, on behalf of the locomotive-engineers, had guaranteed to keep the hammer-blow below a certain maximum. Dr. Lowe-Brown thought that maximum would yet be reduced, but it was probably the best that, in the meantime, Sir Henry Fowler could guarantee. Bridge-engineers had been used to regulating the passage of locomotives over bridges by their weight. Now, of course, they must do so by the weight and hammer-blow combined, and he thought there would have to be some arrangement among the railway-companies by which every locomotive, before being put into service, would have a certificate stating both the weight and the amount of the hammer-blow. He hoped that the hundreds of useful calculations made in the preparation of the tables would not be lost, and that some way would be found of putting them together in a form that could be used by other engineers. It was a pity that the tables had not included the equivalent uniformly-distributed loads for double-heading with loading A (20 tons axle and 5 tons hammer-blow), as he felt sure those would be required sooner or later. The Report showed how the stress should be calculated, but it did not indicate what the working-stress should be. That was outside the scope of the Committee's work, but it was a question that engineers would have to answer. Certain phrases in the Report implied that 8 tons per square inch should be used for all spans, while others appeared to suggest that some allowance should be made for range of stress. Many engineers had defended the high impact-allowances for short spans given by the formula they used by saying that they resulted in giving a low working-stress in short spans; others who had used a range-of-stress formula had said that their range of stress allowed for impact. It might be convenient, when one did not know much

Dr. Lowe-Brown.

about either, to combine the two, but it did not lead to clear thinking. Now that one of the factors had been isolated, there ought to be some guidance to clear thinking on the other. For example, if a working-stress of 8 tons per square inch was all that could be allowed in a bridge of long span where the minimum-to-maximum range was 4 to 8 tons, then in a short bridge, where the range of stress was 1 to 8 tons, a maximum working-stress of 8 tons per square inch was, he considered, too high. He thought most designers would be pretty generous in their sections if they used 8 tons per square inch in short-span bridges. He did not know that there had been any proper experiments on riveted bars for range of stress; and, as a bridge consisted almost entirely of riveted members, it would be advantageous to have some such information. If the highest mean stress in the minimum section of a riveted member was 8 tons per square inch, the average stress over the whole member was lower, but owing to the unequal distribution of stress the maximum local stress round the rivets must be considerably higher. If it was supposed that, owing to some shock, the limit of elasticity was exceeded immediately round the rivets, a permanent set would take place. That would not result in damage so long as the strength of the member had not been too far exceeded; it would only cause a redistribution of stress around the rivet-holes, resulting in a certain amount of distortion, which might be sufficient to cause loose rivets. If those loose rivets were renewed and the new ones remained firm, that showed that the redistribution of stress had been beneficial and that the structure was sound; but if those rivets became loose again and again, it was time for drastic measures to be taken.

Professor Inglis.

Professor C. E. Inglis expressed his appreciation of the very kind words that Dr. Lowe-Brown had just uttered about his efforts. He did not feel that the term "cerebral resonance" applied to himself; from personal experience he realized that his brain was rather heavily "damped"! Sir Alfred Ewing had also referred to his work, and coming as they did from one whose opinion he valued very highly, he could not deny that he was pleased with those references. But, really, he had been one of a crew, every member of which was pulling his full weight; and Sir Alfred Ewing had presided over that rather heterogeneous collection in a very masterly manner. The Author had aptly described his Paper as a signpost, and he would endeavour to supplement what the Author had done in that direction by indicating one or two of the more salient points, particularly in that region where mathematical analysis was apt perhaps somewhat to obscure the vision of those

Professor Inglis.

who were not mathematically minded. The Committee could not claim to have been pioneers in this investigation. In the past, as Dr. Lowe-Brown had mentioned, a mass of experimental data had been accumulated, but in default of any underlying theory sufficiently comprehensive to account for all the phenomena, the results had been meagre and disappointing. The problems, like so many in engineering, called for close co-operation between mathematical analysis and practical experiment. Mathematics was required to indicate the lines along which the experiments ought to proceed, and experiment served to check the validity of conclusions, and prevented mathematics from running off the scent. His contribution to the Committee's labours had been chiefly in the direction of endeavouring to evolve a convenient and practical method of mathematical analysis and making it fit the facts; but without the guidance of experiment no measure of success could have been obtained. Without the guidance of experiment, a mathematician would almost certainly lose his way in the labyrinth of side-tracks that beset that line of research. Sooner or later he would inevitably idealize out of existence factors of the first order of importance, and would probably over-emphasize others which in practice were negligible. Bridges and locomotives, from the mathematical standpoint, were complicated structures, and to render them available for mathematical analysis some preliminary basis of idealization was necessary; but such idealization called for great care, and required to be checked by experiment. As a case in point he might mention that the researches of the Committee had reached an advanced stage before they had appreciated the important effect produced by spring-movement in locomotives in damping oscillations. Any mathematical theory that left spring-movement out of account, though it might be suitable for certain classes of bridges, would in other cases predict results having no connection with reality. The natural frequency of long-span bridges was low, and consequently large oscillations were developed only at low frequencies. Under such circumstances the accelerations associated with those oscillations were not high enough to overcome the friction in the springs of the locomotive, and the locomotive behaved as though it had no springs, the damping due to spring-friction being absent. The idealization necessary in long spans was accordingly comparatively straightforward. The locomotive could be treated simply as a concentrated mass which was subjected to a pulsating force representing the hammer-blow, and damping in the bridge had also to be taken into account.

[The speaker here showed a number of lantern-slides in order to indicate the degree of accuracy attainable by analytical methods. These slides were photographs of figures published in the Bridge Stress Committee's Report, in which they are numbered as indicated in brackets.] Professor Inglis.

The first slide gave deflection records of the Newark Dyke bridge (46), on a main line of the London and North Eastern system, which was taken as a convenient example. The diagrams represented the variation in the central deflection with reference to the horizontal base on which was recorded the position of the locomotive. The loaded frequency of the bridge in question was 2·4 periods per second, and when the locomotive passed over it at 2·4 revolutions per second, a considerable degree of resonance was obtained. The next slide (43) illustrated the dangers of over-idealization. It predicted what would happen on the Newark Dyke bridge if it was assumed that the frequency of the bridge did not alter according to the position of the locomotive. Under that assumption the oscillations mounted up to an exaggerated extent. When damping was taken into account, theory and experiment, for the particular speed of 2·4 revolutions per second in that bridge, were in close agreement, but agreement was not attained for any other speeds. To get agreement over a full range of speeds it was necessary to take into account the fact that the locomotive had mass, which altered the natural frequency of the bridge as the locomotive passed from one position to another. The taking account of mass greatly complicated the analysis, and made a problem, which otherwise might have been solved in a few hours, a question of a week's work. The next slide (45 and 46) showed the result of the working out of a succession of such problems. It illustrated both the actual records and the result of the full analysis, and on the whole the agreement was quite good. The maximum oscillation occurred for a frequency of about 2·4 periods per second, and both sets of records showed that as the speed was raised the oscillations became insignificant. The calculated residual oscillations were not as large as the actual residual oscillations obtained in the experiments; and he had discovered clear evidence that in the actual tests the locomotive had been accelerating and therefore maintaining a condition of resonance longer than theory allowed it to do. That was one of several cases where theory had shown that experiment had departed from the ideal. In many cases also experiments had suggested that data supplied by locomotive-engineers were incorrect; and, when it was a question whether the locomotive-engineers or the experimenters were right, he was bound to say that he thought the advantage of accuracy had been usually with

Professor Inglis.

the latter. The next slide (47), which also related to Newark Dyke bridge, was again a theoretical prediction, but it bore out what was found in practice, namely, that a very small amount of oscillation occurred with a load free from hammer-blow. In the case of four-cylinder or electric locomotives the oscillation set up in the bridge was so small as to be of no practical importance. The good agreement obtained between analytical predictions and actual results suggested the possibility of predicting oscillations, but, as he had indicated, the full theory was far too complicated to become an ordinary utility method. Engineers, however, did not require to know every undulation which occurred in a bridge oscillation. What they were interested in was the greatest oscillation set up by the passage of any particular locomotive. An evaluation of that greatest oscillation, which at any rate did not err on the side of being an under-statement, could be obtained in the following way. Let it be imagined that the locomotive was standing in the middle of the bridge, and that when the throttle was opened the locomotive started skidding its wheels. It would then, by means of the hammer-blows, set the bridge into a state of oscillation, and the oscillation set up was clearly somewhat greater than the oscillations produced by the same locomotive when crossing the bridge at the same wheel-speed. In spans of medium length the damping was usually so heavy that the one or two blows received by the bridge when the locomotive was in the centre developed practically the maximum oscillation the bridge would sustain even if the hammer-blow went on indefinitely. In long spans with comparatively small damping the locomotive could be regarded as being practically in a middle position for much longer, so that again the discrepancy was not great. That substitution of what might be called a skidding locomotive for a travelling locomotive, for predicting bridge oscillations, was important, and when the Committee adopted that device substantial progress was at last made in their ability to predict the maximum state of oscillation. The Author had not mentioned that most important fact—which seemed to be rather a serious omission, because it was the basis upon which all the Committee's conclusions had been founded, and their validity stood or fell very largely by acceptance of that principle. From all the tests that could be applied, it appeared that very little error was involved by substituting a skidding locomotive for a travelling one, and the errors were on the safe side in that they amounted to an over-statement of the stresses. The oscillations of bridges of moderate span, as he had mentioned, might be so violent that spring-friction was broken down, and under those

Professor Inglis.

circumstances the idealization of the moving load became more complicated. It was as shown in the next slide. The load was idealized into a sprung load and a non-sprung load, and it was necessary to investigate the strength, the damping, and the friction of the springs. Spring-movement exercised a profound influence on the amplitude–frequency diagram. On the following slide (50) was shown the amplitude–frequency curve for a 180-foot span. If the springs remained locked, the variation of amplitude on a frequency base would follow the full line on the diagram; when the acceleration of the sprung mass required a greater force than the spring friction could provide, and spring-movement occurred, the amplitude–frequency curve took the form shown in the broken line. Up to a value of slightly more than 2 periods per second the springs remained locked, but from that point onwards spring-movement began, and went on for frequencies up to about 2·5 periods per second. Spring friction absorbed a large amount of energy. For frequencies between 2·5 and 5 periods per second the springs were again locked. At frequencies exceeding 5 periods per second the sprung portion of the locomotive began to move in anti-phase to the movements of the girder, and in a sense that reduced the mass-effect of the bridge and raised its natural frequency. Consequently a high resonance peak occurred. For spans longer than 150 feet the second peak usually occurred at such a high frequency as to be of no practical interest. For a span of 120 feet there was no intermediate stage during which the springs remained locked (51), and the second resonance peak was important. The next diagram (52) recorded a number of amplitude–frequency observations taken on the Langport East bridge, of 112 feet span, together with the predicted amplitude–frequency curve. Though the simplification introduced by substituting a skidding locomotive for a moving locomotive was helpful, yet it did not reduce the problem to a very simple form; for there remained three bridge characteristics and five locomotive characteristics, and it was, under those circumstances, impossible to evolve a simple formula. The nearest approach to a formula of universal application was the dynamic-magnifier formula for k. That had served a useful purpose with regard to the prediction of the two resonance peaks, but it was of no practical utility. At an early stage he had abandoned the use of that formula for getting out actual results, the reason being that the formula was based on the assumption that the damping in the spring-movement of a locomotive was due to fluid friction, whereas actually the damping was caused by solid friction. A certain constant represented the damping in the springs of the locomotive, but that constant

Professor Inglis.

could not be determined until the amplitude of the oscillation was known, and the amplitude of the oscillation could not be determined until the effect of the springs was forthcoming. By a process of trial and error it was possible to arrive at a result, but the application of the process was very laborious. The method eventually found successful was that outlined in Appendix J of the Report. It was not open to the objection he had just mentioned. The problem could be tackled in a way which was straightforward and, he thought, not very laborious or difficult to understand. Appendix J really took the place of the simple formula which, no doubt, many engineers had been hoping that the Committee would produce, but which was denied to them owing to the inherent complexity of the problem. Certainly tables for the benefit of railway engineers of this country had been produced, based upon different types of locomotives and types of bridges of which practical experience had been gained. But to extend the conclusions to the bridges and locomotives of other countries it was necessary to study Appendix J. He was sure that the Committee did not claim to have reached finality in the subject. It hoped, however, that it had taken the problem a real step forward, and removed it from the region of guesswork. He was inclined to think that mathematical analysis would not afford much further help, but there was room for more experimental work. In particular, more information was required concerning that somewhat elusive but highly important factor, spring friction damping.

Professor Dalby.

Professor W. E. Dalby remarked that the Author had given the warning that the actual values of the equivalent uniformly-distributed loading recommended in the Tables of the Report were suitable only for railways where the conditions were similar to those in this country. That meant that for other conditions the values would have to be recalculated. Moreover, the data on which the calculations must be based to follow the method of the Report would be lacking, and not easy to obtain. Without those complete data the mathematical method developed by Professor Inglis in Appendix J of the Report could not be applied; but the Report enabled limits to be calculated between which Professor Inglis's solution would probably lie. The curve showing the dynamic magnifier plotted against the span (*Fig. 10*) embodied practical data which might be widely applied to new conditions, failing data to apply Professor Inglis's method. The values of k plotted were based on the records of the largest deflections observed in the tests under the action of locomotives giving the heaviest hammer-blows. The value of k might therefore be taken to correspond with conditions

Professor Dalby.

of synchronism. Supposing a designer to be at work on a bridge of 240 feet span, a reference to the curve would enable him to say that the deflection against which he was to provide would not exceed $12\frac{3}{4}$ times the deflection caused by the hammer-blow applied statically at the centre. The Report would also enable him to get a good idea of the maximum value of the hammer-blow $E = cn^2$, where c was a constant depending upon the type of locomotive and n was the number of revolutions of the driving-wheels per second. The Committee had found that $c = 0{\cdot}6$ covered all the two-cylinder types of locomotives, and a value of 0·2 was suitable for the four-cylinder types. The deflection caused by the static application of the load E would depend upon the stiffness of the bridge. The equivalent uniformly-distributed load for hammer-blow might therefore be arrived at. [The speaker here gave the mathematical explanation printed below.[1]] The expression (equation 3) for the equivalent uniformly-distributed load would serve for calculating the impact allowance to a sufficiently accurate approximation, k being read off *Fig. 10* for the given span, and c being a constant depending upon the type and number of locomotives. n was the natural frequency of the loaded bridge and equally the revolutions of the driving-wheels per second, since the value of k applied only to synchronous conditions. By way of example he would calculate the equivalent uniformly-distributed loading to allow for hammer-blow for a bridge 240 feet in span. *Fig. 10* showed that for that

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[1] Let y denote the maximum deflection of the girder at the centre due to the hammer blow $E = cn^2$; and let y_1 denote the deflection due to this hammer blow applied statically. Then by definition

$$y = y_1 k \qquad . \ . \ . \ . \ . \ . \ . \ . \ . \ (1)$$

Since the oscillations are small, the deflection y_1 due to the static application of E is proportional to E, that is $E = \mu y_1$ where μ denotes the force which, applied steadily at the centre of the girder, deflects it 1 foot. Thus $y_1 = E/\mu = cn^2/\mu$.

Substituting this value of y_1, equation (1) becomes

$$y = kcn^2/\mu \qquad . \ . \ . \ . \ . \ . \ . \ . \ (2)$$

Again, the deflection due to a uniformly distributed load is proportional to the load for small deflections, so that $\text{E.U.D.L.} = \mu_1 y$ where μ_1 is the uniformly distributed load which deflects the girder 1 foot at the centre. Thus $y = \text{E.U.D.L.}/\mu_1$. It is easily shown that with the assumption of a sine deflection curve $\mu_1/\mu = \pi/2$.

Substituting this value of y in equation (2), and eliminating μ_1/μ

$$\text{E.U.D.L.} = (\pi/2)\, kcn^2 \qquad . \ . \ . \ . \ . \ . \ (3)$$

Professor Dalby. span $k = 12{\cdot}75$. A single-line bridge of medium mass and high frequency, and with a British Standard 15-unit loading, would have a frequency of about 2·35 oscillations per second. The two engines of the Standard loading would apply hammer-blows, assumed to be in phase, equivalent to $1{\cdot}2\ n^2$ at the centre. The constant 1·2 was found from the constant 0·6 for each engine by a method of reduction which he would explain, a hammer-blow cn^2 applied at a_1 feet from a support being equivalent to a hammer-blow $cn^2 \sin(\pi/L)\, a_1$ applied at the centre. Then E.U.D.L. $= (\pi/2) \times 12{\cdot}75 \times 1{\cdot}2 \times (2{\cdot}35)^2 = 124$ tons, which was equivalent to 0·517 ton per linear foot. [To show that the form of the expression (3)—which was derived from a curve plotted from experimental results—was rational, the speaker gave here the mathematical treatment (printed below[1]) of a vibrating system which he had previously

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[1] A beam of negligible mass (*Fig. 15*), freely supported at its ends, and free to vibrate in the vertical plane only, carried a mass M concentrated at the centre. The mass M was supposed to be an engine, self-contained, applying to the beam a vertical pulsating force $E \cos pt$. The system was equivalent to a locomotive of mass M chocked up at the centre of a bridge and acting as an oscillator. The problem examined was to find the maximum amplitude of the oscillation produced by the pulsating force $E \cos pt$. The deflection y, that is the semi-amplitude of the oscillation, was shown to be given by the expression

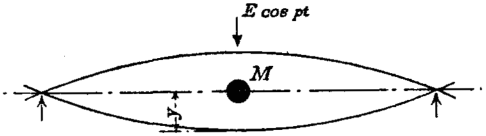
$$y = E\cos(pt - \epsilon)/M\sqrt{\{(q^2 - p^2)^2 + b^2p^2\}} \quad . \quad . \quad . \quad . \quad (4)$$

where p was 2π times the frequency of the applied force, so that if n_p denoted the revolutions of the engine shaft per second, $p = 2\pi n_p$; q was 2π times the natural frequency of the loaded system and equal to $2\pi n_q$; E the maximum value of the applied force, M the central mass, and b the damping coefficient $= \delta/M$, where δ was the force resisting motion when the speed was 1 foot per second. The deflection y was a maximum when p was almost equal to q and for all practical purposes maximum y might be taken as corresponding to $p = q$, the condition of synchronism. In those conditions

$$y = E/Mqb \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The maximum amplitude y was limited only by the value of the damping-coefficient. If b was halved y was doubled.

Fig. 15.



Professor Dalby.

examined ("Balancing of Engines," p. 201), and also demonstrated the action of a model bridge supporting a pulsating load.] The major difficulty of applying equations (4) and (5) to practical problems was the difficulty of choosing a value of b. [The speaker then transformed these equations as below[1] to include the dynamic magnifier k as defined by the Committee.] The investigation he had described applied to the system of the central mass and the central force and the massless beam of stiffness μ. With the assumptions on which the mathematical treatment given in the Report had been developed, a loaded railway-bridge acted upon by hammer-blows from locomotives anywhere on it could be reduced to a central system like that of *Fig. 15*. Those assumptions were: (1) the maximum semi-amplitude of oscillations produced by the passage of a locomotive would not exceed the oscillation produced by the locomotive chocked up at the centre and acting as an oscillator; (2) the girder was of uniform mass per linear foot and the EI value of its cross section was constant; (3) the curve into which the girder bent during an oscillation was a sine curve in relation to the mean position during an oscillation, from which it followed that the semi-amplitude of oscillation was known for every point along the girder if it was given at any one point; and (4) the centre of the girder oscillated in simple harmonic motion. With those assumptions it was easily shown that: (*a*) the dynamical equivalent of an oscillating girder

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[1] Multiply the numerator and denominator of equation (4) by q^2. Then $y = Eq^2/Mq^2 \surd \{(q^2 - p^2)^2 + b^2p^2\}$. But for harmonic oscillations $Mq^2 = \mu$, where μ is the force at the centre that deflects the beam 1 foot. Therefore E/Mq^2 is the deflection y_1 caused by E applied statically. Thus

$$y/y_1 = k = q^2/\surd\{(q^2 - p^2)^2 + b^2p^2\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The right-hand term, therefore, is the theoretical form of the dynamic magnifier in terms of p, q and b. The value of k plotted in *Fig. 10* (p. 70) corresponds to conditions of synchronism. In those conditions $p = q$, so that the theoretical form of k for synchronism is

$$k = q/b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Thus at synchronism the damping coefficient b varies inversely as the dynamic magnifier k at constant frequency. If q/k is substituted for b in equation (5),

$$y = Ek/Mq^2 = Ek/\mu = kcn^2/\mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

This agrees with the expression for y derived directly from the definition of the dynamic magnifier, so that the expression (3) is rational. The value of k for non-synchronous conditions is given by equation (6).

Professor Dalby. of uniform mass per linear foot and constant EI value was a massless girder of the same span and stiffness carrying at its centre a mass equal to half the mass of the girder; (*b*) the mass at the centre equivalent to a mass m_1 distant a_1 from the left support was $m_1 \sin^2 (\pi/L)\, a_1$; and (*c*) the force at the centre equivalent to a force E_1 applied to the girder at a point distant a_1 from the left support was $E_1 \sin (\pi/L)\, a_1$. The general expression for the deflection then became (all the hammer-blows being in phase):

$$y = \frac{n^2 \{c_1 \sin (\pi/L)\, a_1 + c_2 \sin (\pi/L)\, a_2 \ \ . \ \ .\} \ (\cos pt - \epsilon)}{\{\tfrac{1}{2}m_G + m_1 \sin^2 (\pi/L)\, a_1 \ \ . \ \ . \ \ . \ \ .\} \sqrt{\{(q^2 - p^2)^2 + b^2p^2\}}}$$

The value of μ for the girder was not changed by the transfer of the masses and forces to the centre, as it depended wholly on the stiffness of the girder. It was easy to devise a schedule to find the numerical value of the hammer-blow at the centre, which appeared as cn^2, where c represented the sum in brackets. Also the equivalent central mass, M, was the sum in brackets. Thus any loading and any system of forces in phase could be reduced to the equivalent dynamical central system of *Fig. 15*. The first step after finding the equivalent dynamical system was to calculate its natural frequency from the frequency (usually given) of the unloaded girder. If this was denoted by n_q and if n_j was the frequency of the loaded girder, then $\frac{1}{2}m_G q^2 = Mj^2 =$ a constant $= \mu$. Therefore $j = q \sqrt{\frac{1}{2}m_G/M}$, and $n_j = 2\ \pi^{i}$. Then all the factors of the expression for the equivalent uniformly-distributed load were known, and E.U.D.L. $= (\pi/2)\, cn_j{}^2k$. The whole of the preceding investigation had gone on the assumption that the locomotive as a whole oscillated with the bridge, the springs being locked and inoperative. Experiment showed that this assumption was substantially true for long bridges where synchronism occurred at relatively low speeds. But on shorter bridges whose frequency was high, considerable energy was involved in the oscillation of the locomotive at the high synchronizing speed. The transfer of this from the girder to the locomotive mass through the springs was sufficient to unlock them and set them in action. The problem then was to find y with the engine-springs in action. That was the problem of which Professor Inglis had offered the solution in Appendix J of the Report. *Fig. 7* in the Paper (p. 62) showed synchronism at a high value of n with unlocked springs. The difficulty of applying the method still lay in the uncertainty about the values of the damping coefficient and, in addition, in the uncertain data about the natural frequency of the locomotive on its springs, and the damping action of the springs, all of which now entered the problem. An approximate method

Professor Dalby.

Fig. 16.

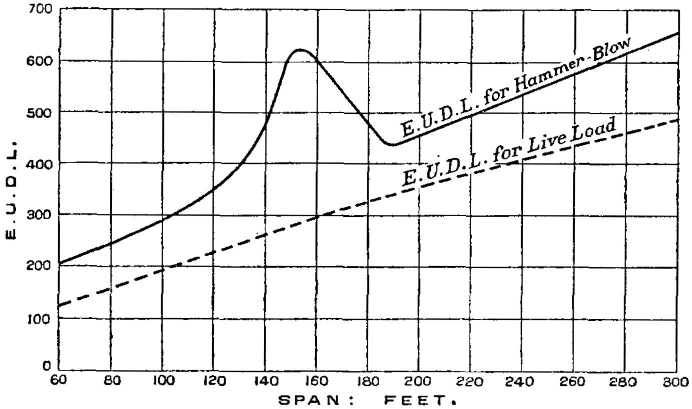


Fig. 17.

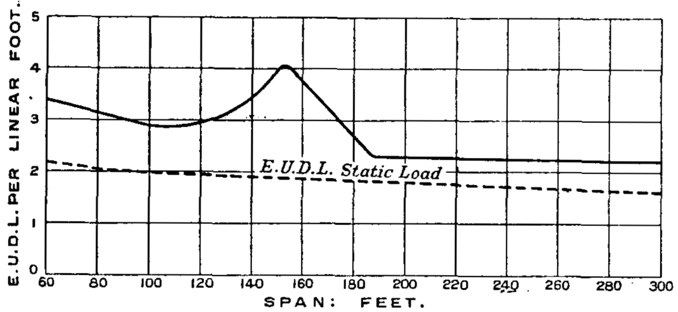
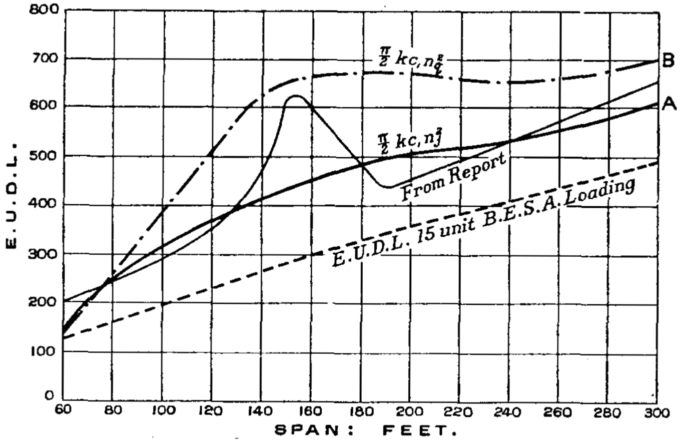


Fig. 18



Professor Dalby. might be used to give a workable value of the equivalent uniformly-distributed loading to allow for spring-action in bridges of high natural frequency where it was expected that the energy of the oscillation would be large enough to bring the springs into effective action. In the absence of data the springs might be regarded as absorbing energy and so reducing the energy transferred through them at each oscillation. The total effect on the maximum value of y would be as though the spring-borne mass was largely reduced in so far as its dynamic action was concerned. The result of reducing the mass on the girder was to increase the synchronizing speed. Spring-action might therefore be met and allowed for by substracting mass from the central oscillating mass M, and so calculating the frequency of synchronism from a reduced central mass. In fact, if the equivalent uniformly-distributed loading was calculated from the frequency not of the loaded girder but of the unloaded girder, a value would be found that was outside the value required for the effect of unlocked or freely-acting springs. In order to test the approximate method just described, he had applied it to the range of bridges postulated in the Report of medium mass and of high frequency. He had assumed the bridges to be loaded with 15-unit British Standard loading and had assumed the hammer-blow from each of the two locomotives to be given by $E = 0{\cdot}6\,n^2$. These assumptions brought his results in comparison with those shown in diagram 2 of Fig. 72 in the Report. *Fig. 16* showed that diagram; *Fig. 17* showed the curve of total equivalent uniformly-distributed loading; and *Fig. 18* showed in curve A the allowance calculated by the method he had described, and in curve B the same method using the natural frequency of the unloaded girder. The curve given in the Report was also included. The comparison showed that an approximate value of the impact allowance for hammer-blow could be obtained easily and quickly in the new conditions not covered by the Report, if the curve for the dynamic magnifier was relied upon. It gave the best data available about damping, because it recorded observations in the field upon a wide range of bridges. The agreement would be closer with bridges of lower frequency.

Mr. Wilson. Mr. J. S. WILSON remarked that there were parts of the Report which were not very clear; but he congratulated the Author on his Paper, which was a concise and admirable guide to the Report. He envied him the opportunity he had had of working with so distinguished a committee, and congratulated him on the very important work he had carried out as Chief Engineer to it. Although it might seem presumptuous on anyone's part to criticize the Report, Mr. Wilson could not help reading it with a critical eye, for he had studied the problem of bridge-design for many years. He had

Mr. Wilson. examined bridges, read reports, compared his own experience with that of others, and followed all the experimental results published on the subject[1]; and in every direction he had found that more and more full-size experiments were required. He had believed that, if only the opportunity could be afforded of carrying out such experiments, the solutions of some of the riddles would be reached. He had contemplated experiments far more ambitious even than those actually carried out by the Committee. When, therefore, he heard that the Committee, backed by the Government and the railway-companies, had been set up, he felt that the experiments would at last be made, and in the circumstances it might be excusable if he, and he thought many others, had felt a little disappointed that the solutions of some of the problems were going to be arrived at over their heads. He had waited anxiously for the Report, and had perhaps expected too much, notwithstanding the large amount of valuable matter contained in it. The Report was a joint effort, and, as was usual with such documents, would be apt to misrepresent the views of individual members of the Committee; and he thought that the Report did contain some unexpected statements. In the first paragraph it was stated that "the terms of reference . . . have to be interpreted with due regard to the condition of engineering knowledge and practice prior to our (the Committee's) appointment." A statement made by a previous speaker in the discussion had rather suggested that bridge-designers had been groping in the dark up to the publication of the Report; and that opening statement, whether referring to engineering knowledge generally or bridge engineering only, did rather suggest a condition of abysmal darkness before the Committee was set up. He did not think that was really meant, but it was rather unfair to experienced engineers who had worked at all the problems for years and had designed bridges which had served their purpose satisfactorily and had been of economical construction. Paragraph 14 of the Report raised hopes by stating that the aim of the Committee had been not only to clear up doubtful points with regard to stress, but also, on the basis of such conclusions, to formulate rules suitable for practical service in the design of new bridges. Those hopes were, however, dashed by paragraph 186 and by the Author's statement (p. 49):—

> The nature of the stresses in various parts of the bridge, when loaded by any given weight, may be the subject of another inquiry. Yet another avenue of research may lead to the solution of the equally important problem of the ultimate strength of bridges.

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[1] See British Association Report, 1923, "Stresses in Bridges," p. 368.

Mr. Wilson.

> Until all these goals have been reached, the factor of safety in every bridge under all types of engines cannot be precisely determined.
>
> All these problems awaited solution when the Bridge Stress Committee was formed; one has been seriously tackled—with results that are now under consideration by engineers. Some data are also given regarding the ratio between calculated and measured stresses in girders caused by engines passing at low speed and causing no impact effects. Although it is evident that rigidity of a riveted structure has the effect of relieving certain members of stress by transferring it to others, in cases much to the advantage of the bridge, the data available were not sufficient for the issue of any rule or recommendation.

A statement in the Report (paragraph 15) with regard to fatigue rather astonished him, namely: "There is no reason to suppose that fatigue becomes operative under the conditions of stress which actually apply in bridges; and there appears to be no experience of failures in steel or wrought iron bridges traceable to this cause." When he read that he felt inclined to ask what else but fatigue did cause failure in a bridge. The only alternative would result in failure under test loading, when either the top boom would crumble up or the bottom boom pull out; but that could only result from some mistake in design. Generally, failure, or the indication of it, came on gradually. A well-authenticated case was the Embabeh bridge in Egypt, investigated by the late Mr. F. E. Robertson, M. Inst. C.E.[1] That bridge was erected in 1892, and serious signs of failure were noticed in 1896. It was tested in 1892 on completion, but only after it had carried traffic for 4 years were serious defects revealed. The late Sir Benjamin Baker had given several instances of failure which he attributed to the effect of the repeated application of load. Mr. Wilson thought that members of the Committee might have meant something different from what he understood to be the meaning of the paragraph referred to. In the Report the word "impact" was, very properly, defined, but he had never understood the word to have the restricted meaning given in that definition. The Report confined the term to the effect on a bridge of the vertical component of the unbalanced revolving weight of the locomotive. Imperfections in the rails, and lurching and other effects, were treated as separate items in the loading to be provided for. That was unquestionably the more scientific way to treat these factors, but although they had not been so treated in the past they had, of course, been recognized, and he had always understood impact to include all those factors; just as the range of stress formulas and the graduated-stress specification of Sir Benjamin

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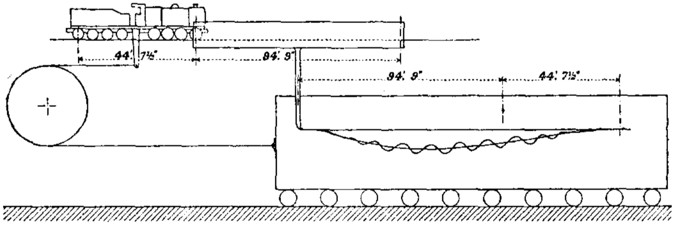
[1] Minutes of Proceedings Inst. C.E., vol. cxxxv (1899), p. 206.

Mr. Wilson.

Baker had all made provision for these defined and undefined effects. That the want of balance in locomotives had been appreciated was illustrated by the instance given in the discussion on Mr. Farr's Paper[1] of an engine which ran away down-hill at high speed and fractured the rails at intervals equal to the circumference of the driving-wheels. He wished to ask one or two questions about the Report. In some of the diagrams giving stress-records a scale of stress was shown, and he wished to know whether the scale could be applied to the record of tension as well as that of compression. Some years ago he had shown[2] by some measurements of strains he had made on a loaded perforated plate, that in deducing the stress, unless a properly adjusted value of E was used, stresses as much as 16 per cent. too high would be obtained. He assumed that the tension records had been adjusted. Regarding the deflection and stress records, he wished to know whether in each case they represented strains or movements caused by the locomotive passing from left to right, and whether a record of deflection might be visualized as being drawn in the manner represented in *Fig. 19*, the card on which the record was drawn by the point fixed to the girder moving from right to left, so that the residual vibration or deflection produced by the train following the engine appeared at the right-hand end of the record. The deflection diagrams, *Figs. 3* and *4*, gave the impression of representing the deflected shape of the girder, but they, of course, represented the curves that would be drawn under the conditions shown in *Fig. 19*, and did not relate to the shape of the girder when deflected. The diagrams in Fig. 54 of the Report, showing the effect of a rail-joint and marking the points at which successive wheels passed over the joint, were not very clear. The wheels were shown with the front of the engine to the left, but the direction of motion was presumably from left to right. The diagram *Fig. 19*, if correct, made it easier to understand this apparent

Fig. 19.



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[1] Minutes of Proceedings Inst. C.E., vol. cxli (1900), p. 100.

[2] *Engineering*, vol. 116 (1923), p. 447.

Mr. Wilson. reversal of direction. In Professor Dalby's Appendix to the Report there was a constant of 1·22, the derivation of which was not explained, though no doubt it represented $4\,\pi^2/g$. On p. 165 of the Report what was apparently intended to be the same figure was given as 2·12. [Professor DALBY explained that the figure 1·22 was the value of $4\pi^2/g$, and 2·12 was a misprint.] Mr. Wilson hoped that some of the more ambitious experiments might still be carried out. For example, instead of scrapping " locomotive K," it should be preserved; and, with the champion bridge which gave 159 per cent. impact in Colonel Mount's tests, and some others, set out in a ring, with the locomotive going over them in succession, extremely valuable experimental results relating to fatigue combined with impact could be obtained. He trusted that the Author's suggestion that the further problems should be tackled would be acted upon, and that the Committee, with the Author's co-operation, would be entrusted with the work.

Sir Clement Hindley. Sir CLEMENT HINDLEY remarked that he had two reasons for intervening in the discussion. The first was that there was something in *Fig. 1* which seemed to call for an explanation from him. The second was that he was very glad to have the opportunity of congratulating the Committee on their work. He could, he thought, say on behalf of all railway-engineers in India that the Committee's work would be of very great value to India in the future. Practical engineers, finding themselves in difficulties in regard to what might be called a " sickness " of their bridges, had called in the specialists, and the specialists after 5 years' diagnosis had done what medical specialists very often did, namely, they had prescribed death and burial—for the locomotive. Bridge-engineers would be glad to find that the specialists, having started to investigate that bridge-sickness, had found that it really was a case of locomotive-sickness, and they had indicated a way out and a method of treatment. He felt, however, that they had been a very long time about it. He was not referring to the 5 years the Committee had been at work, but the 100 odd years since locomotives first began knocking bridges about. The thousands of tons of steel put into bridges because of the few hundreds of pounds of metal put into locomotives, perhaps in the wrong places, was a matter for bridge-engineers and railway-engineers generally to consider, especially as the additional steel in the bridges represented millions of pounds when translated into money. He wished that engineers had had something similar to preventive medicine instead of having to call in specialists when the disease was so far advanced. It was not sound policy for bridge-engineers and locomotive-engineers to have been for so many years working more or less at cross purposes. Bridge-

Sir Clement Hindley.

engineers had had to use what was practically a very big factor of safety in employing the old Pencoyd formula, mainly because there had never really been very close collaboration between those who were designing locomotives, and attempting to balance them, and those who were designing bridges. That was not in any way a criticism of the present Committee's work, because they had been given a practical problem to investigate, and they had done so. It was a criticism of the methods by which engineering practice grew up. The matter had been left to grow until it involved millions of pounds before the specialists were called in to put it right. The demonstration that Professor Dalby had given during the discussion reminded him of the suspension bridge that crossed the pond in St. James's Park. If two small boys stood near the middle of that bridge and jumped up and down in the same way as Professor Dalby's little model had done, exactly the same effect would be produced; and what he imagined to be a dangerous oscillation could be set up. The difference between that experiment and Professor Dalby's experiment was that the latter was quantitative and therefore of great scientific value. The most striking feature of the Report to the ordinary engineer's mind was the idea of the synchronization effect of the hammer-blows on the bridge structure. He could not help feeling that it had taken engineers a very long time to abandon the old mysterious ideas they had had about impact for this very plain and simple matter of synchronization in bridge structures. The line in *Fig. 1* called "Indian Railway Bridge Sub-Committee, 1925," had rather an interesting history. For many years the Pencoyd formula had held sway in India in all bridge-designs. About 1912 a difference of opinion arose between the managements of certain railway-companies in India and the Government of India on the score that the requirements of the Government's inspectors of railways in regard to the design of bridges, particularly for renewals of old bridges, were extravagant. A financial conference which was sitting in India in 1912 recommended that the Government of India should take steps to investigate the problem and endeavour to make bridge-renewals less expensive than they had been before. That had set on foot an inquiry by expert railway-engineers in India, which had lasted unfortunately a good many years. The war had intervened, and the work had been rather put in the background; but much valuable experimental work had been done in India by the use of the Fereday-Palmer extensometer and other instruments allied to it. In India there were many bridges very suitable for that kind of experimental work. In addition, an engineer had been sent to America and he had collected valuable data from work which had been done there. By

Sir Clement Hindley. about 1920 or 1921 a satisfactory volume of material had been collected for analysis, and experiments with stress-recorders were in progress in various parts of India on a fairly well co-ordinated plan. Unfortunately, owing to mistaken motives of economy, his predecessors on the Railway Board stopped that work in 1921, distributed the instruments, and gave up any attempt to co-ordinate the work any further. That had been to some extent a disaster, because the data had been ready for careful analysis. However, in 1923, shortly after he had taken charge of the Railway Board, he was able to get together a small sub-committee to take up that work again. They had not been able to do very much more in the way of experimenting, but they had made a very careful analysis of the data. The problem with which they had been then faced was a very much bigger one than it had been in 1912, because after the war there was an accumulation of bridge-renewal work. There had been progress in increasing axle-loads, and there had been many changes in locomotive-design. So a very large programme of bridge-renewals and bridge-strengthening had been formulated, and at the same time a fairly large construction programme—which meant the building, in some cases, of major bridges—had been undertaken. Having regard to the facts that some of the bridges were intended to last for 40 or 50 years, that the traffic would increase, and that the axle-loads would become heavier, the financial side of the programme had had to be considered very carefully. Therefore he had rather forced the pace with that investigation into bridge-stresses. The sub-committee had recommended that the formula shown in *Fig. 1* should be adopted. That formula was a considerable advance on the Pencoyd formula, and therefore on any practical formula that was being worked to at that time. He might say that in some other ways they had also been in advance of current practice elsewhere, because, while the bridge sub-committee was sitting, he established committees for standardizing locomotives and rolling stock, and had therefore been able to co-ordinate the whole work in the same building. Very valuable results had accrued from that close collaboration. At the moment when they had practically made up their minds to adopt that formula for their new rules for the design and the inspection of bridges, they had been strongly advised to hold their hands and wait until Sir Alfred Ewing's Committee had made its report. It had appeared at that time fairly certain that that Committee would find a real scientific solution of the problem, which might lead to the adoption of greater refinement in design than was possible with the rather rough-and-ready formula evolved by the sub-committee. It had been realized, however, that to wait for

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the Report of the Bridge Stress Committee would mean a delay of 2 or 3 years, and, when a programme involving many million pounds was in question, such a delay could not be faced. The programme had been formulated and the money had been provided; and when money was provided by a legislative body and guaranteed for a certain period, it was well to spend it as soon as possible, because one never quite knew when that legislative body might change its mind. He was afraid that at one time he had been practically alone in desiring to adopt that formula and to go ahead with the new Bridge Rules; but force of circumstances had made others incline to his opinion. That formula had now been in use for some time in all Indian bridge designs. It allowed much greater economies than did the old Pencoyd formula, and it had enabled Indian engineers to maintain in use bridges which would otherwise have been condemned, and to strengthen bridges more easily than had been thought possible before; and in their designs for new construction they had saved many thousand pounds by adopting that wider formula; and they knew from experimental work with extensometers and stress-recorders that the formula was within the limits of safety. The problem that faced any practical engineer who was engaged in bridge-designing was, how far was it worth while to go to the greater refinement which Sir Alfred Ewing and his colleagues had shown to be possible? A certain weight of steel would undoubtedly be saved, but there were occasions when the saving of time and the expeditious spending of money were more valuable. He wanted to pay a tribute to the value of the results which had been obtained in practice from the use of the Fereday-Palmer stress-recorder, which was a valuable adjunct to all the work on bridges in India. The fact that every major bridge that had to be strengthened or renewed was carefully tested by means of those instruments gave a much greater guarantee of safety and economy than could ever be got by having the stresses worked out from diagrams. He had to confess that he had not read the Report of the Committee, but he understood from the Paper that the Committee had been at great pains to work out practical tables for bridge conditions and locomotive conditions in this country. He hoped it would not be forgotten that there were many other parts of the world looking to the Report for guidance, and that the problem of railway-bridges in Great Britain was really a small fraction of the great problem which British engineers had to tackle, not only in the British Empire but also outside it, and he hoped that the work resulting from the Committee's efforts would not be too insular in its outlook. The particular problems of

Sir Clement Hindley. British railway-bridges and of British railway-locomotives were, he thought, largely of interest to this country alone, and it was worth while to make it clearly understood that engineers who were building bridges in other parts of the world would not be helped very much by the tables that had been worked out—unless, of course, they were unfortunately tied to following slavishly British locomotive practice. He did not wish to be thought to be attacking British practice; but it had been found necessary in India to specify a complete set of varied types of engines suitable for Indian conditions, because India had been for so many years dependent on the types of engines sent out from this country. Some time ago they had initiated experiments with four-cylinder engines. He felt that he had rushed into the subject in the proverbial manner of those who did not hesitate to go where angels feared to tread; but the fool, after all, sometimes had to do that, otherwise the angels would not get a fair chance of criticizing. He professed to feel that he was perhaps in better company as an engineering fool than a mathematical or scientific angel.

Mr. Ellson Mr. GEORGE ELLSON observed that, for the benefit of those who had not had time to study the Report in detail, it would not be out of place to go over one or two of the principal points. The first and most important result had been the proving by the deflectometer and the stress-recorder of the theory outlined in Professor Inglis's Paper. He did not think that when the Committee started their work they clearly appreciated the full effects of vibration on the stresses in bridges. They had known that the vibration of bridges was an important factor in the stresses produced by what was called impact, and the first year or two's experiments had proved Professor Inglis's theory in a very striking manner. The actual test records shown on the screen by the Author, which had been further illustrated by the demonstration given by Professor Dalby, had been found to agree very remarkably with that theory. The next step had concerned the locomotive-engineers. The magnitude of the hammer-blow of locomotives had a profound effect on the stresses produced in bridges. The locomotive-engineers investigated the hammer-blows of the various engines; those hammer-blows had been tabulated, and the locomotive-engineers had been able, with that information, to lay down what might be looked upon as the limit of the hammer-blow for future construction. It was also possible to correct the worst offenders among existing engines. They had, further, laid down what might be looked upon as the maximum axle-loads to be provided for in this country for the next few years. The next

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step had been for the bridge-engineers to consider the number of locomotives that could be got on to any one bridge within the limits of reasonable probability. Professor Inglis had dwelt on the importance of the damping effects of spring-friction of engines, and had emphasized the bearing that had on the problem. It was due to Professor Inglis to say that in that respect he had made a very great advance on previous work. Although the tables given in the Report were based on engines with hammer-blows up to 15 tons at 5 revolutions per second, it was not suggested that those values should be used for all bridges or all lines. There might be lines where no such engines would ever pass. Just in the same way, it was not suggested that the high speed of 6 revolutions per second should also be applied to all railways. There were lengths of line for which one need prescribe neither the maximum speed nor the maximum hammer-blow provided for by the Committee. The information now available gave the opportunity of making alternative allowances to suit the actual conditions, and economy could be obtained thereby in designing new bridges. Under the old rules no such economy had been possible; the equivalent distributed live load was taken, and an allowance was made for impact, and it was universally applied. The present work was the first step towards clearing up a number of important problems in bridge-engineering. The Committee had been criticized for not doing this, that, and the other; but the work they had done had been slow, arduous, and expensive. The Committee were fully aware of many other avenues which called for exploration, but unfortunately they had neither the time nor the funds to extend their researches. For instance, the question of distribution of stresses in bridge-floors would repay further investigation. Then there was the problem of arch bridges, which had not even been touched upon in the recent experiments. Exactly what happened in an arch bridge—and particularly stone or brickwork arches—was very obscure. Brick or stone arches had carried railways for many years, and they had been subject to ever-increasing speeds and loads. Now signs of fatigue were appearing. Fortunately such bridges did give warning. Signs of movement were seen, and therein lay the safeguard in regard to that class of bridge. For the first time bridge-engineers had a positive and rational basis on which to work in designing their structures. The main aim of the Committee had been to get something which could be applied in an ordinary drawing-office, and he thought that that result had been reached. Before the Report was available, the diversity of practice had been very wide, as was shown by *Fig. 1*. For that result alone the Committee's

Mr. Ellson. work had been well worth while. The co-operation of the locomotive-engineers was extremely valuable in regard not only to bridges but also to permanent way. The results were considerably more valuable with regard to permanent way than to bridges, because the amount spent upon bridges was but 5 per cent. of that spent on permanent way.

Sir Henry Fowler. Sir HENRY FOWLER remarked that he felt that the whole of the weight of the locomotive, both static and dynamic, was pressing upon him that evening, and he regretted that the weight was not shared by some of his locomotive colleagues. The Author had stated that, in view of the kindness of locomotive-engineers in assisting the Committee, he would not indulge in harsh criticism. But he must say that he thought that whatever criticism could be directed against the locomotive-engineer was equally applicable to the bridge-engineer. Sir Clement Hindley had referred to the fact that it was nearly a century since the hammer-blow problem was first considered. Whatever the exact period was, he knew that in the forties attention was called to it, and the locomotive-engineers began at once to give it some attention. The difficulty had been, however, that there had not been that co-operation in times past which he felt that the present Report was going to bring about in an increasing degree. A story would explain his point. A bridge-engineer had said to his colleague who looked after the locomotives, "I am sending a man across to see the locomotives weighed." Obviously he did not believe the figures given to him. The mechanical engineer replied, "I shall be delighted to see him. At the same time I am sending one of my draughtsmen, who is a mathematician, to see how you calculate the stresses on your bridges." That was not at all the way in which the matter should be handled, and he felt that the co-operation shown during the work of the Committee would undoubtedly help materially in the future. Certainly the locomotive-engineers would welcome co-operation. In the past, as far as he knew, they had never been asked to co-operate. They had only been asked casually what the hammer-blow of the locomotive was; and what they had been told was that 20 tons was the maximum load allowed on an axle. The curves that had been given by the Railway Engineers' Association were based solely on the static weight. Locomotive-engineers would like, were it possible, to have a curve which would combine the static-load and hammer-blow effects in order that they might work to it. In times past their great trouble in the design of a locomotive had been to make it come within the allowable limits of the curve with which they had been supplied—which was known as the 1908

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curve—especially on spans between 40 and 80 feet. He had asked some of his bridge-engineer friends why they said that certain locomotives were rough on bridges; what it was in the bridge that caused the bridge, not to fail—because no bridge ever did fail—but to show signs of distress; and what made them decide to scrap a bridge, or reduce the speed, or stop certain locomotives from passing over it. It seemed to him that there were two troubles, to one of which Mr. Wilson had referred, namely, the question of fatigue. He would like to have more information with regard to that, if it was a cause. The other was the question of corrosion. He had heard that sometimes bridges had had to be renewed on that account. The locomotive could not be wholly blamed for that, although one did appreciate the fact that stresses affected corrosion to a large extent in very many structures. Locomotive-engineers had been told that in order to reduce the hammer-blow they should use alloy steels. He trusted the bridge-engineers were also going to use alloy steels, which, he believed, would help them in regard to the factors which made a bridge require renewal or attention. There was at present a great movement with regard to supplying steels which he felt would, for each of those purposes, be useful to bridge-engineers. There were the various types of silicon steel, rightly or wrongly so-called, which would help with regard to fatigue, and which would allow of a lighter bridge; and there were the copper-bearing steels which practical experience had shown were of very great help with regard to resisting corrosion. Bridge-engineers could use alloy steels that were much cheaper than those which locomotive-engineers could use, because the alloy steels of which he was speaking were much cheaper than the high chrome-nickel steels which locomotive-engineers had to consider. It was only necessary to look at the locomotives that had been built recently for the four great systems in this country to appreciate the advantages which had already accrued from the Report. Engines were running now with axle-loads that would not have been tolerated before the investigation, the results of which were embodied in the Report. He trusted that the investigations would be extended still further, especially with regard to some locomotives: there was one which produced no hammer-blow at all, and it happened to be on the railway which owned "locomotive K"! Unquestionably, larger locomotives could be built in the future which would practically cause no hammer-blow. A difficulty met with, however, was the question of springs. The locomotive-engineers had been investigating that matter with the object of making the springs more effective. The trouble was that Professor Inglis said "Don't do that: make a spring so that

Sir Henry Fowler. it is not resilient." In certain high-speed locomotives all the springs were of the laminated type and compensated. He would like to know what the effect of that was, theoretically at all events, as compared with a locomotive which had a separate spring for every axle. Again, certain locomotives had been used which had been sprung purposely with coil springs on the driving-axles. It had been felt that it was easier for the crank-axle of the locomotive if coil springs were used. He appreciated what Sir Clement Hindley had said with regard to weight having been put in the wrong place. When one was tied down in the matter of weight in the way locomotive-engineers were sometimes, one sympathized with the designer of "locomotive K" when he put no balance-weights at all in the wheels. Perhaps if locomotive-engineers had been allowed to put a little more weight in certain parts it would have been better for both sides. A colleague had asked him to say a word about "locomotive K." That locomotive had been designed to haul 1,000 tons at an average speed of 17 miles per hour. It would be appreciated that when it was run at 2½ times the speed it had been designed for, it would not behave so well as when running at its normal speed. Therefore he thought there was something to be said for that locomotive, although it had been condemned, and its execution was in progress—but not because of the hammer-blow. He also appreciated Sir Clement Hindley's remark that locomotives in this country were not entirely suitable for India. British locomotives were built for British conditions. Now they were going to be built so as to be able to fulfil those conditions even better than they had fulfilled them in times past. He felt with Mr. Ellson very strongly that the Report marked only the first step. Engineers were thankful indeed that that step had been taken.

Mr. Martin. Mr. H. M. MARTIN observed that the Author suggested (p. 48) that fatigue formulas should be abandoned. Mr. Martin considered that a very dangerous doctrine; and, taking that view, the Author would, presumably, oppose the practice of allowing higher unit stresses for occasional wind and temperature loads than for normal everyday loadings. Sir Benjamin Baker could, however, be quoted in support of this practice, and his experience was probably unparalleled. It extended into the pioneering period of metal bridge building, when failures were more frequent than they were to-day; and it was an accepted truism that more was to be learnt from one failure than from a dozen structures which stood, with a margin of safety which was necessarily somewhat uncertain. He also thought that certain statements contained in the Committee's Report were contrary to the facts. It was there stated (p. 8) that there appeared

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to be no experience of failures of steel or wrought-iron bridges traceable to fatigue. That involved the implication that Sir Benjamin Baker, in asserting the contrary, did not know enough to distinguish between a fatigue fracture and the other type. That was incredible, and the assumption implicit in the Committee's Report seemed not a little audacious. A fatigue fracture was in fact unmistakable. The metal parted with no contraction of area, and with no indication of ductility. A typical case was provided by the tie-rod responsible for the fall of Charing Cross station roof. There a faulty weld had led to such a concentration of stress on the sound metal that the fatigue limit was exceeded, and failure became a mere matter of time. Indeed, he doubted whether under service conditions a tension member ever failed save by fatigue. Of course, an accident by which a tie was subjected to a load never contemplated by the designer might lead to a ductile fracture, but such a break could not be attributed to normal service conditions. Again, a failure might start as a fatigue fracture and finish with some show of ductility, but it was fatigue that started the trouble. Fortunately, failures in tension were rare. He thought it would have been better if the Committee, instead of ignoring Sir Benjamin Baker's evidence, had noted it, and given their reasons for believing him to be mistaken. It was difficult, however, to imagine what these could have been, since Sir Benjamin had seen the failures and the Committee, of course, had not. There was an astonishing statement on p. 145 of the Report, namely, that "the working stresses . . . are at their maximum too far from the elastic limit to allow their repetition to have any deleterious influence." As a matter of fact the elastic limit was probably exceeded in some part of every existing metal bridge. Sir Benjamin Baker had given the working-stress upon the Britannia bridge as 6 tons per square inch; but that was an average figure. It was now known that, were not the elastic limit exceeded, a stress of 18 tons per square inch would be found round some of the rivet-holes. That figure was in excess of the elastic limit of wrought iron. Of course, the Committee knew that perfectly well, and what they had apparently meant to say was that the range of stress in metal bridges was generally well within fatigue limits. To put the matter thus, however, would have involved recognition of the fact that the range of a stress and its frequency were matters of importance; and for some unexplained reason they seemed very reluctant to admit that. He desired to call attention to what he thought was the absurdity of basing factors of safety on the elastic limit. He had never come across any defence of that practice which took account of the fact

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Mr. Martin. that if a small hole was drilled through a tie-bar the stress at the edges of the hole might be three times the average stress on the whole cross section. A striking instance of the absurdity of the practice criticized had been shown in some Continental tests of the strength of dished ends for steam-drums, made 2 or 3 years ago. It was found that if those ends had manholes formed in them the elastic limit was exceeded in certain narrow zones under no excessive test pressure. The experimenters accordingly made the fatuous remark that when such dished ends were proportioned by the usual rules the factor of safety was zero. As a matter of fact those dished ends would stand before rupture quite double the pressure at which the large strains were observed, and in practice failures never occurred in those theoretical danger-zones, the cracks being located in regions where the fatigue was a maximum—which was not necessarily the place where the stress was a maximum. The insistence on the all-importance of the elastic limit seemed due to the fact that some engineers, having learnt to solve complicated stress problems, were reluctant to admit that there was not necessarily any close connection between calculated stresses and actual strengths. He believed he had dealt with as complicated cases of stress-calculation as most investigators, but he had long recognized that the last word lay with experience and not with calculation. Moderate calculated stresses might be an indication of safety, but high calculated stresses did not necessarily indicate danger. On p. 48 the Author said that certain engineers had ignored impact. That was hardly correct. What they did was to lump in provision for it with the general factor of safety, just as they did for corrosion. It was by no means clear that this procedure was not just as sound as that recommended by the Committee, whose conclusions he regarded as somewhat hasty and irrational. They were apparently based on the assumption that a stress was a stress, and was equally as dangerous whether it came on once in 20 years or twenty times an hour. In this matter, as in most others, a qualitative as well as a quantitative factor was involved, and the quality of a stress depended on, amongst other things, its range and frequency. The data secured by the Committee were extremely interesting, but it would have been well had their recommendations been put forward as suggestive and tentative rather than as final.

Mr. Rowell. Mr. H. S. Rowell remarked that he had been engaged for the last 8 or 9 years on a problem closely similar to that dealt with in the Paper; and he believed the Department of Scientific and Industrial Research had sent some of his Papers to the Bridge Stress Committee, so perhaps he might add some words of explanation to them. The problem of resonance in bridges was closely related to the problem of resonance in motor-cars. If one considered the body and frame of a motor-car suspended on springs, the springs resting on the axles, and the axles resting on the tires, one had there in its simple aspect a model of a bridge system, where the car-body represented the locomotive-engine and boiler, the car-springs were the locomotive-springs, the mass of the axle of the car was the mass of the bridge and the locomotive-wheels, and the elastic elements of the bridge were equivalent to the elasticity of the pneumatic tires of the car. That system was very simply represented in *Fig. 20* by a mass M_2 supported by a spring of stiffness S_2 carried by a second mass M_1 supported by a second spring of stiffness S_1, which gave all the factors required to work out the frequencies of that simple system. The system had two degrees of freedom, and therefore two frequencies. Everyone had seen how a motor-car would sometimes oscillate gently up and down on its springs; and if it were watched closely enough, the axle would be seen moving in phase with the body, bulging the tires when the body descended. Conversely, when following a car on a bad road the axle could be seen vibrating up and down very rapidly indeed, while at the same time the body would be seen to be moving—as the Committee had described the locomotive motion, very concisely—in anti-phase, that was, the body moved down while the axle moved up. It could be shown very simply that the two radian frequencies of a system of that sort were given by :—

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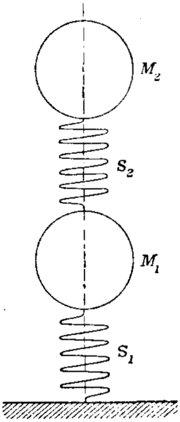
Fig. 20.



$$\omega^2 = \frac{1}{2}\left(\frac{S_2}{M_2} + \frac{S_1 + S_2}{M_1}\right) \pm \sqrt{\left\{\frac{1}{4}\left(\frac{S_2}{M_2} + \frac{S_1 + S_2}{M_1}\right)^2 - \frac{S_1 S_2}{M_1 M_2}\right\}}$$

In the bridge problem, S_2, the stiffness of the locomotive-springs, was usually very much less than S_1, the stiffness of the bridge. That was to say, the static deflection of the locomotive-springs was very much greater than the static deflection of the bridge under the weight of a locomotive. Therefore S_2 could be neglected in both the terms $(S_1 + S_2)/M_1$. The terms under the root then became a perfect square, giving

$$\omega^2 = \frac{1}{2}\left\{\left(\frac{S_2}{M_2} + \frac{S_1}{M_1}\right) \pm \left(\frac{S_2}{M_2} - \frac{S_1}{M_1}\right)\right\}.$$

Mr. Rowell. Thus, approximately the two radian frequencies were $\sqrt{S_2/M_2}$ and $\sqrt{S_1/M_1}$, or in periods per second $(1/2\pi)\sqrt{S_2/M_2}$ and $(1/2\pi)\sqrt{S_1/M_1}$ respectively. The first was the frequency of the locomotive on its own springs, and the second was the frequency of the bridge by virtue of its own mass and elasticity. With those facts in mind he had been greatly surprised to find no mention whatever in the Paper of the frequencies of the locomotives. Turning to the Report, and making a careful search, he had found only a casual remark on p. 98 that the natural frequency of a locomotive on its springs was usually between 3 and 4 periods per second. It was to be regretted that the frequencies of all the locomotives used in the experiments had not been tabulated with the frequencies of the bridges. Had that been done, the equation he had given could have been applied, and the range of resonance very much more closely determined. With that point of view in mind, it would seem that the entire Committee had been somewhat dominated by the bridge. The stresses, the movements, the frequency, and so on, of the bridge seemed to have occupied their minds far more than the properties of the general system. It was therefore especially interesting to notice a phrase in Lord Balfour's introductory note to the Report, namely, " Fixed bridges and trains in motion become for brief periods parts of a single mechanical system." That view of the bridge and the locomotive as a single mechanical system was strangely absent from the Report. Had that very simple view of the bridge and locomotive system been used, two or three apparent complexities in the Report would have been made clear and simple. For example, on p. 26 the Committee said " It was found that the frequency of certain bridges, when loaded with these engines, was so much less than the unloaded frequency that the difference could only be accounted for on the assumption that the whole mass of the locomotives oscillated with the bridge." In that passage was seen the unfortunate concentration on bridge frequency, instead of regarding the bridge and the locomotive as one doubly periodic system. As an example, he had worked out three typical frequencies for an assumed bridge and locomotive, each having the same frequency, namely, 3 per second. If the springs were locked solid, as the Committee suggested, the frequency would be 2·12 per second. If the springs were moving freely, the lower frequency would be 1·89, and in the anti-phase condition the frequency would be 4·35. So that the facts that the first two frequencies were nearly equal and the springs were sluggish explained some of the very flat portions of the curves in the amplitude–frequency diagrams. The Committee in their Report said there was evidence

Mr. Rowell.

supporting the belief that the springs remained locked solid under certain conditions. With that view he could not agree without seeing the evidence on which the opinion was based. He had carried out a great many experiments on the friction of springs, and had found that a relatively slight amount of vibration would suffice to neutralize practically all the friction in a spring. Another reason for doubting that statement was that, if locomotive-engineers fitted to locomotives springs that were solid, even when vibrating on bridges, what would be the use of such springs on ordinary level tracks? A far more striking result was concerned with long-span bridges. The Committee, in classifying bridges, asserted (pp. 27, 89 and 90 of the Report) that long spans were only subject to the lower critical speed. On p. 98 of the Report an even more unfortunate assertion occurred, namely, that for bridges of the order of 200 feet span the high-frequency peak was off the diagram. Now in long-span bridges, the mass M_1 of the bridge was very large indeed compared with the mass M_2 of the locomotive (*Fig. 20*, p. 117). Applying this fact to the frequency equation, very nearly equal roots were obtained when $S_1/M_1 = S_2/M_2$, so that, when the frequency of the locomotive on its springs coincided with the unloaded frequency of the long-span bridge, there was only one natural frequency, namely, that of the bridge or the locomotive. Assuming that the frequency of the locomotives was 3 to 4 per second, and referring to *Fig. 6* of the Paper or Fig. 71 (p. 128,) of the Report, it was seen that this was the frequency of spans of 160 to 230 feet. That was why the frequency of the locomotive on its springs was so very important, especially if it came near the frequency of the long-span bridge. One further point related to Appendix J of the Report, by Professor Inglis. The difficulties of applying mathematics to any such problem were really immense. The data could not be expressed mathematically, and, since that was so, it was very doubtful whether it was justifiable to proceed to handle them mathematically. After all, mathematics were very like a sausage-machine. If one put in good meat one got out good sausages: if the data applied to the mathematics were indifferent, precise results could not be expected. In the spring problem one of the fundamental difficulties was that the friction was not fluid friction (which was very simple mathematically); it was not even solid friction (which was not so difficult mathematically); it was some peculiar mixture of the two. There was grease, rust, roughness, and intermittent contact. In an attempt to deal mathematically with the spring, Professor Inglis had expressed the assumed solid friction correctly by means of a curve of Greek key pattern (Fig. J4 of the Report). That meant

Mr. Rowell. that, as the spring oscillated, the friction was negative and constant, then positive and constant, and so on. Then, in order to simplify the mathematics, Professor Inglis assumed that that pattern was developed into a Fourier series. The first term of the series had an amplitude of $4/\pi$ times the solid friction, and then that single sine curve was put into the calculation as a close approximation. It was, however, a precise representation of perfect fluid friction—proportional to the velocity of the motion. Therefore, in proceeding with the solid friction and taking only the first term of the Fourier series representing that solid friction, Professor Inglis had come back unconsciously to fluid friction and applied it to the springs that were said to be locked solid at all but very high speeds. Despite these criticisms he wished in conclusion to pay a tribute to the splendid field-work of the Author and his staff.

Mr. Parkinson. Mr. R. M. Parkinson remarked that if there was a factor of safety of 4 or 5, a small difference in the matter of hammer-blow could not make much difference to the bridge. Had the Author used instruments on the engine itself? He took it that a locomotive when it was sent out of the shops was weighed; so that if there was, say, 9 tons on each of four driving-wheels, the weigh-bridge on which a wheel was put would register 9 tons. If when any one of those wheels was raised by a force equal to, say, 7 tons, the spring was thereby compressed 1 inch, it followed that when, in actual running, that spring was compressed 1 inch, instead of 9 tons there was 16 tons on the wheel. Did not that spring movement give the measure of the weight on the wheel at a particular moment when the engine was rolling? As rolling transferred weight from one rail to the other, surely the amount of any hammer-blow was shown by the spring-movement. He supposed the hammer-blow expended itself like the action of dynamite. It not only went down on to the rail, but also up into the spring, so that the full effect that the Author had described was perhaps obtained.

Colonel Sir Gordon Hearn. Colonel Sir Gordon Hearn remarked that parts of the discussion had rather reminded him of the aerial "dog fights" that used to take place during the war; they were carried on at a great height, and the infantry on the ground were not able to make out quite what was going on. But, in spite of occasional high flights on the parts of Professors Inglis and Dalby, he thought the members were able to appreciate what a great effect hammer-blows had on the deflection of a girder. He would like to point out what a wide difference there was between the Committee's recommendations and the old formulas. If the Pencoyd and similar formulas were abandoned, nothing would remain in the top left-hand corner of

Colonel Sir Gordon Hearn.

Fig. 1—that part which showed impact allowances for short spans, according to the Committee's methods of calculating impact allowance. In the Correspondence on Mr. Anderson's Paper, Professor F. E. Turneaure had shown[1] a diagram of experimental results plotted in the same way as in *Fig. 1*, and in the left-hand top corner of that Figure was a mass of records made by strain-recorders on short spans. Above the plotted results had been drawn what the Author described as an enveloping line, which had resulted in the American Railway Engineering Association formula; and Professor Turneaure had gone a stage further by adding about 10 per cent. The discrepancy between those results and the findings of the Committee was very puzzling. Was it really the case that a stress-recorder could grossly exaggerate to that extent? The Committee appeared to have depended on the deflectometer, and to have condemned the stress-recorder. He though that required some explanation. Was it really the case that rapidity of application of the load had no impact value? The Committee had dealt with speeds up to 6 revolutions per second. That was very high, being equivalent to train-speeds ranging up to 87 miles per hour. In India there were many bridges approached by long banks on which nothing like that speed was attained. A speed of 4½ revolutions per second would be quite enough for an express engine with the heavy loads hauled in that country. The Author had remarked very significantly that if a girder of a certain span was weak, it did no good to restrict the speed—the inference being that the synchronization of the revolutions of the driving-wheels and the vibration of the girder would make matters worse. Was it logical to go in the other direction, that was, to increase the speed in order to avoid the lower frequency and get the anti-phase frequency? A 150-foot span under the Committee's loading had to be exceptionally strong. That span-length was common in India, where the cost of the superstructure was equated to the cost of the substructure to determine the most economical span-length. Was it really the case that a span of 200 feet should be adopted rather than 150 feet?

The Author.

The AUTHOR, in reply, remarked that a number of speakers had kindly elaborated and explained matters with which it had not been possible to deal fully in the Paper, and he was grateful to them for their assistance in that respect. As he had pointed out at the beginning of the discussion, it had been quite impossible within the limits of a Paper to deal completely with a subject of such magnitude. He

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[1] Minutes of Proceedings Inst. C.E., vol. cc (1915), p. 271.

The Author. would like to express his gratitude for Sir Alfred Ewing's very kind references to the work which he had been able to do for the Committee. Sitting by Sir Alfred's side for 5 years had been a very liberal education, not only in the particular work under discussion, but also in the writing and speaking of English. Sir Alfred was never guilty of an over-statement except when the kindness of his heart led him to pay too generous a tribute to those who had helped him. Professor Inglis's clear exposition of the principles on which he had based his calculations, and of the various steps by which he had approached the complete solution of his difficult problem, had supplied what it had not been possible for the Author, even if he had had the ability, to include in the Paper. He accepted Professor Inglis's criticism that he had omitted to refer to the fact that the Committee's tables were based on the assumed condition of a "skidding" locomotive, and not on a travelling one. That this was an important fact, scientifically, must be admitted, and it was necessary to point out at the same time that there were grounds for adopting such an assumption, in that the error thereby introduced was negligible and the gain in simplicity of calculation was very great. What was undoubtedly a difficulty had been mentioned by Sir Clement Hindley and other speakers, namely, the application of the information given in the Report to the special conditions obtaining in countries where railway practice was not similar to that upon which the allowances in the tables were based. The Author would not admit that the Report was "insular," in that it gave tables applicable to the conditions in this country alone. The main purpose of the Committee's work had been to discover and define the principles, rather than to calculate allowances for any particular set of conditions. It would clearly have been impossible for the Committee to calculate tables of loads for the many different kinds of engines and bridges of different gauges all over the world, and it was the duty of the various administrations to ascertain what the conditions were for which impact allowances must be calculated, and then to make the necessary calculations. The Committee had been asked at one stage of their labours to calculate curves for Indian railways, based upon an assumed engine having axle-loads and hammer-blows of such severity that he shrank from the contemplation of its effect on bridge-design in India. The Committee, however, had declined—he thought rightly—to do so, because, among other reasons, such a work would have postponed the publication of the Report for some time. A useful contribution towards the solution of this difficulty, however, had been made by Professor Dalby, who had described a

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method by which an approximately correct allowance could be calculated. That would no doubt be followed up by engineers who had to deal with bridges and engines for which the tabular loads were inapplicable. Professor Dalby's practical illustration of the effect of a synchronized hammer-blow had been much appreciated by members present; they had seen in a vivid manner the importance of avoiding synchronous conditions. Some speakers, particularly Mr. Fereday, had complained that the Committee had not gone further with the matter of secondary stresses. The reasons for that were fully set out in the Report, and he would not allude further to them. So far as the instruments were concerned, he hoped that nothing in the Report or in the Paper would be taken as detrimental to the Fereday-Palmer instrument, for which the Committee had been very grateful and which they had valued highly; but he thought Mr. Fereday had himself admitted that the work to which the Committee had put that instrument was not exactly the work for which Mr. Fereday had originally designed it. The Committee, perhaps, had required something more than could reasonably be expected from the instrument. They had tested it. They had no prejudice in the matter whatever. A very great deal of care had been taken, and a very long series of tests had been made, which were fully described in the Report. There was nothing in the Paper which he would wish to take back so far as the instruments which had been used were concerned. He was unaware of any infringement of any patent and was at a loss to understand Mr. Fereday's remarks, seeing that the Committee had not invented any stress-recorder, but had endeavoured to develop and improve the instruments which were available; and the only instrument which they had employed as an alternative to the Fereday-Palmer stress-recorder was based on an entirely different principle and did not appear to have anything in common with it. Several speakers had referred to the question of working-stress. The Committee had not made any recommendation with regard to working-stress, for the reason that they had deliberately distinguished between the two factors of load and stress, which for various reasons had been somewhat confused in the past. They had been charged with the duty of ascertaining what were suitable impact-allowances expressed as additional loads on the bridge. The appropriate working-stress in the bridge was another matter, that could be decided upon having regard to the nature of the materials used in bridge-construction. In that connection he would refer to the remarks of certain speakers who had taken exception to the statements in the Paper concerning "fatigue." Nobody had a greater regard for Sir Benjamin Baker

The Author. that he had, and he would not for a moment suggest that anything Sir Benjamin Baker had said was not perfectly true and founded on fact. All the Committee had said was that they themselves, after inquiry and on the advice of the railway members of the Committee, had not heard of any reliable evidence that bridges had suffered from what was commonly known as fatigue. At any rate, if such occurrences had been met with, they were so extremely rare that there was no need to take them into account in prescribing rules. The fatigue question had arisen because the Committee had been of opinion that a formula based on fatigue was an entirely unsuitable type of formula to use for estimating impact effects. If fatigue occurred in railway-bridges, clearly some adjustment must be made for it in prescribing the working-stress. But if fatigue occurred in bridges there seemed to be no reason whatever why a formula based on range of stress should be used as a measure of impact effects which were very clearly due to a force applied to the bridge, having nothing whatever to do with the strength of the material or the behaviour of material under stress. What the Committee had done was to set out the impact-formula in rational form, and to express their opinion on the unsuitability of a fatigue formula for estimating impact effects. He would want to have some very sound evidence that fatigue effects did occur and did bulk largely in the strength of bridges. In his experience he had never seen any sign of that in old bridges which had been taken out or which had been strengthened. There were high stresses near rivet-holes, and there had been plenty of instances of rivets working loose; but the deterioration of materials by fatigue was not a factor which the bridge-engineer in this country, to the best of his knowledge, was concerned with to any extent. Regarding the important point of the comparative value of stress readings and deflection readings, it was known that the stresses recorded by an extensometer did not agree with simultaneously-recorded deflections if the stresses were read at only one point in the cross section of a member. Reference to the diagrams in the Report would show that stresses had been measured at two or four points around the members under test and that the mean of the readings had been compared with the results of the deflection tests. The individual stress-readings had, of course, been affected by local bending-moments which did not appreciably affect the deflection of the girder. The Committee were satisfied that the deflectometers gave a true record of impact effects, both in main girders and in floor members, although both systems of measurement had been continuously employed; and in certain instances, the stress-recorders had been

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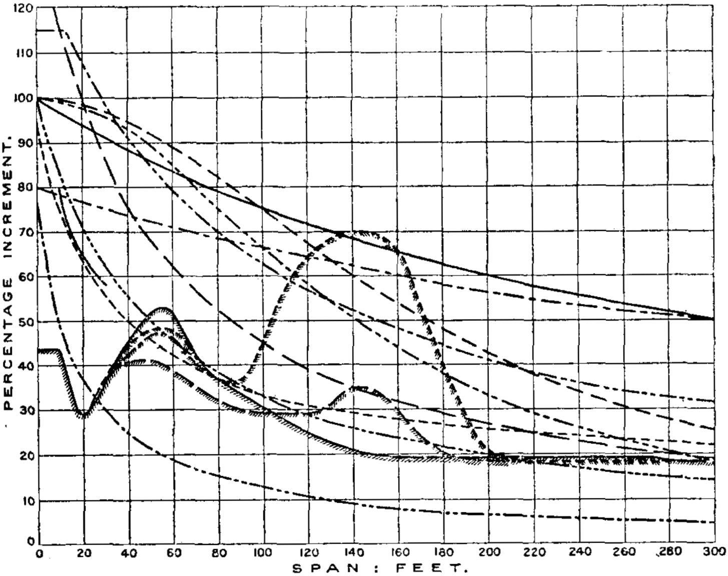
found to be more convenient than the deflectometers. This statement did not, of course, suggest that the actual stresses in a bridge and in its various component parts could be ascertained with any degree of accuracy by means of deflectometers. The curves in *Fig. 21* represented by heavy lines were based on the limiting speed of 6 revolutions per second: to have included the curves for the lower limits of 4·5 and 3 revolutions per second would have complicated the diagram. If, however, the C loading, which included a hammer-blow of 15 tons at 5 revolutions per second, were excluded, the differences between the various curves on account of the three speed-limits were not very great. Fortunately it was possible in this country to exclude the 15-ton hammer-blow almost, if not entirely. If this loading had to be taken into account, and a speed of 6 revolutions per second had to be allowed for, it was found that spans of 120 to 200 feet were somewhat expensive. He had had the assistance of Professor Inglis in replying to the remarks of Mr. Rowell, which were entirely directed to the method which Professor Inglis had developed for calculating impact effects. Mr.

Fig. 21.



Lines Hatched Show Impact Allowance Recommended by the Bridge Stress Committee as a Percentage of Twenty Units of British Standard Loading. The Other Lines are as Shown in *Fig. 1*, p. 49.

The Author. Rowell had presented a formula for determining the two natural frequencies which occurred when a bridge carried a spring-borne load, and his subsequent observations had rather suggested that the Bridge Stress Committee had given insufficient attention to this point. The consideration of the natural frequencies of the bridge in this condition had been very prominently in the minds of the Committee, and in working out the impact allowances the effects of possible spring-action had always been taken into account. In Chapter VI of the Report this point was discussed to the fullest possible extent, and the formula there given for determining the two natural frequencies was much more comprehensive than that stated by Mr. Rowell, in that the effects of damping, both in the bridge and in the locomotive, were taken into account. Mr. Rowell had in fact given an excellent example of the dangers of over-idealization referred to by Professor Inglis. Arguing from the analogy of a motor-car, he had concluded that the friction in the spring-movement of a locomotive was negligible. In that respect locomotives differed very much from motor-cars. The chief source of spring-damping in a locomotive lay in the friction between the axle-boxes and their guides, which was increased, when the engine was running, by the thrust or pull in the connecting-rods and side rods. The bridge-oscillator showed that, unless a bridge was shaken to a very pronounced extent, no movement whatever of axle-boxes in their guides could be detected. Further, when a locomotive was picked up by a crane and dropped a small distance for experimental purposes, the motion was found to be almost dead beat. The springs of a locomotive no doubt served a useful purpose in taking up shocks caused by rail-joints, etc., but they were not as a rule stimulated into movement on a bridge having a slow natural period. When they did come into action, the absorption of energy in overcoming friction was considerable. A theory which was idealized to the extent of leaving out of account that consideration of spring-friction was worthless for predicting bridge-oscillations, and in view of the careful attention given to that point in the Report, it was a little strange that Mr. Rowell suggested that the Committee had concentrated their attention on bridge characteristics to the exclusion of those of the locomotive. In criticizing the method of dealing with spring-friction employed in Appendix J of the Report, Mr. Rowell apparently missed the real point. He stated that in taking the first term of the Fourier series representing solid friction, Professor Inglis had come back "unconsciously" to fluid friction. Mr. Rowell might rest assured that there was nothing "unconscious" about that process. Certainly the fact that the

The Author. variation of the frictional force was sinusoidal in time was a point of similarity with fluid friction, but there the similarity ended. In fluid friction the coefficient of friction was independent of the velocity, but if the first term of the Fourier series representing solid friction was to be regarded as a fluid friction, this reconciliation could only be effected by conceiving a fluid whose coefficient of friction varied with the extent of the oscillation. The coefficient accordingly could not be determined until the state of oscillation was known, and vice versa. That difficulty could be overcome by a trial-and-error method which had been explained[1] by Professor Inglis. Experience, however, had soon revealed the laborious character of that method and caused the substitution of the more direct process set forth in Appendix J.

Correspondence.

Mr. Blyth. Mr. C. E. Blyth remarked that he had no data of local observations in relation to this subject that would be useful in the discussion. With two or three exceptions, most of the bridges on the Egyptian State Railways were adjacent to stations where trains were timed to stop, so that naturally train-speeds over those bridges were generally low. He had, however, personal experience of excessive wear of locomotive-tires, which, on careful investigation, had been traced to incorrect balancing of the engines, and he could readily understand that such bad balancing must produce severe hammer-blows and consequently set up serious stresses in bridges due to vibration, especially if the frequencies of the hammer-blows synchronized with the periods of vibration of the bridge; and that pointed to the need for co-operation between locomotive-engineers and bridge-engineers when designing engines or railway-bridges.

Mr. Colam. Mr. H. N. Colam observed that as a bridge-engineer who had spent a good deal of time in the investigation of impact, he wished to record his admiration of the work done by the British Bridge Stress Committee. At the same time he thought that both the Report and the Author's Paper were unfair, to say the least of it, to the work done before. The Report referred to the work done in India, but omitted all reference to the Indian Report of 1925, which summarized and co-ordinated the results, and to a very large extent

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[1] Proceedings Roy. Soc., Series A, vol. cxviii (1928), p. 60.