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

Correspondence.

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

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

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

Mr. Colam. anticipated the work since done by the Bridge Stress Committee. The Indian formula for impact derived in 1925 was, on bridges where synchronism occurred,

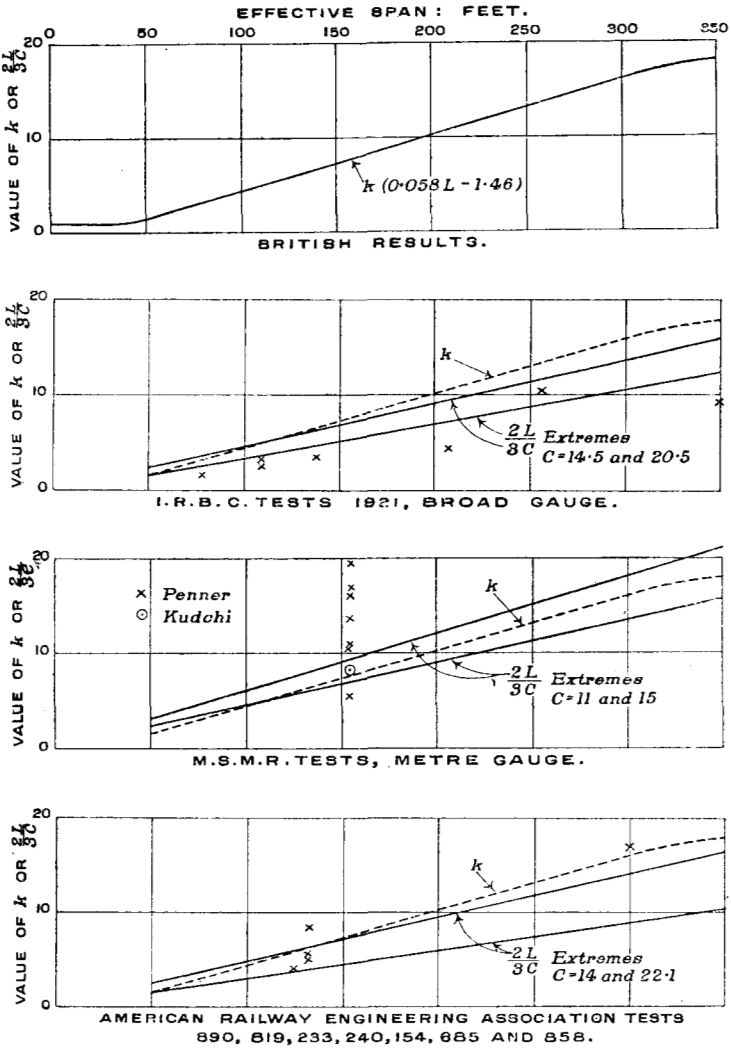
$$\text{Impact (per cent.)} = \frac{1050\,P_1}{(w+p)\,cd}$$

where P_1 denoted the hammer-blow in tons at 1 revolution per second, w the dead load and p the live load in tons per linear foot, c the circumference of the driving-wheels in feet, and d the static deflection in inches; this represented the square of the critical frequency, the stiffness of the bridge, and the number of hammer-blows during the passage of the locomotive. A minimum value of 0·2 had been fixed for P_1. Considering that this formula was published in 1925, it was a little surprising that it had not been referred to; and still more surprising was the statement that no formula previous to that now published took proper account of the hammer-blow. Admittedly the second critical frequency had not been covered, but at present speeds in India it was not reached. In the derivation of the formula there was a term $2L/3c$ (L being the span in feet) which had disappeared by conversion to other terms in the final form. This factor $2L/3c$ had been arrived at from the study of a large number of impact tests, and it was an interesting fact that, if it was replaced by k, the Indian formula for the added deflection was exactly the same as the British formula. In other words, $2L/3c$ corresponded to k, and, multiplied by the hammer-blow at the critical frequency, gave the total effect on the bridge (*Figs. 22*). On the face of it $2L/3c$ looked more rational than a single coefficient, because the impact must depend on the number of blows struck, which differed on any span with locomotives having wheels of different diameters. The figures given in the following Table showed that $2L/3c$ agreed with the values he had been able to work out, as well as, or better than did k. As a matter of interest he had applied the Indian formula to the first three bridges dealt with in the Report, namely, Runcorn, Queen Alexandra, and River Aire, with the following results :—

	Added Deflection : Inch.		
	Runcorn.	Queen Alexandra.	River Aire.
Near girder calculated . .	0·112	0·086	0·157
,, ,, observed . .	0·112	0·095	0·140
Far girder calculated . . .	0.036	0·045	0·066
,, ,, observed . . .	0·037	0·053	0·091
Mean calculated	0·075	0·082	0·116
,, observed . . .	0·075	0·074	0·116

Considering that these figures had been worked out in a few minutes without any calibration of the bridge, they seemed fairly Mr. Colam.

Figs. 22.

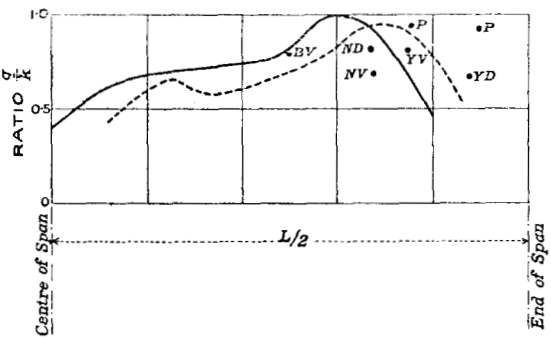


DYNAMIC MAGNIFIER.

satisfactory. The inclusion of the Indian curve $65/(45 + L)$ in *Fig. 1* suggested that it was merely an empirical covering curve as

Mr. Colam. were the others. It was not. It had been derived in the way that the final recommendations of the Bridge Stress Committee had been derived. The worst broad-gauge locomotives were found to have a hammer-blow of 0·6 ton at 1 revolution per second. The impacts were worked out for a train of the heaviest wagons the standard bridges would take, headed by two of those worst engines, and the results were expressed as a percentage of the live load. The covering curve $65/(45 + L)$ was drawn over those results, and the curve was really meant for design purposes only. Whether the results were expressed as a uniformly-distributed load or a percentage of the live load was immaterial, as the one was readily convertible to the other, though it must be admitted that talking in percentages

Fig. 23.



DYNAMIC INCREMENT FOR WEB MEMBERS.

Aire Bridge, 210 ft., verticals.
,, ,, ,, Diagonals, plotted to centres.

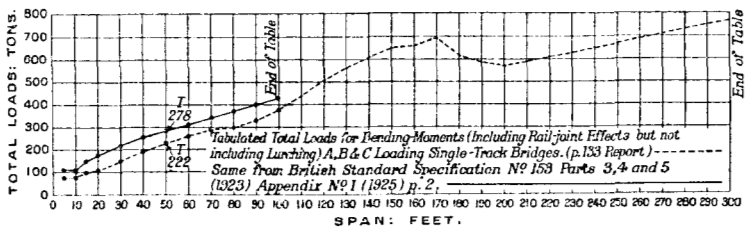
N	Newark	V vertical.	D diagonal.
Y	Yalding	,,	,,
B	Bridgwater	,,	,,
P	Blackthorn plate girder	,,	,,

sometimes led to false impressions. For spans shorter than those where synchronism occurred, the formula $65/(45 + L)$ was admittedly empirical. The question of the balancing of locomotives in India had also been gone into, and many locomotives had been found with no part of the reciprocating masses balanced, and there was even a negative effect in some metre-gauge locomotives. Those facts were now being considered, to decide whether it was really necessary to balance two-thirds of the reciprocating parts, or whether some of this fraction could be exchanged for an increase in static wheel-load and tractive effort. The Author's remark that the makers' statements regarding the balancing could not be depended upon

was fully confirmed by the Indian results. The measured balancing was generally a surprise, and the balancing could often be rearranged without detriment to the locomotive and with very great benefit to the track and bridges. The conclusions about the impact in shear members were new and extremely interesting. The method of calculating it was, he feared, not likely to be of use to the practising bridge-engineer, and must be simplified. From the somewhat meagre figures given in the Report it would appear that a relation of q to k might be established which would immensely simplify matters (*Fig. 23*), and he suggested that the Committee should examine their records with that in view. Mr. Colam.

Mr. R. E. S. Cooper felt that even those who, like himself, had more or less followed the labours of the Committee, had hardly yet had time to grasp completely the full significance of its very carefully, and, if he might say so, very economically-worded Report. Mr. Cooper.

Fig. 24.



The first thing that struck him was the triumphant vindication of the theories set out by Professor Inglis in the Paper contributed about 5 years ago. When he read that the Committee did not feel that the results justified the adoption of a higher working stress, he had a sense of disappointment, which was but little allayed when he realized what a very much better position engineers were in to investigate actual stresses than they had been before, as evinced by the remarkable accordance between theory and experiment revealed by the Report. He wondered whether the Committee as a whole had realized the significance of the interesting experiments that Professor Dalby had described[1] just before the Report appeared. He found it hard to reconcile the very greatly increased precision in the computability of stresses with adherence to the 8 tons per square inch on normal structural steel, and that more particularly as the bogy of "fatigue" seemed, so far as bridges were concerned, to be finally laid. Nevertheless, as the Author stated, it was a matter

[1] Minutes of Proceedings Inst. C.E., vol. 227 (1929), p. 25.

Mr. Cooper. for congratulation that this long research pointed the way to an actual saving in the cost of bridges. He had checked through the Table No. 1 on p. 133 of the Report up to 100 feet span, and found that the figures given therein for the extreme case, 6 revolutions per second, A, B, and C loading, for the equivalent uniformly-distributed load were notably lower than similar figures derived from the equivalent uniformly-distributed load in Appendix No. 1 to the British Standard Specification for Girder Bridges, No. 153, when the allowance for impact based on the Ministry-of-Transport formula was added (*Fig. 24*). In matters of maintenance the results of this research would point the way to very great economies. Anyone who had had experience on British railways would be able to point to bridges which were "refractory," owing to their initial design or perhaps to the method of erection, when one wanted to find out the stresses by calculation. There were also bridges which might possibly undergo the delicate operation of being strengthened in situ, or, again, interesting questions were afoot as to how far strengthening in situ had been effective. The solution of questions such as the foregoing would not in future be accepted as valid until one of the stress-recorders that had been so well tested in this research had given its unbiased verdict.

Mr. Everall. Mr. W. T. EVERALL remarked that the report was of particular interest to bridge-engineers in India because in 1925, as the result of several years' work on this subject, the Railway Board published, as Technical Paper No. 247, a considerable amount of information and data from which a new impact formula was evolved, and that formula had since been adopted by the Government of India for railway-bridges. The results of the Bridge Stress Committee's work very largely confirmed the pioneer work previously done by the Indian Railway Bridge Committee, for which Messrs. L. H. Swain and H. N. Colam, Assoc. MM. Inst. C. E., and C. Harrington, were largely responsible. The types of span selected by the Bridge Stress Committee for testing were not as suitable as the more modern single-track types generally in use in America and India. Most of the English tests had been carried out on double-track spans with only one track loaded, and that considerably below its full capacity. The results produced were unsymmetrical loading in the main girders, complicated by torsional effects in the flooring-system, factors which all tended to mask the true impact results. Further, in the flooring-systems of large through spans, owing to the interaction between the chords of the main girders and the floor-system, the rail-bearers under the train load (when rigidly connected throughout the bridge) carried chord-tension stresses in addition to the ordinary stresses

induced in them in their capacity as rail-bearers. As the speed of the load increased the longitudinal deformations of the bottom chord under the vertical oscillations of the main girders induced alternating stresses in the rail-bearers, which might synchronize with the ordinary stresses in them. The stresses in the rail-bearers were greatest near the centre of the span. Such conditions must produce considerable vertical oscillations. He did not see any mention of such stresses in the Report. Extensive use of the Fereday-Palmer stress-recording instruments was made in India, and gratifying results had been obtained in many hundreds of tests made with it. Considerable care was, however, taken to see that the instruments were calibrated and properly attached by skilled operators who had been specially trained for the work. The Author's criticism of the custom of providing roller bearings in spans exceeding 100 feet and his suggestion as to substituting sliding bearings were unusual. Would the Author suggest that sliding bearings could, with advantage, be used for a span of 300 feet where the temperature-range was at least 130° F. and, in addition, considerable braking-forces were likely to occur ? Mr. Everall.

Mr. EDWARD GAGEL, of New Haven, Connecticut, observed that American railway practice in the important matter of impact was largely guided by the investigations of the American Railway Engineering Association's Bridge Committee. The impact curve assigned on p. 49 to the Association was not the one now being used by it. The Association's recommended practice, as contained in bridge specifications issued by it, was the impact formula shown in *Fig. 1* as the Turneaure formula. In the American investigation of impact stresses it had been quite well recognized that the principle of critical speeds on certain spans had a very definite relation to the impact on those spans, on account of the synchronism of the natural period of vibration of the structure with the effect of successive blows of the engine counterweights. The tendency to create higher values of impact stress, particularly in spans of the range from 80 feet to 125 feet, was met by the American impact formula, which made considerably higher allowances than did the Indian Railway Bridge Sub-Committee's curve, proposed in 1925. Considerable additional information, affecting the counterbalancing of locomotives, had been obtained in America by the investigation of the American Railway Engineering Association and the American Society of Civil Engineers Joint Committee on Stresses in Railroad Track. That investigation was leading, in America, to more careful consideration of the counterbalancing of locomotives by their designers. It was also recognized, in America, Mr. Gagel.

Mr. Gagel. that, as pointed out in the Paper regarding British practice, the impact effect of three-cylinder locomotives and of electric locomotives was much less than that of the two-cylinder steam-locomotive. In America the general proposition that a lower allowance for impact was permissible in the longer spans had been recognized, after a thorough investigation along somewhat similar lines to the investigation of the British Committee, and had been accepted by the adoption by the American Railway Engineering Association of the Turneaure formula as recommended standard practice, in place of the Pencoyd formula, which for a long time had been recommended practice in America. The new American formula provided for a somewhat higher impact allowance on shorter spans than did the old Pencoyd formula, but from 100 feet upwards the present American formula reduced the impact allowance materially below the old Pencoyd value. The Author's note that "conditions of loading and standards of axle-load and hammer-blow chosen by the consideration of data originating in this country cannot be applied to railways abroad without some discrimination," seemed particularly applicable to American conditions, as it was believed that the American operating equipment, both locomotives and freight-cars, imposed such varied conditions that American engineers were not disposed to draw lines quite as fine as were suggested in the Paper, nor did there appear to be any present tendency to reduce the impact allowance now generally used in America.

Mr. Hay. Mr. J. B. M. HAY observed that he could not but admire the results of the research into impact as published in the Report of the Bridge Stress Committee. Early in 1925 he began some experiments at Manchester University on impact on short-span beams. The range of spans and speeds of loading were necessarily limited. Sufficient data were obtained, however, to satisfy him that the mathematical theory of transverse oscillations of girders, as developed by Professor Inglis in his admirable Paper, was in agreement with results given by those short-span beams. He had worked out the relationship which should exist between an actual bridge and a scale model so that the impact effects might be predicted. Unfortunately, he had not yet had time to determine whether the results were in accordance with the findings of the Bridge Stress Committee; but he hoped to be able to present the results of such an investigation in the near future. The relationship might be expressed by stating that if

$$\frac{p}{p'} = \frac{n_u}{n'_u} = \frac{n}{n'} = \frac{l'v}{lv'} = \text{a constant},$$

the impact allowance would be the same for the bridge as for the

model, where p denoted the frequency of the pulsating load on the actual bridge, n_u the unloaded frequency of the bridge, n the loaded frequency of the bridge, l the span of the bridge, and v the speed of the load crossing the bridge, the accented symbols being the corresponding quantities for the model. The pulsating loads must also be of the same type. The effect of damping had not been considered in the foregoing relationship, and it was possible that damping might make a considerable alteration in the formula given. Mr. Hay.

Mr. H. PARRY JONES remarked that impact was a subject with which engineers in India generally, and on the Bombay, Baroda and Central India Railway particularly, were greatly concerned. Although the general regirdering of the bridges on the main line of that railway (about 550 spans of 60-foot and 30-foot girders) had been completed before the revision of the Bridge Rules in 1903, which revision provided for lighter live loads than those called for under the Rules of 1898, to which the girders had been designed, he had for the last 3 years been faced with the problem of dealing with them. Up to the issue of the First and Second Interim Reports of the Indian Bridge Committee,[1] those girders had been calculated as to strength with the aid of the Pencoyd impact factor, the result being that, to carry the heavier loading aimed at, the girders would have to be either quadrupled or triplicated. For the former, four of the girders would be placed under one track and new girders of the requisite strength would be obtained to replace those removed. If triplication were carried out, each of the old girders would be strengthened to take the higher shear stresses and new girders would be obtained to take the place of those removed for triplication. That would be a very costly work to carry out over 350 miles of main line under traffic. The adoption by the Government of India of the recommendations of the First and Second Interim Reports, permitting the use of an impact factor based upon the hammer-blow, had entirely changed the situation, and proposals had now been put forward which, if approved, would enable the company to retain the girders in service (when strengthened in shearing resistance by a small amount of electric welding) under axle-loads of 22·5 tons, or even more. The Author stated that it was perhaps remarkable that unequal wear of tires had not directed attention to the defect of overbalancing. On inquiry he found that that had been noticed, but he could not learn that any steps had been taken to rectify the state of affairs revealed. Mr. Jones.

[1] Indian Railway Board Technical Paper No. 247,

Mr. Lang. Mr. P. G. LANG, jun., of Baltimore, observed that when the Report of the Bridge Stress Committee came to hand he had just completed a report to the International Railway Congress Association on American practice with respect to static and dynamic stresses in railway-bridges. A comparison of British and American practice would show that substantially the same final results were obtained, so far as the bridge was concerned. The truth of this conclusion was demonstrated by the fact that, while independent investigations by British and American committees had been made upon the factors of live load and impact, the final total stress, for dead, live, and impact loads, was approximately the same. By means of diagrams, using one recommended formula as a basis, he proposed to show that no material divergence existed, and that consequently the bridge-engineers of both countries could well afford to harmonize minor differences and use identical formulas. The Bridge Stress Committee's Report clearly indicated that very valuable work had been done, with extreme thoroughness. The Report was amplified in an admirable manner by the Paper. It would seem that, in America, the same subject was covered by the tests and reports of Committee XV (Iron and Steel Structures) of the American Railway Engineering Association.[1] The Bridge Stress Committee had very thoroughly covered all the factors that produced impact stresses in bridge structures. It had appeared quite evident that the sum total of impact could not be obtained solely through mathematical processes, and consequently tests had been made, with a view to determine in various structures, under a variety of conditions of loading, the actual maximum impact obtained. With ideal conditions for both structure and loading, or at least with conditions approximating to those assumed in the mathematical analysis, a good check between theory and practice was secured. Perhaps the more important part of the railway bridge-engineer's work was the maintenance of existing structures, and he should be capable, as a result of calculations and experience, of obtaining the longest possible life from such a structure, combined with the greatest carrying-capacity, bearing in mind that considerations of safety must always be uppermost. The function of a railway was to transport goods and passengers from one point to another. In connection with that movement a bridge was but a part of the problem. Between two points, many bridges of various kinds and carrying-capacity and in different conditions would be encountered.

[1] Proceedings Am. Ry. Eng. Assoc., vol. 12, pt. 3 (1911), p. 26; and vol. 17 (1916), p. 116.

Mr. Lang.

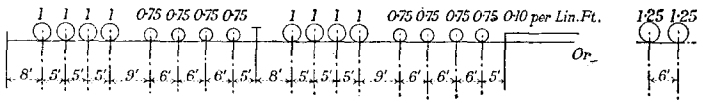
It was necessary to use engines of various types. The engines would vary, so far as their counterbalance and axle-loads were concerned, and any given engine might, from time to time, have its counterbalance or reciprocating parts and individual axle-loads changed, thus producing changes in its dynamic and static effects upon bridge-spans, although the total weight of the engine would remain constant. While, in general, a reduction in speed reduced the impact effect, speed-restrictions could not in practice be placed promiscuously upon structures. The foregoing general statement, merely attempted to indicate the practical considerations that must be met, without any thought of detracting from the splendid work done by the investigators of this subject. Such investigations were extremely valuable and necessary. A distinction must be drawn between the investigator and not only the designing engineer but also the engineer who rated existing structures in order to obtain the extreme limit of their safe carrying-capacity, considering economical railway-operation. It would seem, therefore, that finally a general rule must be evolved, resulting from such mathematical investigations and series of tests as the Bridge Stress Committee had made. Further, it would seem indisputable that extreme refinement was necessary in exceptional cases, involving large and costly structures. However, he believed that the majority of railway structures were rather small; in fact, in that portion of the United States lying between the Atlantic seaboard and the Mississippi, and north of the Ohio and Potomac rivers, which was the portion most densely populated and thoroughly covered with railways, 97 per cent. of all bridge-spans were less than 120 feet. For such short spans plate girders were largely used. Such structures had proved to be of sturdy construction, and extremely satisfactory from a maintenance and operating standpoint. While it was highly desirable to have close and constant co-operation between designers of locomotives and designers of bridges, it was questionable whether the bridge-designer could always control engine-design within narrow limits, in so far as such design would affect bridge structures. Past experience indicated that, while that was an ideal worthy of achievement, it was not likely to be attained. Further, the necessity for various types of engines would always exist. From these considerations it would seem that a simple, usable formula was required, which could be readily applied to at least the majority of bridge structures, and which would cover all causes of impact, both those susceptible to mathematical analysis and others which depended not only on the design but also on the maintenance of track, rolling stock, and railway structures. It would

Mr. Lang. seem clear that the proposed formula $120/\{90 + \frac{1}{2}(N + 1)L\}$ met those requirements. That formula provided, in multiple-track structures, for live load and impact combined, stresses which were always in excess of N times the live load. Undoubtedly, careful consideration had been given to the use of the factor N (the number of tracks), although it would appear that, for the sake of simplicity, that factor might be taken as unity, thereby giving a formula $120/(90 + L)$ which would be applicable to a single-track structure, and a coefficient K could then be selected for use for multiple-track structures. It appeared to be intended that N should take account of a reduction in impact due to the fact that rarely, if ever, would a synchronization of operations on the several tracks of a structure occur, producing maximum impact. The question arose whether it was also intended that the factor N should provide for the extreme possibility of the simultaneous loading of several tracks to produce the maximum live-load or static stress. It might be well to amplify this point for the sake of clarity, or to give consideration to the omission of the factor N and the substitution of a coefficient K, to be applied to multiple-track structures, which coefficient would take into account the reduction not only in impact or dynamic stresses but also in static or live-load stresses on such structures. Referring to Fig. A–1 (p. 144) of the Report of the Bridge Stress Committee, it was true that the formula $20{,}000/(20{,}000 + L^2)$ had at one time been given consideration by the American Railway Engineering Association, but, so far as he knew, it had never been actually used. Professor F. E. Turneaure had been associated with the Committee of that Association dealing with the matter for many years, and undoubtedly he was in a large measure responsible for the formula $30{,}000/(30{,}000 + L^2)$. Before 1920 the American Railway Engineering Association's specification provided for the use of the formula $300/(300 + L)$. Since that time the general specifications of the American Railway Engineering Association for steel railway-bridges for fixed spans less than 300 feet in length, and subsequent revisions, provided for the use of the formula $30{,}000/(30{,}000 + L^2)$. The specification in which that formula appeared was definitely limited to spans not exceeding 300 feet, and apparently the Bridge Stress Committee likewise limited their conclusions to spans not exceeding that length. That seemed highly desirable, as undoubtedly longer spans should have special consideration. The formula $120/\{90 + \frac{1}{2}(N + 1)L\}$ was limited to 115 per cent. for single-track spans less than approximately 14 feet in length, and it allowed 100 per cent. at 30 feet span. Apart from the question whether impact in excess of 100 per cent. actually

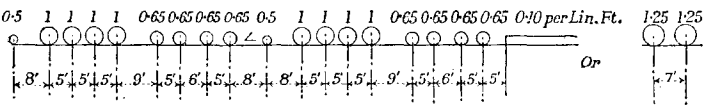
occurred, he believed that it could be limited to 100 per cent. in design for spans 30 feet or less in length, as such spans were of sturdy construction, having reserve strength or carrying-capacity. The standard unit loading for railway-bridges promulgated by the British Engineering Standards Association approximated very closely to the standard loading in use in the United States and Canada, namely Cooper's class loading, which was typified by two 2-8-0 type engines, followed by a uniform load. As the Cooper class loading had been used in the United States for many years, numerous costly and voluminous bridge records were based on that loading. Not only did a typical engine loading reflect in the layman's mind a

Mr. Lang.

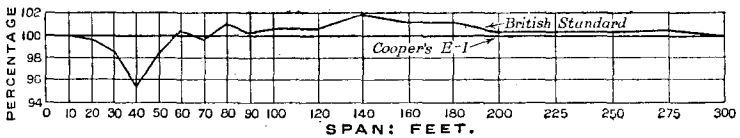
Figs. 25.



BRITISH STANDARD LOADING FOR RAILWAY-BRIDGES.



COOPER'S E-1 LOADING (AMERICAN RAILROADS).

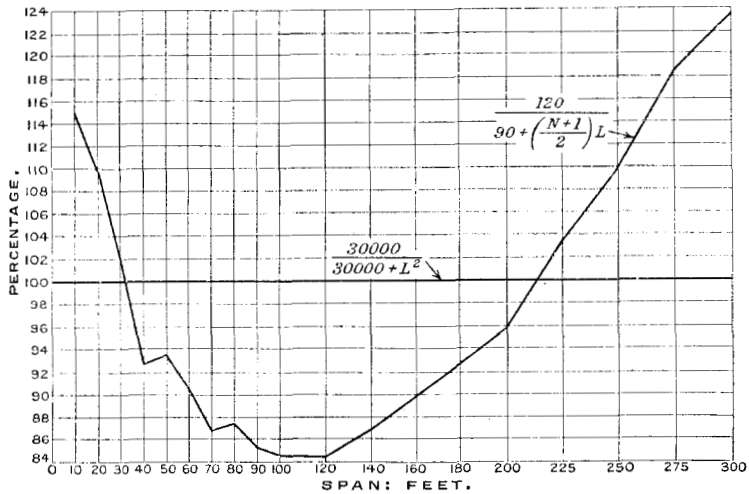


MOMENT CURVES—LIVE LOAD.

definite engine, but further, it had been found that the Cooper loading, and presumably the British Standard loading, produced an effect on bridges very closely approximating to the actual effect of the various types of engines in use. An interesting comparison between the British Standard diagram of unit loading and the Cooper loading could be made, indicating that for spans up to 300 feet in length, so far as live load was concerned, the maximum variation was small (*Figs. 25*). Likewise, a comparison between the impact formula $120/\{90 + \frac{1}{2}(N + 1)L\}$ and the American formula $30{,}000/(30{,}000 + L^2)$ indicated that, for single-track spans up to 300 feet, a variation of only 22 per cent. was obtained,

Mr. Lang. as indicated in *Fig. 26*. The heaviest engine-loading proposed for use in design in Great Britain involved an axle-load of 20 tons or 44,800 lbs., comparable with what was known in America as Cooper's

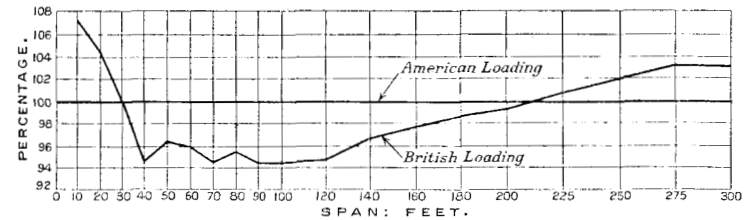
Fig. 26.



VARIATION IN IMPACT.

E–40 loading, for which the engines had an axle-loading of 40,000 lbs. In Great Britain the unit stress in design was 8 tons or 17,920 lbs. per square inch: taking for purposes of comparison a unit stress of

Fig. 27.



VARIATION IN TOTAL DEAD-LOAD, LIVE-LOAD, AND IMPACT ALLOWANCES BETWEEN BRITISH AND AMERICAN FORMULAS.

16,000 lbs. per square inch, and taking on the one hand the British Standard unit loading, using 27 units, that was 27 tons or 60,480 lbs. per axle, and the impact formula $120/\{90 + \frac{1}{2}(N+1)L\}$, and on the

other hand Cooper's E-60 loading, having an axle-load of 60,000 lbs. and the American formula $30{,}000/(30{,}000 + L^2)$, it would seem that, considering all the factors of dead load, live load, and impact, for single-track spans up to 300 feet in length, a variation in the bridge only between 107 and 95 per cent. was obtained, as shown in *Fig. 27* and the Table on p. 142. It would seem that, as a practical consideration, too much attention should not be given to slight variations in typical diagrams of live load used for design and in impact formulas, and that the ultimate results should always be kept in mind. British practice appeared to coincide closely with American and Canadian practice. A large part of the railways of the world was located within those three English-speaking countries. Close co-operation between such large groups of English-speaking peoples seemed desirable, and the question arose whether it would be possible, for the sake of uniformity, to cast aside small and rather immaterial differences, and agree on definite and uniform standards for design loading and impact formulas, uniformly applicable in at least those countries.

Mr. Lang.

Mr. J. N. D. La Touche remarked that the alliance between theory and experiment, on which the Paper was based, marked a great advance in the study of impact problems. The collection of formulas given by the Author reminded him of the relics of an ancient civilization to be seen in a museum; though the formulas were the result of long experience and much brain-work, they had not been hitherto sufficiently supported by field experiment. About 1895 the Government of India became alarmed about the stability of the girder bridges on their railways, and ordered a general check by calculation, in which he, at that time a deputy consulting engineer, took part. The impact coefficient prescribed was simple: the actual wheel-loads were to be doubled. As that would have condemned most of the girders in use, and as very few indeed showed any signs of over-stress, much less of failure, that coefficient was soon modified, the modifications being based rather on theoretical than on practical grounds. He was impressed at the time by the importance of ascertaining by actual field tests the effect on the metal of running a train over a girder bridge, and was convinced that, if enough tests could be made, the theory of impact would be considerably modified and rendered more trustworthy. The carrying-out of such work was hindered by the delicacy and high cost of the instruments required. To render a test of any value several recorders should be used simultaneously, and when even one cost more than the ruling body would sanction, such a procedure was out of the question. In an attempt to remedy

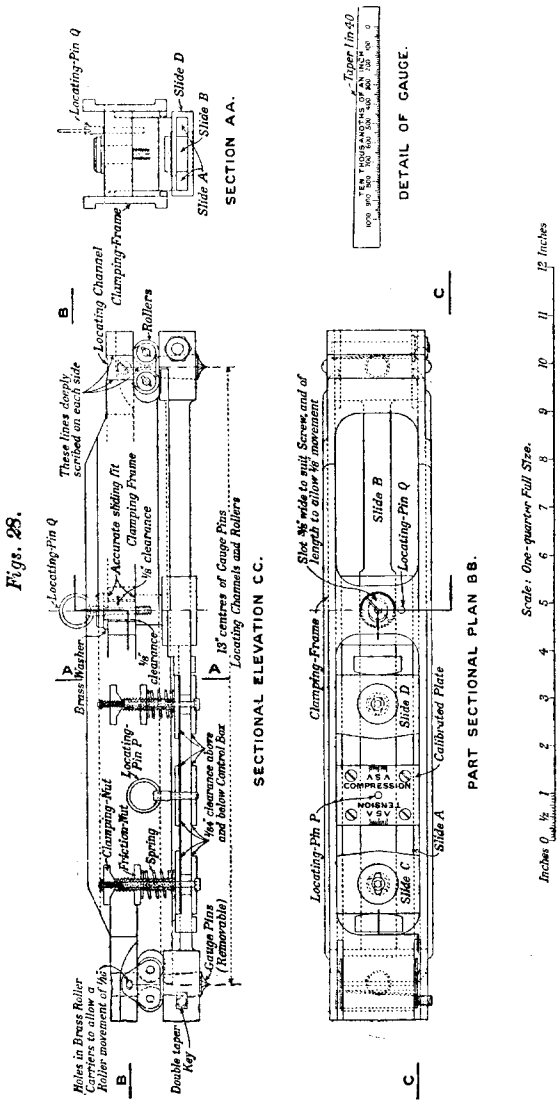
Mr. La Touche.

Mr. La Touche.

1	2	3	4		5	6	7		8	9
Span: Feet	Dead-Load Moment.	Live-Load Moment for British Standard 27-unit Load.	Impact.		Dead-Load + Live-Load + Impact Moment. (b)	Live-Load Moment for American E. 60 Loading.	Impact.		Dead-Load + Live-Load + Impact Moment. (a)	$\frac{b}{a}$
			Per Cent.	Moment.			Per Cent.	Moment.		
10	5	94	115	108	207	94	100	94	193	107·3
20	25	309	109	337	671	310	99	307	642	104·5
30	60	609	100	609	1,278	616	97	598	1,274	100·3
40	115	942	92	866	1,923	983	95	934	2,032	94·6
50	195	1,407	87	1,222	2,824	1,426	92	1,312	2,933	96·3
60	317	1,957	80	1,567	3,841	1,950	89	1,736	4,003	95·9
70	521	2,556	75	1,917	4,994	2,560	86	2,202	5,283	94·5
80	732	3,270	71	2,320	6,322	3,241	82	2,658	6,631	95·3
90	982	4,020	67	2,690	7,695	4,006	79	3,165	8,153	94·4
100	1,275	4,860	63	3,060	9,195	4,830	75	3,623	9,728	94·5
120	2,340	6,960	57	3,970	13,270	6,922	68	4,707	13,969	94·9
140	3,430	9,450	52	4,910	17,790	9,294	61	5,669	18,393	96·6
160	4,800	12,060	48	5,790	22,650	11,934	54	6,444	23,178	97·7
180	6,480	14,910	44	6,560	27,950	14,754	48	7,082	28,316	98·7
200	8,500	17,880	41	7,340	33,720	17,784	43	7,647	33,931	99·3
225	11,549	21,960	38	8,340	41,849	21,858	37	8,088	41,495	100·8
250	15,235	26,490	35	9,290	51,015	26,340	32	8,429	50,004	102·0
275	19,615	31,440	33	10,400	61,455	31,248	28	8,749	59,612	103·1
300	24,750	36,600	31	11,350	72,700	36,600	25	9,150	70,500	103·0

that state of things he designed the instrument shown in *Figs. 28.* With the exception of the wedge micrometer, the tool could be

Mr. La Touche.



La Touche Strain-Gauge.

made in any workshop having a shaping-machine and a lathe. It would also, in his opinion, founded on more than 500 tests on spans of 6 feet and upwards, meet the difficulty mentioned by the

Mr. La Touche. Author, of obtaining a trustworthy record when the effect of the impact was that of a sudden blow, especially in cross girders and where violent vibration was set up. The strain-gauge could be set in position by one person; the readings gave the stress in tons per square inch directly, without calculation; and the gauge was so simple that any bridge-inspector could be trained to use it. There were no moving springs or multiplying-levers, so that the risk of disturbance by vibration or shock was minimized. The reason for choosing 13 inches as the distance between the points of attachment was that, the modulus of elasticity of ordinary structural steel being 13,000 tons per square inch, the alteration in length due to a stress of 1 ton per square inch would be 0·001 inch in 13 inches. The micrometer measured this alteration in ten-thousandths of an inch, giving the intensity of stress in tons and tenths. Was it not possible that the double peak shown in *Fig. 7* might be due, in part at least, to the establishment of a node, so that the girder vibrated in two or more equal parts? He could imagine the formation of such a node when an engine first struck a girder, and its suppression later when the span was more fully loaded.

Mr. Lindenthal. Mr. GUSTAV LINDENTHAL observed that the effect of impact from locomotive and train loads on bridges could not be solved on the basis of static stresses; it belonged to the region of dynamic stresses, in which theory alone could not be relied on to give useful results. Empirical facts were the only safe guide for them. For illustration: the ordinary rail was a bridge of short span. It would forever bear with perfect safety a standing locomotive which stressed the steel nearly to its full elastic limit. The same rail would not last a day under locomotives rolling over it at ordinary speed. What was the difference in the stressing of the steel? Engineers did not know, although they might theorize. The only practicable way to make the rail safe for the rolling stock was to make it heavier and stronger. How much heavier and stronger? Again a safe answer could not be given from *a priori* reasoning. But it could be found by experiment, taking a stronger and stronger rail until it did not break under the fastest locomotive. If the stresses in the rail were then computed on statical principles, that was, with the locomotive standing still, the static stress would be probably only one-fifthof the elastic limit. Thus the ratio of static to dynamic stress was established empirically as 1 : 5 or more. That was, making the rail five or more times as strong as would suffice for a stationary locomotive provided a safe rail for the moving locomotive. So it was with axles for cars and locomotives. Mathematical reasoning *a priori* would lead nowhere: experiment

alone gave safe results. And so it was with impact in bridges. The investigations described by the Author were interesting attempts to find the stresses from impact theoretically. But the engineer should not rely on them: he should be guided by safe empirical methods resulting from experience and observation. That had been his underlying thought in the devising of a safe formula for impact on railway-bridges,[1] which had been used in the designing of the Hell Gate bridge in New York, carrying four tracks. That structure had been designed for the heaviest locomotives and train-loads moving abreast on the four tracks, and it was thus the strongest railway-bridge of long span in existence. Another railway-bridge for which that impact formula had been used was the Sciotoville bridge over the Ohio river (two tracks), designed for the heaviest locomotives and coal-wagons moving abreast. Such heavy loads abreast might never occur on that bridge, or on the four tracks of the Hell Gate bridge; but, if they did, the bridges would be perfectly safe. It was the business of the engineer to make them so. In those two structures the problem of impact was exceptionally important because of the long spans and the heavy loads.[2] The exact paring down of steel in bridges, on the basis of nice theoretical assumptions, could not be regarded as good engineering practice, albeit it might have the appearance of greater scientific accuracy. Nevertheless, research was not to be discouraged, and experiments might furnish much-needed explanations of the behaviour of steel under sudden stress. On all the American trunk lines, most of the iron and steel bridges built before 1900 had been replaced by stronger structures. Some railways already had the fourth generation of metal bridges. Bridges always failed first in the floor, and weakness in the trusses followed because of insufficient provision for impact stresses from heavier and faster locomotives and trains. It did not pay to dimension metal railway-bridges too closely to the economic conceptions of the day. But the limit had probably been reached, although heat-treated 150-lb. rails for the track were under consideration on the Pennsylvania Railroad. Axle-loads of 80,000 to 90,000 lbs. had been attained in special locomotives, but the railway-bridges built within the last 15 years could safely carry them without deterioration.

Mr. Lindenthal.

Mr. H. J. NICHOLS observed that, while there could be no question about the value of obtaining a clear understanding of the causes

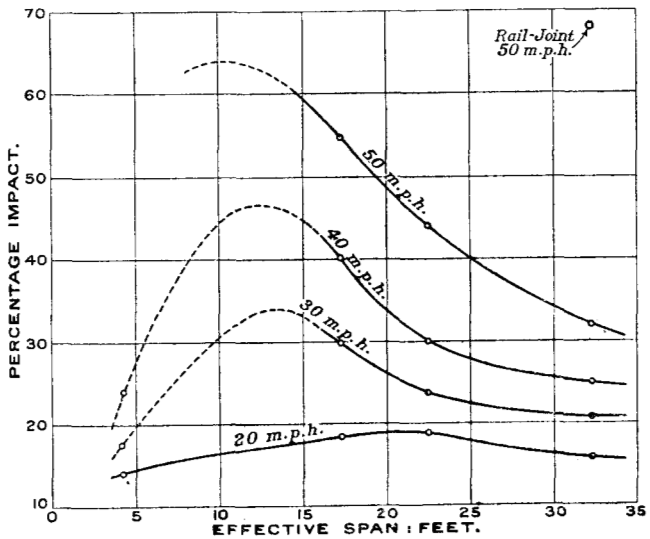
Mr. Nichols.

[1] *Engineering News*, vol. lxviii (1912), p. 216.

[2] "The Hell Gate Arch Bridge," Trans. Am. Soc. C.E., vol. 82 (1918), p. 852; and "The Continuous Truss Bridge over the Ohio River at Sciotoville, Ohio," *ibid.*, vol. 85 (1922), p. 910.

Mr. Nichols. of impact and of its intensity in bridge structures, it would appear that for purposes of design the tabulated hammer-blow allowances were rather idealistic. Such tabulated allowances would be of more real value in checking the strengths of existing bridges when carrying existing locomotives. The bridge-engineer was, in fact, asked to go into great detail to design and build a bridge to suit an engine which did not yet exist and which might never exist. That would appear to be putting the cart before the horse. It would surely be more rational to allow the bridge-designer to continue to use tables of equivalent uniform loads and to send the hammer-blow tables

Fig. 29.



PERCENTAGE IMPACT UNDER ELECTRIC ROLLING STOCK.

to the locomotive-designer to do as he pleased with, provided he kept within given bending-moment and shear curves. In spans of 150 feet and over the stresses due to impact constituted only a very small fraction of the total stresses in the main members, and until further knowledge was gained, the wisdom of attempting excessive refinement in this small fraction for design purposes appeared doubtful. Dead-load stresses might constitute 50 per cent. or more of the total, but at present their distribution was taken for granted. Secondary stresses and relief of stresses which might be caused by dead load, live load, and impact, were ignored. He had measured secondary stress amounting to 25 per cent. of the live-load stresses in a 183-foot span. Relief of stresses amounting to 20 per cent. had also been

obtained. These factors, until they were accurately accounted for, rendered abortive any proposed adjustment of impact-allowance, which in the longer spans could not amount to more than 2 or 3 per cent. of the total stress in a member. Much, however, might be done towards correcting or entirely eliminating overbalance in future engines. It would be of interest if the Author could state whether any experiments had been carried out with two-cylinder engines balanced for rotating masses only. Such experiments were reported from America, and it appeared that, with the exception of a " nosing " period at about 22 miles per hour (the diameter of the driving-wheels was not stated) the engines ran remarkably smoothly. No allowance for wheel flats appeared to have been recommended by the Committee ; presumably the rail-joint allowance was intended to cover that. With regard to short-span bridges with the girders directly under the track, although hammer-blow was incapable of causing resonance, he had observed in tests that the deflection of the rail between successive cross sleepers constituted a definite cause of vibration, and with sleepers spaced 2 feet 6 inches apart between centres it was conceivable that resonance might be established. *Fig. 29* showed impact percentages as measured by Fereday-Palmer recorders under electric rolling stock with 21½-ton axle-loads and a heavy unsprung weight. The speeds shown were approximate only. He was of the opinion that in such cases impact was almost entirely a question of track- and wheel-maintenance. On a 17-foot span, under comparatively light rolling stock with very bad wheels but at moderate speeds, an impact of 143 per cent. had been obtained during the tests referred to. It might be suggested that standards of bridge-design should be based more on the weight of the paying load to be carried and less on the weight of the locomotive. In the case of the Indian Heavy Mineral Standard of Loading, the heaviest section of the locomotive (23 feet long) amounted to 6 tons per linear foot plus hammer-blow, as against 2·3 tons per linear foot without hammer-blow for the wagon load.

Mr. Nichols.

Mr. GEORGE RICHARDS considered that the outstanding feature of the work carried out during the period 1923–1928 by the Author and the Bridge Stress Committee was their solution of the problem of hammer-blow. Under various names hammer-blow had long been recognized as the biggest component of impact when two-cylinder steam-locomotives were used ; for instance, in the American tests of 1907–1910, Professor F. E. Turneaure found [1] at least 80 per cent. of the total impact effect to be due to " unbalanced locomotive

Mr. Richards.

[1] Minutes of Proceedings Inst. C.E., vol. cc (1915), p. 269.

Mr. Richards. driving-wheels." The Committee's work on the other causes of impact, detailed in Chapter VII of their Report, appeared to be less exhaustive than that on hammer-blow; but they had published in Chapter VIII a tabulation of loads covering allowances for impact which were likely to be adopted by designers of steel railway-bridges in Great Britain for many years to come. It was admitted, on pp. 111 and 112 of the Report, that some of the allowances for impact were empirical and that they were meant to apply only to the standards of permanent way and design usual in Great Britain; so that the tables of loads printed on pp. 133–138 of the Report could not be accepted for all railways in India and other countries overseas. The Author drew attention to the need for discrimination in applying the tables to railways abroad. However, with the aid of the Report of the Bridge Stress Committee and the Author's Paper, the Railway Board of India should now be able to deduce from records of bridge-tests made in India, in 1921 and subsequent years, not only the allowances for impact suitable for the design of new bridges on railways of all their gauges and standards, but also, approximately, the probable impact effect of any train-load on any old iron or steel railway-bridge in India. Whether the Board were or were not in a position to do this at once depended mainly on the extent to which the bridge-tests recommended in paragraph 19 of the Fifth Report of the Indian Railway Bridge Committee had been carried out. He could not say how far that had been done, because 5 years had elapsed since he left the Board and India. From a remark near the top of p. 3 of the Report of the Bridge Stress Committee it appeared that the Committee had been induced to tabulate their allowances for impact in the form of equivalent uniformly-distributed loads for the sake of convenience in design. The tabulation of allowances for impact for new bridges in any other way might raise objections in a designer's office; but for dealing with old bridges carrying loads for which they were not designed, Mr. Richards still liked to visualize impact effect as (1) the effect of a concentrated load moving across a bridge on a driving-axle of a locomotive, and producing approximately the same maximum effects as the "hammer-blow" (if any) of that locomotive, plus (2) the effect of a load uniformly distributed over the span and producing approximately the same maximum effects as the "other causes of impact."

Mr. Swain. Mr. L. H. SWAIN remarked that, as a member of the Bridge Standards Committee of India, he had been particularly interested in the Report of the Bridge Stress Committee. He had been struck with the systematic and thorough way in which the work had been done and the high degree of accuracy which had been aimed at in

Mr. Swain.

determining what the dynamic increment was. He might perhaps be permitted to add as a caution that, in the scientific interest of that close pursuit of the dynamic increment, there was some risk that the broader requirements of providing a basis for legislation, in the establishment of a reasonable covering formula, might be overlooked. The Report was of particular interest to railway-engineers in India because in 1925, as a culminating result of the work on the subject which was begun in 1917, the Railway Board published[1] a mass of data and a new formula for impact, which, in comparison with the results now put forward, showed that the latter were to a large extent a confirmation of the Indian formula. The Indian formula had been very favourably reviewed[2] by Professor R. Desprets, and it was a further gratification to the members of the Indian Committee to find confirmation in the present and latest work on the subject. The Bridge Stress Committee had found that impact effect was conveniently measured by the increase of central deflection of the girders when the train ran over the bridge at a critical speed, over and above the deflection measured when the train passed over dead slow. It was a reasonable assumption that the increase of stress would be proportional to the increase of deflection, and that seemed to be borne out by the tests. It was also the view of the Indian Committee; and tests in India showed that it was accurate enough for practical purposes. However, when any lateral oscillation of the span occurred, owing to racking or hunting of the engine, such oscillation caused increase of stress in the booms which was not registered by increased deflection, and to that extent the increase of central deflection was not a full measure of impact. Another matter of interest, which was not mentioned in the Report, was the time-lag that occurred while the wave of stress set up by the pulsating load travelled to and from the points of support of the girders. That time-lag was appreciable in long spans and represented an interval of time between the application of the load and the setting-up in the girder of the corresponding stresses which it produced. It was mentioned in the Report that the work generally had been guided by the basic mathematical analysis of Professor Inglis. In the correspondence on Professor Inglis's 1924 Paper he had pointed out[3] that the weight of the train had been neglected by Professor Inglis in determining the frequency of vibration of the girders, and that that fundamental omission would have to be rectified before theory could be brought into line with facts. He was therefore interested to see in the present Report that the mass of the

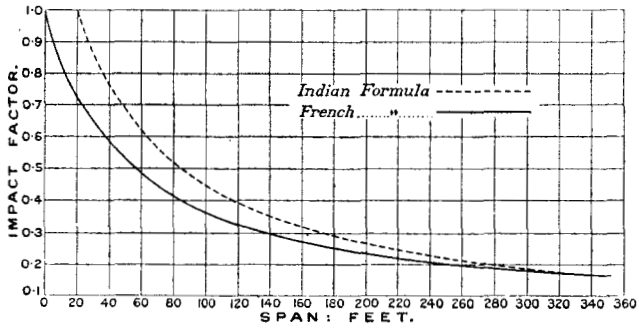
[1] Technical Paper No. 247.

[2] Bulletin International Rly. Congress Assocn., vol. 9 (1927), p. 367.

[3] Minutes of Proceedings Inst. C.E., vol. ccxviii (1925), p. 310.

Mr. Swain. train had been fully considered, and that, when that had been done, the calculation of the dynamic increment from resonance agreed closely with experimental results. Actually that merely confirmed the conclusions and the results of the Indian Committee published in 1925. An extremely interesting result brought out in the present Report was the occurrence of higher impact effects at a second critical speed. The existence of a second critical speed had been known in India, but it had only a scientific interest, since in actual tests the impact obtained was never so high as at the first critical speed, and it was not observed at all in the range of spans from 80 to 140 feet specially mentioned in Clause 62 of the Report of the Bridge Stress Committee. The Report made it clear that the effects recorded could only be obtained at extremely high speeds (up to 90 miles per hour) which were beyond the range of legislation

Fig. 30.



COMPARISON OF INDIAN AND FRENCH FORMULAS.

in India. Perhaps the most important conclusion that appeared to be drawn in the Report was that the impact allowance on short spans could be reduced. The primary argument on which that was based was that resonance in short spans could not occur, on account of their extremely high natural frequency, and that impact was merely represented by a single hammer-blow effect and such other increments as might arise from lurching of the locomotive and irregularities of the track, including rail-joints. Experience in India up to the present was that there was no calculable or well-defined impact on short spans, but that in certain instances it might rise to high values. Actually in India at present 100 per cent. was allowed on spans up to 20 feet, that allowance being entirely empirical but in conformity with previous experience. Some remarks in the first paragraph of Clause 15 of the Report appeared to be a sweeping condemnation of impact formulas in general, and the

American and Indian ones in particular, chiefly on the ground that impact was not (it was alleged) proportional to the live load. He disagreed with that view, because, unless there was a fundamental alteration in the design of locomotives, the hammer-blow and the dynamic effect generally were roughly proportional to the weight. At any rate, for general purposes of legislation no other assumption could safely be made, and from a practical point of view a formula of the Pencoyd type, modified to agree with actual requirements, was the most satisfactory way of specifying and covering impact effects. One of the criticisms that might be directed against a formula of the Pencoyd type concerned its application to central web members, in which application, by taking the loaded length for L in the formula, a higher allowance for impact was made on central web members than for the span as a whole. This might at first sight appear illogical and incorrect; but the truth of the matter was that the higher allowance was demanded by actual experience. The additional allowance, which was intended to cover the dynamic effects of reversal of stress in those members, which occurred in the passage of the locomotive, was most conveniently made by taking the loaded length in the impact formula. It was a case where the requirements of practice took precedence over theoretical requirements. *Fig. 30* showed a comparison of the Indian Railway Board's formula $65/(45 + L)$ with the latest French formula applied to Indian conditions. The latter formula was $i = \{0{\cdot}4/(1 + 0{\cdot}2\ L)\} + \{0{\cdot}6/(1 + 4\ P/S)\}$, where L denoted the span in metres, P the dead load, and S the live load.

Mr. Swain.

Mr. W. H. THORPE remarked that, interesting as were the conclusions summarized by the Author, there yet seemed to be some want of appreciation of their relative values. Prominence was given to the effects of resonance, but that that was of comparative unimportance was suggested by the history of bridge structures. It would be difficult—if indeed it were possible—to cite an instance during the last 80 years or so in which any main girder span had failed from misbehaviour due to that cause. Failures of small girders, or of the floor-structure of larger bridges, had occurred, owing to various inimical influences, to overloading, or to wastage; but experience showed resonance to be no serious menace. On the other hand, the influence of the hammer-blow was of manifest importance, and perhaps the one salient result of value was the information presented under that head. Reduction of the hammer-blow might not only prove advantageous to small girder-work, but also materially reduce the cost of road-maintenance.

Mr. Thorpe.

The AUTHOR, in reply, observed that Mr. Colam and others who had been interested in the work of the Indian Bridge Committee

The Author.

The Author. had commented on the omission of any reference in the Report to the impact formula contained in the Indian Report of 1925. The Author would express his regret for having omitted to refer to that formula in his Paper, and took this opportunity of commenting upon it. He had had the advantage of conferring with Professor Inglis, who had kindly given him the benefit of his views. It appeared that the Indian formula $I = 1050\ P/(w + p)\ cd$, in which P_1 denoted the hammer-blow at 1 revolution per second in tons, w the dead load and p the live load in tons per linear foot, c the circumference of the locomotive driving-wheels in feet, and d the static deflection in inches, had been evolved as the result of a study of Professor Inglis's Paper,[1] and it contained as a consequence the shortcomings of the theory which had been reached at that stage. Its true application, for instance, was limited to bridges of long span on which locomotive spring-movement did not occur, and where double-heading had to be considered, that was, to spans which exceeded, say, 250 feet. To medium- or short-span bridges the formula could have no general application. Further, the formula either took no account of damping or treated all bridges as having the same amount of damping—an obvious error. The compilers of the formula, judging from Mr. Colam's statement that "impact must depend on the number of blows struck," had not appreciated that damping imposed a limit on the amplitude of the oscillations and that this limit was reached after a few blows had been given, in fact, by the time the locomotive had reached the middle of a long bridge. It was true that under idealized conditions, such as were assumed by Professor Inglis in his Paper referred to, the extent of the oscillations depended on the number of hammer-blows, but such an assumption was untenable as the basis of a working formula. Since the Author's Paper was read, Professor Inglis had compiled the following formula, which gave the increased central deflection for the case when resonance occurred as the locomotive reached the centre of the bridge. This formula applied to long-span bridges in which the locomotive behaved as though it were springless, and was of no help in prescribing the impact allowance for medium or short spans. The maximum dynamic increment to the central deflection was

$$\frac{P_1 N}{4\ \pi^2 M_g} \cdot \frac{n_b}{n^2 + n_b^2} \times 12\ g \text{ inches}$$

where P_1 was the hammer-blow in tons at 1 revolution per second, M_g the total mass of the bridge in tons, N the loaded frequency of

[1] *Loc. cit.*

The Author.

the bridge, $n = v/2\pi$, and n_b was a coefficient defining the damping in the bridge. The close agreement obtained by Mr. Colam when comparing the results of the tests by the Bridge Stress Committee on three long spans and the impact effect predicted by the Indian formula was, as he suggested, satisfactory, and went to show that his formula was near the mark for the long spans. An interesting comparison of the practice in the United States and in this country in regard to live load and impact was given by Mr. Lang; but he took as a basis, so far as this country was concerned, the British Engineering Standards Association formula, which, for reasons given in considerable detail in the Report of the Bridge Stress Committee, was considered by the Committee as not only illogical but also inaccurate. Although there was much to be said for Mr. Lang's plea for simplicity and a broad view of the problem, he did not appear to the Author to have given any justification for the definite excess of metal and consequent expense which were entailed by continuing to apply the British Engineering Standards Association formula to bridgework in general and particularly to floor-systems. Mr. Lindenthal's statement that "the investigations described by the Author were interesting attempts to find the stresses from impact theoretically," did less than justice to the work of the Committee, seeing that the basis of their recommendations was the results of a long series of practical tests. There was, unfortunately, considerably more difficulty in ascertaining the ultimate strength of large girders, in relation to dynamic stresses, than in dealing with the strength of rails in the manner suggested by Mr. Lindenthal. Some fuller particulars of "the safe empirical methods resulting from experience and observation," which Mr. Lindenthal recommended as a guide for engineers, would be welcomed. The only remark of Mr. Swain with which the Author was not in agreement was his statement that the hammer-blow and dynamic effect of locomotives in general were proportional to their weight. So far as this country was concerned, nothing could be farther from the truth. The majority of the modern "giant" locomotives on British railways had more than two cylinders, and, as a result, their hammer-blow was quite small. Mr. Swain's statement, however, was not even true of two-cylinder locomotives. The practice of locomotive-engineers in prescribing the proportion of reciprocating parts to be balanced had varied widely, and there was a school of thought that recommended that the proportion of reciprocating parts which remained *unbalanced* should be related directly to the total weight of the engine—a practice which produced the exact opposite of the state of affairs erroneously suggested by Mr. Swain.