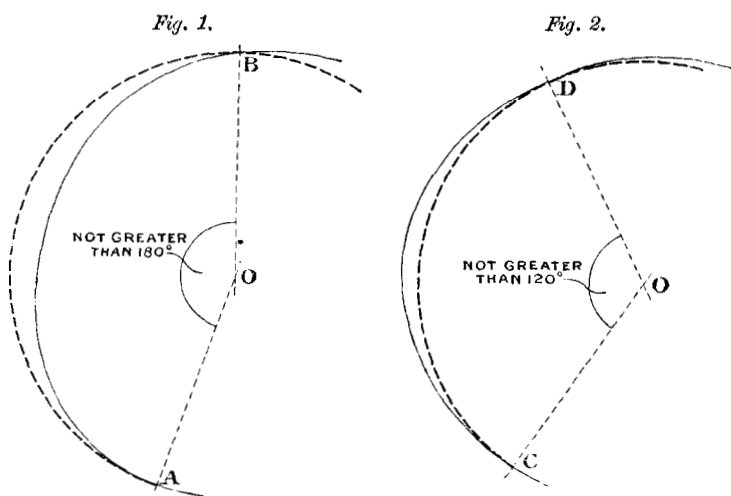


# Discussion.

The PRESIDENT moved a vote of thanks to the Authors for their two very useful Papers on a subject which was perhaps not in the forefront in England. The President

Mr. SHORTT, after exhibiting several lantern-slides showing diagrams of curves and junctions plotted to the distorted scale, explained that, as the method described in his Paper was based on a mathematical approximation, its use could not be extended beyond certain limits. In general those limits would not be exceeded if it were made a rule when working with the distorted scale never to let the improved or the original alignment curve through more than



180° from a point of contact or intersection without cutting one another, or to allow more than 120° between two adjacent tangent-points. *Figs. 1 and 2* would render this clearer. In each case the full line represented the original alignment and the thick dotted line the improved alignment. In *Fig. 1*, if the two curves touched one another at A, and cut one another at B, the angle AOB must never be allowed to be more than 180°. The same thing applied if the curves cut one another at both A and B. In *Fig. 2*, if the original alignment and the improved alignment were tangential at C and also at D, the angle COD must not be allowed to be more

Mr. Shortt. than  $120^{\circ}$ . The larger set of scales of 2 feet, 20 feet, and 200 feet to the inch had now been adopted as the standard, instead of the smaller scales of 4 feet, 40 feet, and 400 feet to the inch, mentioned in the Paper.

Mr. J. W. JACOMB-HOOD.

Mr. J. W. JACOMB-HOOD remarked that it would be a commonplace to say that the subject was interesting; from the point of view of very many engineers it was important, and it was quite time that it was discussed by The Institution. At the same time, it was a subject full of difficulty and a delicate one to handle, and if he did not make himself quite clear in the few remarks he ventured to offer, he would claim the indulgence of the members. The subject was important for two among many reasons, the two being opposite in their effect. It was important, in the first place, because the idea of increase of speed in the minds of the public rendered it desirable that anything tending to interfere with or restrict speed on railways should, if possible, be removed. There was a suspicion in the mind of the man in the street that curvature on existing lines of railway was more restrictive in this respect than it might be, and it was very desirable that the effect of the forces at work should be examined, and that that misinterpretation should, if possible, be cleared away. Again, it was important because, as was pointed out in both the Papers, when a train or a tramcar entered a curve a large amount of energy must be expended in resisting the lateral forces which tended to disturb the equilibrium of individuals carried in the vehicle. The necessary amount of power depended on the abruptness with which the lateral forces were applied, but of their application the individual was largely subconscious, if not unconscious, although the force exerted was probably very large. An idea of the energy expended might be formed by considering that every man, woman and child in the country, a population of forty-four millions, travelled in a train or tramcar sixty times every year, and the average length of each journey was about 15 miles. Assuming there was a divergence from a right line in every mile of railway or tram-line, the amount of energy expended in overcoming the effect of lateral forces must be very great indeed, and therefore all possible steps to avoid such excessive waste of power were fully justified. Mr. Spiller's Paper investigated all the circumstances and conditions that had to be met in considering the effects of curvature on railways, and Mr. Shortt showed how those forces could be counteracted to some extent. The subject was especially interesting to him because it had been his duty and privilege, with other members of The

Institution, to conduct officially a similar investigation; and it was satisfactory to feel that, although they had travelled over quite different ground, they had arrived generally, though not in particular detail, at much the same conclusions as Mr. Spiller, on whose Paper he wished chiefly to speak. The investigations he had carried out had been not only theoretical but also practical. It might be a little superficial, but the first criticism he had to make was that the title was a trifle inconsistent. Mr. Spiller called his Paper "High Speed on Railway-Curves," but there was a somewhat conservative atmosphere when the Author came to consider the possibility of limiting speeds, and Mr. Jacomb-Hood would suggest the title should be "The Lowest Possible Speed on Railway-Curves." It was satisfactory to see that Mr. Spiller proposed to trace only the nature and extent of the forces; he did not seem to care much about going into questions of maximum speed-limits, and Mr. Jacomb-Hood thought that was a wise limitation. In the earlier part of the Paper the Author examined the question of disturbances to regular running due to track-deformation, and gave a very clear and correct analysis of the effect of isolated deformations in rails in bringing about lateral oscillations. He also drew attention very properly to the causes of disturbances which might be very difficult and dangerous; but he omitted one cause to which attention should be drawn—one that under certain conditions might bring about derailment, and was much more likely to bring about trouble than were lateral oscillations—namely, the effect of low joints on curves. With two deformed places in a road, opposite each other, a condition of plunging was set up, especially with heavy locomotives, which might bring about a far worse state of things than any lateral oscillations, because there might be such a disturbance of loads on the different wheels that anything might happen, and perhaps at any speed. The moral was that the greatest possible care should be taken to see that the joints were kept in good alignment and level. Mr. Spiller did not touch, as he might have done, upon possible derailments due to bad balancing in rolling stock. At different times there had been a good deal of discussion on the question of irregularity in balancing, particularly of locomotives, and it was practically admitted now that some of the mysterious derailments that had occurred all over the world at quite low speeds were traceable to want of balance, either temporary or permanent. There were well-known cases where derailments at even low speeds had been brought about by irregularity of balance. He had in mind a case that had occurred again and again within his own knowledge where a

Mr. Jacomb-Hood.

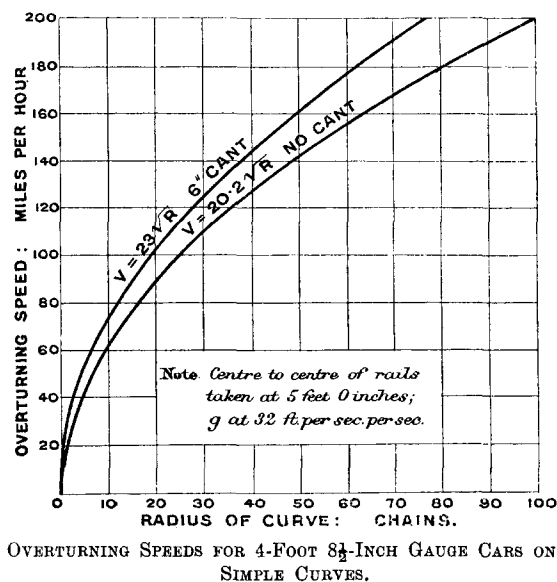
Mr. Jacob-  
Hood.

locomotive loaded partly and irregularly with coal would go off on quite a flat curve with the greatest regularity; whereas if the bunker happened to be full of coal well distributed it would take a curve at high speed without any difficulty. In another case he had in mind the curve was of about 15 chains radius and the super-elevation was about 3 inches, and there trains ran regularly in complete safety and comfort at 35 or 40 miles per hour; but when an engine ran round at 3 or 4 miles an hour it went off the outside of the curve, owing of course to unequal loading of wheels due to superelevation of the outer rail. The next matter dealt with by Mr. Spiller was the lateral forces due to curvature, and he arrived at the interesting result that of the whole force, which he divided into two component parts, the force  $F_2$  was independent of speed and the force  $F_1$  was independent of cant. He quite agreed with Mr. Spiller in that; it was a condition of things that was very likely to be disturbing and looked unusual to most people. It certainly paved the way to the conclusion Mr. Spiller came to, that within limits speed had really very little to do with questions of derailment on curves, except from one point of view. Mr. Spiller went very naturally from the question of speed and lateral forces to the question of super-elevation or cant. This question occupied a very important position in the mind of the average man in the street and of many railway men. The settlement of proper cant or superelevation was one of the most difficult mechanical problems that responsible railway men had to deal with, and involved taking into account many considerations. It was a little disconcerting to find people all over the country—people who ought to know better—adopting the view that super-elevation was the end-all in curvature. He had heard official views expressed again and again, that because there was so much super-elevation the permissible speed on a particular curve should be so much; but Mr. Spiller had knocked the bottom out of that argument by showing—and he agreed with him—that canting hardly possessed the virtues generally ascribed to it. Attention was drawn in the Paper to the really very shadowy advantage to be gained on curves by the superelevation that was generally possible, the Table set out in the Paper showing that, with 4 inches of superelevation, overturning due to centrifugal force would take place at twenty-three times the square root of the radius in chains, and that if there was no cant at all the same thing would happen at about twenty-one times the square root of the radius. He had arrived at the same result, and he had put a diagram upon the wall to illustrate it (*Fig. 3*). In that case the limit of cant was

extended from 4 inches to 6 inches, 6 inches being the limit that was considered advisable in the service in question. Assuming 6 inches of cant, the advantage of cant could be seen clearly in the diagram. At a speed, in miles per hour, equal to 23 times the square root of the radius of curvature, a train or vehicle would be just balanced; if there were no cant at all it would just balance at 20.2 times. The advantage of superelevation was therefore very shadowy. When Mr. Spiller turned from the question of centrifugal force to the much more important question of derailment due to "climbing" the rails from lateral pressure, it

Mr. Jacobb-Hood.

Fig. 3.

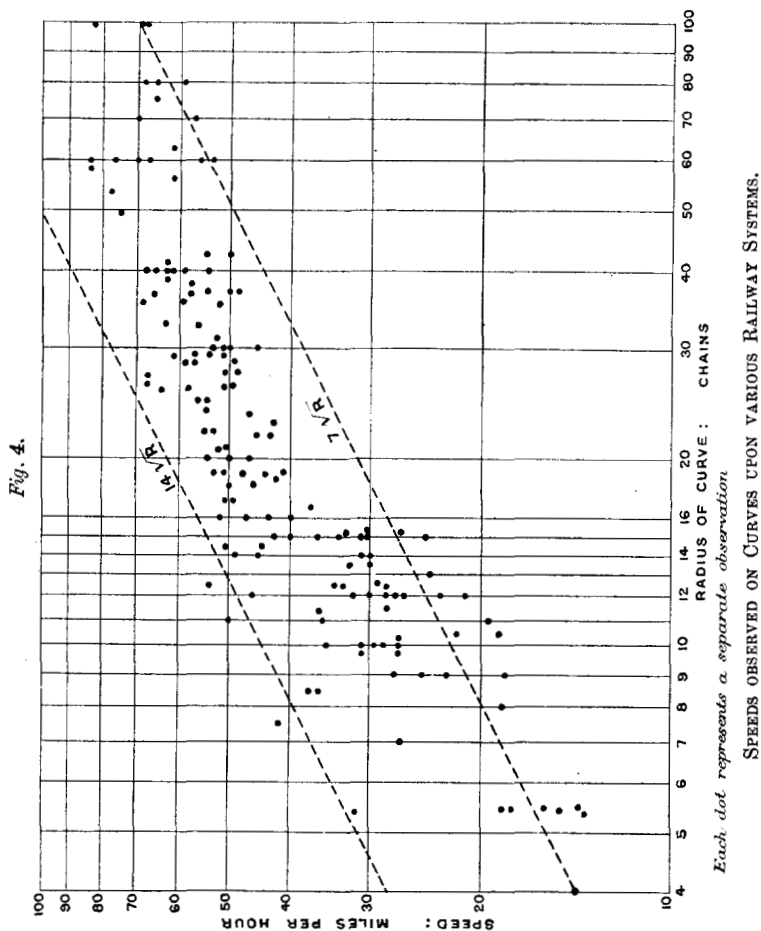


(Height of Centre of Gravity above Rails, 6 feet.)

would be seen that the danger of climbing was clearly present at nearly all speeds, and that speed had only a limited effect upon it, as was shown mathematically by Mr. Spiller's investigations. It was also shown, and was worth emphasizing, that under certain conditions it was far safer to have no cant at all, if there was a danger of climbing, than to have cant; because with cant there was a liability to throw more weight than was desirable on the inside rail, and less on the outside rail. It followed that the two effective factors to obviate climbing

Mr. Jacob Hood. were high speed and no superelevation. It seemed paradoxical but it was true. In connection with climbing, Mr. Spiller arrived at a result that required a little reconsideration: he did not recognize the fact, as he would do when it was pointed out to him, that at the critical speed due to climbing which he arrived at, the ratio of  $w$  to  $W$  would vary owing to the centrifugal force. This ratio was taken in his formula as 1 to 15, but with the increased weight on the outside wheel due to centrifugal force, it would actually be about 1 to 9; so that the critical speed at which overturning would take place would not be  $1.743\sqrt{r}$ , or  $14.6\sqrt{R}$ ,  $r$  and  $R$  being the radius in feet and chains respectively, but would be more like  $17\sqrt{R}$ . When Mr. Spiller came to divide the critical speed by a working-factor of 2 he was out of court. If he went to the railway-authorities responsible for speeds and asked them to adopt such a low limit as about seven times the square root of the radius in chains they would laugh and say they would chance a higher speed than that—in fact, that they had to go much higher or stop the railway. With regard to that particular point, he had placed on the wall another diagram (*Fig. 4*) in which were shown two lines indicating respectively the speed which Mr. Spiller suggested was the critical one with regard to climbing, namely,  $14\sqrt{R}$ , and the speed which he suggested should be the limit. Between those two lines were a number of black dots which represented observations of actual speeds on certain curves. It would be seen that in practice the speeds on most curves were considerably above the limit Mr. Spiller proposed, and none of them could be regarded as excessive except the three above the upper line; and they indicated that in everyday working the speed thought to be a critical one and liable to bring about derailment due to climbing was very nearly reached. He had no doubt cases like that occurred by the thousand—in fact he knew of a case where every day the speed over a curve of 12 chains radius was 50 miles per hour, or nearly  $15\sqrt{R}$ . That had been going on for years and was likely to go on for many more years. Great care was taken in preserving the alignment and level of the road, and the superelevation was high, say about 8 inches. He thought, therefore, that Mr. Spiller must reconsider the limit at which he thought the critical speed would be reached. It was very important that any attempt at settling speeds should not be too dogmatic. No one interested in the subject deprecated investigations into the forces involved in running trains round curved lines; indeed, it was important that those forces should be investigated in the fullest degree: but if any attempt were made to fix maximum limits of speed, he suggested they

should be fixed not at the lowest but at the highest possible level. Mr. Jacomb-Hood.  
 Railway practice certainly could not accept the level of speeds suggested in Mr. Spiller's Paper: both theory and practice were against it. It was pointed out by Mr. Spiller, and Mr. Jacomb-Hood wished to emphasize the truth of the remark, that within



limits, and excluding one cause of difficulty, trouble, and danger at curves, speed was not of great importance. He also wished to emphasize another fact enunciated by Mr. Spiller, that super-elevation was to a large extent—excluding, of course, that one cause of difficulty—independent of speed. The view of the average

Mr. Jacobm-Hood.

railway man, that the speed-capacity of any given curve should be measurable entirely by the cant, was an idea that ought to be disposed of by the discussion. It caused considerable difficulties when an attempt was made to measure the effect of a curve, and, as the Author had shown, it was not, strictly speaking, a fair method to adopt in arriving at suitable speeds on curves. That brought him to the subject of superelevation. He had said that superelevation must not be regarded as a fetish, but it was still of great value. From the point of view of safety it might be very largely disregarded, but from the point of view of comfort it was difficult to place too much importance upon regard for superelevation. Whether superelevation should be large or small depended greatly on the conditions at any particular point, just as did speed. It was important to remember, however, that the possible range of superelevation was considerable. The difficulty of the question was illustrated by a story he had heard a short time ago, of an engineer with very wide experience of railway-maintenance in India, who was applied to by letter, by a Royal Engineer officer temporarily in charge of a length of line, for assistance and advice under a difficulty. The officer had found in Molesworth's Pocket-book a formula which, applied to a particular curve, would give a superelevation of  $3\frac{1}{8}$  inches; but on turning to Camp's Notebook he found a formula which would give a superelevation of  $3\frac{1}{2}$  inches, and he was troubled to know which he should apply. Having referred to the books in question, his friend replied that if the inquirer looked at the books again he would see that one formula applied to a 5-foot gauge and the other to a 4-foot  $8\frac{1}{2}$ -inch gauge, and, after all, he thought it would be quite safe if the  $\frac{1}{8}$  inch and the  $\frac{1}{2}$  inch were disregarded. With regard to the unequal loading of opposite wheels, which was certainly a cause of derailment on curves, perhaps another illustration would explain his point—a case that occurred in his own practice a short time ago. A branch line was under construction, and not far from the junction of the branch line there was a long curve of 12 chains radius which was worked round regularly by stock of all kinds, including four-wheeled coaches with a 13-foot wheel-base. As a rule such coach stock, being 8-ton trucks, on four wheels, were fairly uniformly loaded, and there was no difficulty in working them round that curve and many other curves of shorter radius. But on one occasion a truck in the middle of a train was derailed when going at quite a low speed—about 10 miles per hour. The cause was a mystery until the resident engineer in charge discovered that that particular truck was loaded with a steel girder weighing about 30 cwt., disposed on the truck



diagonally so that the load was on the inside leading wheel and on the outside trailing wheel. Immediately the truck reached the curve in question it went off the line on the outside of the curve. The truck was unloaded, placed upon the rails again, and tried in both directions, when it took the curve very easily. It was then loaded again in the same way, and again it went off. It was then unloaded again, replaced, and loaded properly, after which it ran quite well. That proved that the unequal loading of opposite wheels would produce an unexpected result. That condition was suspected to be not unusual, and might be a contributing cause to many otherwise mysterious derailments on curves at all speeds. When the results of the forces due to curvature had been cleared up as far as possible, there still remained the question how to remedy the difficulties that arose. Both Papers dealt with that in an excellent way so far as one cause of derailment and danger was concerned. There were, in fact, two main remedies with which those who were responsible for the maintenance of roads suitable for high speed were concerned. The first was undoubtedly very careful attention to alignment and level. Too great care could not be taken to see that curved roads on which high speeds obtained were kept in perfect circular curvature in order to overcome the possibilities that Mr. Spiller drew attention to—derailment due to oscillations, lateral and fore-and-aft. The second remedy was the only other remedy railway men had in their hands, namely, transition-curves, which were, in fact, the main subject of the Papers. Transition-curves were proved by the daily work of railways to be not only important but also a natural remedy for the trouble. It had been pointed out again and again that nearly every circular arc came, sooner or later, by the common sense of the man in charge of the maintenance, to have at each end a rough and irregular transitional spiral curve. But of course the irregular transition-curves provided by the average trackman's work were not as satisfactory as those laid out by such methods as the study of the question suggested. All English railways, he believed—and certainly all railways in the United States—had realized the importance of providing in a suitable and methodical way spiral curves at the approaches to all circular curves on which high speeds obtained, and of keeping roads in the best possible condition for running. The difficulty that most railway men had experienced had been to make it possible, in the press and rush of daily work, with lines full of traffic, to examine the curves in order to see exactly their condition, and so to lay out approaches to them that the work could be done fairly quickly and economically. It was in

Mr. Jacob-Hood.

Mr. Jacomb-Hood.

investigating that question that Mr. Shortt had arrived at the method put before The Institution. When Mr. Shortt suggested his method, Mr. Jacomb-Hood's Company had been examining the curvature of their line for some little time and had set aside a small staff for the purpose. The first thing they found was that all circular arcs had become very irregular, and that some of the long circular arcs had greater variations of radius than ought to exist; in fact were so irregular that the condition Mr. Spiller pointed out was readily set up. The condition of the curves having been found and recorded exactly, consideration was given to the methods by which improvements could be made, and then Mr. Shortt put forward the plan he now described. That method of course was subject to criticism. It might be called rough and ready; but Mr. Jacomb-Hood ventured to think that the results of its application were such as to quite justify it. To put the method very briefly, it might be described thus:—Having arrived at the best possible form of spiral for the entrance and exit of curves by the method described in the Paper, the setting out of that transition-curve was fairly simple, because every transition followed exactly the same rule; that was, the shift of the tangent from the circular arc was a constant in every case, and the length of the transition was always referable to the radius of the circular arc. It was directly proportional to the square root of the radius. The offsets of each transitional spiral were exactly similar in every case. They were very convenient figures to adopt in applying the particular method, and it was obvious that such a method saved time. The arrangements, in fact, were simple and ingenious, and he could vouch for the success of the results; and he thought the circumstances that had led up to the use of the method quite justified the presentation of the matter to The Institution in the form in which Mr. Shortt had presented it.

Mr. Burge.

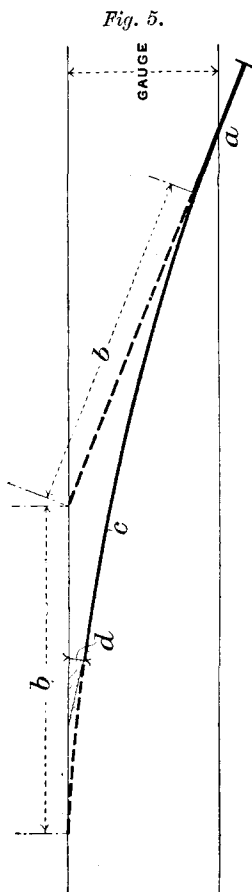
Mr. C. O. BURGE thought the members might like to hear some of the experience gained in Australia with regard to transition-curves, which had been used in that country for many years—even before the date of Mr. Glover's Paper. After a good deal of investigation the conclusion was come to that the best curve was the cubic parabola as recommended by Mr. Shortt, and it was also thought that a uniform length for all transitions would meet all requirements and obviate a great deal of calculation in the field. In the Colonies such high speeds as in England were not usual, and it might be said that transition-curves were hardly necessary where the speeds were low, but in the case he had in mind it was necessary

to adopt very sharp curves in order to cheapen the cost of construction; so that to a certain extent one thing balanced the other. A somewhat remarkable anticipation of Mr. Shortt's conclusion was arrived at, namely, that a 4-chain transition-curve was about the most suitable one for radii of 10 to 20 chains, and the formula given on p. 101 would lead to much the same result. Beyond 20 chains radius no transitions were used for any curves. The use of these curves had been established in New South Wales for the last 15 years, and about 1,000 miles of railway with such curves had been constructed during that time. He had travelled over them very frequently, and could hardly notice the curves, the entrance being so gradual. The calculations for those curves, and for the deflecting angles and offsets, were contained in Papers<sup>1</sup> read before the Royal Society of New South Wales. He wished to corroborate what Mr. Jacomb-Hood had said about unnecessary limitation of speed in connection with super-elevation of the outer rail. About 20 years ago there was a fearful accident on a railway in Australia, in the neighbourhood of Sydney. The brakes of a very heavy excursion train failed, and the train ran without control down an incline more than 7 miles in length with gradients ranging from 1 in 75 to 1 in 40, the greater part of the declivity being 1 in 40. The actual accident was the result of collision, not derailment. He was not in the train, but he saw the accident, and estimated that the train was travelling at least at 80 miles per hour. It ran over several reverse curves of 11 chains radius and still kept the road, although the super-elevation was not adapted to any excessive speed. He thought that case went far beyond Mr. Spiller's critical curve.

Mr. E. BENEDICT said that, so far from agreeing with Mr. Jacomb-Hood that Mr. Shortt's system was a rough-and-ready one, he would call it exceedingly refined; but then he belonged to two generations ago, when such things were not thought of. All he was asked to do in those days in setting out a curve was to make it miss the tangent and ease it in in the last few chains. Such methods accounted for the rough transition-curves found on old lines. With regard to setting out, he had always been taught to use a theodolite and angles rather than measurements from tangents, and it seemed natural that the former method should be preferable, because on a curve a tangent very soon got off the formation and could not be measured. In his opinion theodolite work was much more accurate and reliable.

<sup>1</sup> Journal and Proceedings of the Royal Society of New South Wales, vol. xxii (1888), p. 89; vol. xxix (1895), p. 51; vol. xxxi (1897), p. 56.

Mr. Benedict. The way of arriving at the curve was admirable, but the setting-out should be done with a theodolite. Another point was how to maintain the curves. In station-yards and in a cutting there would not be much difficulty, but how was it to be done on a bank of any height, because there was no bank that did not give way more or less? Then



there was the natural degradation of the road, and the lifting that had to be done, all of which would interfere with the very delicate pegs put in both for direction and for superelevation. With regard to the latter, he had to plead guilty to being the author of the anecdote Mr. Jacomb-Hood had related, and he wished to correct Mr. Hood in one little matter. The gentleman who referred to him mentioned the difference between the two pocket-books to the third decimal of an inch, and was told in reply that the first engine that went over that curve would alter the superelevation much more than the third decimal of an inch. With regard to turn-outs, the diagram illustrated what he considered was the right way to set them out. The method had been criticized, but he had never been told where he was wrong. The curve began at the end of the crossing *a* (Fig. 5), and therefore, the distances *bb* were equal, and *c* represented the curve. The place for the heel of the switch was where the offset *d* equalled the opening of the switch, say  $4\frac{1}{2}$  inches, quite irrespective of the length of the switch. The longer the switch the better. It was the angle and length of the crossing and nothing else that fixed the position of the heel of the switch and the radius of the curve.

Mr. Carus-  
Wilson.

Mr. C. A. CARUS-WILSON observed that Mr. Spiller rightly pointed out the great importance of the question of the wheel climbing the rail, and he would like to recapitulate briefly Mr. Spiller's argument in a slightly different form. Mr. Spiller began by drawing attention to the fact, illustrated in Fig. 6 (p. 88), that the critical question was how much side pressure was permissible

for a given weight on a wheel; and he laid down the rule, according to his calculation, that the wheel would be derailed if the force on the flange was 1.36 times the weight on the wheel. Then he said that in a bogie locomotive the weight on the leading outer wheel of the bogie was about one-fifteenth, or 6.7 per cent., of the weight of the locomotive. Putting that figure in the equation, a value was obtained for the derailing force on the flange equal to 9 per cent. of the weight of the locomotive. Mr. Spiller went on to say that the derailing force was made up of two quantities, one of which he called the lateral force, the other being the centrifugal force; and he gave reasons why he thought that the lateral force on the leading outer wheel of a bogie was one-twenty-fifth part, or 4 per cent. of the weight of the locomotive. Deducting that from the permissible 9 per cent. of flange-pressure which would cause derailment, it left 5 per cent. for the centrifugal force. Mr. Spiller then said that the centrifugal force on the front wheel of the bogie was one-fourth of the whole centrifugal force, so that if it were possible to have 5 per cent. on the leading wheel it would be possible to have 20 per cent. altogether. Taking the case of a 50-chain curve, 20 per cent. centrifugal force meant 100 miles per hour. In that way Mr. Spiller arrived at the limit he gave, shown in *Fig. 7*, that on a 50-chain curve the train would be derailed when 100 miles per hour was reached. That, briefly speaking, was his argument. With regard to safe running, Mr. Spiller said that for safety the speed should be limited to 50 miles per hour. It looked at first sight as if that gave a factor of safety of 2, but it did not. The factor of safety was really, of course, the relation between the force which, supposing the calculation to be correct, would derail, and the force which was really acting on the flange. At 50 miles per hour the force on the flange *qua* centrifugal force was one quarter of what it was at 100 miles per hour, namely, about 1.3 per cent. Then came in the lateral force, the 4 per cent. which Mr. Spiller said was independent of speed and therefore remained the same at 50 miles per hour as at 100, making a total of 5.3 per cent. The ratio of the derailing force, 9.1 to 5.3, was 1.7, so that according to Mr. Spiller there was a factor of safety of 1.7 at 50 miles per hour. He therefore said that on a 50-chain curve the speed should be limited to 50 miles per hour, and at that speed there was only a factor of safety of 1.7. Mr. Carus-Wilson ventured to think that those were two propositions which would not be accepted by railway-engineers; they would decline to limit the speed on a 50-chain curve to 50 miles per hour, and if they did so limit the speed,

Mr. Carus-Wilson.

Mr. Carus-  
Wilson.

they would not admit that they had only a factor of safety of 1.7. He had endeavoured to ascertain how it was that Mr. Spiller arrived at results which were so obviously out of touch with actual practice, and he thought it was due to two main causes. In the first place there was the vexed question of the lateral force, which, Mr. Spiller said, in a bogie locomotive was 4 per cent. of the weight of the locomotive. It would take too long to give his reasons for thinking that Mr. Spiller had altogether overestimated the value of that force, and he would only refer briefly to *Fig. 1* (p. 78). In calculating that force, Mr. Spiller said in effect, "Here is a truck moving round a curve from right to left; in order to get that truck to accommodate itself to the line of the rails a sliding force must be exerted, since if the truck rolls from A to B it must slide from B to C; it is easy to calculate the force required to produce that sliding by multiplying together the weight on the rail,  $W$ , and the coefficient of friction  $\mu$ ." That was the force Mr. Spiller took, but was it correct? It would be interesting to see how this method would apply to the tangential sliding that took place on the outer rail of a curve. Taking the case of a 50-chain curve, the outer wheel had to slide, to make up for the extra distance, about 2 inches in every 100 feet. What force was required to make that wheel slide? Following the Author's argument, the weight of the wheel multiplied by the coefficient of friction gave the force; but that was the force required if the wheel was absolutely skidding on the outside rail, and not rotating at all. The question was how to calculate the force required to make a wheel skid under the peculiar conditions when the actual skid was only a small percentage of the distance covered. It would take too long to go into the mathematics of the subject, but it was obvious that the real force required was only a small percentage of  $\mu W$ . The same thing was true of the force required to slide radially. He would confine himself to saying that under the circumstances that force could not exceed 1 per cent. of the weight, and that in many cases it did not exceed 0.5 per cent., and the Author's figures in his opinion were four to eight times too large. With regard to the influence that would have upon Mr. Spiller's calculation, on a 50-chain curve engineers would admit in practice a speed of 70 miles per hour, and at that speed the centrifugal force was 10 per cent., that was,  $2\frac{1}{2}$  per cent. on the outer wheel of the bogie. Adding to that the lateral force, which he submitted could not exceed 1 per cent., gave a total force of  $3\frac{1}{2}$  per cent. The factor of safety was the relation between the 9 per cent. which would cause derailing and  $3\frac{1}{2}$  per cent., or 2.6 to 1. Therefore a train could run round

a 50-chain curve at 70 miles per hour with a greater factor of safety than the Author had calculated for 50 miles per hour. But there was another point. So far nothing had been said about superelevation. He did not understand how the Author could say, as he did on p. 90, that cant would reduce the vertical loads on the outer wheels without diminishing the lateral forces to the same extent, and would therefore increase the tendency of the wheel to mount the rails. As a matter of fact, the effect of cant was perfectly well known. Taking the 50-chain curve at 70 miles per hour, the superelevation to compensate for the centrifugal force would be 10 per cent. or about 6 inches, with the result that when compensation had been made there would be nothing left but the lateral force, which was only 1 per cent., and the factor of safety, therefore, would be 9 to 1 were it not for the fact—and so far he agreed with the Author—that the effect of superelevation undoubtedly diminished the load on the outer wheel. A rough working rule was that 10 per cent. superelevation reduced the weight on the outer wheel by 20 per cent. Consequently, instead of being 9 to 1, the factor of safety was 6·4 to 1. There could be no questioning the fact that trains did run round 50-chain curves at 70 miles per hour, and they did so with a factor of safety certainly of not less than 6 to 1. It was obvious that when superelevation was adopted the amount of the flange-pressure remaining after compensating for centrifugal force was of the greatest possible importance. According to the Author it was in this case 4 per cent., or four times as much as actually existed. In conclusion, he wished to refer to some figures relating to curves on the Paris-Lyons-Mediterranean Railway, near Dijon, bearing on the question of the flange-force left after superelevating for centrifugal force. The radius of the curve was the same as he had mentioned, 50 chains, and the trains had to round that curve at 60 kilometres, or 37 miles, per hour. In the first instance the superelevation was designed to counterbalance exactly the centrifugal force, which was 2·7 per cent., and the outer rail was superelevated this amount. But there was an audible grinding of the locomotive as it rounded the curve—the locomotive had a rigid wheel-base of 18 feet 4 inches—and it was found that in order to get rid of the grinding the superelevation had to be increased from 2·7 to 4·7 per cent., an increase of 2 per cent., which meant 1 per cent. available for the leading-wheel of the locomotive. Thus an increase of 1 per cent. in the inward force on the leading outer wheel of the locomotive balanced the flange-force, and that bore out the statement he had made that the flange-force in such a case was only about 1 per cent. According to the

Mr. Carus-  
Wilson.

Mr. Carus- Author's calculations the flange-force in this case would have been  
Wilson. 8 per cent. He submitted that the flange-force, which was an exceedingly important quantity, had been very largely over-estimated by Mr. Spiller, and this explained how it came about that trains actually ran round curves at very much higher speeds than were possible according to his calculations.

Mr. Shelford. MR. FREDERIC SHELFORD had been struck by the fact that the discussion was the first upon the subject of easing curves that had taken place at The Institution, about 80 years after the introduction of railways and 40 years after high speeds came into use. It might be said that transition-curves might have been used without any discussion upon them in The Institution; but as a matter of fact, the older engineers who constructed the railways of England, and those in many countries abroad, did not use transition-curves, and consequently in many parts of England the passengers were subjected to shocks when a train went off the straight on to a curve. There were many instances; he could mention to Mr. Jacomb-Hood, Eastleigh in particular, where, on a journey to Southampton, a shock was always felt, although he believed there had been some improvement of late. Again, on the London, Brighton and South Coast Railway, in journeying to Arundel, the passenger suffered a nightmare of shocks. The first reference to transition-curves that he noticed in the Proceedings was among the Foreign Abstracts in 1891. It would scarcely be anticipated that lines in the Colonies should adopt such refinements as easement-curves. In out-of-the-way spots there was often a large amount of curvature and high speeds were not required. During the last 5 or 6 years he had endeavoured, in the Colonies with which he was connected, to introduce easement-curves. He said "endeavoured," because it was one thing to issue instructions and another thing to have them carried out on the ground. Instructions might be issued to the resident engineer to use the easement-curve, the resident engineer instructing the engineers under him; but the assistant engineers who actually set out the curves were inclined to think they were a fad which arose in the office at home in Westminster; and even if they thought transition-curves were good things, they were apt to think they could be put in afterwards. They had to find a good line; the country was unknown to them; and they were inclined to say that the easement-curves could wait until a good line had been found, and the platelayers could put them in afterwards. The point he wished particularly to emphasize was that easement-curves could only be put in, as far as he could see, before the line was actually staked out; they could not be put



in afterwards without sharpening the curve somewhere. The Mr. Shelford diagram he had used corresponded almost exactly with the diagram given in Mr. Spiller's Paper, the only difference being that Mr. Spiller worked out the length of his curve, which depended upon the square of the speed and varied inversely as the radius of curvature, for each case. To simplify operations in the field Mr. Shelford fixed the length of the transition-curve at 132 feet and worked out the lateral shift for each case, according to the curvature. For a 5-chain curve there was a lateral shift at the tangent of  $3\frac{1}{2}$  links, and for a 40-chain curve about  $\frac{1}{2}$  link. It was worked out in links for convenience in the field. The easement-curve, obtained after a considerable amount of calculation, unless there was a diagram or formula to work from, was not really the centre-line of the track required; it was the desired path of the centre of gravity of the engine or the vehicle, and the centre of the track should be rather outside that line. If the centre of gravity was a foot inside the curve, the centre-line of the track ought to be about 1 foot outside the easement-curve, so that the centre of gravity could go along the easement-curve itself. With regard to Fig. 11, Plate 2, which Mr. Spiller put forward as an example of a safe junction for high speeds, he wished to point out that if there was high-speed running on the right-hand road and there was cant, the rails on the left-hand side were higher; therefore on the left-hand road, where the cant was required on the right-hand rail, they would conflict. It seemed to him that that junction could only be carried out if there was no cant at all and the rails were level all through. The difficulty came in at the elbow, not at the crossings.

Colonel H. A. YORKE wished to make a few remarks on the Colonel Yorke. question of rail-climbing. Mr. Spiller and several of the speakers had referred to the question of derailment due to the climbing of the rail by the flanges of the wheels. Mr. Spiller began by referring to the accidents at Aylesbury, Salisbury and Grantham. On p. 95 he stated that two of the derailments alluded to at the beginning of the Paper occurred at junctions, and on p. 90 he referred to the critical speed for a wheel mounting a rail, saying that the danger of the wheel mounting the rail was much greater than the danger of overturning of the engine, and adding "This accords with practical experience, many more accidents having occurred through derailment than through vehicles overturning under the action of centrifugal force." Putting those remarks together, Colonel Yorke gathered that the Author meant that the accidents he referred to had occurred owing to the wheel climbing the rails, and he would like to know upon what grounds the Author based

Colonel Yorke that assumption. Those two accidents occurred undoubtedly at junctions, but they were not junctions of the ordinary description, where one line of rails bifurcated and continued to separate from the other line of rails; both at Aylesbury and at Grantham they were junctions between two parallel lines of rails, and were laid out in the shape of an **S** curve, usually called a reverse curve. In both cases the engine got round the first portion of the **S** curve without any difficulty, that was, without overturning or mounting the rails, and in both cases the derailment occurred on the curve of contra-flexure. Therefore those derailments were, in his opinion—he investigated one accident and his colleague Lieut.-Col. von Donop investigated the other—shown fairly conclusively to be due to the reversal of the curvature of the rails, and there was no question of rail-climbing at all. He would like to ask Mr. Spiller whether he could point to any case of an accident that had been absolutely proved to be due to rail-climbing pure and simple, without any other disturbing elements in the case. Of course derailments had occurred, but there were various factors that had to be taken into account. There was the disturbing factor of switches and crossings; there might be a weak road, insufficient ballast, bad fastenings, and many other disturbing elements that might assist or cause the wheels to mount the rails; but he did not consider that accidents under such conditions were cases of pure rail-climbing. In nearly every accident he had had anything to do with the chances were that the wheels would not have mounted the rails unless there had been present one or more of the disturbing factors to which he had alluded. He believed that derailment due to simple climbing of the rail by a wheel was exceedingly rare, and that at any rate it was desirable, in a technical Paper of so much importance and value as Mr. Spiller's, that vague statements should not be allowed to pass unchallenged. There was another point in regard to which the Paper was vague. Was the Author considering the case of an engine, or the case of trucks, or passenger-vehicles? If the case of an engine, was it a bogie-engine or a six-wheeled rigid-wheel-base engine? If wagons or other vehicles, were they four-wheeled or six-wheeled vehicles, or long vehicles with bogies at each end? He ventured to suggest that the conditions of rail-pressure were exceedingly diverse in accordance with the length of the wheel-base. He had happened that morning to be looking at Mr. Wellington's book, where the subject of curvature was treated at great length, and he found that Mr. Wellington stated that increasing the length of the wheel-base of a vehicle from, say, one gauge—that was, a length equal to the gauge of the line—to two

gauges increased the curve-friction by 58 per cent. That showed Colonel Yorke. that increasing the wheel-base must have an extraordinary effect upon some of the lateral forces referred to in the Papers. Mr. Wellington barely referred to the case of a six-wheeled vehicle, and most of his remarks were based upon the conditions applying to bogie-engines; but in England six-wheeled engines were very common and six-wheeled tenders far more common, and the conditions that applied to a bogie-engine were surely not quite the same as those which applied to a six-wheeled tender. He thought the thanks of The Institution were due to the Authors of the two Papers, because the subject was one of the utmost importance and interest; it was almost fascinating, but unfortunately its fascination seemed to be equalled by its complexity. He suggested that only the fringe of the subject was touched in the Papers, and of course it could not thoroughly be thrashed out in the present discussion, but he hoped it would be investigated and discussed with increasing zeal and frequency. Abstract theory was hardly sufficient; theory and practice should go hand in hand. The theoretical conclusions arrived at in the Papers could be tested by occurrences that had taken place in the past. In every notable railway-accident where derailment had occurred the facts were fairly well established; every possible theory could be considered in connection with it, and even the conditions under which the accident occurred could be reproduced on the ground. An enormous amount of valuable information could thus be obtained, if theory and practice were taken together, to illustrate some of the very startling occurrences which had happened on the railways. He hoped nothing he had said would result in discouraging the Authors of the Papers from pursuing their investigations; his hope was that they would continue to investigate the subject and would give The Institution, on some other occasion, the benefit of their further studies.

Mr. R. PRICE-WILLIAMS considered it a highly gratifying fact in the history of The Institution that the Papers should have been read at a time when the President, the acknowledged head of the profession, was the chief of one of the most important and famous railways in the kingdom. The discussion had been somewhat of an academic nature, and he wished to deal more with the practical aspects of the question. Allusion was made in the Paper to accidents at junctions, but no evidence was given as to the actual causes. The mere fact that the accidents had been cited had given the Papers a certain importance, because undoubtedly a strong feeling of uneasiness was still experienced by the public in connection with trains travelling at high speeds through junctions.

Mr. Price-Williams.

Mr. Price-Williams.

Mr. Spiller had very clearly shown that the proper curves at junctions must have the character of parabolic curves, but that view was not new. In the early days of railways, in the late forties, when he shouldered a theodolite and set out curves, it was generally known that in setting out a long curve from the two tangent-points with a theodolite the points on the curve never coincided at the middle, and the error was attributed to the fact that the central angle had to be bisected. He devised a little instrument which overcame the difficulty, by utilizing the whole of the traverse of the central angle and mechanically halving it to obtain the exact angle of the chord and tangent. That instrument was shown to one of the profoundest mathematicians of the time, Professor Whewell, who quite approved of the principle, but remarked that the true curve for railways was a parabola, and proved it very clearly. The instrument (which Mr. Price-Williams exhibited) had done very good service in its time with circular curves. Some years later, when Mr. Robert Stephenson was visiting Leeds, Mr. Price-Williams happened to be the resident engineer there on the Great Northern Railway, and Mr. Stephenson expressed a wish to see the instrument. He agreed that it was accurate, but also made the remark that his ideal of a railway-curve was a parabola, especially at the entrance to a junction. The difficulty with regard to the application of a parabolic curve was, Mr. Stephenson said, setting it out on the ground, which might be obviated. He thought the instrument shown might very easily be modified and adapted. Indeed, so much did Mr. Stephenson think of the ideal curve that he asked Mr. Price-Williams to call upon him on his return from a voyage he was making with Mr. Bidder for the benefit of his health, and said he thought he saw his way to get over the difficulty in regard to the vernier-plate. But Mr. Stephenson returned only to die, and the meeting never occurred. From that time Mr. Price-Williams had taken no further interest in the subject, and The Institution had kindly accepted the "cyclograph," as something that had met with Mr. Stephenson's approval, more than for any other reason. When the terrible accidents alluded to in the Paper occurred, his thoughts reverted to Mr. Stephenson's remark that he saw his way to adopt a parabolic standard curve, and he was happy to say that he had now devised a very simple method which no doubt was in Mr. Stephenson's mind. The design and explanation of the principle he had sent to The Institution, and he had received, quite properly, the reply that the mere description of a new invention was not germane to the objects of The Institution; but the

occurrence of a full discussion on so important a subject gave him an opportunity of drawing attention, not merely to the merits of the invention, but to the urgent necessity of the case. He had listened with the greatest interest to Mr. Jacomb-Hood's remarks, and the practical method so well described which he had adopted in improving the character of junction-curves on the London and South-Western Railway; and no doubt the President, in his dual capacity as the chief of one of the foremost British railways and President of The Institution, would be able to assure, not only the members but also the public, that everything that engineering skill and resource could do would be done to minimize the danger, if any, of derailment at railway-junctions. With regard to the diagrams on the wall, the Great Northern main line where the accident alluded to occurred was straight, and the adoption of the transition-curve there would involve the curvature of the main line in an **S** form. He had had a great deal to do with permanent way, and had found that with switch-tongues 30 feet long and a radius of curvature of 109 chains there were very grave practical difficulties, not merely in regard to the length of the tongue. The radius of curvature was governed by the space of  $2\frac{1}{2}$  inches, which Mr. Spiller proposed to reduce to  $2\frac{1}{4}$  inches, which was the width of the tire-flange path. The effect of this would be to increase the entrance radius of curvature to  $72\frac{3}{4}$  chains, and the 30-foot switch tongue would be a long and attenuated rail planed away to a knife-edge, which, even with a 60-chain entrance curve, would be a 1-in-15 angle at the first facing crossing-point. The other transition-curve, with its 109 chains radius and  $2\frac{1}{4}$  inches of tire-flange path, would need a switch-tongue 37 feet in length, to which, with the present class of steel rail-section, it would obviously be impossible to get the rail tapered down at the point. The question was a serious one and should be treated in a practical way, and he had not the slightest doubt that the difficulties connected with junctions would be very satisfactorily settled.

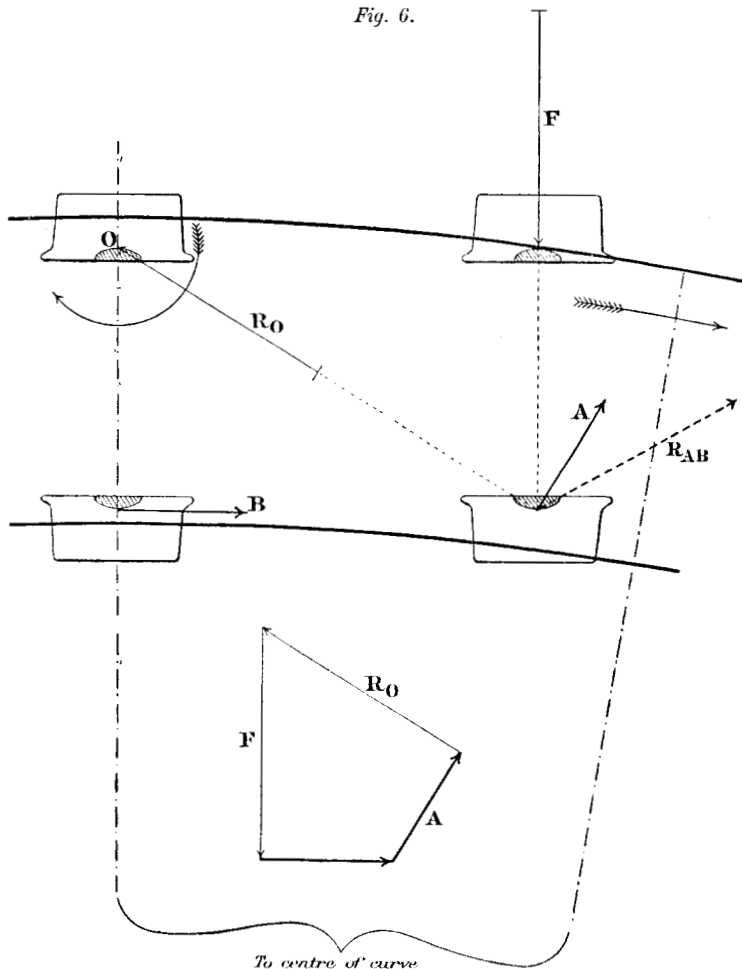
Mr. W. H. SHORTT had been keenly interested during the past 4 or 5 years in all the questions discussed in Mr. Spiller's Paper, and he therefore wished to say a few words on the points raised. In investigating the behaviour of a four-wheeled truck on a curve Mr. Spiller explained that the motion of a car might be considered as consisting of a straight roll forwards together with a horizontal rotation, and stated that while the position of the axis about which the rotation took place could not be definitely determined it would probably coincide with the point of contact of the rear outer wheel with the rail. In that he had followed other authorities on the

Mr. Price-Williams.

Mr. Shortt.

Mr. Shortt. subject, but unfortunately that particular position was one of those which, in Mr. Shortt's opinion, the axis of rotation could not possibly occupy. For, as would be seen from *Fig. 6*, the resultant of the frictional forces A and B, which opposed the sliding of the

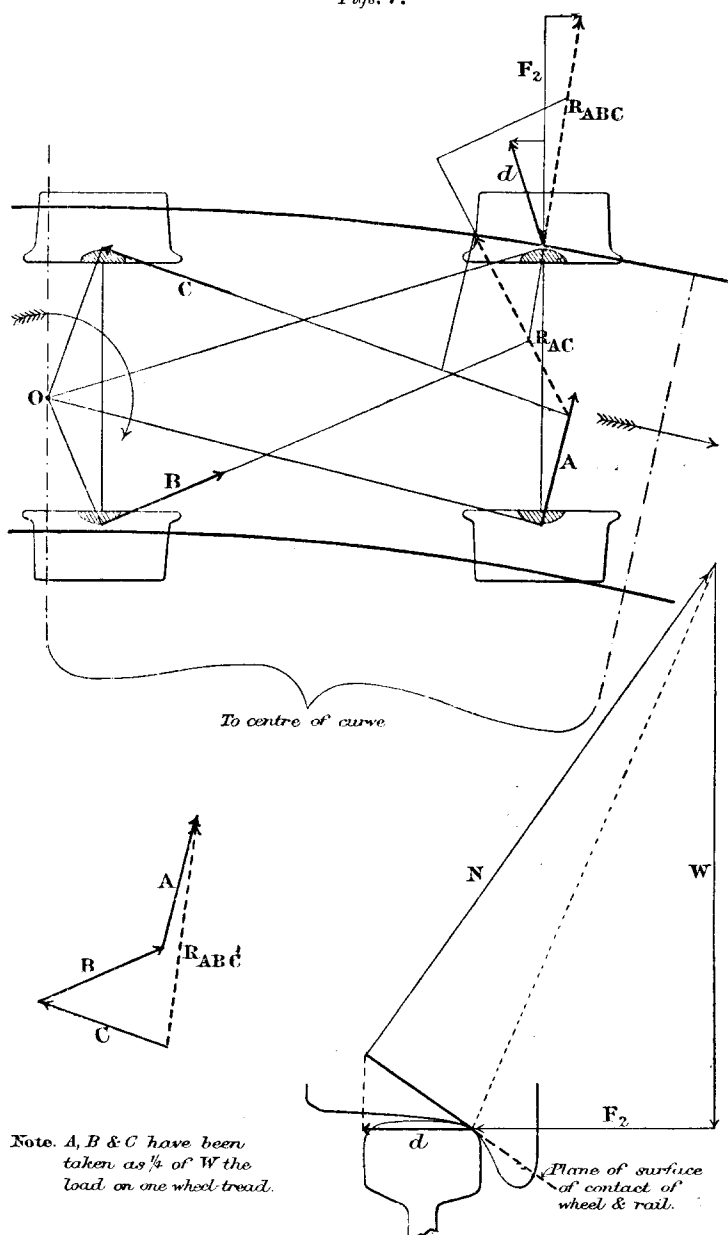
*Fig. 6.*



inner wheels, did not pass through the point of contact of the outer leading wheel with the rail, and consequently the lateral thrust of the outer rail could not balance it without the aid of a third force passing through the intersection point of  $R_{AB}$  and  $F$ , and also through the axis of rotation. If that force had not been large

Figs. 7.

Mr. Shortt.



Note.  $A, B$  &  $C$  have been taken as  $\frac{1}{4}$  of  $W$  the load on one wheel tread.

Mr.Shortt. enough to slide the rear outer wheel on the rail all would have been well, but, as would be seen from the triangle of forces  $R_o$ , the force required to bring about the equilibrium was larger than A or B, and consequently the rear outer wheel, which was assumed to be stationary, must be sliding on the rail. Therefore the truck could not be rotating about the point O. *Figs. 7* showed a possible if not the true position of the axis of rotation under normal conditions for a bogie-truck with wheel-base of 8 feet. It would be seen that the resultant  $R_{ABC}$  of the three frictional forces between the wheels and the rails passed through the point of contact of the outer wheel and the rail, and consequently could be balanced by a lateral thrust without requiring the aid of any force liable to shift the axis of rotation. Of course such a force was out of court, since the axis of rotation was now a virtual one. The outer leading wheel was sliding in the direction opposite to D, and consequently  $R_{ABC}$  must be acting in the direction shown in order that the two might be balanced by a force normal to the rail. The lower triangle of forces showed the forces keeping the outer leading wheel of the truck shown in *Figs. 7* in equilibrium on the rail. An important point which Mr. Spiller had not mentioned, was that the position of the axis of rotation definitely determined the position of the car on the curve, and vice versa; for the axis of rotation must always lie on a radius parallel to the axles as in *Figs. 6* and *7*. If both outer wheels of the car were hard up against the rail, then the radius parallel to the axles would come straight down midway between them, and the axis of rotation would necessarily lie on it. With regard to the position taken up by the car on the curve, he did not agree with the explanation of the way in which it was reached. The car might be close to the inner or outer rail instead of central with the track when entering the curve. Consequently, as the final position was the same in each case, the statement on p. 78 that when a leading outer wheel reached C (*Fig. 1*) it made the same angle with the rail as it did at A, could hardly be correct. He thought the true explanation of the way in which the car reached the position taken up on the curve was simply that the leading axle, in its endeavour to roll straight forward, tended to set itself as much askew to the track as it could, and the rear axle, in the absence of any external force, helped it until that axle reached the radial position as shown in *Fig. 6*; but once the rear axle had passed that position it commenced to journey towards the outer rail, and as both it and the front axle were trying to do the same thing, the one acted against the other and equilibrium was quickly established. The true position of the car would therefore not be quite as shown in Mr. Spiller's *Fig. 1* (p. 78), but such



that the rear axle was slightly in advance of the radial position, as Mr. Shortt. shown in *Figs. 7*. He was afraid he must also disagree with equations (2) and (3), pp. 81 and 82, which dealt with the lateral pressure exerted by the leading bogie-wheels of an engine. In the first place, when determining the lateral force  $P$  at the centre pin, Mr. Spiller had taken the frictional force at about 100 times its true value, for the coefficient of friction for lubricated bearings was nearer 0.01 than 0.05, and since friction was already acting round the axles in a circumferential direction, it could not be assumed to be acting with more than  $\frac{1}{20}$  of its full value in a direction at right angles to this.  $P$  as defined by the Author, when correctly calculated, was practically negligible. Secondly, Mr. Spiller had taken no account of the lateral thrust at the centre pin of the bogie due to the guiding action always demanded of the frame by the driving-wheels, even if there was sufficient play for their flanges to be up against the outer rail. Thirdly, the action of any thrust at the centre pin would alter the weights on the bogie wheels and shift the axis of rotation from its normal position, so that equation (1), which was approximately correct for the simple truck, would probably not apply. In spite of the fact that he did not agree with Mr. Spiller's equations for obtaining the flange-pressure, he thought that  $W/25$  was a very good estimate of its average value for modern express engines. On p. 89, in order to calculate  $\mu$ , Mr. Spiller had assumed that the flange of a badly worn wheel 4 feet in diameter might touch the side of the rail at a point about 7 inches in front of the point of contact of the tread of the wheel and the rail. Owing to the fact that flanges never got worn vertical, it was practically impossible for a worn flange to touch the rail more than 2 or 3 inches ahead of the point of contact of the tread and rail. This could be confirmed by anyone who drew out proper cross-sections of an actual worn flange and rail. Were it possible, as suggested by Mr. Spiller, there would be constant derailments through flanges climbing the points of switches and crossings. With regard to the danger of derailment when passing through junctions, he would like to point out that Mr. Spiller's equation No. 20 (not No. 24) applied to the case of the rear wheel, which was rolling away from the rail. Taking  $\theta = 65^\circ$  and  $\mu = 0.2$ , it would be found that the lateral thrust required to get that wheel over the rail was equal to four times the load on the wheel, so that it was practically impossible for it to be forced over the rail and consequently for derailment to take place in the manner suggested. Mr. Spiller said that Salisbury was perhaps the only case on record of derailment by overturning under the action of centrifugal force alone, but a very clear case had occurred in March

Mr. Shortt. 1907, on the Tralee and Dingle Light Railway, and he thought Grantham and Shrewsbury were also clear cases of overturning. In conclusion, he thoroughly agreed with Mr. Spiller's statement that the value of cant was greatly overrated. Cant was certainly desirable for the comfort of passengers, but the average passenger did not experience any discomfort when the actual cant was as much as 3 inches less than that theoretically required, provided the 3 inches was not taken away or returned very suddenly. He was sure that in the case of terminal stations, it would be much better practice to lay the whole of the complicated permanent way dead level, instead of following the present method of canting short lengths wherever possible and saying nothing about it where it was impossible. At terminal stations there could never be any danger of overturning, and the provision of cant would not prevent climbing. Fortunately, the engineer had the check-rail to use, and if it were used properly—that was, put in so that the rubbing side of the head was always 4 feet  $6\frac{3}{4}$  inches away from the outer rail, whether the gauge was exactly right or slack  $\frac{1}{2}$  inch or more, instead of being put in always  $1\frac{3}{4}$  inches away from the inner rail—it formed a very effective means of preventing climbing.

Mr. Robertson. Mr. F. E. ROBERTSON thought it was now time to consider from what point of view the Papers were being discussed. It was perhaps inevitable that the sad accidents which everyone deplored should have been used as a text for Papers of the kind, but he really could not see that they had much to do with the subject. The accidents had not happened because the road was badly laid, or because there was any particular defect in the rolling stock; something else had gone wrong, an accident had been bound to happen, and an investigation as to the last straw was not, he thought, of much profit. There was no more reason for discussing curves on the basis of those accidents than there was for considering the abolition of terminal stations because engines had been known to butt their way through and fall into the street. If engineers did not use transition-curves it was not for want of information. When he started as an engineer, more years ago than he cared to think of, he invested in a Rankine, and found in that book all that it was necessary to know in order to lay out a proper transition-curve. When he was directly in charge of permanent way, he took a certain amount of interest in the various formulas that were put forward from time to time, especially in the current journals; but some time ago he came to the conclusion that a transition-curve was no more a proper subject for discussion than was perpetual motion; because, so long as the platelayer was permitted to use some easement, it did not matter

whether a cubic parabola, or a spiral, or a sine curve, or any other particular geometric form was used. One speaker had expressed the wish that the subject should be thoroughly investigated and discussed, but Mr. Robertson maintained, with all deference, that the subject had been very efficiently discussed and investigated on the mathematical side. He had at home—not to mention plenty of other literature on the subject, such as Papers in the Institution Proceedings, in the Proceedings of the American Society of Civil Engineers, and in the technical journals—three volumes, foolscap size and about  $\frac{1}{2}$  inch thick, of translations from the German of articles on the action of trains on curves. He would be happy to present those volumes to the member who thought the subject had not been sufficiently discussed, or to anybody else who would like to have them. There were sufficient equations in those books for any one to wallow in. Except from the broadest possible point of view, such mathematical exercises were fruitless, because it was impossible to insert into them all the data. The most elaborate of them omitted many considerations of importance which vitally affected the question. There were, however, other aspects than derailment, from which easement-curves might be considered, such as ease in riding, and the broad question whether there should be an easement at all. Without wishing to hurt anyone's feelings, he would like to mention a splendid curve known to many, just this side of Boulogne. When that curve was taken by a train at a high speed the passengers were nearly thrown out of the windows, and he thought Mr. Shortt's Paper would do the gentleman who was in charge of that section a great deal of good. Mr. Shortt's Paper was a very practical and useful one in the way the Author handled the questions of the length of easement-curve required, and when such a curve was not required. He hoped that all the young engineers who had poorly-laid curves under their control would now set to work and put them right. If they had little excuse before for slovenly work they had still less now. To anyone who desired to measure the influence of acceleration on entering a curve with greater accuracy than by means of his personal feelings, he recommended the use of the so-called dynamometric pendulum, which was used by Mr. Desdouts<sup>1</sup> in some of the numerous and elaborate experiments the French engineers had made on the motions of trains on curves. It was a disk with a weight that could be shifted either way, the disk rolling on a steel bed. That description was, he thought, sufficient to indicate the

<sup>1</sup> *Annales des Ponts et Chaussées*, vol. xi, 1886, p. 371.

Mr. Robertson. nature of the machine, from which extremely accurate results could be obtained. Something might be said from a practical point of view on the question of superelevation, or cant, because it seemed to be a regular fetish among railway working men. On the Bolan railway an Abt track was laid, and with that arrangement much cant could not be used. The 800-foot curves were laid with only  $\frac{3}{4}$  inch cant. As the gradient of that line was 1 in 25 it was thought it would be interesting to loose a wagon down it, and a covered goods-wagon was accordingly allowed to run down two or three times, acquiring a speed of 55 miles per hour round the 800-foot curves without going off. He had looked up the superelevation that would be required according to two reputable formulas, one of which gave 15 inches and the other 16 inches; so that it would be seen there was a considerable margin of safety, and as a previous speaker had said, it was not necessary to trouble about sixteenths of an inch. Mr. Shortt's Paper referred practically to curves as viewed in plan, but Mr. Spiller referred more to the forces which tended to derail a train. He did not propose to go into the mathematics, with which he had no quarrel; the results as set forth in *Fig. 7* were very reasonable. But he wished to point out that the very detached nature of the calculations rendered them of little value in practice. In considering the geometry of two surfaces in contact with each other, one necessarily omitted most of the things that in practice led to derailment, such as imperfections in the track, oscillations of the vehicles, surging of water in the tenders, one vehicle jerking another, and last but not least, the unequal loading of wheels, which he believed to be the cause of all so-called mysterious derailments. Engineers were not concerned with derailments in cases where a train ran amok at 60 miles per hour, where the speed should be 20 miles at most; something was sure to go then: what they really were interested in were the petty, annoying, avoidable derailments, mostly in station-yards, and generally at comparatively low speeds. He quite agreed with Mr. Spiller that most of those accidents were due to the leading outer wheel mounting the rail, which was generally due either to unequal loading or to unequal action in the springs. A six-wheeled tender with equally spaced wheels was a troublesome vehicle; the slightest derangement of the track would incite it to go off. One speaker had quoted the case of a wagon with a girder placed diagonally across it. Clearly in that case the wagon carried all the weight on two diagonally opposite wheels; the other two wheels, having very little load on them, were free to mount the rails. That condition was present more or less whenever the loads on the wheels were unequal. The trouble might arise also from springs,

from unequal distribution of the load in the wagon, or from an Mr. Robertson. unequally distributed load arising from springs of unequal strength or unequal initial camber. A load of 3 tons placed at one side of an empty wagon would take all the weight off the opposite wheel, and when, as was the case in India, the load carried by the wagons was 24 tons that was not a very large proportion of it. When a mysterious derailment took place, he believed that if the vehicles were weighed with an Erhardt weighing-machine it would generally be found that there had been unequal weights upon the wheels. Just lately an Indian friend, thinking the wagons which were being sent to him were not good enough, set to work to design a wagon for himself. He increased the loads and strengthened the springs, leaving them the same length. When he put those wagons on the road they hopped gaily off, so that he had to withdraw them from service and alter the springs. If the deflection of a spring under its full load was assumed to be 1 inch, and one of the wheels passed over a bad joint in the track that was lowered  $\frac{1}{2}$  inch, it was clear that half the weight was removed off that wheel; and not only so, but the weight removed went to the other side of the wagon, to help to lift the already lightly loaded wheel. If the spring were more flexible and the deflection were 2 inches, the disturbance of  $\frac{1}{2}$  inch took off only one-quarter of the load. He believed that one speaker had objected to the notion of the flange climbing the rail, but it seemed to him there could not be any doubt about that action. He remembered a temporary track on a sandbank leading down to a wagon-ferry, with a very sharp curve, where wagons were always getting off the road. It was impossible to keep them on the track until a coolie was stationed at the curve with a swab to grease the inside of the rail. On the Continent it was not an uncommon practice on lines where there were very sharp curves to adopt means for lubricating the engine-flanges. Apropos of Mr. Spiller's remarks with regard to derailments at crossings, in India diamonds as flat as 1 in 10 had always been used, and he was not aware that any inconvenience had ever been experienced. Continental engineers also adopted at elbow crossings the practice of raising the guard-rail about 3 inches above the other rails, sometimes using a channel-bar for the purpose. It seemed rather a good device. He hoped the gentlemen who had furnished the diagrams exhibited on the wall would not suppose that he had treated their exercises too despitely: he merely wanted to suggest a general view of the question. He thought that the philosophy of maintaining railway roads could be put into very little. It did not matter

Mr. Robertson. much what formulas were used for easement-curves so long as an easement was put in, or for cant so long as it was not overdone and was run easily right through. Further, rolling-stock men should see that their springs were of equal camber and equal strength when they were put in; and the traffic men should see that they did not pile up heavy girders diagonally across wagons. If all those points were attended to, he thought matters would go on very well so far as easy riding and freedom from derailments were concerned.

Mr. Glover. Mr. J. GLOVER agreed with the last speaker that the subjects of the Papers scarcely lent themselves to a public discussion. They were more easily dealt with in the study, where the Papers could be carefully worked through page by page; and in the case of Mr. Shortt's Paper the results might then be transferred to the field. He presumed that track-engineers would be more interested in Mr. Shortt's Paper than in Mr. Spiller's, because it did not present those divergences between theory and practice which appeared in Mr. Spiller's excellent Paper. As the Author of, he believed, the first communication printed by The Institution on transition-curves, Mr. Glover was naturally more interested in Mr. Shortt's Paper, and he wished to express the pleasure that the reading of the Paper had given him. He considered that Mr. Shortt's method of fixing the best length of a transition-curve by the consideration of personal sensitiveness to the increase of rate of acceleration; his criterion of the efficiency of the curve; his investigation of the limits of radial steps where uniform curves could not be obtained; and last, and most important of all, his practical method of studying the actual state of affairs by means of distorted scales, were excellent. To adopt transition-curves on new railways, though necessary and praiseworthy, was a simple matter. The difficulty arose when existing conditions, and the limitations due to existing structures, had to be considered. To adapt transition-curves to that state of affairs was a difficult matter, with which Mr. Shortt's Paper clearly dealt, and to that extent it justified its title. Railway-engineers could now have no excuse for neglecting to improve their curves on fast-running lines and bring them up to the highest standard of perfection—a standard which was effectively pointed out by theory, and was also easily and cheaply attainable in practice. He could not agree with Mr. Robertson that any curve was good enough for a transition-curve. Where an engineer was not limited, one curve must be better than others, and it was surely the duty of an engineer to adopt the best.

Mr. R. ELLIOTT-COOPER would not attempt to enter into any discussion of the formulas which formed practically the whole of Mr. Spiller's Paper, because he did not think that such a discussion, even if he could undertake it, would lead to any practical result. In order that a formula might be of any service at all, it must be prepared upon known and definite lines. It was useless to discuss a formula based on conditions which might be totally different from the conditions under which the formula had to be used. In considering the question of safe speeds on railway-curves, it was essential first of all to consider the nature of the rolling stock. The Author stated that he adopted ideal conditions, but Mr. Elliott-Cooper was at a loss to imagine what he meant by that expression in ordinary railway practice, with the different kinds of locomotives, and trains made up of various vehicles. The only set of ideal conditions of which he was aware—and with which he had had something to do in a discussion before Parliament—was that presented by a single rail and a single wheel. Under those circumstances, ideal conditions were obtainable so far as a weight travelling round a curve was concerned, because the greatest of all the difficulties, namely, a fixed wheel-base, had not to be contended with. It was evident that when a locomotive had to travel round a curve of, say, 30 chains radius at a speed of 50 miles per hour, a great deal depended on the flexibility of the engine—its capacity to “turn round the corner,” if he might put it so. The locomotive which accomplished that under the best conditions in ordinary practice nowadays was of the type having a four-wheeled bogie in front, which gave as much play as in any locomotive at present in use. With regard to vehicles, there was no question that of late years the old-fashioned six-wheeled rolling stock with a fixed wheel-base had become practically obsolete. Modern passenger rolling stock was composed of bogie-coaches having a four- or six-wheeled bogie at each end, and the ease with which a vehicle of that kind would go round exceedingly sharp curves was well known. Such vehicles went round 5-chain curves in South Africa without the slightest difficulty; and it appeared therefore rather curious to him that a diagram should be put on the wall giving the safe speeds at which trains could go round curves of 10 or 15 chains radius, when every day they were going round considerably sharper curves at those speeds. There were, broadly speaking, only two forces which had to be taken into consideration in a vehicle travelling at any high speed round a curve, namely, the weight of the vehicle acting in a vertical direction, and the centrifugal force acting in a horizontal direction; and the pressure between the rail and the flange of the

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outer wheel was the resultant of those two forces. Taking the case of two wheels of a locomotive, with a weight of 20 tons on the axle, going round a 30-chain curve at a speed of 50 miles per hour, a centrifugal force was produced equal to 8·7 per cent. of 20 tons, or 1·7 ton, as a horizontal pressure. He therefore thought the Author was wrong when he said that, in the case of a vehicle going round such a curve at that speed, the lateral pressure was very great: in relation to the vertical pressure it was comparatively small. It was due to this fact that there were very few cases—he doubted if there were any case—where it could be shown that derailment had taken place solely on account of the centrifugal force causing the outer wheel to mount the rail: at any rate, Colonel Yorke had already said that, in the long range of his experience, he had never known a case where it could be shown that derailment had been due solely to the speed of a vehicle going round a curve. Of course derailments did occur on curves when trains were travelling at high speeds; but, as Mr. Robertson had mentioned, there were usually other contributing circumstances which had nothing to do with the theoretical question that was supposed to be dealt with in the Paper. With one or two points in Mr. Spiller's Paper he could not agree. One was the statement that the effect of super-elevation of the outer rail was to throw such a weight upon the inner rail that a tendency was caused for the train to mount the curve on the outer rail. He could not understand that. The cant was the amount of elevation which was supposed to put the resultant force at right angles with the table of the rail, and if the speed at which the train was travelling was such that the vehicle was bearing on the lower rail, there could not be any tendency to mount the outer rail because it would not be touched. Whether he had misunderstood Mr. Spiller's meaning or not, one thing was quite certain in his opinion, namely, that the cant, if it could be used in connection with a uniform speed and carried through in a uniform manner, practically enabled a vehicle, provided it was sufficiently flexible, to go round at almost any speed. The only tendency of a train to leave a curved track was its natural tendency to run off at a tangent. Therefore, if the cant was increased to the amount which would bring the pressure vertical to the rail, the effect of the centrifugal force was practically done away with, because the centre of gravity was so brought inwards that the resultant of the two forces came on the rail. He said "on the rail" on the supposition that it was a single rail. The effect of cant in the case of a surface was to be seen, for instance, at the Brooklands motor-track. At that place cars



could be seen running round the track at nearly 120 miles per hour. It really did not differ from a train running round a rail-track except that the steering was done by hand on the steering-gear whereas the direction of the locomotive was effected by means of the flange on the wheel. The track was so arranged that it was possible to run at any speed without any tendency to skid by reason of the centrifugal force. Cars were to be seen there running at very high speeds at an angle of about  $30^{\circ}$ . It was quite evident that, if it were not for the transverse slope, a car could not be kept on the track where it was curved when running at such high speeds. The object of the superelevation of the track on the outer side was merely to bring the resultant force within the base of the vehicle. He had referred at the beginning of his remarks to the Parliamentary inquiry—the only inquiry that he had ever known upon a detailed matter of that kind—which was held when the Bill for the Manchester and Liverpool Express Railway was before Parliament. In that case very elaborate diagrams were placed before the Committee, which at a later period had to be submitted to the Board of Trade (who afterwards approved of the drawings). These showed that it was possible to run round curves of 30 chains radius at 110 miles per hour. During the day he had looked up a diagram, prepared for the Board of Trade, in which information was given as to what the centrifugal force was, in tons, due to a weight of 40 tons travelling at 110 miles per hour on curves of various radii. The figures were the following:—

Radius of Curve.	Centrifugal Force.	Radius of Curve.	Centrifugal Force.
Chains.	Tons.	Chains.	Tons.
30	16·3	50	9·3
35	14·0	53	9·2
38	12·9	55	8·9
40	12·2	60	8·2
44	11·1	70	7·0
		80	6·1

The radii of the curves were given in that irregular manner because they were the actual curves on the projected railway. Those centrifugal forces had to be dealt with by so canting the permanent way that an excessive lateral strain was not created. He remembered that, in the course of his cross-examination before the Parliamentary Committee, counsel asked him what the effect would be upon a passenger when he was going round one of the sharp curves at a speed of 110 miles per hour, the suggestion being made that he would be flung out of the window. His reply was that the only

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effect upon the passenger would be that if he were a 12-stone man he would become a 13-stone man, that was, the weight of the man in conjunction with the centrifugal force gave a resultant force of 13 stones. In other words, an extra stone was put on the seat. He had perhaps gone a little away from the subject of the Paper, but he had mentioned these points in illustration of what had to be faced in practice in connection with the question of a vehicle running round a sharp curve. He wished, in conclusion, to refer to one or two passages in the Paper to which he took some exception. The first was the statement "Cant will reduce the vertical loads on the outer wheels without diminishing the lateral forces to the same extent." The amount by which it would decrease the lateral force was, of course, a simple matter of the resolution of forces. The locomotive in *Fig. 4* (p. 84) looked as if it was just about to overturn, the diagram showing a cant of a little more than 1 foot; whereas the cant that would be required for the purpose which it was intended to illustrate would be about 4 inches. If the locomotive were canted to the angle shown, it would probably indicate a speed more like 80 miles per hour than 50 miles. One point mentioned in the Paper was the difficulty of dealing with superelevation on rails passing through a station. That was insuperable. It was impossible to continue the superelevation of the outer rail on a curve passing through a station where sidings went off at different angles; and this, he believed, was generally the cause of the uncomfortable jar which occurred on arriving at a station, and not the effect, as was so often supposed, of running through points and crossings. For instance, when a train which had been going round a curve with superelevation and therefore with very little uncomfortable effect upon the passengers, suddenly arrived at a station where the outer rail was dropped to the level of the inner rail, the result naturally was that the flange of the wheel struck against the outer rail. He thought that was one of the reasons why the very unpleasant feeling was experienced of a series of small side jars in passing through a station after having run on a curve with comparative smoothness. With reference to Mr. Shortt's Paper, he was sure all agreed that transition-curves were a great improvement on the older plan of running the curve direct out of the straight; but it was difficult, particularly in connection with stations, to arrive at a mathematical curve that would meet all cases.

Prof. Smith. Professor R. H. SMITH remarked that apparently there still lingered on this side of the Atlantic a superstition that the centrifugal force of trains running round curves was wholly or mainly balanced by the component of gravity due to cant. The two Papers

under discussion gave no countenance to that idea, and the only Prof. Smith. fault he found with them was that they did not give numerical illustrations of the impossibility of that physical effect being attained. Mr. Robertson had mentioned a case in which 15 or 16 inches of cant would be required, and he would give two further numerical illustrations. For a speed of 68 miles per hour, with a radius of 2,000 feet,  $8\frac{3}{4}$  inches of cant would be required to balance the centrifugal force; and at so low a speed as  $54\frac{1}{2}$  miles per hour, with a 1,000-foot radius, no less than 11.2 inches of cant would be needed. Such illustrations showed that it was quite impossible to get the effect that was apparently so much sought after. Mr. Spiller showed that the speeds calculated from the overturning moment were of much greater importance than those calculated from cant. Again on p. 84 he put down the maximum practical cant as 4 inches. That, on a  $59\frac{1}{4}$ -inch gauge, which he used over the outside of the rails, was a cross gradient of 0.072, which was much less than the coefficient of friction between the rail and the wheel. So that with the maximum cant which Mr. Spiller thought was practicable, the component of gravity obtained was not nearly sufficient to overcome the friction of resistance to side slipping. A slight verbal error occurred on p. 85. Near the foot of the page Mr. Spiller spoke of one-third of unity, when it ought to be one-third of a foot. He agreed with the Authors that cant was of very little use, and that nearly the whole of the really useful guidance obtained was by rail-pressure. He thought that had been fairly well substantiated by the prolonged American researches upon the subject, which showed that the sinuous running of trains between rails was really the dangerous element that had to be provided against. He did not speak with the authority of special railway experience, although he had taken a great interest in the accidents that had occurred, and the brake-actions that had been employed; but in his opinion, safety at high speeds depended mainly upon three things: first, on rigidity and even laying of the track; secondly, on good balancing of the locomotive, and especially the avoidance of sinuous motion and climbing the side of the rail; thirdly, on rapid and efficient action of the brakes, in which was to be included the human element, which was excessively important in the manipulation of the brakes. With regard to Mr. Spiller's calculation of overturning moments, and the maximum safe speeds calculated in that way, he thought a proper allowance was not made for one thing. The Author calculated his most dangerous overturning speed with a high outward wind across the curve. Professor Smith did not think it was

Prof. Smith the habit of any good locomotive-driver to run round sharp curves at the highest speeds when there was a dangerous wind outwards. He thought reliance could be placed upon the common sense of drivers to vary the speed at which they ran round sharp curves in a very dangerous wind, according to whether the wind blew outwards or inwards upon the curve. He believed that such a difference was always made. In discussing the tendency to mount the outer rail he thought one thing had been overlooked in the calculation of the frictional resistance and the grinding between the flange and the rail, namely, that it was the common practice to increase the gauge slightly on curves; and that must make a difference, he thought, in the frictional resistance which came into the problem, looked upon with regard to the grinding between the wheel and the flange. Mr. Glover's Paper of 1899 was an extremely pretty and perfect piece of mathematics. He agreed with Mr. Robertson that, so long as small angles of deviation were being dealt with, it did not matter much what transition-curve was adopted. It was a mathematical fact that all the forms of transition-curve that had been proposed resolved themselves practically into a cubic parabola, so long as the curve ran through only a small angle of deviation. It might be noticed in the Papers, and in Mr. Glover's Paper too, that in the practical application of the equations given the Authors were forced to make approximations which really reduced the mathematics to the mathematics of a cubic parabola. He wished to point out, however, certain restrictions that were imposed by the use of the cubic parabola. In improving curves, if not in laying out new lines, it was desired to start a transition-curve at a specified point of the main curve. When that was done, two things were specified: the distance from the main tangent, and the angle made with that main tangent at the point of junction between the main curve and the transition-curve. Those two elements determined the whole of the transition-curve, and from them alone the length of the curve could be calculated, as also the radius of the transition-curve at the junction between it and the main curve, and the slip between the main curve and the main tangent. All those things followed immediately from a mathematical calculation based upon the two given elements alone. Therefore the curve did not leave the desired amount of freedom of choice. It was possible to fit a cubic parabola on to any given main circular curve, but it was not possible to do this at a given point of that main curve. Again, another restriction or disadvantage imposed by the use of the cubic parabola was that the minimum curvature in a cubic parabola occurred at an angle of deviation of 24 degrees

6 minutes. Beyond that the curvature began to decrease again.\*Prof. Smith. Most transition-curves did not go through so large an angle as that, but for his own part he did not see why railway-curves should not be transition-curves right through. Each half of the whole curve would be better made a transition-curve. The third objection was that the rate of change of curvature decreased from the beginning of the transition-curve proceeding along the curve. Mr. Shortt based the whole of his calculations of the most desirable transition-curve to be used upon the rate of change of curvature along the curve, and it was certainly undesirable that this should decrease instead of increase gradually from the starting of the curve. He wished to show to the members a curve that obviated these three disadvantages. The curve was constructed on the principle that the radius of curvature increased uniformly along the length of the curve, per chain, or per 1,000 feet. The radius of curvature increased uniformly instead of the curvature itself increasing uniformly. This curve, like all other curves used on railways, when used through small angles of deviation, resolved itself into a cubic parabola. Its one disadvantage was, that in using it the radius of curvature at the beginning of the transition-curve could not be made infinitely long; it could only be made as long as was desired: in other words, the curve would not satisfy the theoretical mathematician, but would satisfy the engineer. In these curves the curvature increased from the start of the transition-curve all the way through, and it never reached any minimum. The peculiarity of the curves was that they had the same shape throughout: one part of the curve was precisely the same as another, only to a different scale. It seemed to him that if a good shape was obtained for one part of a transition-curve, one could not do better than adhere to it throughout the rest of the length of the curve. The curves he showed had a special facility in this respect. They were scaled and marked with the radii of curvature all along the scale. The scale was put upon both sides so that the curve could be reversed, and two exactly symmetrical halves could be obtained with extreme ease. They were called homogeneous curves because of the sameness of shape throughout the whole length; they were also called reversible curves because of their facility for reversing; they were also called scaled curves because they were scaled; and for all three reasons they were called "R.H.S." curves.

Mr. W. B. WORTHINGTON thought that the main interest of the Mr. Worthington.  
Papers and the discussion lay in bringing out Mr. Shortt's extremely simple and effective method of applying transition-curves to existing railways. Of course an engineer would not think of making a new

Mr. Worthington. railway without them, but it was quite a different matter to get them in on the awkward corners of existing railways. He confessed that he had seen no method of carrying this out more easily and simply than by the one explained in Mr. Shortt's Paper, and for that reason he welcomed it cordially. The Papers also dealt with the related question of maximum speeds on curves. He was not prepared to fix the maximum speeds on curves everywhere by a mathematical formula, because there were so many other grounds than the mathematical formula based on theoretical considerations on which the speeds had to be fixed. It appeared to him that three things regulated the speed on any particular length of railway: first, safety; secondly, the comfort of passengers; and thirdly, economy in relation to the permanent way and rolling stock. It was a question whether railway-dividends would not be higher to-day if the managers of railways had given more attention to the third, for he was not at all sure that the speeds at which trains were run conduced to dividends. He also thought that the comfort of the passengers and the economy of working would regulate the speed before the point was reached at which it was governed by considerations of safety; in other words, proper provision for the comfort of passengers and for economy of working left a margin in respect of safety.

Mr. Thorpe. Mr. W. H. THORPE considered that the conclusion drawn by Mr. Spiller from his investigation of the maximum cant permissible, namely, that it would seem advisable to limit the cant to a maximum of 4 inches, was rather a lame one. If the vehicle was just on the point of overturning when the cant was  $4\frac{1}{2}$  inches with a wind-pressure of 20 lbs. per square foot, the reasonable conclusion to draw was that light vehicles should not run in such a wind. To knock off the odd  $\frac{1}{2}$  inch and say that the limit of superelevation should be 4 inches was not quite the result to deduce from the figures. In commenting upon formula (27), on p. 90, Mr. Jacomb-Hood, if Mr. Thorpe had understood him aright, had said that the results arrived at by the application of that formula were quite at variance with common practice, and that the constant (1.743) used to find the critical speed at which an engine would just mount the rail should be rather ten times as great. Mr. Jacomb-Hood's diagram on the wall (*Fig. 4*, p. 125) showed a number of results between two lines which had been plotted to express, respectively, fourteen times and seven times the square root of the radius; and Mr. Jacomb-Hood had concluded that, as the diagram appeared to be quite at variance with the formula which Mr. Spiller had given, that gentleman's expression was entirely wrong. Mr. Thorpe thought

there was a simple explanation of the apparent divergence in the Mr. Thorpe. conclusions. Mr. Spiller had, he thought, taken the radius of the curve in feet; Mr. Jacomb-Hood had taken the radius of his curve in chains. Applying the necessary correction, it would then be found that the top line was in strict agreement with Mr. Spiller's figure, so that there appeared to be no divergence between the two results. With regard to the use of distorted diagrams, referred to in the Appendix to Mr. Shortt's Paper, although probably the use of distorted scales was quite original as applied to the plotting of railway-curves, the principle of the method was stated many years ago by Professor Fleeming Jenkin in his article upon Bridges in the ninth edition of the *Encyclopedia Britannica*. It was used there for the plotting of deflection-curves, and Mr. Thorpe had himself used it for that purpose. His experience of the use of this method in plotting such curves justified him in saying that, as a matter of draughtsmanship, great accuracy could be obtained; so that he could readily understand that Mr. Shortt got precise and satisfactory results by adopting it.

Mr. SPILLER, in reply, remarked that the formula giving the Mr. Spiller. critical speed had given rise to more discussion than any other portion of his Paper. That formula depended largely on the ratio of  $w$  to  $W$ ; and the value which he had given,  $\frac{1}{1.5}$ , had been chosen after the actual ratio had been calculated for several classes of engines of the 4-4-2 and 4-6-0 type. Mr. Jacomb-Hood, in the course of some very closely-reasoned remarks, had suggested that the ratio should be  $\frac{1}{3}$  instead of  $\frac{1}{1.5}$ . In engines of the 4-4-0 class the ratio  $\frac{1}{3}$  was perhaps correct; but for engines of the 4-6-0 class, similar to the one illustrated in the Paper, the value  $\frac{1}{3}$  would give a result much lower than was actually the case. The ratio he gave,  $\frac{1}{1.5}$ , was the lowest value he had found for several classes of locomotives, and the lowest value had been chosen because it was thought that, in fixing speed-restrictions on a long length of line, it would be necessary to be guided by that class of locomotive which seemed to give the lowest factor of safety on curves. Perhaps the average value for that ratio would be  $\frac{1}{1.2}$ : if this was assumed and also (as would be the case on flat curves) that intermediate drivers were in flange contact and thus took up the lateral pressure due to the axle weights, the critical speed then worked out to  $19\sqrt{R}$ , where  $R$  was the radius in chains, giving  $9.5\sqrt{R}$  as the maximum safe speed. This gave higher speed restrictions on curves than were in force on the German State Railways. Mr. Jacomb-Hood had suggested that the ratio of  $w$  to  $W$  would vary greatly owing to the effect of centrifugal force. Some additional

Mr. Spiller. load would undoubtedly be thrown on the outer wheels, but to nothing like the extent imagined by Mr. Jacomb-Hood; while cant would have the effect of reducing this additional load. Mr. Jacomb-Hood referred to two contributory factors to unsteady running not mentioned in the Paper, namely, imperfect balancing and low rail-joints, but neither materially affected the conclusions reached. Imperfect balancing would not affect the load on the bogie-wheels to a great extent, while plunging due to low rail-joints should not be excessive on well-maintained railways. Mr. Carus-Wilson had criticized the value given to the force  $F_2$ , and stated it should only be 1 per cent. of  $W$  instead of 4 per cent. He had not explained how he arrived at this percentage. If a truck when entering a curve faced north and south, and if the central angle of the curve was  $90^\circ$ , the truck would face east and west when it left the curve, and since cylindrical wheels of equal diameter rigidly fixed to parallel axles rolled in straight lines it followed that slipping of the wheel-treads across the rail must take place in order that the truck could change from its north-south position to its east-west position, and the frictional resistance overcome could not be less than the product of the weight and the coefficient of friction. Both Messrs. Carus-Wilson and Elliott-Cooper had implied the possibility of canting sufficiently to prevent flange-contact between the outer leading wheel and the rail, but unless the plane of the rails was tilted to an angle greater than the angle of repose of wheel on rail there would always be flange contact between the leading outer wheel and rail whatever the speed and whatever the radius. Mr. Elliott-Cooper could prove this at Brooklands, if he wished, by attempting to negotiate the curves by aid of the banking alone and without any assistance from the steering-wheel of the car. Mr. Elliott-Cooper had taken exception to the statement, "Cant will reduce the vertical loads on the outer wheels without diminishing the lateral forces to the same extent," and Mr. Carus-Wilson, in giving some calculations which he had made, had allowed quite a large percentage of decrease due to superelevation. But this, the Author contended, was taking exception to the laws of motion, for although it was possible by canting the track to bring the resultant of the weight and the centrifugal force normal to the plane of the rails, this resultant still had a component acting at right-angles to the plane of contact of wheel-flange and rail, causing pressure between these two surfaces, and in addition to this no amount of cant within the limits of practice would materially reduce the lateral thrust of the leading wheel flange due to the force required to produce the horizontal movement of rotation. Mr. Elliott-Cooper had said



that, broadly speaking, there were only two forces which had to be taken into consideration in a vehicle travelling at any speed round a curve, namely, gravity and centrifugal force; but for all speeds except those approaching the critical speed, centrifugal force was less than the force producing the movement of rotation, and it was this force which was often responsible for those "petty derailments generally at comparatively low speeds" which Mr. Robertson had referred to. The lateral pressure, which Mr. Elliott-Cooper suggested was comparatively small, was sufficient to shear the spikes holding down the outer rail of the curve in the case of the Woodlawn accident in America; and actual tests<sup>1</sup> made on the West Jersey and Sea Shore Railroad by the Pennsylvania Company in 1907 proved that the lateral pressures of the flanges against the outer rail of a 1° curve, canted for a speed of 70 miles per hour, were from 4·81 to 7·36 per cent. of the weight of the locomotive (Atlantic type 4-4-2) when travelling at speeds varying from 75·5 to 92·3 miles per hour. With regard to the factor of safety referred to by Mr. Carus-Wilson, it was simply stated in the Paper that if speeds up to half the critical speed were permitted, probably there would be a sufficient margin of safety, and that seemed the simplest way of dealing with the matter. If the engine-driver knew that his engine would not derail until he reached a speed equal to twice the permitted speed, he did not care much what his mechanical factor of safety happened to be. As for it being 6·4, as suggested by Mr. Carus-Wilson, the Woodlawn accident and the tests on the West Jersey and Sea Shore Railroad already referred to, proved that it must be very much less than this. It would, of course, simplify matters if it could be assumed, as Mr. Elliott-Cooper seemed to think, that each axle took up its share of the centrifugal force, but as long as locomotives had rigid frames such an assumption could not be made. Several speakers had stated that formula No. 27 (p. 90) was of little value because it did not apply to all classes of rolling stock. It was, of course, only applicable to the locomotive whose wheel-base was illustrated diagrammatically in *Fig. 3*, and a careful perusal of the Paper made this clear. In no part of the Paper was it suggested, nor did the Author wish to imply, that locomotives did not exist with a higher critical speed of derailment than was given by formula No. 27. Every type of locomotive required a different constant in the equation, for it was evident that the lateral forces would not be the same in a bogie with a fixed centre-pin as in a bogie possessing lateral play sufficient to allow the leading

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<sup>1</sup> *Engineering Review*, vol. xx (1909), p. 48.

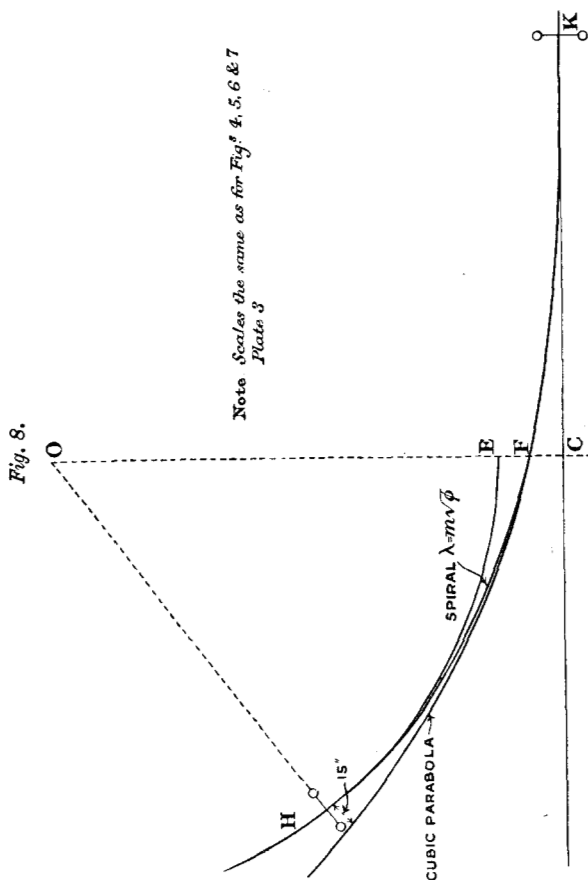
Mr. Spiller, driver to become the guiding wheel. In the latter case the vertical load on the driving-wheel would be perhaps double that on the leading bogie-wheel, but the lateral pressure would also be increased: first, because the guiding force being applied nearer the axis of rotation the lever arm was shortened and the force increased; secondly, the guiding force was so near the centre of gravity of the locomotive that practically the whole of the lateral pressure due to centrifugal force was concentrated at the leading driver. In one particular class of engine it was found the ratio of lateral pressure to vertical load was the same whether the bogie or the leading driver steered the locomotive. This was perhaps an exceptional case, where the leading driver was lightly loaded; generally, bogies with lateral play would give higher speed-limits than the fixed centre-pin type. It had certainly not been the Author's intention, as Colonel Yorke seemed to suppose, to draw any conclusions from the accidents referred to at the commencement of the Paper. Mention was made of them merely to emphasize the importance of the subject, and not as proof of the conclusions subsequently reached. The justification for the statement that many more accidents had occurred through derailment than through vehicles overturning under the action of centrifugal force was contained in Major Pringle's report to the Board of Trade on the Salisbury accident, wherein he said, "there is no other known case where overturning has been caused by high speed." Colonel Yorke's reference to Mr. Wellington appeared to be somewhat beside the point, for the increase of curve-resistance by 58 per cent., when the wheel-base of a car was increased from one gauge to two gauges, did not involve any alteration of the lateral forces referred to in the Paper. It was shown by Mr. Wellington that this increase of 58 per cent. was due to the fact that the actual distance slid by the wheels in the second case was 58 per cent. more than in the first, when the cars travelled over equal lengths of the same curve. It was certainly difficult to follow Mr. Shortt's contention that the axis of rotation must always lie on a radius parallel to the axle. Probably, as stated in the Paper, its position was constantly changing, since there would be in practice continual variation in the wheel-loads and in the coefficient of friction between wheel and rail. There was a well-known theorem in mechanics which stated that "any system of forces acting on a rigid body in one plane can be reduced to a single force acting at an arbitrary point and a couple." Whatever difference of opinion existed as to the amount and direction of the lateral forces, in view of the above law unanimity must prevail that their result was exactly the same as that of a single force, applied at

the centre of gravity of the vehicle, causing the movement of Mr. Spiller. translation about the centre of the curve, and a couple producing the movement of rotation. The axis suggested by Mr. Shortt necessitated the sliding of an additional wheel across the rail, but it was quite justifiable to assume that the truck would remain in a position entailing a less resistance to motion. Mr. Spiller quite concurred in the opinion expressed by Mr. Worthington that a speed of discomfort was reached long before the critical speed. It was of course impossible, as Mr. Shelford had pointed out, to cant one of the roads through the junctions illustrated in Figs. 10 and 11, Plate 2, except at the expense of the other; the intention was that all roads should be laid level throughout the length covered by the crossing timbers. Mr. Price-Williams had misunderstood Mr. Spiller's intentions in regard to the 30-foot switches. Straight tongues were suggested with the curves tangential to them at the heel, or  $4\frac{1}{2}$  inches opening; it would not be necessary to extend the planing more than about 18 feet from the point. Mr. Price-Williams appeared to think there were grave practical objections to the use of switches of such length, but Mr. Spiller had supervised the laying of 30-foot switches 7 or 8 years ago, and no difficulties had since arisen.

Mr. SHORTT, in reply, thanked the members for their reception of Mr. Shortt. his Paper, and remarked that, before dealing with the various points raised, he wished to clear up Mr. Thorpe's reference to Mr. Jacomb-Hood's diagram (*Fig. 4*, p. 125). The critical speed given by Mr. Spiller as  $1.743 \sqrt{r}$  was intended to be represented by the highest line on the diagram marked  $14\sqrt{R}$  ( $R$  being the radius in chains), and the recommended safe speed by the lower line  $7\sqrt{R}$ . The reason that Mr. Spiller's figures had been converted was that it was much easier to realize what the speeds actually were when the radius was expressed in chains. He thought Mr. Thorpe was therefore under a misapprehension when he attributed an error to Mr. Jacomb-Hood. The first point with which he would deal was the length of the transition-curve. Both Mr. Burge and Mr. Shelford had adopted a constant length for the curve, but if the centrifugal force was always to be gained at the same rate, the length of transition must be proportional to  $V^3/R$ , so that only under very exceptional circumstances could a constant length be suitable. There was little doubt that a transition-curve 132 feet in length was far too short to be adopted as a standard: for instance, the shift required when the radius of the main curve was 40 chains was so small that it seemed hardly worth while putting in a transition-curve. If 45 miles per hour was the maximum speed attained on railways in Australia, then perhaps Mr. Burge was right in regard to not using

Mr. Shortt. transitions longer than 4 chains, but it was difficult to see why, if a 20-chain curve required a 4-chain transition, a 21-chain curve required none at all. When laying out new lines it seemed a mistake to use shorter transitions than those given by the formula  $L = \sqrt{R}$ , at any rate for curves less than 50 chains in radius. With regard to the form of the transition-curve, Mr. Burge had spoken of "the cubic parabola as recommended by Mr. Shortt," and Professor Smith had stated "that in the practical application of the equations given the Authors were forced to make approximations which really reduced the mathematics to the mathematics of the cubic parabola." This was not the case with the spiral  $\lambda = m\sqrt{\phi}$  used by Mr. Shortt, and there was no mention of the cubic parabola in his Paper. It was certainly true that in the practical application of the spiral  $\lambda = m\sqrt{\phi}$  the offsets from the tangent to one-half of the transition and from the main curve to the other half, as shown in Fig. 5, Plate 3, had been assumed to be directly proportional to the cube of the distances of those offsets from the respective tangent-points, but that was very far from reducing the whole curve to a cubic parabola. The radius of the transition shown in Fig. 5 decreased all the way round, while the deviation was  $48^\circ$ , or practically twice that permissible with the cubic parabola; in fact, the method of plotting described in the Paper gave a very close approximation to the spiral  $\lambda = m\sqrt{\phi}$ . Fig. 8 showed what would have been the result if the transition curve of Fig. 5 had been plotted as a cubic parabola; it would be seen that it missed being tangential with the main curve by about 15 inches. With reference to Professor Smith's contention that the rate of change of curvature should decrease instead of increase gradually from the start of the transition-curve, as trains might travel in either direction over the curve, surely the one was as bad as the other. The proper method was to make the rate of change of curvature uniform, as was the case with the spiral  $\lambda = m\sqrt{\phi}$ , which was undoubtedly the simplest form of transition-curve. The curve described by Professor Smith was an ordinary logarithmic spiral of the form  $r = Ae^{b\theta}$ , and in Mr. Shortt's opinion was quite an unsuitable spiral for transition purposes. It certainly never approximated to the cubic parabola as suggested, and the rate of change of curvature decreased in an inverse ratio to the square of the radius instead of being constant, while the curve was of little use between a circular curve and a straight, or between reverse curves, owing to the radius only becoming infinite at infinity. It had been suggested that in the ideal alignment the curves should be made transition-curves throughout, there being no circular portions at all. Mr. Shortt could not agree with that, however, since nothing would

appear to be gained by using longer transitions than those given by the Mr. Shortt. formulas in his Paper, while unless sharper curves were introduced, the percentage of curved line would be increased. Mr. Shelford's observation that transition-curves could not be put in after the line was staked out without sharpening the curve somewhere was theoretically true—excepting the obvious possibility of sluing the



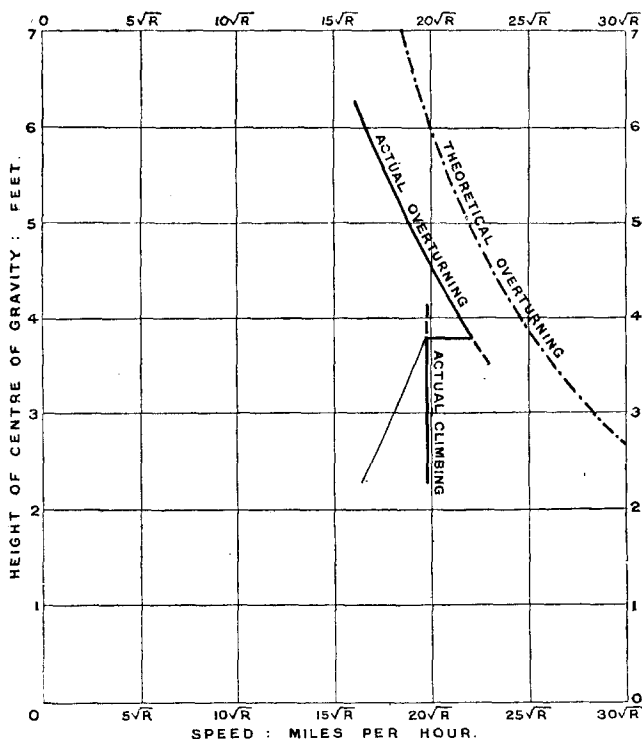
straight; but in practice it had been found, owing to the irregularities of curves which had been in use for many years, that a transition-curve could generally be inserted without sharpening the curve or moving the straight, as had been done in the case of the first improvement illustrated. Also, Professor Smith had remarked that in improving curves it was desired to start a transition-curve at a

Mr. Shortt. specified point of the main curve ; but in practice that was not the case ; what was generally wanted was to insert a transition-curve of given length, and the exact position of the tangent-points did not matter in the least as long as the curve was got in. Of course, with the logarithmic spiral it was necessary to start at the tangent with the main curve, because that was the only definite point on the curve. Mr. Benedict was certainly right in saying that the use of the theodolite was preferable to setting out a curve by offsets from a tangent which rapidly got off the formation, but Mr. Shortt did not set out his curves by offsets from such a tangent, but by offsets from the existing curve, and on the average those offsets were never more than 7 feet, including the addition of a constant length of 5 feet to get the pegs out of the way. A theodolite was an excellent thing on new works, but its efficient use to improve existing curves where the traffic was heavy was practically impossible. If he restricted himself to circular curves when setting out turnouts, Mr. Benedict's method was quite correct, although there was no reason why the whole length of the crossing rails should be kept straight ; but he had overlooked the fact that a transition-curve might be used with advantage instead of the circular curve. With modern long-rail switches, as used on the London and South-Western Railway, having no joint at the  $4\frac{1}{2}$ -inch point, the curve could be extended right down to the heel of the planing of the tongue. The length of the switch was not an independent quantity, for the curve of the turnout should always be tangential to the switch tongue at the heel of the planing, and also tangential to the stock-rail. The fact pointed out by Mr. Thorpe, that the relation between the distorted scales shown in Appendix I had been recognized and made use of in another connection by Professor Fleeming Jenkin, was quite unknown to the Author, and had he been aware that the truth of the relation had been recognized by so eminent a man, Appendix I would have been replaced by a reference. It was true, as Mr. Shelford had pointed out, that owing to cant the centre of gravity of a vehicle did not follow the centre-line of the track ; but, assuming the cant commenced at the entrance to the transition-curve, increased uniformly throughout, and reached its maximum at the commencement of the main curve, the alignment of the path actually followed by the centre of gravity was quite as good as that of the transition actually set out. If lateral force, and not centrifugal force, was meant by Professor Smith when he said that the Authors gave no countenance to the idea that it was wholly or mainly balanced by cant, then Mr. Shortt agreed : there was, however, no reason why the centrifugal com-

ponent of the lateral force should not be balanced by cant, as a Mr. Shortt. speed of  $11 \sqrt{R}$ , the maximum regularly attained in practice, only required a theoretical cant of  $7\frac{1}{4}$  inches. Professor Smith's practical illustrations were hardly examples of schedule running. While there were many points on which he thoroughly agreed with Mr. Robertson, he thought that gentleman's introductory remarks were a little contradictory, for having stated that the behaviour of trains on curves had been very efficiently discussed and investigated on the mathematical side, he went on to say that these investigations were practically fruitless because conditions which vitally affected the question had been omitted. Mr. Robertson's summary seemed to be that it was useless to try and treat the action of trains on curves scientifically, and that in order to maintain curves in an efficient condition it was simply necessary to instruct the plate-layer to ease the ends by eye and put in more or less cant according to the curvature and the speed of the traffic. With such views the Author must strongly disagree, as, were they generally accepted, progress could not be hoped for. The unsatisfactory results of leaving the alignment of curves to the eye of the plate-layer had so far been realized in the United Kingdom that the engineers of the principal railways were putting in permanent reference-marks alongside the curves. Colonel Yorke's advice that theory and practice should go hand in hand had been fully realized by the Author, and *Fig. 9* showed the results of some tests made during 1908 on the method of derailment and derailling speeds of a model four-wheeled bogie-truck with an adjustable centre of gravity. The model had properly-sprung wheels and was made to a scale of one-twentieth of full size, and the radius of the curve corresponded to 6 chains. It would be seen that as long as the equivalent height of the centre of gravity was more than 4 feet above rail-level, derailment always took place by overturning. When the centre of gravity was between 4 feet and 3 feet 6 inches above rail-level, sometimes the car overturned and sometimes it climbed; while with the centre of gravity below 3 feet 6 inches the car always climbed. The speeds were accurately and automatically determined by means of a chronographic recorder, and with each position of the centre of gravity tests were made at gradually increasing speeds until a derailment took place; this test was then repeated, and if derailment did not occur again the speed was increased, and so on, until finally a speed was attained at which derailment took place every time. The speeds at which the overturning derailments took place were perfectly definite, but in the case of climbing, accidental derailments occurred before the definite climbing speed was reached:

Mr. Shortt. the fine line on the left of the "actual climbing" line on the diagram showed when these accidental derailments commenced. Knowing the angle of the wheel-flanges ( $33^\circ$  with the vertical), and the climbing-speed, the total lateral thrust of the leading wheel-flange against the rail could be calculated, the portion due to the centrifugal force deducted, and the thrust defined by Mr. Spiller as  $F_2$  obtained. Working in this manner it had been proved that the experimental

Fig. 9.



car could not have been derailed by climbing unless the force  $F_2$  was equal to at least 10 per cent. of the weight of the truck when the centre of gravity was 2 feet 6 inches above rail-level, and about 15 per cent. when the centre of gravity was 3 feet 9 inches above rail-level. This result, which was rather a striking confirmation of Mr. Spiller's estimates of the force  $F_2$ , was contradictory to Mr. Carus-Wilson's views, and Mr. Shortt could not help doubting the suggestion, based on the Paris-Lyons-Mediterranean experiment,



that it was possible to balance the force  $F_2$  (flange-force Mr. Carus- Mr. Shortt. Wilson called it) by increasing the cant 2 per cent. With reference to Mr. Elliott-Cooper's suggestion that if the speed of a train were not as high as corresponded with the cant the flanges of the outer wheels would not touch the outer rail and therefore could not have any tendency to mount, had this been so it would have been practically impossible to obtain any derailments of the model car by climbing, at any rate unless the height of the centre of gravity was much less than corresponded to 2 feet 6 inches. A practical experiment with a four-wheeled truck with parallel axles, the flange on the outer leading wheel having been removed, would demonstrate beyond doubt that this flange played a most important part in guiding the car round a curve, as it would be found quite impossible to get it to go round any unchecked curve even if canted as much as 6 inches or 8 inches at any speed, high or low, without derailment. In view of *Fig. 9*, the non-derailment of the vehicles and trains mentioned by several speakers was explained, since in no case would the centre of gravity have been sufficiently low for derailment to take place by climbing, and the overturning speeds were never reached.

The PRESIDENT remarked that as he had had something to do with The President both old railways and new railways he would like to refer to one or two points. One of the most important results of paying attention to cant—which he did not treat so lightly as some of the speakers—was the comfort of the travelling public. It was much more pleasant to put the additional stone on the gentleman Mr. Elliott-Cooper had mentioned in that way than in the shape of a shear. As a matter of practical politics, the difference in the comfort of travelling if cant was properly attended to and continuously maintained, coupled with a transition from the cant on either straight line or another curve, was very considerable. His Company had had a good deal of experience in that connection, particularly in South Devon, where on one part of the line 2 miles in length there were eleven reversals of curvature. It could be easily imagined how much the proper treatment of such a piece of line meant to the comfort of the travelling public. With regard to the curves in *Figs. 9 and 10* of Mr. Shortt's Paper (*Plate 3*), in his opinion it was far better to put up with a curve of somewhat greater curvature, so that there was a piece of straight between the reverse curves, than it was to run two curves of less curvature into one another. He noticed that the magic of the transition-curve was supposed to get over that difficulty; but really a domestic straight line would do it better. It was far better to have a 30-chain instead of

The President. a 36-chain curve as far as the running was concerned. He would be glad if Mr. Shortt would put on the diagrams the actual cants which were used in the several curves illustrated. Another point which he could not help feeling was very important was the character of the engine, a point which had been touched on more or less. He remembered a type of engine formerly used on the Great Western Railway, with which derailments frequently occurred, mostly at low speeds. At one time they had a good many engines of a type in which the leading wheels were the drivers, four wheels coupled, with a trailing bogie. Those engines so distinguished themselves through derailments—sometimes when running at a considerable speed—that they had had to be taken off. He could not help feeling that that was due to a large extent to the diameter of the leading wheel, which seemed to him an important factor in the question whether a given engine would travel safely round a curve at a particular speed. The same engine as he had referred to, built with the bogie the other way and the weights adjusted, went round quite smoothly. It was well known that the bearing of the flange against the rail was much shorter with a small wheel than with a large wheel; and the result of a long grip of the flange was greater grinding-action and increased tendency to mount the rail. In another case, on a length of line full of curves from end to end, where the cant had been allowed to disappear, the wheels sheared on the outer rail to such an extent that the whole of the permanent way of the upper rail had to be taken out and relaid. That was done a good while ago and the cant replaced, with the result that the wear on the higher rails had been much diminished: so that proper adjustment of the cant had a certain influence on the wear of the rails. Mr. Worthington had made a hit at managers for running trains at too high speeds; but, like all such questions, this had two sides. If Mr. Worthington was thinking of running coal-wagons or goods-trains he might be right. Whenever high speed was attempted, every part of the machine must be mechanically fairly perfect. This was easy of attainment, and it was now being done. For instance, it was now usual practice for the journals of common coal-wagons to be put on an emery-wheel and finished almost mathematically correct at a cost of about 9d. per axle. When journals were treated in that way, he did not think there was any harm in running even a coal-wagon at high speeds. He had tried experiments at the high speed of 84 miles per hour running round 15-chain curves—keeping well out of the way while the experiment was being conducted! The experiments took

place on the Princetown railway, where the problem to be solved was, whether a truck blown away by the wind would go down an incline, mount another, and get over a summit. The result was that a 12-ton wagon, loaded with 10 tons of granite, moved beautifully across at 84 miles per hour and round a 15-chain curve. He thoroughly agreed with the remark one speaker had made about the important influence that sinuosity of motion had. He confessed that it was a matter to which sufficient attention had not been paid in the balancing of the machine. He had had many opportunities of watching trains running at high speed on straight and other lines, and had noticed that the swaying, and the period of the swaying, varied very much indeed. That was one of the things that he hoped would be gradually overcome, when running at high speed, by the use of four-cylinder engines; whether they should be compound or simple was another matter.

Mr. SHORTT stated that in the case of the curve entered from the straight and the reverse curve illustrated in his Paper the actual cant used was  $4\frac{1}{2}$  inches. As the improved alignment was, and should be always, settled entirely without reference to cant, except in so far as it was liable to cause fouling of the construction gauge, cant had not been shown on the diagrams.

## Correspondence.

Mr. ROLLO APPELYARD remarked that although the equations at which Mr. Spiller arrived were, in some respects, out of agreement with results observed in practice, his method of examining the general conditions that determined derailment at curves was helpful as a basis for further research as to the direction and magnitude of the complex forces concerned in these disasters. Details of serious derailments were recorded by the Board of Trade and by railway-companies, but before they could be rendered available for such generalizations as those at which Mr. Spiller sought to arrive, they required to be co-ordinated and brought into conformity with modern conditions as to speeds and roads. Until this was done there must be great difficulty in evaluating the constants and the variables of any equation that was to predetermine critical speeds. Nevertheless, Mr. Spiller had succeeded in indicating the nature of the problem, and he had given a fair approximation to one of its partial solutions. It might be useful to suggest how a further step

Mr. Rollo  
Appleyard.