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-

Professor Bulleid.

borne) for the vertical oscillation; and for the horizontal oscillation the girder should be imagined laid on its side and deflected by its own weight, but no account should be taken of the wind load, which did not add to the mass. This was a well-known method of calculating the free period of such bodies as turbine-rotors, but as he had never seen it referred to in connection with bridges, it seemed worth while to call attention to it.

Mr. Fraser.

Mr. W. A. FRASER remarked that Professor Inglis was to be congratulated on the excellent Paper presented by him to The Institution. The importance of impact allowances and their amount was a matter which had occupied the attention of bridge-engineers for a considerable time. All concerned with the vexed question of impact in its application to the design of new railway underbridges, and the calculation of the strength of existing railway underbridges, would welcome the mathematical truths expounded, and would hope that, so far as practicable, the testing of structures with the aid of accurate recording-instruments would be directed towards establishing these truths from a practical standpoint. The work in the Paper was based on the deflection of structures, and not on the stresses developed in the material; and it could be safely accepted that comparative deflections were a generally reliable indication of the probable comparative stresses. Many of the conclusions drawn in the Paper accorded with the views of bridge-engineers, but the very low values of impact and the comparative absence of live-load effect, for practical speeds, were contrary to the views generally held. The case of the 24-inch by $7\frac{1}{2}$-inch rolled steel joist gave a remarkably low impact value, which was almost negligible. The influence of axle-spacing was clearly shown on pp. 258 and 259, and the remarkable absence of live-load effect for the closer spacing of axles was interesting. The expression developed for the central deflection, $\frac{2P_1l^3}{\pi^4EI}(2Nn_0^2)$, contained all the essentials of a satisfactory impact formula, and was an improvement on most of the existing formulas, which, in many cases, included only one variable factor. The nature of the driving-wheels and their size were very important factors, as the effect of rotating masses representing the balance-weights had a considerable bearing on the impact effect. The proposed formula for impact effect, as distinguished from live-load effect, embodied all the principal factors bearing on the question. With the advent of new and larger locomotives, the necessity for establishing a satisfactory impact formula once and for all was of first importance. Within the last year the weight of locomotives had increased from about 120 to

Mr. Fraser. 147 tons, a fact which of itself showed the importance of establishing a reliable impact formula, so far as the maintenance of existing structures was concerned. The introduction of heavier locomotives had drawn attention to the necessity for more scientific balancing, so as to compensate, by a reduction in the impact, for the effect of the increased loads. It was perhaps too much to hope that the amount of relief which balancing of engines would afford could be set off against the extra loading which had to be carried by the structure. Yet this was what would seem to be implied in the Paper under review. If, then, such a result could be demonstrated by practical testing, the results would be valuable.

Mr. Remfrey had made a most interesting contribution to the literature on the subject of the stresses in bridges. Mr. Fraser had had a good deal of difficulty in the past in reconciling the calculated stresses in the chords of trusses with the actual stresses ascertained by the use of accurate measuring-instruments. By making allowances much on the lines laid down in the Paper, a reasonable amount of agreement between the calculated and observed stresses was established.

Mr. La Touche. Mr. J. N. D. La Touche observed that in the course of his duties as Inspector of Railways to the Government of India, he had taken a large number of deflection diagrams of girders, over a considerable range of spans. These diagrams were taken on cards, placed in a plane at right angles to the axis of the girders, by means of a sharply-pointed pencil held in an arm clamped to one boom. (He had found a needle and a smoked brass plate better than the pencil and card.) In no case could he remember to have seen signs of oscillation in the vertical plane; on the contrary, the even movement of the pencil, especially in the cases of the larger spans, was remarkable. Of course, in a short span the deflection took place quickly, but always smoothly. Any tendency to vertical oscillation was damped out by the weight of the moving load. There was generally well-marked horizontal vibration, mainly in the lower half of the diagram; but this was seldom of much extent, the maximum being about $\frac{1}{8}$ inch. The effect of the passing of the train seemed to be similar to that of pulling a violin string to one side and holding it there, rather than to the vibration set up by the bow. He had often thought that the dynamic effect of the moving load as a whole—that was, apart from the effect of the balance-weights in the driving-wheels, lurching, and such secondary causes—depended on a comparison of the time in which the weight would fall through the height of the deflection with the time taken by the load to run half-way across the span; and that this effect would be greatest

Mr. La Touche. when these times equalled the natural period of vibration of the girder. It had been shown by strain-gauge tests that the maximum effect was produced on all spans at a speed of about 40 miles per hour, which seemed to point to some cause of this kind, as the time of fall, and that of a single vibration, would vary nearly directly with the length of span. He would like to add a word of warning as to crawl tests. In some hundreds of trials with his strain-gauge he had found it impossible to get consistent crawl readings, due he believed to the lurching of the engine, which could not be avoided, even at the slowest possible pace. He finally gave up this test, and compared the observed stress under the moving load with the calculated stress under the same load at rest, which, after all, was what was wanted.

Mr. Leitch. Mr. W. O. LEITCH remarked that the Papers seemed of great interest, that on oscillations and impact being of special importance. One of the conclusions, however, namely, that too great an allowance had hitherto been made for impact on short spans, did not seem to be acceptable. Where bridge decks were enforced by law, and ballasted track was carried over the bridge, the impact effect was reduced, but there must be throughout the world a large majority of bridges in which the bridge-ties, or longitudinal timbers, rested directly on the girders. A front view of an approaching locomotive showed plainly the unsteady lurching motion; and on viewing short spans, say up to 30 feet, from below, the girder was seen to receive a considerable shaking, quite severe in the case of spans under 20 feet with free ends. Embankments were apt to settle after heavy rains, but the abutments did not do so, and the result might be to cause unsteady running on the short-span girder. It had also become customary to lay track with staggered instead of square joints, and as even the most perfect joint must yield a little, the average joint probably set up oscillations, which might be important. Consequently, for short spans it seemed premature to reduce the old impact allowance. Even for longer spans, in the case of, say, 100-foot-span deck girders, with end diagonal members, so that the girder was supported at the top, oscillation sideways of the bottom flange took place ; hence it would appear to be necessary to continue the investigations by taking a large number of deflections and movements of bridges in service. Where bridge-decks were not enforced by law, it was very doubtful if the metal in plated decks was used to best advantage ; would it not be better to put it in the girders ?

Mr. Lindenthal. Mr. GUSTAV LINDENTHAL remarked that in the past insufficient attention had been given to the fact that the floor system participated in the chord stresses from passing loads. The stresses were

Mr. Lindenthal. assumed to be negligible, until the discovery of lateral fractures in the flanges of floor-beams, which could be produced only by lateral bending. Such was the case, for instance, in the floor-beams of the big truss bridge of the Baltimore and Ohio Railroad over the Susquehanna river. The floor-beams rested on top of the top chords of a deck bridge with 500-foot spans. When the stresses were analysed, it soon became evident that, as the top chords shortened under compression, the floor-beams were subjected to heavy lateral bending stresses, which caused lateral fractures in the flanges of the floor-beams near the riveted connections with the outside stringers (rail-bearers). When the old bridge (too weak for the increasing loads) was replaced with a new bridge, care was taken, in the new floor system, to provide all stringers at one end with a sliding connection, to avoid lateral bending stresses in the floor-beams. The floor system was now independent of the stresses in truss chords. The Paper by Mr. Remfry dealt only with the stresses induced by expansion and contraction of the chords. But the lateral bending stresses in the floor-beams due to the tractive force of locomotives or to braking of trains were as important, and sometimes larger. There were also temperature stresses when the floor system was covered by the track structure and the chords were exposed to the sun, so that chords and stringers were subject to different rates of temperature-change. It was the practice in the United States to analyse these different lateral forces in the floor system, and to design the details in such a way that harmful bending stresses were avoided. In most cases the stringers had riveted connections at one end and sliding connections at the other end. The diagonal wind-bracing in the plane of the floor between the truss chords was, as a rule, riveted to the floor system, so that it might take up the traction or braking forces and transmit them directly to the chords and to the truss bearings on the abutments or piers, rather than allow these forces to travel through the stringers, which would cause the kind of bending stresses in the floor-beams analysed by Mr. Remfry. Deck and buckle plates had expansion-joints where necessary, with bolt connections in slotted holes, which permitted of sliding.

Turning to Professor Inglis's Paper, the difficulties and complications in the deduction, by purely mathematical research, of a rational formula for impact and oscillatory stresses in iron bridges had long been recognized. The first known investigation of this kind was one by Dr. H. Zimmermann in Berlin in the early nineties, but his method had not found general acceptance. Professor Inglis stated that theory indicated that a rational formula for impact allowance should at least take account of :—

Mr. Lindenthal. (1) The length of the bridge; (2) the natural fundamental frequency of the bridge; (3) the nature of the balance-weights on the driving-wheels; (4) the size of the driving-wheels; and (5) the axle-loads and spacings. Iron railway-bridges were not built for any specific nature of balance-weights on driving-wheels, or specific driving-wheels, or specific axle-loads and spacings. In short, bridges were not built for laboratory use, but for the rough work of transportation, and they should be strong and safe enough for all the ordinary variations and incidents of loads and speeds. For practical engineering, the impact formula must give a maximum value, including all kinds of loads, different speeds, and different groupings. The difference between a structure dimensioned according to highly refined analytical methods for a certain set of assumptions and a bridge dimensioned as a safe and durable structure for every kind of ordinary railway load involved no advantage worth gaining. The former did not save material or cost of labour, or give better service. This, however, should not discourage analytical research work on the problem of impact stresses. Every fact and relation brought out by patient theoretical investigation and experiment added to knowledge, and enabled the engineer to judge more surely whether he was on the safe side of the problem. In the United States the need of an impact formula was met on an empirical basis more than 20 years ago, by the so-called Pencoyd formula. It agreed fairly well with observed facts in spans of middle length, but gave results too small for short spans and too large for long spans. Mr. Lindenthal had deduced, therefore, for his practice, an impact formula which gave results more nearly in agreement with observed behaviour of structures under heavy and fast train-loads, on all lengths of span, and with any number of tracks. Particulars of this method were published [1] in 1912; a reprint, embodying his recent amendments, was in the Institution library.

Mr. Lloyd-Jones. Mr. C. W. LLOYD-JONES considered Professor Inglis's Paper to be an important contribution to a subject which had been engaging the attention of bridge-engineers for many years. The Author had placed the mathematical theory of the subject on a satisfactory basis, and had indicated the method for any further development which might be required. The distinction between "impact allowances" and "live-load allowances" was a suggestion which might usefully be accepted to distinguish between the effect of a smooth-running load and a load whose intensity varied with time. Although the Author

[1] *Engineering News*, vol. lxviii, p. 216.

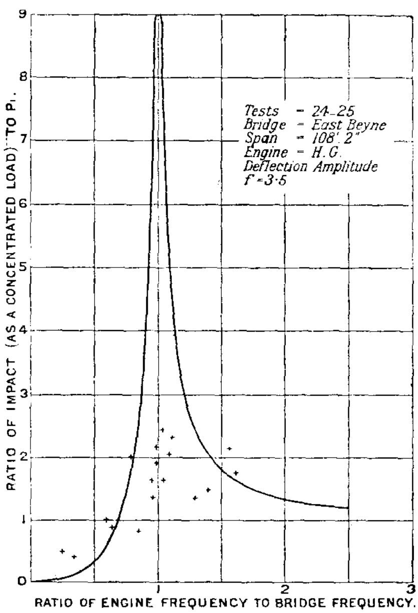
Mr. Lloyd-Jones. expressed several of his results as a percentage of the total live load, he took care to point out that an impact factor based on the total live load was theoretically unsound. That was the view emphasized by the Indian Railway Bridge Committee in its Fourth Report. The different effects of spring-borne live loads and of non-spring-borne live loads, to which the Author drew attention, had not always been fully appreciated by writers on this subject in the past, and it had frequently been assumed that the total load on a bridge was always effective in reducing the frequency of bridge vibrations. The Indian Railway Bridge Committee, appointed by the Government of India in 1917, investigated *inter alia* the allowance to be made for impact in the design of railway-bridges. The Committee deduced certain theoretical relations, and carried out a considerable number of experiments, with a view to verify these principles and ascertain the values of the constants involved. The theoretical principles which that Committee attempted to verify were in accord with the results obtained by Professor Inglis, and the experimental results arrived at by the Committee were, therefore, of interest in connection with Professor Inglis's deductions. In the experimental and theoretical investigations of the Committee, the effect of impact was measured as a vibration of the bridge and also as a variation of the stress in both the chord and the web members; and in order to correlate these different measurements, the impact effect was expressed as a concentrated load which, when passing slowly over the bridge, would cause the same effect as that measured. That method was adopted because it was seldom, if ever, that the whole of the live load contributed to the effect measured, and in the case of vibrations set up by the unbalanced parts of a locomotive, a concentrated load would more nearly represent the force causing the vibration than would a uniform increase of all loads on the bridge. The Committee deduced from theoretical considerations that the live-load allowance due to a single load P coming on to or leaving a bridge was equivalent to a central load $P/2N$, and that a uniformly-distributed load rolling over the bridge would require no live-load allowance. In this expression N denoted the number of free vibrations of the bridge which would take place in the time taken for the load P to pass over it.[1] These results were the same as those obtained by Professor Inglis (p. 257). Professor Inglis considered that the maximum cumulative effect from a succession of loads was equal to four times

[1] Indian Railway Board, Technical Paper No. 225. Fourth Report of the Indian Railway Bridge Committee, vol. ii.

the effect of a single load. The Committee estimated the cumulative effect at $qP/2N$, where q was a numerical coefficient depending on the damping coefficient, probably having a value ranging from 6 to 9. With a view to verify this result, the Committee carried out a few experiments, which were referred to on p. 25 of the Fourth Report. The conclusion was that the live-load effects, measured experimentally, were in excess of the value deduced from the foregoing relations; but the experiments were too few to be conclusive. Most of the experiments were carried out by the Committee with a view to investigate impact allowance due to locomotives. The following were the theoretical formulas which the Committee deduced and

Mr. Lloyd-Jones.

Fig. 3.



attempted to verify experimentally. The method of arriving at the formulas was explained in Appendixes 1 and 2 of the Fourth Report. The notation used was that of Professor Inglis.

$$F = n_0{}^2 P_1 \left\{ \left(\frac{n_0}{nq}\right)^2 + \left(\left(\frac{n_0}{n}\right)^2 - 1\right)^2 \right\}^{-\frac{1}{2}} \quad . \quad . \quad . \quad (1)$$

At critical speed

$$F = qn_0{}^2 P_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

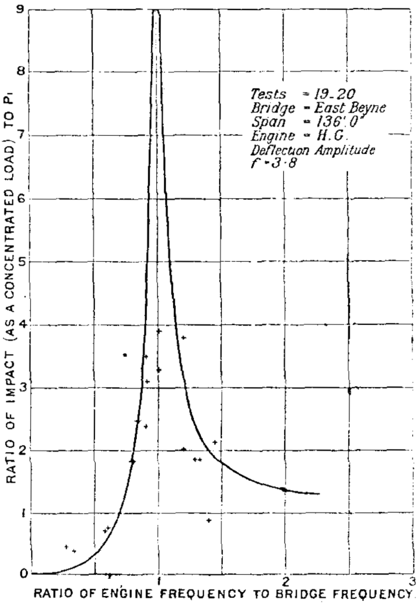
Mr. Lloyd-Jones.

Neglecting the damping coefficient and not at critical speed—

$$F = \frac{\pm P_1}{\dfrac{1}{n^2} - \dfrac{1}{{n_0}^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In these formulas F denoted the concentrated load passing over the bridge which would produce the same effect as the impact due to the locomotive, and q was a numerical coefficient dependent on the damping; q was found to be less than 9 for all the bridges tested. It was eventually concluded that for spans under 200 feet q depended

Fig. 4.

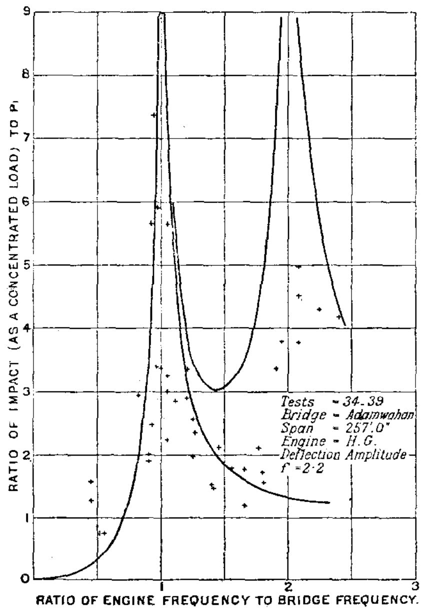


mainly on the number of revolutions made by the driving-wheel while the engine crossed the bridge.[1] The quantity P_1 was that defined by Professor Inglis, namely, the centrifugal force due to the balance-weight when the driving-wheels were rotating at one revolution per second. It would be seen that formula (2) was the same as that given by Professor Inglis on pp. 264 and 267, substituting $2N = q$. The Committee's experiments were devised to ascertain the values of q, n_0, and P_1, so that the particular interpretation of

[1] Indian Railway Board, Technical Paper No. 228. Fifth Report, p. 15.

q did not affect the investigation. Formula (3) might be derived from Professor Inglis's formula at the top of p. 265 by substituting $1/N = 0$. It was also identical with the result obtained on p. 241, if the free vibration was neglected. It was concluded, therefore, that the theoretical relations on which the Committee's experiments were based were not inconsistent with the results obtained by Professor Inglis. The results of those experiments were briefly the following:—The relationship between F and n was verified by a series of tests with the same engine and bridge at different speeds. Some of the results obtained in this way were shown in *Figs. 3–6* Mr. Lloyd-Jones.

Fig. 5.

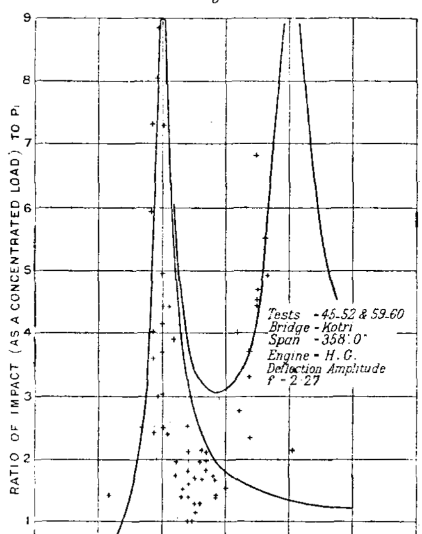


The impact effects were determined both as a vibration of the bridge and as a stress-measurement, and there was no essential difference between the results obtained from the stress-recorders and from deflectometers. Nearly all the diagrams, except those at critical speeds, when resonance occurred, showed a considerable variation in the frequency of vibrations, and vibrations of several different frequencies were usually superimposed. As a case in point, the experiments on the Kotri bridge, 358 feet span, carried out early in 1921, might be cited. Stress-measurements were made on five chord members and six web members, and deflection was measured

Mr. Lloyd-Jones.

at the centre and at one-quarter of the span. Four separate ranges of frequency could be detected, namely, 2 to 2·3, 6 to 8, 40 to 50, and 60 to 70. The slowest vibration was usually absent in the web members, although the vibrations having a frequency-range of 6 to 8 were strongly in evidence. It would be seen from *Fig.* 6 that there a second critical speed appeared to occur at double the bridge frequency, and not at four times the frequency, as would have been supposed. From the experiments carried out in 1921 it seemed extremely likely that, for bridges of more than 200 feet span, high-frequency vibrations might be of importance. The frequency of

Fig. 6.

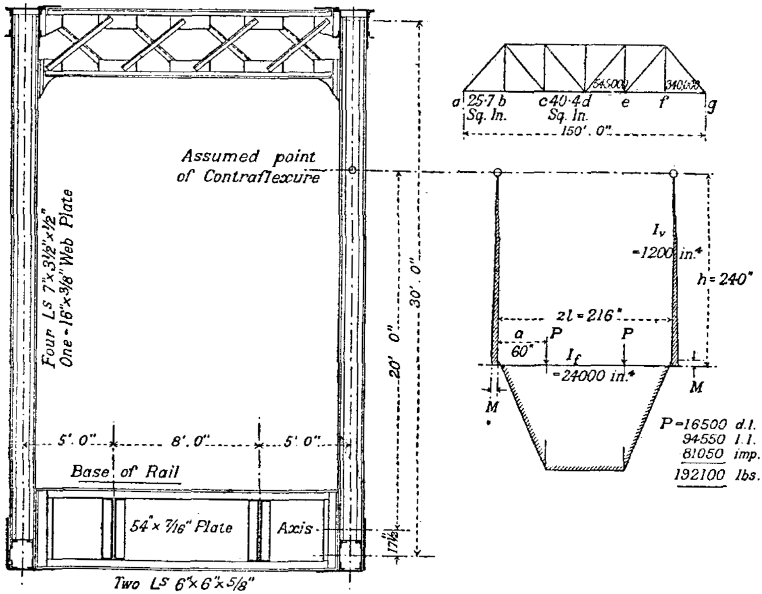


the Kotri bridge was 2·27, and it might be assumed that resonance effects would occur at a frequency of 9, and therefore the vibrations of this frequency might be of importance in the case of engines passing over the Kotri bridge at high speed. It was suggested that in such cases the foregoing formulas might still give a suitable impact allowance if F were calculated for the higher value of n_0 and the girder were assumed to have a third support at the centre. An allowance of this kind would have an important effect on the design of web members of large spans. It appeared unlikely from the Committee's test that the factor q was proportional to the span, as suggested in Professor Inglis's Paper. It seemed more probable that in the case of long spans q tended towards a value (about 9) determined by the damping-coefficient. For short spans in which $n < n_0$, formula (3) applied, but Mr. Lloyd-Jones had suggested that if formula (2) were considered as the product of a bridge coefficient (qn^2) and an engine coefficient (P_1), it would be sufficient to determine the former for different spans, and the latter for types of engines; and he was still of opinion that that was sufficient for practical purposes. Mr. Lloyd-Jones.

Mr. P. L. PRATLEY remarked that Mr. Remfry's Paper was a reasonable presentation of those features of bridge-construction and the resulting interaction of parts which depended upon more or less rigid connections. The question had engaged the attention of many engineers who were continually concerned with the design and maintenance of railway-bridges. Mr. Remfry was to be congratulated upon his courage in advancing opinions and assumptions based upon observation and "sensibility," as an alternative to theoretical considerations. Assumptions were often objected to by the self-styled practical man, but in reality a well-thought-out, or "well-felt-out" assumption, was often a valuable guide in a search for the true interpretation and understanding of facts, and should not always be classed as arbitrary. That the floor system must and did have its effects on the distribution of stresses in the main girders or trusses had long been accepted, but the question persisted, what was to be done about it? Mr. Remfry saw this point, and acknowledged the difficulty of dealing with these effects in the processes of design and manufacture. It was well, however, that such a Paper should be published, and at some length, in order to bring out in connected form some indication of the conditions actually arising, and some suggestions as to their analysis and treatment. Mr. Pratley would personally have preferred to see more of the calculations included in the printed Appendixes, as the early essentials in the study of such complex phenomena were an appreciation of relative magnitudes, whether areas, stresses, or stiffness-factors, and an idea of the range of their variation in practice. Apart from the results of deliberate standardization, bridge superstructures were individualistic, and the various features such as length of span, length of floor-panel, depth of girders or trusses, width of truss members, and nature of connections, all contributed in their own way, and in varying degrees, to the complexity of the whole fabric. Nothing less than a multiplicity of examples, covering closely the entire range of practice, could provide adequate material for study, or the necessary evidence as Mr. Pratley.

Mr. Pratley. to facts. The difference in general practice, in so far as it controlled principal dimensions, connection details, and workmanship, struck him as an all-important item. For instance, deck plates, referred to by the Author as common in modern practice, were a rare exception in Canadian or American construction and were practically confined to short solid-floor spans over city streets or canals. The fixity of cross beams and their lateral stability or flexibility were also largely affected by the governing practice. To illustrate this point, Mr. Pratley had selected a very usual 150-foot through truss span, similar to hundreds on the Canadian National or Canadian Pacific

Figs. 7.



Railways, and had made a few simple comparative calculations, enough perhaps to set alongside those published in the Paper.

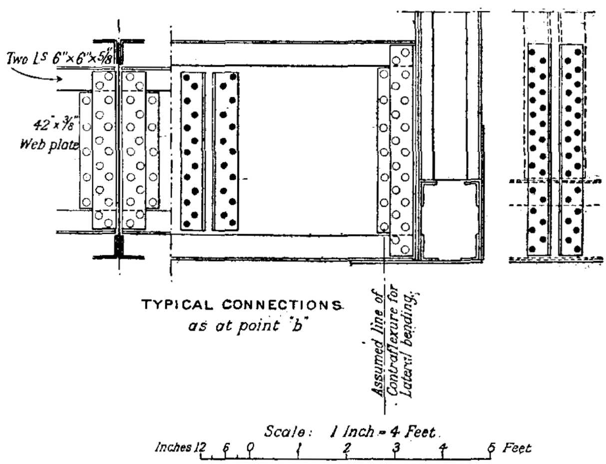
Treating, first, of the end fixity on the intermediate cross beams, and taking the actual dimensions and material shown in *Figs. 7*, the partial derivative $\frac{\delta W}{\delta M}$ of the work function, $W = \Sigma \int \frac{M^2}{EI} dx$, yielded, when equated to 0, the value—

$$M = \frac{Pa\ (2l - a)}{2l + \frac{2I_f}{3I_v}}$$

in which M was the fixity-moment acting on both the cross beam and the vertical. The effect of chord-twisting was neglected as unimportant. By examination of this expression it was seen that:—(1) as I_f/I_v decreased, M increased; and (2) as h/l decreased, M increased. In British and Indian practice, as contrasted with Canadian and American, both these conditions obtained. The cross beams were usually shallower, and the truss members usually wider, making I_f/I_v much less. The trusses themselves were also shallower as a rule, so that these dimension features led to relatively greater fixity and consequently greater relief on the cross beam in British and Indian practice than in American. For the case illustrated, the full moment on the freely-supported cross beam was $192,100 \times 60 = 11,526,000$ inch-lbs. The end moment, if perfectly fixed, would be $P\left(a - \frac{a^2}{2l}\right) = 8,320,000$; whereas the solution of the work differential gave $M = 2{\cdot}74 \times P = 526,000$, corresponding to 6·3 per cent. of fixity and providing only 4·56 per cent. of relief at the centre of the beam. The resulting fibre stress on the vertical was 3,500 lbs. per square inch. With such a small degree of end-constraint, the point of contraflexure was actually outside the girder, namely, at $2\frac{3}{4}$ inches from the axis of the truss, and the rivets in the connection had to carry a moment of about 1,134,000 inch-lbs. at the face of the truss member and some 2,042,000 at the centre of moment of the web connection. These moments could be accommodated without undue stress on the rivets, so that it might be asserted that this 6·3 per cent. of fixity represented fairly closely the actual situation developed in the structure. With regard to the lateral bending of the cross beams, the type of connection again played an important part. With no deck plates—indeed, with no connection at all between the flange of the cross beam and the truss—the fixity in the lateral plane was also very slight. Further, the connection of the stringers to the web-plate only of the cross beam was of very common occurrence in America, and was in striking contrast to the equal-depth, flange-fitted connections referred to by Mr. Remfry. Thus in the case illustrated, which was actual and very typical, it was problematical whether the full lateral stiffness of the cross-beam flange-angles was really available to cause or resist longitudinal stress in the stringers. From a "sensing" of the case, the point of contraflexure for lateral bending would seem to be at or near the rivet line in the filler plates outside the angles; that was, $7\frac{3}{4}$ inches from the back of the connection angles. Assuming this location, and using the full lateral moment of inertia, 10,000 lbs. at the stringer produced Mr. Pratley.

Mr. Pratley a deflection there of 0·44 inch relative to the end of the beam. Applying the method used by Mr. Remfry in Appendix I, the correct solution involved a longitudinal stresses of 7,750 lbs., 12,890 lbs., and 15,490 lbs. in the stringers *ab*, *bc*, *cd*, respectively, these figures being 2·28, 3·79, and 2·84 per cent. of the total bottom-chord stress, while the cross beams, all of the same section, and all assumed equally fixed, suffered lateral fibre stresses of 10,650, 7,050, and 3,580 lbs. per square inch. In the end cross beam the combined unit stress was 10,650 + 9,770 = 20,420 lbs., and in the second, at *b*, it was 7,050 laterally + 12,360 vertically = 19,410 lbs., using full impact for the vertical loading and maximum stresses in each case. Strictly, of course, these stresses were not possible simultaneously in all chords

Fig. 8.



and both cross beams, but the error was not great. The double-web construction of the end cross beam, adopted to reduce the lateral bending, was a practicable feature worthy of notice, but it must be used with care, as its effects were not wholly beneficial, particularly when it was applied to cases similar to that shown in *Fig.* 8. The continuity of stringers certainly existed, and was developed to a degree determined by the strength of the connections. In the examples cited by Mr. Remfry, the connections were generally capable of accommodating the full continuity moment, or even the full value of the stringer; but in the typical Canadian case they were limited in capacity to about one-third of the value of the girder at units within the bounds of elasticity. The neglect of this continuity value in designing did not remove it, and he was in full sympathy with Mr. Remfry when he suggested that not only should it be recognized, but the detailing should also strive to make use of it. By computing the stringer as freely supported, and then, by its web connection, inefficiently constraining it, bridge-builders were merely soliciting the loosening of connection rivets and a gradual breakdown of what was not merely an unrequired reserve bending strength, but also the only, and very necessary, shear-transfer device. Failures did not occur, it was true, in floor systems nowadays, but this was because the deck systems were usually too highly penalized by the impact formulas, and the provisions demanded by these considerations involved deeper and more closely riveted connections. Thus, at the expense of doubly unnecessary metal towards the centre of the girder, the neglected continuity was not only established but also protected. Thus the question what was to be done about it still remained. The situation was partially realized, at least. Tests on certain spans on the Canadian Pacific Railway had substantiated the analytical results as regarded deflection of cross beams and relief of chord stresses, but it was difficult to see that any predetermined provision could be made in any general way for these interaction factors. If each bridge were designed individually by careful and competent engineers, sympathetic to the co-ordination of designing practice and actual fact, the situation would be different. But time and cheapness entered into the matter, and standardization was the spirit of the day. Specification and more specification was the cry on all hands, and under such circumstances he felt that definite provision for interactions would, so far as most bridgework was concerned, be indefinitely deferred—on the continent of America, at least. Mr. Pratley.

Mr. L. H. SWAIN observed that Professor Inglis had presented a most able mathematical analysis, on a subject which had a direct economic importance. Mr. Swain was surprised to find that Professor Inglis made no reference to the work of Mr. C. W. Lloyd-Jones, Assoc. M. Inst. C.E., in connection with the researches of the Government of India, and as far as Professor Inglis's Paper went, Mr. Swain did not think it represented any advance on the masterly analysis of Mr. Lloyd-Jones.[1] Professor Inglis made reference to the problem of whirling of shafts, which he stated would appear at first sight to be analogous to the problem of oscillations in girders. It must be evident, however, that there could not possibly be any analogy between the two problems. The Mr. Swain.

[1] Fourth Report of the Indian Railway Bridge Committee, vol. ii.

Mr. Swain. important fact was that, whereas in a shaft a fixed and constant mass was in operation, in which synchronous vibrations could be introduced at a certain fixed critical speed depending on the natural period of vibration of the mass, in a bridge under a train load the mass (or weight) of the train and its effect on the bridge must obviously alter in a very fundamental way the natural period of vibration of the free girders. Moreover, since the mass of the train varied with its position on the bridge, the natural period of oscillation of the combined mass of girders and train was never constant, but varied throughout the passage of the train. Hence there was no definite critical speed of oscillation for a railway-bridge, and any calculations which were based on an assumed constant natural frequency were liable to be very wide of the mark. The most that could be stated was that there was a critical frequency for the unloaded bridge, which frequency was *a priori* of no practical importance, and there was also a critical frequency depending on the maximum load which occurred on the bridge. This second frequency, however, was a very different figure from the unloaded frequency, and, at any moment in the passage of a train, the frequency had some value between the two, but was never constant for a second. He ventured to think that this important consideration was liable to be somewhat overlooked in investigating the subject of the possible synchronism of locomotive driving-wheels with bridge vibrations. Even in cases where cumulative vibrations did actually occur during the passage of a train, it was rare to find the maximum amplitude or impact effect occurring at the position of maximum load; and this, after all, was the important consideration. The possibilities of synchronism with cumulative effect occurring on a bridge during the passage of a train could only be investigated usefully and scientifically if the important fact of varying natural frequency were taken into consideration.

Mr. Remfry had succinctly stated in the first two lines of his Paper a fact to which increasing attention had been drawn as the result of actual tests on bridges. In the course of his Paper he explained some of the reasons why the stresses under load which a stress-recorder indicated were rarely as great as the ordinary calculations would show. That fact had been recognized in the British Standard specification for Girder Bridges, paragraph 12, in which it was stated that in the case of riveted truss girders a reduction not exceeding 15 per cent. in the primary stress (as calculated by rigid-body statics) might be made, at the discretion of the engineer. The basis of this proviso was, he believed, the fact that engineers now realized that the deformations, both primary and secondary, which occurred in a riveted truss represented work done, that was, energy stored in the girder. It was further computed that 15 per cent. of the energy stored by the weight of the bridge and the load in its vertical deflection movement was taken up in bending the rigid members, and there was, therefore, a corresponding relief in the amount of work done in primary stress. Mr. Remfry, however, went further than this, and showed that in some cases important relief was afforded to main girders by the fact that part of the lower-boom stress might be carried by, or rather transmitted to, the floor system by simple virtue of its contiguity and connection thereto. The amount of relief afforded would, of course, depend entirely on the nature of the floor system and on its connection to the main girders. In the case of a flat-plated floor (which, however, rarely occurred) it was not unreasonable to suppose that a fair proportion of the section of the plating could be added to the effective boom section. Alternatively, if the span had specially heavy wind-bracing, that bracing must share in the load on the booms. In actual practice each case would have to be treated on its merits, and an allowance for relief could only be made when it was clear from the design or by experiment that it was tenable and operative. The subject was of considerable interest, on account of its possible influence on bridge-design. The tendency in America had been towards development of the "scientific" or pin-connected structure, in which rigid-body statics could be made to apply as nearly as possible; and Americans showed anxiety to make their structures act in a "scientific" manner, even to the extent that in the new Hell Gate arch over the Niagara Gorge on the Michigan Central Railroad the floor system was deliberately made in disconnected sections, to prevent its sharing any load with the main chords. This tendency was quite the opposite to that advocated by Mr. Remfry; but Mr. Swain could not help thinking that, from a common-sense point of view, the unification advocated by Mr. Remfry was the direction in which improvement and economy could be effected. **Mr. Swain.**

Mr. W. H. Thorpe observed that, while conceding the value and suggestive character of Professor Inglis's Paper, he desired to emphasize the fact that the investigation developed dealt with but one phase of the allowances made in bridge-design to secure adequate strength, and was not sufficient reason for discounting the character of the Pencoyd formula. In that formula an overall cover for various effects was provided, which appeared to meet the teachings of long observation of the behaviour of metallic bridges. In so far as Professor Inglis's conclusions differed materially from conclusions based on common experience, by so much it would **Mr. Thorpe.**

Mr. Thorpe. appear reasonable to hold them suspect, for the facts of experience remained incontrovertible. Ten years of bridge-maintenance had indeed made it quite unacceptable to Mr. Thorpe to modify the Pencoyd results in favour of reduction of impact allowances for small spans, as against allowances for those that were larger—giving to the term "impact" its usual significance. The conclusions reached with respect to spans of 100 to 120 feet appeared to be based upon a concatenation of circumstances so improbable as likely to occur but seldom, and having but little effect upon the structure's continued usefulness. In small spans such adverse influences as existed were more likely to occur together, since the necessary combination to induce maximum effects was less complex; and they constituted, when they did occur, a larger proportion of the total effects.

Referring to Mr. Remfry's Paper, it might be permissible to say that the question of the interaction of the deck and main girders had not been neglected quite so much as was suggested, though not recognized to the extent of reducing boom sections.[1] Designers of experience had been accustomed to bear this relationship in mind, more particularly with a view to avoid connections liable to develop faults. It was apparent that any cross or longitudinal girder connection should, if rigid, and secured to rigid parts, be sufficiently strong, or, if designed for flexibility, be flexible without the development of damaging stresses. Any connection which was both rigid and weak was bound to suffer.

Professor Timoshenko. Professor S. P. Timoshenko remarked that the Paper by Professor Inglis was important at the present time. The problems were too complicated to be solved by purely analytical methods; nevertheless, a rational theory such as was outlined in the Paper might be useful in guiding experimental work on vibration in bridges now in progress in the United States and in England. Professor Timoshenko had solved[2] several problems on vibration in bridges, by using the method[3] now mentioned by Professor Inglis. On the basis of this study, different impact formulas were subsequently discussed,[4] and it was then shown that an impact factor based upon an addition to the total live load on the bridge was theoretically unsound. It was therefore proposed to divide the problem into two parts, one dealing with short bridges of the girder type and also with the cross beams and rail-bearers of

[1] "Anatomy of Bridgework," pp. 22–31. London, 1906.

[2] "Bulletin of the Polytechnical Institute in Kiev," 1908. (German translation in *Zeit. für Math. u. Phys.*, vol. lix (1911).

[3] "Philosophical Magazine," vol. xliii (1922), p. 1018.

[4] Bulletin of the Engineers of Ways of Communications, Petrograd, 1917.

Professor Timoshenko.

long bridges, and the other dealing with bridges of comparatively long span. In considering short bridges and girders submitted directly to the action of driving-wheels, the additional deflection due to the vibration could be neglected in comparison with the variation in load produced by the rotating locomotive balance-weights, by the impact effect due to the rail-joints, and by flat spots on the wheel-rims. Due to the last cause, large impact stresses could be produced [1] in the girders, and it was difficult to agree with Professor Inglis's statement that impact formulas of the Pencoyd description penalized short-span bridges to an extent which theory quite failed to justify. In order to take account of these high impact stresses, the lowering of the working-stresses to 5 tons per square inch was proposed for girders of this type in the discussion of the specifications mentioned above. In the cases of bridges of longer spans, the vibrations produced by moving loads and by the balance-weights must be taken into consideration. Formulas for the corresponding additional deflections produced were also given, exactly in the form now proposed by Professor Inglis on pp. 257 and 243. It was proposed also to consider the horizontal vibrations of bridges of long span. These bridges having comparatively low frequencies of vibration in this direction, the moving pulsating couples in a horizontal plane due to balance-weights were thought to be able to produce in them large vibrations. For an analytical study of the natural and forced oscillations, so far as the first harmonic component was concerned, the approximate Rayleigh method [2] might be very useful. Such problems as forced oscillations in a girder carrying concentrated masses, considered by Professor Inglis, could be easily solved by using this method. Taking for the fundamental tone a simple sinusoidal deflection curve of the girder, the equation of forced vibration could be represented in the form

$$\ddot{y} + k^2 y = \phi(t).$$

The solution comprising combined forced and free vibrations produced by the force $\phi(t)$ would be given by the formula

$$y = 1/k \int_0^t \phi(t_1) \sin k(t - t_1)\, dt_1.$$

He felt that the same method could be used also in calculating

[1] This question was discussed by Professor Timoshenko in publications on dynamical stresses in railway track. Bulletin of Engineers, Petrograd, 1915. (Abstract in French. *Le Génie Civil*, vol. lxxix (1921), pp. 555–556.) See also Proc. Amer. Soc. C.E., vol. xlix (1923), p. 1279.

[2] "Theory of Sound," vol. i, p. 113, London, 1894.

Professor Timoshenko. the natural period of vibration of bridges. It was necessary only to take a suitable form of deflection curve during vibration and to calculate the corresponding amounts of kinetic and potential energy of the structure, which could be done with sufficient accuracy. Combining this method with Mr. W. Ritz's method, further approximations could be obtained.[1] The problem of forced vibration produced in beams by rolling masses was more difficult than those relating to vibrations produced by moving forces, and so far a satisfactory method of solving this kind of problem had not been found.[2] The actual condition of the vibration of bridges was much more complicated than those taken usually in analysis, and theoretical solutions could only be considered as a guide for experimental work. In order to obtain general conclusions from experiments a preliminary mathematical investigation of the particular cases, and construction of deflection–time diagrams as proposed, together with study of locomotives selected for experimenting, were of great importance. It was necessary, however, to have suitable measuring instruments for recording simultaneously variable stresses in the several bridge-members during motion of the locomotive. The electric telemeter developed recently in the Bureau of Standards[3] seemed to be very promising for this purpose. In contributing to the discussion of this interesting Paper he recognized the importance of the subject. He had attempted to bring forward the conclusions arrived at elsewhere. Other workers in this sphere such as Messrs. A. Kriloff[4] and H. Reissner[5] might be mentioned. It was not his intention in putting forward those other names to detract from the important contribution by Professor Inglis, but he felt that these expressions of opinion might assist in further development of the subject.

Mr. Tudsbery. Mr. H. T. Tudsbery expressed appreciation of the considerable trouble taken by Mr. Remfry in giving a detailed analysis of the relief afforded by the deck system of a bridge to the adjacent booms,

[1] Professor Timoshenko used this method in calculating vibrations in the hulls of ships. See his book "Theory of Elasticity," vol. ii, p. 222. Petrograd, 1916. Abstract of this work is given in Mr. N. W. Akimoff's paper "On the Vibrations of Beams of Variable Cross-Section." Trans. Society of Naval Architects and Marine Engineers, vol. xxvi, p. 111, 1918.

[2] A step-by-step process for calculating deflections produced by a moving mass, applied by Professor Timoshenko in a particular case ("Zur Frage nach der Wirkung eines Stosses auf einen Balken," Zeit. für Math. u. Phys. vol. lxii (1913), p. 198), is too tedious for practical calculations.

[3] Technological Paper No. 247 of the Bureau of Standards.

[4] *Mathematische Annalen*, vol. lxi (1905).

[5] *Zeitschrift f. Bauwesen*, vol. liii (1903), p. 135.

which included in its scope both a through type and a deck bridge, with and without deck plating. While much of the calculations had to be based on assumptions, e.g., degree of fixity of the ends of the cross girders, apportionment of longitudinal stress between stringers, etc., the results obtained appeared to have been confirmed by practical tests. It was well known that considerable relief might in practice be afforded by the decking to the adjacent booms, but it had not been usual in the past to count on this in design, such relief as might be afforded being considered incidental, and, as such, ignored. From the theoretical point of view Mr. Remfry appeared to present a complete case for taking advantage of such relief, but Mr. Tudsbery would like to have his further views on certain practical considerations which must be taken into account when designing any structure. Consider the through span, the stringers of which formed the rail-bearers; such relief of longitudinal stress (tension) as was afforded to the main booms must be provided by the stringers in full. Moreover, both tension and relief were cumulative, being a maximum at the centre of the span. From certain points of view it was hardly possible to imagine anything more unsuited to take longitudinal stress than was the usual stringer. It was cut up into comparatively short lengths, fitted in between the cross girders, and riveted thereto with end cleats, and these rivet-heads would be thrown into tension by longitudinal stress. In addition to this defect, such sections would be subject to the hammering action of concentrated rolling loads, which very often caused the rivets to work loose. Further, from their position and considerable surface area, these stringers were far more liable to corrosion than was equivalent metal in the flanges of the booms. It would therefore appear that the relief given would not be very dependable, except in special cases. Decking would, of course, be able to assist in taking tension; but, supposing that decking was used, and in order to avoid corrosion was of stainless steel, it should act well until an engine was derailed, when the shock of the wheels thumping down on to the decking might easily cause the riveting to give, so immediately destroying the tension value of the deck plating. Under such critical circumstances it might mean all the difference between whether the bridge stood or collapsed, according to whether the designer put in good flange-area in the main booms, or trusted in the relief that the decking might give. It appeared that a ton or two of metal saved in the flanges of the main booms might cause the loss of very many times its value at a later date, and that the saving would not in general be worth the risk run. The main stresses in any structure should Mr. Tudsbery.

Mr. Tudsbery. be provided for in the most direct manner, and in such a way that such provision was exposed to risk only of necessity and not from choice. The interaction between cross beams and posts was on rather a different basis, and further investigation of the best form of connections between these, with a view to reducing to a minimum the secondary stresses, might be advantageous. Probably the simplest and safest method would be the use of some form of extensometer. It was curious that Mr. Remfry appeared to mention neither wind nor temperature stresses in his Paper.

Professor Inglis. Professor INGLIS, in reply to the Discussion and Correspondence, remarked that the interest displayed in the subject-matter of his Paper had acted as a stimulus to further research, and he found it difficult to resist the temptation to discourse on these new explorations, particularly as in some cases they provided answers to questions raised in the discussion. Mathematical analysis necessarily had its limitations, but it was capable of being advanced considerably beyond the stage reached in the Paper. For instance, three circumstances of outstanding importance in determining bridge oscillations which came within the scope of mathematical analysis were :—

(1) The inertia effects of the moving masses.
(2) The damping effects (*a*) when the bridge was loaded, (*b*) when the bridge was unloaded.
(3) The torsional oscillations set up by lack of symmetry in the loading.

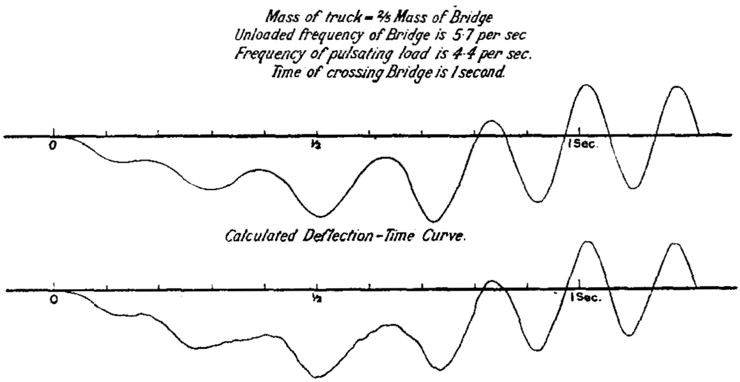
With regard to (1), Professor Timoshenko drew attention to the fact that moving masses were much more difficult to deal with than moving forces. Professor Inglis, as the result of much painful experience, fully endorsed this statement. The "step-by-step" process mentioned by Professor Timoshenko was one of the paths explored, only to be abandoned as being too arduous for practical purposes. More recently, a fairly concise and manageable solution had been achieved, and the results obtained fitted in with experiment to a degree of accuracy which was almost calculated to provoke suspicion. In these experiments the condition of a 120-foot span plate-girder bridge had been reproduced in a model 12 feet long. The truck had a non-spring-borne mass of about one-third the mass of the bridge, and it carried an arrangement of balance-weights to produce pulsating loads during its passage across the bridge at a speed of about 10 feet per second. On the assumption that the mode of vibration of the bridge was a simple sine curve, the deflection at the centre was calculated, taking into account the main effect of

Professor Inglis.

the moving load. How well the calculations were supported by experiment was indicated by the two curves given in *Fig. 9*, and this was only one example of many verifications equally coincident. Mr. Southwell put in a plea for experimental work on a model scale. With this plea Professor Inglis was in full agreement. If principles of similarity were respected, experiments with models led the way to valuable fundamental conclusions, and were achieved at a comparatively small expenditure of time and money.

(2) The problem of damping introduced no analytical difficulty, but at present the analysis could hardly be used for lack of reliable data. It was hoped that in the near future such data would be forthcoming ; and, with this end in view, the Bridge Stress Committee presided over by Sir Alfred Ewing was constructing a bridge

Fig. 9.



Observed Deflection-Time Curve, drawn to same scale as above

oscillator. Vertical periodic forces set up by masses rotating in opposite directions would be applied to the bridges under test, and by varying the speed of rotation the natural frequency and damping-coefficients for bridges both loaded and unloaded should be obtainable. It was hoped, moreover, that this oscillator would help to clear up the question how much of a locomotive could be regarded as spring-borne, and to what extent the free oscillation of a bridge was slowed down by the mass it supported. Recent evidence brought to the notice of Professor Inglis and the interesting remarks made by Mr. Fereday indicated that, in its effect on the period of vibration of a bridge, a locomotive behaved almost as though it had no springs. On the other hand there was evidence that the springs had a pronounced damping effect, and that the presence of

Professor Inglis.

a train on a bridge greatly increased the apparent damping-coefficient. Professor Inglis understood that the suggestion of a bridge oscillator emanated from Mr. Lloyd Jones some little time ago. With regard to Mr. Swain's comment on the absence of Mr. Lloyd Jones's name from the Paper, he would like to say emphatically that this omission did not imply any lack of appreciation. Mr. Lloyd Jones's activities in connection with the Indian Railway Bridge Committee had been so valuable, and were so well known, that eulogy appeared unnecessary.

(3) The torsional oscillations noticeable in a double-track bridge, due to the passage of a load along one track, could be treated readily by the methods of harmonical analysis developed in the Paper. The torsional rigidity of a bridge might be difficult to predict, but it could certainly be determined by direct experiment. It was probable that the natural frequency for torsional oscillation was much lower than for vertical oscillation, in which case torsional oscillation would not play a prominent part in prescribing impact allowances.

As might be expected, some disapproval had been expressed at the revolutionary suggestion that short-span bridges were possibly unduly penalized by existing rules. He had prefaced this remark, however, by suggesting that track-irregularities might figure prominently in such cases, and it would appear that for short-span bridges the impact allowance must turn on the degree of imperfection which might be assumed to exist in the track. It would always be possible to justify a claim for a 150-per-cent. allowance by presupposing a sufficiently bad track.

Mr. Palmer and Mr. Lindenthal rather suggested that the saving in cost effected by more refined calculation was perhaps hardly worth while. If capital cost and initial cost were the main considerations, this statement would call for no comment. It was when the question of scrapping a bridge arose that factors of safety—or factors of ignorance—came into prominence, and it was then that a knowledge of the principles underlying impact and live-load allowances became so desirable and important. In conclusion he would like to add how much he appreciated the generous spirit in which his efforts had been received. For reasons which were sufficiently obvious Sir Alfred Ewing's approval was particularly welcome.

Mr. Remfry.

Mr. Remfry, in reply, observed that the studies on interaction referred to in his Paper were directed to the investigation of average spans, and could not, without further investigation, be considered as reasonably applicable to spans greater than, say, 200 feet. Large spans, such as the 500-foot span referred to by Mr. Lindenthal, Mr. Remfry.

had therefore not been considered. It might be found that a treatment which was applicable to spans of 200 feet and less was only of limited application to spans such as 500 feet.

Mr. Lindenthal's statement that the longitudinal stresses caused lateral fractures in the flanges of the cross girders (floor-beams), near the riveted connections with the outside stringers, was interesting. It was not stated how many stringers there were, but apparently at least four were used. In Appendixes 1 B and 1 C it would be seen that the outer stringers carried nearly the whole of the longitudinal stresses borne by the deck, and it was possible that the inner stringers might carry a small longitudinal stress of opposite sign to that in the outer stringers. The maximum stress in the cross-girder flange would therefore occur at the outer stringer where the fracture took place.

Stresses due to traction of locomotives, braking of trains, wind, and temperature-variation were not considered in the Paper, partly on account of the shortage of space. Traction and braking stresses were in the nature of direct stresses on the deck system, although they were naturally transmitted as quickly and directly as possible to the main girders. Temperature did not affect the interaction results largely, except in so far as variation in temperature between the exposed and shaded portions of the structure was concerned. Local variations of stress due to temperature, approximating to 2 tons per square inch, were to be expected under Indian conditions.

He much appreciated Mr. Pratley's views, and quite agreed that, owing to the individuality of each structure, only the working-out of a multiplicity of examples, covering closely the entire range of practice, could provide adequate material to guide the designer. In that respect he fully realized the limitations of the studies made. He had taken, he admitted, examples of a type in which the assistance given by the deck system was likely to be considerable; and, although he had indicated the very important influence exerted by a flat deck plate, it must not be lost sight of that in the open deck spans he had also assumed that the rail-bearers and cross girders were of one depth and adequately riveted together. In the majority of bridges that did not occur, and discretion must be exercised and reasonable assumptions made in each case, according to the characteristics of each structure.

Mr. Pratley, in his example, showed an end fixing of the cross girder of 6·3 per cent. That would be increased slightly in practice, owing to the resistance offered by the booms to twisting. Under

Mr. Remfry. Indian conditions the increase due to that factor might be 25 per cent., raising the value of the fixing to about 8 per cent. In Canadian and American practice the additional resistance due to that factor would be less, and perhaps the fixing might work out to 7 or $7\frac{1}{2}$ per cent. In the 150-foot through Indian span with deck plating the corresponding fixing-percentage was nearly twice as much. The fixing in the lateral directions of the ends of the cross girders in the Canadian type illustrated was considerably less than when a deck plate was used. Mr. Pratley's calculations led him to expect an average relief in the booms of about 3 per cent. in that structure. It was certain the relief would not be large, and from inspection of the details given and comparison with the results of Appendix I, Mr. Remfry would not have expected a relief of more than 5 per cent. The example brought out clearly the importance of considering each structure on its merits. The very deep cross girders used in Canadian spans brought the upper edge of the cross-girder connection well up the vertical, and the stresses in the verticals due to twisting should not be lost sight of. It was questionable whether, in view of such twisting stresses in these types, sliding joints in the rail-bearers would not be advisable. The Canadian type illustrated had not the reserve of strength of the Indian type, which was fitted with a deck plate. The two designs showed very clearly the characteristic difference between a span in which the deck was adapted to assist the main girders and one in which it was not so adapted. Each type had its adherents, and each had certain desirable features. If, however, a solid-deck structure was to be used because of certain inherent qualities, it was only reasonable so to design it that it would be fully effective in all respects and would "pull its weight."

It was most important, if the deck was to be capable of resisting longitudinal stresses, that it should be specially designed for that purpose. Mr. Tudsbery pointed out how very unsuitable certain forms of construction were to resist such stresses, and drew particular attention to the unreliable nature of the connection between the cross girders and rail-bearers. In Mr. Remfry's practical experience of bridgework-maintenance the realization of such deficiencies had been brought home, and an attempt had been made to develop a type of connection which would avoid those defects. The result was a type in which the rail-bearers were of the same depth as the cross girders and were framed between the latter, with cover-plates passing above and below the cross girders to make the rail-bearers practically continuous beams from end to end of the span. The development of this type of connection had led him to investigate its influence on the rest of the structure, and the Paper was the outcome of those investigations. Mr. Remfry.

Some apprehension was also felt by Mr. Tudsbery in regard to reduction of the section of the tension booms of a through span, owing to the provision of a plated deck designed to assist such booms. In Mr. Remfry's opinion, if the structure were designed upon the lines he advocated, it would have a far greater chance of surviving a bad derailment than would a structure with an open deck. Derailments of wagons or coaching stock on a plated deck were not very dangerous, as the wheels usually got over the span, damaging the sleepers, but not smashing them completely. In the case of an engine derailment at high speed the probability was that the sleepers would be smashed completely. If they were supported throughout on deck plating, they were likely to survive severer punishment than would sleepers which were only supported at two points where they rested on stringers. Assuming, however, that the engine derailment was serious and that the sleepers were completely smashed, the engine would drop through an open deck and crash into and destroy the cross girders, possibly overturning in the process and destroying the main girders. If a deck plate was used, it might give the necessary support to a derailed engine, and prevent it from wrecking the cross girders. The deck plate would suffer; but it would probably guide the wheels over the cross-girder flanges. It was unlikely that even an engine-derailment on a bridge with a flat deck plate would cause the deck plate to lose all its capacity for assisting to resist longitudinal stresses. Appendix II showed that the deck system of a single-span 150-foot through bridge of the type advocated might assist the booms to the extent of about 30 per cent. without a deck plate, or 57 per cent. with a flat deck plate. If in an accident the deck plate were damaged so that it was only capable of giving half of its original help, the deck system would still carry 30 to 40 per cent. of the longitudinal loads.

Referring to the economics of the question, the following figures were given as an indication of the relative weights of a 150-foot through 5-foot 6-inch gauge railway span. Two main girders, 100 tons, of which the tension booms constituted 22·25 tons; cross girders, 16·75 tons; rail-bearers, 19·5 tons; deck plating, 16·0 tons. Hence the total weight of the deck system was 52·25 tons; bracings, 7·25 tons; expansion bearings, 7·0 tons; total weight of span, 166·5 tons. In this span the working-stresses as usually calculated

Mr. Remfry. would be in the tension boom: under dead load, 2·0 tons per square inch; under live load and impact, 6·0 tons, a total of 8·0 tons per square inch. By adding 2·5 tons to the deck system in the way of covers to rail-bearer joints, the deck would assist the tension booms to the extent indicated in Appendix II, namely, it would carry 57 per cent. of the longitudinal stresses. In the present state of knowledge it would not be advisable to reduce the section of the tension booms of the main girders by 57 per cent., or 12·75 tons; but it would be reasonable to reduce the section by 33 per cent., or by 7·25 tons. The net saving in weight would be about 5 tons, which, although not large, was appreciable. The structure would be more solid and less liable to injury in the case of accident. In the tension booms as altered, the actual stresses under full loading would be: dead load stress, 2·9 tons; live-load stress plus impact, 4·0 tons; total, 6·9 tons per square inch. If in an accident the deck system became totally ineffective to resist longitudinal stress, but the structure still carried the train, the stress in the tension booms would rise to about 12·0 tons per square inch. It had been indicated that, even after an accident, the deck system was likely to be effective to a very considerable extent; and it was not likely that the stresses in the boom would exceed 9·0 tons per square inch.

Although sound engineering opinion would not agree, in a new design, to reduce the main-boom sections to the extent that calculations and experiment indicated they might be reduced, owing to the action of the deck system, it was reasonable to move a certain distance in that direction. A more rational design would result, wherein the metal would be better distributed and have greater capacity to resist impact and fatal damage in accidents. In deciding the life of old bridges, studies of this nature were of the greatest importance, as they might assist in coming to a decision to retain in service a structure which might otherwise be condemned. With the future possibility of better-balanced locomotives, or electric traction, and consequently reduced impact stresses, such reprieve might be extended almost indefinitely.