Discussion.

The CHAIRMAN, in moving a vote of thanks to the Authors, observed that The Institution was much indebted to them for their excellent Papers. The usual practice was to make no allowance for the reduction of stress in the main girders due to the action of the floor-system. It was true it was a very real reduction; but engineers had to exercise discretion in its use. His own firm had recognized that action for some time, and an endeavour to evaluate the stresses taken up by floor-systems had been particularly useful to them in examinations made to determine the stresses in existing bridges, and in deciding whether those bridges were capable of carrying the loads which were being imposed upon them, or any greater loads. There were stress-recorders now in existence, with which the actual stresses in members of girders could be determined with considerable accuracy, and on the results his firm had been able to analyse the effects of the loads and to give extensions of life to many bridges—some in this country, but more particularly in India. To illustrate the importance of this, he might mention that one of the bridges which had been given an extended lease of life consisted of fifty-six spans of 150 feet. It was evident that Professor Inglis's Paper had been prepared with the greatest care. It was a valuable contribution to the literature dealing with the vexed question of impact allowance, being a considerable advance on anything that had been published hitherto on the subject. On p. 227, however, Professor Inglis made it quite clear that he was dealing with ideal conditions. Unfortunately, those conditions did not always exist. Neither in bridgework, in locomotives, in rolling stock, nor in track, was there perfection. However, with deeper knowledge of what the actual stresses were, it was possible to make allowance for them—with some considerable difference in the cost of the bridge and in the design of the structure. As the Papers dealt only with railway bridges, he had prepared a few figures showing to what extent the expenditure on bridges formed a part of the capital cost of a railway. Engineers should not lose altogether their sense of proportion in dealing with the matter of steel girders, important as it was. In India, fortunately, valuable information was available as to the capital cost of all the lines built, which included four gauges, and he had taken out the cost of

The Chairman.

The Chairman. about 25,000 miles of the 5-foot 6-inch, and metre-gauge lines. The costs in India were kept under the following twelve separate headings, which had been consistently used throughout the railway-construction life of India :—

Heading.	Heading.	Heading.
Preliminary.	Fencing.	Collieries.
Land.	Telegraphs.	Plant.
Formation.	Permanent Way and Ballast.	Rolling Stock.
Bridges.	Stations and Buildings.	General Charges.

Of those twelve, the five items of Formation (13 per cent.), Bridges (15 per cent.), Permanent Way (27 per cent.), Stations and Buildings (10 per cent.), and Rolling Stock (16 per cent.) alone made up 81 per cent. of the total cost. The Bridgework figure (15 per cent.) included a number of all-masonry bridges, culverts, arch bridges, etc. ; and 12 per cent. might be fairly taken as representing the steel bridges. There was really no detailed sub-division as between masonry and steelwork in steel-span bridges, but it was considered the best construction to design such a bridge so that the cost of the metal spans was equal to the cost of the piers. Therefore the 12 per cent. was reduced to 6 per cent. for steelwork alone. If it were assumed that by improvements in methods of design a saving of 10 per cent. on the cost of the steelwork would be possible (he thought that was a generous allowance ; Mr. Remfry estimated 15 per cent. for the main girders, but the increased weight of the floor-system would probably bring that down to a maximum of 10 per cent.), the fact emerged that the ultimate improvement obtainable was *one-tenth of 6 per cent., or 0·6 per cent. on the capital cost* of the railway. He thought it was necessary to call attention to that conclusion. Anything that could be done, however, in the way of reducing capital expenditure was all to the good. Therefore he welcomed the information and the valuable data which the Authors had given.

Professor Inglis. Professor INGLIS explained a series of wall-diagrams prepared to illustrate his Paper. He remarked that, of the five factors, enumerated on p. 267, of which account should be taken in the consideration of impact formulas, the last three were locomotive characteristics ; and the determination of dynamic allowances

Professor Inglis.

would be very greatly helped if the British Engineering Standards Association would specify a locomotive to be used for calculating impact effects. To determine the dynamic effect of any particular locomotive on any particular bridge, it was necessary to work out the live-load and impact effects for each axle separately, and to superpose the results, to get a resultant deflection–time curve, as had been done in the case of *Fig. 18* (p. 269). Such work might sound rather laborious, but it was not prohibitively so. When facility had been gained in plotting the necessary formulas, a case of that sort, which involved ten axle-loads, could be evolved in the course of a day's work. Throughout the investigation no attempt had been made to dogmatize or to lay down a specific impact formula for general purposes. In the light of the experimental work which was going on in this country, such action would be both premature and undesirable. But if the results which emerged from the analytical methods were found to be in substantial agreement with experiment, he thought some real progress would have been made. If, furthermore, the problem were simplified by the specification of a standard locomotive for calculating impact effects, there was a reasonable hope that rational formulas for impact and live-load allowances, which satisfied both theory and experiment, might be forthcoming at a not-too-distant date.

Mr. Richards.

Mr. GEORGE RICHARDS desired to associate himself with the remarks which the Chairman had made about the value of the Papers. He considered the subject-matter of the Papers was based on Clauses 5 and 12 of the British Standard specification for Girder Bridges. Clause 5 stated that something had to be added to the live load to bring it to an equivalent static load, and it put in a formula which was admittedly tentative. Clause 12 dealt with the relief of stress in the following terms: "In determining the maximum stresses in a member, any relief of stress afforded the member or part of the structure under consideration by adjacent members or parts may be taken into account, subject to the approval of the Engineer." Mr. Remfry had given a very good idea of what relief of stress could be expected in main girders on account of the deck system, and Professor Inglis had made an advance towards getting a satisfactory impact formula. It had to be remembered, however, that Professor Inglis was using the word "impact" in a slightly different way from that adopted hitherto. In 1915 a Paper[1] was contributed by Mr. C. W. Anderson, M. Inst. C.E., who defined "impact" in the sense in which it was used in the Indian bridge

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[1] Minutes of Proceedings Inst. C.E., vol. cc, p. 178.

Mr. Richards rules. The definition of impact in those rules was: "The difference between the vertical effect on a bridge of a load moving over the bridge and the effect of the same load at rest on the bridge." That difference could be measured by extensometers and deflectometers. Professor Inglis was practically confining the word "impact" to the effect of counter-weights on the wheels of a locomotive. In the Correspondence on Mr. Anderson's Paper, the different causes of impact were summed up very well by Professor F. E. Turneaure,[1] who enumerated six different causes of impact—impact according to the definition which Mr. Richards had just quoted. Professor Turneaure reckoned that 80 per cent. of the whole effect was due to counter-weights.

Professor Coker. Professor E. G. COKER observed that Professor Inglis's Paper might be looked upon as a small treatise on impact, or as a small treatise on partial differential equations; and in both aspects it presented some serious problems. It seemed to him that Professor Inglis's criticism of the Pencoyd formula and his suggestion of the elements which would have to be put into future impact formulas were very sound, and he hoped that later the work of Professor Inglis, and others associated with him, would produce a much better formula than the Pencoyd one, which seemed very arbitrary and inconsistent. The Paper contained a strong argument for the electrification of railways, since Professor Inglis showed that in regard to the influence of a live load, such as was produced by an electric train, the impact effect was practically negligible. The expressions for oscillation of both the main girders and cross beams had been found by mathematical methods which appeared to be above suspicion, but it seemed that they were all taken separately in considering the oscillation of the bridge. He would like to know the effect of the complex oscillation which he imagined must be going on when the whole of the bridge was oscillating—the cross girders at one periodicity and the main members at another. Theory appeared to indicate that in such a complex system there was a change in the fundamental oscillation of the main trusses. Perhaps Professor Inglis would explain what, if any, change did take place. It was suggested on p. 270 that one of the ways of minimizing the evil effects of oscillation produced by wheel-spacing, such as was indicated in ***Fig. 11*** (p. 258), would be, in the case of double-heading, to choose locomotives whose driving-wheels were of different diameters. That would be a solution, but he would like to know whether it would be practicable to maintain express speed when one locomotive

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[1] Minutes of Proceedings Inst. C.E., vol. cc, p. 268.

Professor Coker.

had coupled wheels, say, 6 feet 6 inches in diameter, and the other 5-foot 6-inch wheels. He had been making some experiments in a very tentative way to investigate stresses occurring in simple bridges. He had come across a difficulty which he would like to mention, as perhaps the solution might be found in Professor Inglis's Paper. He had been using a model of a bridge, which consisted of two plate girders, 12 inches long, supported on free ends. A rolling load was pulled over by a system of pulleys. By using a transparent material for the beam and looking at it in a ray of polarized light, he was able, by properly proportioning the size of the bridge to the load, to get a well-marked colour-band in a convenient position. What he had found, and had not been able to understand, was, that when he plotted the velocity of the rolling load against the stress at a particular point on the centre-line of the bridge, the value of the stress decreased very slightly as the velocity increased up to a certain value, but above this value the stress began to increase again. These experiments were different from those described in the Paper, because, if the formula $\frac{EId^4y}{dx^4} + m\,\frac{d^2y}{dt^2} = f\ (x, t)$ were considered, the material being very light, the value of m in comparison with EI was so small that it rendered the second term negligible. Of course that did not absolutely prevent the occurrence of a term causing an oscillation effect, but he had not been able to find any oscillation worth mentioning, under the circumstances described. It was a case which more or less approximated to the one considered by Sir G. G. Stokes in a Paper[1] in 1849, where the mass of the girder presented considerable difficulties. He thought it would be useful if Professor Inglis would prepare a short Appendix outlining the methods of solution of some of the later equations, as it was rather difficult to follow these completely without some such aid.

Mr. Gribble.

Mr. Conrad Gribble remarked that the two Papers dealt with three problems of bridgework which had exercised the minds of bridge-engineers for a very long time, the main problem being perhaps that of impact, which was between 70 and 80 years old. It was disappointing to him that there was not more experimental verification of the figures given by Mr. Remfry, although the Paper was most useful in giving a lead as to the direction in which research should be made, to solve the two problems with which Mr. Remfry dealt. Such experience as he himself had had in experimenting on bridges had, he was afraid, very largely destroyed his preconceived

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[1] "Discussion of a Differential Equation relating to the breaking of Railway Bridges." Proceedings Camb. Philosophical Society, vol. i, p. 83.

Mr. Gribble. ideas. It was necessary to be a little cautious in taking the figures in the Paper, particularly regarding the assistance given to cross girders by reason of their fixation. It was possible to calculate moments in the verticals of a lattice-girder bridge to which the cross girders were attached—that had been done on very sound theory; but the calculations assumed that the frame comprising the cross girder, the two verticals, and the overhead member, stood alone: it was practically impossible to take into account also that they were a part of the bridge. The torsion of the main chords of the main girders, and the assistance given to the cross girders by stringers, rendered the calculations rather theoretical, and he did not think any such calculations should be taken very seriously without a good deal of experimental verification; they could only give an approximate idea. Mr. Remfry dealt only with one type of bridge, and undoubtedly the same effect occurred to a very much less degree where there was no overhead bracing. Also, in a lattice-girder bridge with overhead bracing there were certain verticals which had no overhead member attached to them, and the conditions in the various cross girders must certainly depend on whether they were under an overhead member. A great deal of the work, although useful, should not be taken literally. A series of tests had been made on the North Eastern Railway a year or two ago under Mr. C. F. Bengough, M. Inst. C.E., which were directed to solving the two problems exactly. Three wrought-iron bridges and three steel bridges were tested, the main object being to ascertain the extent of the fixation which the cross girders received, and also to what extent a continuous decking assisted the structure. In the cases of the older bridges the object of the test was to ascertain more exactly how strong the bridges were, in view of increasing loads; and in connection with the newer bridges it was desired to test the calculations made in the course of their design, seeing that a certain fixation of the cross girders had been assumed. The first of the old bridges was of 50 feet span, and had wrought-iron girders and a continuous decking; and it was possible to make certain experiments which could not usually be made. The decking was cut away, and each cross girder was isolated from its fellows, destroying the continuity which the decking possessed. Extensometer tests were carried out in both the original and the altered states. It was thought that the continuous decking would add considerably to the strength of the bridge, and would affect the stresses in the floor and main girders. That was one of the cases where preconceived ideas were found to be wrong. Neither to the cross girders nor to the main girders did the cutting of the decking

Mr. Gribble.

make any appreciable difference. The next test was to see how much the cross girders were affected by the fixation of their ends. The rivets were cut out and replaced by bolts, and tests were made both with the bolts tight and with the bolts slacked off, so that the cross girder was freely supported at its ends. No difference was detected. That was, perhaps, explained by the fact that the original connection with the main girder was not good. It was found, however, that a point load was widely distributed over the floor. The test was made with a single axle supporting the end of a long trolley wagon, which stood over the cross girder. There were no stringers of any sort, but the rails were carried on sound longitudinal timbers. When the axle was immediately over a cross girder, the latter only took 42 per cent. of the total point load, the rest being accounted for by the assistance from the distribution of the load by the timber and the rails, which was far more than had been anticipated. Another test was on a new double-track steel bridge, with very heavy cross girders, strongly attached to main girders about 100 feet long. It was thought that a certain degree of fixation would be obtained at the ends of the cross girders, but very little was found, that in the cross girders in the middle of the bridge being considerably less than in those at the ends. A greater distribution of load over the cross girders was found than had been anticipated, although the fixation-moments at the ends were much less. There was, however, no more stress in the middle of the cross girder than had been calculated in the designing, because of the largely increased distribution of load along the floor. For calculations of cross girders of that kind, it seemed that it was not necessary to take concentrated loads; the same evenly-distributed loads assumed in the design of the main girders would give results not very far from the mark. Although the assumptions made were wrong in two ways, the instinct of the engineers had been right, because the tests proved that the girders had been well designed. Another fact which arose from those tests was the very great twisting-moment that appeared in the cross girders, at their connections with the main girders, when a single load passed over the bridge. When the load was symmetrically over a cross girder, the connection was in a state of vertical shear. When the load passed to one side of the cross girder, the latter was tilted over by the flexure of the rail-bearer. That sideways effect of the cross girder did not cause any bending-moment at the middle, but it created a very considerable bending-moment at the ends; and the extra metal that was put into the ends of the cross girders to allow for the reverse bending-moment, which was not there in the case of most of the

Mr. Gribble girders, was actually required for the twisting-moments, which had not been previously considered. The cross girders, of uniform section throughout, were not badly designed, but the test showed that calculations, however carefully made, might be based on assumptions that were quite different from the conditions actually occurring in practice, and that these were widely different even in a single bridge.

He wished to suggest the application of Professor Inglis's Paper in three directions—first, as an assistance to research. The great difficulty in conducting research on bridges was to make up one's mind as to what was actually the object in view. It was easy to make a vast number of tests with extensometers and deflectometers, and to create masses of records which were very difficult to digest—such as the Americans obtained in 1908. The impact formulas which had been used in the past in this country were obviously illogical, being based on the total live load on the bridge. This applied to the American formulas of the Pencoyd type, and also to range-of-stress formulas based on the ratio of dead load to total load. The results obtained in America and in India, and Professor Inglis's theoretical analysis, pointed to the fact that the important factor was the counterbalance weights of the locomotive, and therefore no formula of the old type could be really considered at all correct. Professor Inglis's work was extraordinarily useful in giving a definite lead and a method which could be applied to research, and Mr. Gribble thought that research should be directed towards finding the limitations of Professor Inglis's theory—that was to say, finding how far the theory actually was borne out in practice. An examination of the records obtained in America and India showed that the synchronous oscillations which were built up as the engine passed over the bridge did not increase during the whole passage of the train, but, after a certain number of pulsations, decreased or vanished altogether. A research should first find the law of that action and the limits within which the formula applied. If that was accomplished by means of deflectometers which gave the movement of the whole girder, it was necessary also to see, by means of stress-recorders on the individual members, how far the effect observed on the whole bridge was observed in the individual members composing it. The second application of Professor Inglis's Paper was in the design of a bridge. Bridge-engineers might be a little alarmed at the prospect of using such a very complicated theory in their ordinary work; but he did not think that was what Professor Inglis suggested. After the necessary research had been made, the results could be put into

Mr. Gribble.

the form of a loading table which could be compiled in a logical form, without the empiricism of the old impact formula. The extra effect produced on a bridge by impact was an additional deflection, which could be represented by a load on the girder, either distributed or concentrated. Personally, he thought it could be quite well represented by a concentrated load; and so a loading table could be obtained, consisting of a distributed load representing the static weight of the locomotive on the bridge, and a concentrated load, to be superimposed, representing the effect of the counterbalance, which would have no relation to the total load on the bridge, but would be a function of the locomotive and also of the bridge, and would be expressed in tons. Such a loading table would take a good deal of compilation, and could only be produced as a result of research. Of all the locomotives in this country, only a small proportion would have to be considered, and there might be some heavy engines which had a comparatively small total effect. Of the engines considered for the static load table drawn up some years ago, comparatively few had to be included in the tabulated data, and not many more would have to be considered for the dynamical loading table. The third application was from the point of view of the maintenance-engineer, whose problem, Mr. Gribble thought, was comparatively simple, since he had to deal with existing bridges and locomotives, the particulars of which were known. By testing individual bridges, he could quite easily estimate and check the impact effect that was likely to occur. If the theory put forward was borne out by research, or its limitations were arrived at, it would prove to be the clearest method, and probably the only correct method, of dealing with the impact problem. Regarding the suggestion that 100 feet was a critical span-length, the results of American tests gave the frequencies of a large number of bridges, which were as low as 4 or 5 per second on a 60- to 80-foot span. That was a point which only experiment could prove, but it would appear that for shallow bridges it was possible to obtain synchronous speed on girders of considerably less span than 100 feet.

Sir Alfred Ewing.

Sir ALFRED EWING, K.C.B., observed that the two Papers were welcome as being relevant to what was recognized as an urgent engineering problem at the present day. Professor Inglis had contributed an original and fundamental treatment of a question of the utmost practical interest. The Institution was to be congratulated on being the medium of publication of a Paper of such exceptional quality and importance. It was a personal gratification to himself that such a Paper should have its origin in the Cambridge

Sir Alfred Ewing. Engineering Laboratory, and that Professor Inglis should be so well maintaining the traditions of research which were established by his predecessor, the late Professor Bertram Hopkinson, M. Inst. C.E. There was another reason why he particularly welcomed Professor Inglis's Paper, namely, that he happened to be Chairman of a Committee which had recently been established, through the co-operation of the railway companies and the Department of Scientific and Industrial Research, to investigate bridge stresses with special reference to moving loads—a Committee whose composition might claim to be a happy blend of experience and theory. Its object was to bring the matter out of the region of empiricism. He hoped he was guilty of no disrespect to the usefulness of the Pencoyd formula if he asked what justification it had ever had. Mr. Gribble had called it obviously illogical, and he could endorse that opinion in the light of the immense mass of work which had been already done on the subject. There was the big American report with its thousands of observations, a long series of Indian reports, and the recent experiments of the Ministry of Transport, conducted under the ægis of Colonel J. W. Pringle, M. Inst. C.E. The experience acquired from that body of experiment had been of great value to the new Committee. At the outset it had been found to be desirable, notwithstanding all that had been done in the past, to face anew the problem of selecting and even of designing instruments which would accurately record deflections of the bridges and stresses in the members during the passage of moving loads; and a good deal of the preliminary work of the Committee had consisted in a quest for instruments. They had found the Fereday-Palmer recorder an instrument of much utility, but they did not feel that any instrument previously tested met the case entirely. It had been necessary, therefore, to embark upon experiments which had not yet reached a conclusion; but enough had been done to make them realize that it was not by the mere accumulation of data, as Mr. Gribble had well said, that the problem was to be solved, but rather by giving the experiments such a direction that they would elucidate conclusions which had been already anticipated on grounds of theory. It was by that method, he believed, that a satisfactory conclusion for the purpose of practical engineering would be arrived at. Professor Inglis drew special attention to the importance of determining the free period of vertical vibration of a bridge, both loaded and unloaded, and also of determining the damping of the oscillations. That was a point now engaging the Committee's attention. There was reason to believe that the alarming results exhibited in *Fig. 15* (p. 264) did not arise in normal practice, because

Sir Alfred Ewing.

of two factors, namely, the damping, which in itself would prevent excessive amplitude of vibration, and the fact that the very movement of the load rendered the period of vibration variable and tended thereby to prevent the cumulative effect of synchronism. The distinction which Professor Inglis drew between live-load effect and impact effect was of great importance—all the more, perhaps, because the distinction was entirely ignored in the otherwise valuable American report, in which all the effects of a moving load were merged under one name, impact. Professor Inglis wisely did not attempt to lay down any simple formulas, but Sir Alfred Ewing trusted that they would come later, when the work which Professor Inglis had so well begun was supplemented by the experiments of the Committee. He also hoped that Mr. Gribble's prophecy might be correct, and that the Committee would serve as a buffer between the mathematics of the investigator and the work of the drawing-office. He could well believe that the ordinary designer did not wish to have to apply the methods of harmonic analysis on every occasion that he made a design, nor would it be necessary. He warmly congratulated Professor Inglis on having made a new departure which would provide direction to future work on the subject, and serve as a guide in the interpretation of all experimental results.

Mr. Fereday.

Mr. H. J. FEREDAY remarked that the relief of stress in main girders, dealt with in Mr. Remfry's Paper, had been put, as a permissive clause, in the specification for Girder Bridges (No. 153, Part 3), recently issued by the British Engineering Standards Association. The relief was most evident when there was a plated floor, and still appreciable when there were only cross girders and rail-bearers; and, as a matter of fact, the main girders were not so heavily stressed as calculations would indicate, even when there was no floor at all. Mr. Fereday had been associated with the tests made, under the auspices of Colonel Pringle, for the Ministry of Transport. Controversy had raged ever since, as to the discrepancy observed between the stresses as calculated and those recorded by the stress-recorders. Unfortunately, he was unable to reconcile them, because, except in one case, the deflections under load were not taken; but he had been successful in reconciling many of the stresses in the bridges tested in India by the Railway Bridge Committee, by taking certain precautions and adopting certain methods. The agreement between a modification of the calculated axial stresses in the whole cross section of a member and the corresponding stress in the whole member, deduced from the recorded fibre stress, was within about 5 per cent. When a train came on to a bridge, deflection took place in the main girders, and that deflection was a measure

Mr. Fereday of the work done by the train; but it was always less than the deflection as calculated by the principle of least work, because that principle was based on rigid-body statics and assumed that the whole of the work done was axial deformation of the members. Rigid-body statics could not be accurately applied to an elastic structure. He found that the ratio of actual to calculated deflection in the Indian bridges ranged from 0·75 to 0·85, depending on the type of bridge. Now, if the whole calculated stress in a member of any bridge were multiplied by this factor, the true axial stress was obtained, and it could be compared with that deduced from the fibre stress recorded by stress-recorders. Mr. Remfry stated (p. 206) that what was so recorded was the extreme stress over the minimum section of a tension member, compared with the average stress over a length of 20 inches as recorded by the stress-gauge. Mr. Fereday preferred to say that the fibre stress recorded was the stress in tons per square inch on the average section of the member between the points of the instrument. To obtain the total stress in the member, the average cross section of the member must be multiplied by the recorded fibre stress. That was the correct method of making a comparison, because calculated stresses per square inch, particularly in the net section of a member, could not be compared directly with the recorded stress. If any attempt were made to compare them, and agreement were found, it was only a coincidence. Somewhat similar comparisons must be made in comparing stresses in plate girders.

He did not know whether to admire more the mathematics or the deductions in Professor Inglis's Paper. When he commenced reading the Paper, he had not understood how the method of harmonical analysis could be applied to statical deflection, but, following the mathematics, he found it, of course, correct. He compared the method employed by Professor Inglis with the old formula which had been in use so long. By taking three terms of the converging series, he found the comparison exact. The method, he thought, was a very elegant piece of mathematics. The most interesting and speculative part was the reason why the vibrations did not increase unduly as a train ran over a bridge. He thought there were many reasons for that. In the first place, there was the natural frequency of the bridge; secondly, the variation of that frequency when that part of the locomotive which was not on springs travelled across; and, thirdly, the fact that the greater part of the load was spring-borne and had a summation of general frequencies of its own: so that the actual vibrations occurring in a bridge when a train passed over were extremely complicated. Another reason which might be

Mr. Fereday.

taken into consideration was the momentum of the locomotive itself. He had worked out the calculations for a 105-foot span under the heaviest locomotive in India; the two together giving a critical speed of about 41 miles per hour. The statical deflection under the

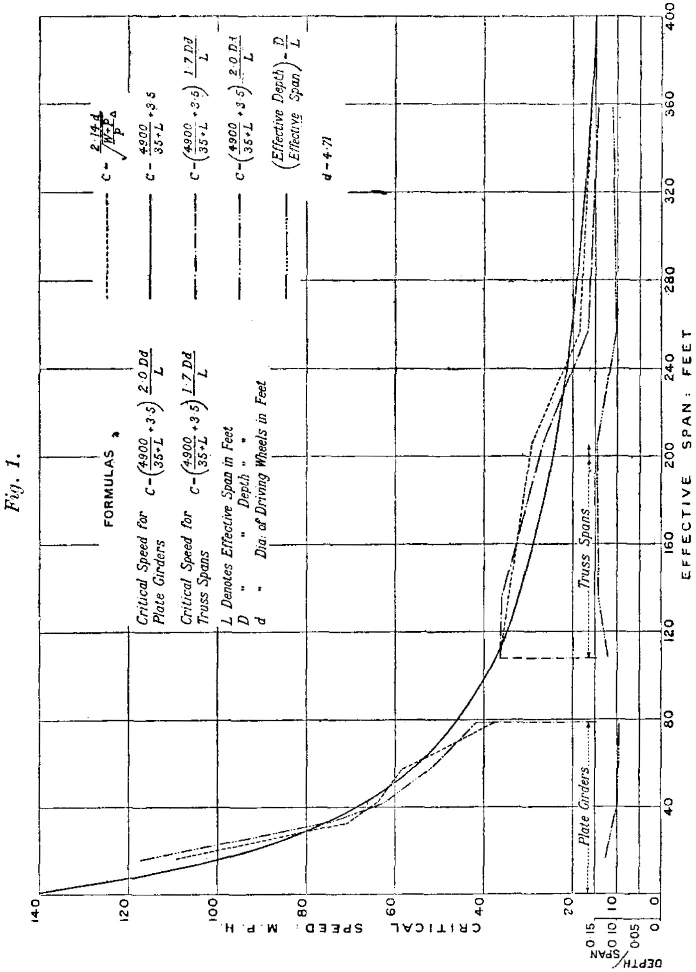

Fig. 1.

locomotive was 0·77 inch; the extra deflection corresponding to the stresses of impact effect under the Pencoyd formula was 0·575 inch. This deflection was equivalent to 116 inch-tons of work, and he had assumed that this was performed by the locomotive in half the time of crossing the bridge. Putting this roughly into horse-power, the

Mr. Fereday. locomotive had to develop in some way about 50 HP. more in crossing the bridge than when running along a level track. He could not imagine the locomotive, while passing over the bridge, developing 50 HP. and allowing the vibrations to accumulate. Professor Inglis stated that the frequency of a bridge increased approximately in the inverse ratio of the span. Mr. Fereday had calculated the critical speeds for all the bridges tested by the Indian Railway Bridge Committee, from the data they gave, and had plotted them in *Fig. 1.* The ordinates to the dotted line represented the critical speeds. The full-line curve was one of inverse ratios based on the economical depth of the spans, and corresponded only very approximately with the actual results. The bottom curve represented the ratio of depth to length for all the spans. By applying this bottom curve to the inverse-ratio curve, it would be seen that there was remarkable agreement between the actual and the modified calculated critical speeds. He therefore thought it more correct to say that the critical speed varied directly as the depth and inversely as the span squared. That was of some importance, because a formula for impact effect must embrace plate-girder spans in which the depth was as little as one-fourteenth of the span, and truss spans in which the depth might be as much as one-fourth. Professor Inglis stated that he had explored many paths and had scrapped them because they led to nowhere. That certainly showed a high gift of imagination, which was so necessary in research work. He himself might also exercise some imagination and predict to some extent what the new impact formula was likely to be. He was sure the gentlemen who were getting out this formula would produce a very scientific one: he was afraid, however, that there would be one factor in it which would convert an otherwise beautiful expression into an empirical formula, which, as was well known, was an anathema to all scientific men.

Professor Dalby. Professor W. E. Dalby remarked that no one could doubt the value and importance of Professor Inglis's Paper, not only from the point of view of the methods used, but also because of the clearer view which he had presented of the actions and reactions between moving loads, impact, and deflections. Few engineers, unless they had specially studied the question, realized what a powerful means of investigation was offered by the Fourier series, which could represent any periodic function and was useful in many problems, e.g., the distribution of power between the cylinders of a four-cylinder dynamically-balanced steam-engine so as to give as uniform a turning-effort as possible. Professor Inglis had for the first time, he believed, tackled the problem of moving loads on bridges by means of that series. In his statement on p. 227 Professor Inglis was

Professor Dalby.

unduly modest. Both the method he had adopted and the results he had achieved were, so far as Professor Dalby knew, distinctly new, original, and of great value. Without going into the details of the investigation, one thing stood out: Professor Inglis brought out with certainty and clearness the fact that, if any progress was to be made in the subject of impact on bridges, then the wave-form corresponding to the forced oscillation produced by the application and removal of the weight on the cross girder as the load passed over it must be combined with the wave-form corresponding to the free vibration of the girder. It was, in fact, the mathematical problem of the combination of forced oscillation with a natural oscillation, and carried with it all the known consequences. The elements involved in such a combination were[1]: the frequency of the forced oscillation; the frequency of the natural oscillation; and the damping coefficient, which was usually taken as proportional to the velocity of the parts. He concluded that one of the problems to be dealt with in bridge oscillation was whether any data could be tabulated from which damping-coefficients were likely to be deduced. Considering a bridge as a whole, the vibrations produced by the load were not necessarily vertical: a large girder could vibrate vertically or laterally. He had been observing recently the deflection of a bridge due to the passage of an engine passing over it at various speeds. The deflection was practically constant at speeds up to 50–60 miles per hour; in one experiment, however, when the engine was travelling at about 80 miles per hour, the deflection seemed hardly as much. But there was more energy stored in the bridge, and he noticed a considerable lateral vibration, in addition to the vertical vibration, produced by the passage of the engine. Were these lateral vibrations produced by the swaying action of the engine, or were they transformed from vertical into lateral vibrations after the manner which was evident in the oscillation of a Wilberforce spring?

Professor Inglis.

Professor INGLIS remarked that Mr. Remfry called attention to two ways in which the deck system of a bridge influenced the main girders, one of the effects being beneficial and the other the reverse. The decking, owing to the fact that it possessed some longitudinal strength, no doubt assisted the main girders to resist bending-moments. The bending-moment introduced into the vertical posts at their attachment to the cross girders constituted an unwelcome, though perhaps necessary, intrusion. He considered that Mr. Remfry

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[1] The speaker demonstrated by means of apparatus how the final amplitude of oscillation of a beam supporting an alternating load depended on these factors, and also the effect of hammer-blow due to the balance-weights of a two-cylinder locomotive.

Professor Inglis.

had performed a valuable service in giving prominence to these two particular effects; but, if he might be allowed to make a mild criticism, he thought the value and interest of the Paper would have been enhanced if the treatment had followed rather more general lines. The investigation dealt with particular cases, and particular cases formed a treacherous ground for general conclusions. The bending-moment set up in a vertical post due to its attachment to the cross girder could be estimated by the formulas:

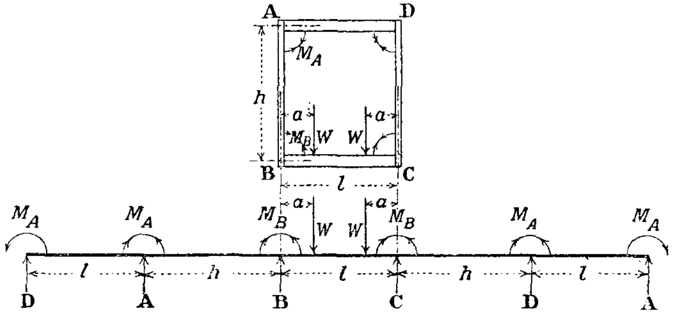
$$M_B : M = 1 : \left(1 + \frac{h}{l} \times \frac{I_{BC}}{I_{AB}} \times \frac{2lI_{AB} + hI_{AD}}{3lI_{AB} + 2hI_{AD}}\right),$$

and

$$M_A = - \frac{1}{2\left(1 + \frac{3lI_{AB}}{2hI_{AD}}\right)} M_B.$$

In this formula I_{AB}, I_{BC}, I_{AD} were the moments of inertia for the sections of the members AB, BC, and AD depicted in *Fig. 2*. This formula was readily obtained when it was recognized that the bending-moments required to maintain the right angles in the rectangular framework were the same as the bending-moments introduced over the points of support for the continuous beams DABCDA indicated in *Fig. 2*. Mr. Remfry's determinations of M_B involved much laborious calculation, and even then the stiffness of the overhead cross member was neglected. The application of the general formula written above would have simplified and shortened the calculations to a considerable extent. The problem of the longitudinal rigidity of the decking also admitted of generalization. An idealized decking which had approximately

Fig. 2.



M is the bending-moment required for the complete fixation of the ends of BC.

For the case shown, $M = \frac{W(l-a)a}{l}$.

Professor Inglis.

the same elastic properties could be achieved by considering a continuous distribution of cross girders and stringers having the same average lateral and longitudinal strength respectively as the cross girders and stringers in the actual decking. It could then be deduced that the equivalent section of the decking was approximately $\frac{1 \cdot 6 l^{0 \cdot 50}}{b^{0 \cdot 75} p^{0 \cdot 25}} I^{0 \cdot 25} \mathrm{A}^{0 \cdot 75}$, assuming the ends of the cross girders to be fixed rigidly, and half this value if the ends were free. In this formula l was the length of the deck, b its breadth, A its cross section, I the moment of inertia of a cross girder for lateral bending, and p the pitch of the cross girders. He did not feel enthusiastic about Mr. Remfry's suggestion that the decking of a bridge should be designed to assist the main girders. The ideal to be aimed at, was the simplification of the interactions between the various members ; and in the ideal structure all such interactions should be capable of computation by the principles of rigid-body statics. Rather than take the step proposed by Mr. Remfry, he would prefer to see the decking hung from the main girders in such a way that the vertical posts could not receive any bending due to the attachment of the cross girders. The relief provided by the decking might form a reasonable pretext for prolonging the life of a bridge which was in the last stages of senile decay, but he contended that in the design of a new bridge the duties of the main girders and the duties of the decking should be kept distinct, and any attempt to make these duties overlap seemed in his opinion a retrograde step in the evolution of bridge engineering.

Mr. Southwell.

Mr. R. V. SOUTHWELL remarked that Professor Inglis, in addition to stating the analytical theory, had made a start with the experimental verification of his results, and had done so with a model. Anyone who had compared *Figs. 5* and *7* in his Paper must have felt increased confidence in the theory of elasticity, which confidence, he believed, was always warranted if the theory was applied in its proper sphere, and consideration was limited to small strains. That could be done with problems of vibrations and elastic stability, but could not properly be done in dealing with questions of ultimate fracture. Since the experiment cited had given such satisfactory results, he felt that it should be very carefully considered whether useful information could not be obtained by something which would be a guide to full-scale research—not in any way meant as a substitute for it—by experiments on models. It seemed to him that the application of dimensional theory to the problem of impact in bridges was much simpler than to problems in marine architecture or in aeronautics. If the load on the bridge was reduced

Mr. Southwell. in the ratio of the square of the scale reduction, and if that load crossed the bridge at the true speed, then all the conditions for dynamical similarity were satisfied, and the stress at corresponding points was the same at corresponding intervals of time. Should it be found impossible to advance the model across the bridge at the full-scale speed, dynamical theory suggested that the difficulty could be obviated if the effective density of the model bridge were increased without increasing its stiffness, by loading the girders on the model bridge. Mr. Gribble had mentioned that the problem was about 70 years old: looking up the literature of the subject the other day, he found that model experiments of the kind which he was suggesting had been put in hand at Cambridge 70 years ago by Professor Willis, who evidently realized the importance of the inertia (that was, of the free period) of the bridge, because he provided for varying the effective inertia of his bridge by methods similar to those Mr. Southwell had suggested. The main conclusions he had gathered from Professor Inglis's Paper were :—(1) That only theory could give a rational impact formula, which would be safe in application and yet be fair to bridges of such span that resonance could not occur under practical conditions. (2) That the dominant factors in the problem, so far as the bridge was concerned, were the natural frequency and the damping. Once those quantities were measured, it would be a question for consideration whether they were not to some extent within the control of the designer. It was very doubtful whether he could control the frequency: there was a dimensional law which limited his design. But he might increase his damping artificially, and it would be very interesting to see whether, as a result of the work now in progress, bridges would in fact be provided with better damping than they had at present. (3) That the stresses in the main members of the bridge were almost certainly deducible from the central deflection, in the sense that the stresses in the main members would keep step with the deflection and would be almost in proportion with it. A somewhat controversial point which he wished to put forward was that, in his opinion, it was quite essential in connection with such work to separate the two problems of (*a*) impact effect, and (*b*) the uncertainty introduced by fixation of the joints. If measured stress were compared with the theoretical stress, then uncertainties both as to the impact figure and as to the stress-calculations would be present. He felt very strongly that the impact problem must be separated, and that the measured stress under the rapidly-advancing load must be compared with the measured stress under the crawl test, so that any uncertainties in regard to the stress problem on

Mr. Southwell. the bridge itself were eliminated. The question then arose whether the crawl deflections were what theory would dictate; but that seemed to him an entirely separate problem. Therefore the numerical measurement of stress was not so important for the impact problem as a stress-measurement which could be relied upon to give the relative values of the crawl stress and the impact stress; and, if it was not necessary to have the exact numerical values, but only the exact ratio of the two stresses, there was little doubt that the problem of measurement was immensely simplified.

Mr. Wilson. Mr. J. S. WILSON remarked that in 1850 a very interesting discussion[1] took place on early work done in connection with the subject. The discussion was interesting because the members of The Institution and the Council were on the point of taking "direct action"! The Commissioners of Railways had not passed the Torksey bridge because the stress in it exceeded 5 tons per square inch. During the discussion Mr. G. P. Bidder remarked that "He agreed in the observations on the small effect of vibration, by railway trains, passing over bridges; he believed it to be a mere ghost, raised by mathematicians to frighten engineers as to the strength of their structures, and he thought the engineers were bound, as standing between the mathematicians and the public, to apply to their deductions the principles of common sense." He did not wish in any way to disparage the mathematical part of Professor Inglis's Paper; it was a communication of the greatest importance, and he thought The Institution was to be congratulated on the Paper and the valuable discussion that had followed.

Mr. Remfry. Mr. REMFRY, in reply, remarked that studies such as those made in his Paper were of far greater value when considering old bridges, which in course of time had to carry much larger loads than those for which they were originally designed, than they were when considering new spans. This point was brought out most strongly in Mr. Palmer's remarks. For instance, in the case of the single bridge of fifty-six spans of 150 feet each, studies such as these, taken in conjunction with strain-gauge investigations, might possibly prolong the life of the structure for, say, 10 years. To replace such a structure now might cost, say, Rs. 30,00,000, or £150,000. This, at compound interest for 10 years at 5 per cent., would be increased by about 60 per cent., and the delay might save the railway administration about Rs. 18,00,000, or £90,000, if prices did not rise in the interval. It was quite possible that the original girders of such a bridge had cost only little more than half

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[1] Minutes of Proceedings Inst. C.E., vol. ix, p. 233 *et seq.*

Mr. Remfry. the cost of replacement now with heavier girders. If that were so, the amount saved by a more rigid investigation of the actual stresses in bridges and the possibility of prolonging their individual lives by, say, 10 years, might save railways an amount equal to the original cost of the girders in place. This amount Mr. Palmer estimated as 6 per cent. of the original cost of the railways. The Indian Railways had cost £465,000,000, and it might be possible by investigations on interaction to save the equivalent of £28,000,000. The calculation made by Mr. Palmer, that in the limit the saving in the capital cost of railways in a country like India obtained by applying methods such as were advocated in Mr. Remfry's Paper might be 0·6 per cent., was very suggestive. But, even if the saving were half of this, or 5 per cent. in the weight of each large bridge, it would be worth striving for. The capital cost of the railways of India was £465,000,000, and 0·3 per cent. of this was £1,400,000. He wished to emphasize the fact that, although a present saving of 5 per cent. in the weight of new bridges might or might not be considered worth while, if, by the study of the interaction of the parts, bridges could be so constructed that they would have a reserve of strength which would lengthen their lives, there should be no doubt that the design ought to be such that the reserve would be available when wanted. Some engineers pinned their faith to the theory as to frictionless joints, and disliked a design in which the stresses did not readily admit of computation; they would prefer to throw away any possible assistance obtainable from the deck, by having sliding joints in the rail-bearers and pin connections for the cross girders. Such expedients might render it possible to evaluate stresses in the main girders with greater accuracy, and in the case of really large spans, or even of spans over 200 feet, they might be necessary; but in many cases such expedients merely threw away possible sources of assistance and led to the design of structures of a less rigid and reliable type. The experiments made by Messrs. Gribble and Bengough were very interesting. It was certainly surprising that in the case of a bridge with a deck plate the experiments did not indicate that the deck plate helped the main girders. Possibly in the case in question the riveting of the deck plating was loose; that in the connections of the cross girders was stated to have been so. Loose rivets introduced a give which altered the distribution. Such give in the riveting would be much more important in a small span (such as the 50-foot span experimented on) than in a larger one. A working of the rivets in the rivet-holes amounting to $\frac{1}{30}$ inch towards the ends of the span would make the plating in a 50-foot span useless for helping

Mr. Remfry.

out the main girders when loaded to produce an extra stress of 2 tons per square inch in the booms. In the case of a 150-foot span, the corresponding give in the rivets at the ends would have to be $\frac{1}{10}$ inch. The experiment gave very unexpected results, as it was scarcely to be imagined that even in the rivets in the deck plating of an old span a give of as much as $\frac{1}{30}$ inch could occur. With regard to the fixity of the cross girders which were tested, Mr. Gribble had not said whether the bridges in question were deck spans or through spans. In the case of through spans, even with absolutely sound riveting, the fixity of the ends of the cross girders was not large. Mr. Remfry's calculations showed, in a 150-foot through span, 11·3 per cent. fixing, falling to 8·5 per cent. when the overhead bracing was removed and the breathing of the upper booms was allowed for. Further calculations, taking into consideration the resistance of the main booms to twisting, increased these fixing-percentages by 27 per cent., or to 14·4 per cent. and 10·6 per cent. respectively, for a through span with or without overhead bracing. The importance of the twisting of the cross girders, referred to by Mr. Gribble, was not to be overlooked. The approximate formulas suggested by Professor Inglis would certainly be helpful. The work involved in solving the stresses in a cross frame was, however, not very much less than that needed in Mr. Remfry's method. Further, it could not take into consideration the additional assistance afforded to the fixing of the cross girders by the resistance to twisting offered by the booms. A study of these formed part of the original Paper; but, for want of space, it was omitted from the printed copy. In an investigation of interaction, generalization was misleading, and each case must be considered on its merits. For example, although the formula suggested by Professor Inglis for a rough approximation to the longitudinal rigidity of the deck might give some ideas as to what was occurring, which would be extremely valuable where no ideas existed previously, it would be very rough, and might be misleading. If the end cross girder differed markedly from the intermediate cross girders in lateral rigidity, taking the average would lead to a very different result. The variations in the boom section made a great difference. This factor did not figure in Professor Inglis's formula. It made large differences whether there was a deck plate or not. The deck plating made an immense difference to the lateral rigidity of the cross girders. It also made them vastly stiffer on one flange than on the other. This added to the labour of solution. Finally, the plane in which the deck lay, relatively to the plane of the boom which was helped, affected the question. He certainly regretted that he

Mr. Remfry. had only had time to touch the fringe of the subject in his Paper. The examples taken represented fairly general practice over a large railway-system embracing 2,750 miles of track in India. It would be absurd to suggest that the results could be applied generally; they were applicable only to girders designed somewhat similarly. They were, however, he hoped, suggestive enough; and sufficient material had been given, even in its abridged form, to assist others to apply similar studies to their own bridges.

⁂ Professor Inglis's reply will be found at p. 316.—SEC. INST.C.E.

Correspondence.

Professor Bulleid. Professor C. H. BULLEID observed that Professor Inglis, in his interesting and valuable Paper, showed how important it was to be able to calculate the frequency of the free oscillation of a girder. The following remarks were offered to facilitate the rapid estimation of this quantity. If a mass m were placed on a massless girder, its weight would cause a certain deflection δ. If now the girder executed vertical oscillations, the period could readily be shown to be $2\pi\sqrt{\frac{\delta}{g}}$ seconds. The number of oscillations per minute, with δ expressed in inches, would be, therefore, $\frac{187}{\sqrt{\delta}}$. Now the free period of a uniformly-loaded girder was $\frac{2}{\pi}\sqrt{\frac{wl^4}{gEI}}$ seconds, and its static deflection was $\delta = \frac{5}{384}\,\frac{wl^4}{EI}$. Hence the free period was $\frac{2}{\pi}\sqrt{\frac{384\delta}{5g}}$ seconds, and the number of oscillations per minute was $\frac{60\pi}{2}\sqrt{\frac{5\times g\times 12}{384\delta}}$, where δ was measured in inches, which reduced to $\frac{212}{\sqrt{\delta}}$. The simple formula $\frac{200}{\sqrt{\delta}}$ lay between these values for the two extreme cases, and differed from them by only 6 per cent. Hence the number of oscillations per minute of a girder, however loaded, might be calculated with sufficient accuracy by dividing 200 by the square root of its static deflection in inches. In calculating this deflection, account should be taken of the weight of all masses which oscillated with the girder. This would appear to include the weight of the girder itself and of all non-spring-borne loads (but not the spring-