The following concluding notes may be of help in tackling design problems.

Summits or Peaks.—The Ministry of Transport recommend a minimum distance of mutual visibility of 500 feet. This is a safe distance for a speed of 52 miles per hour. For higher speeds a greater distance is advisable.

Valleys.—As an average value for design, a maximum vertical acceleration of 1·0 foot per second per second may be adopted ; but for motorways and trunk and similar roads the maximum vertical acceleration should not exceed 0·50 foot per second per second. Only if exceptional difficulties in design arise should the value of 1·50 foot per second per second be adopted.

The gradients or grade changes should be worked out to the third place of decimals.

Slide rules can usually be used for calculating the value of Ax^2 and Bx, especially if the road is one with a tarmacadam surface, where errors of 0·01 foot and 0·02 foot are not considered serious.

The Paper is accompanied by three drawings, from which the Figures in the text have been prepared.

Discussion.

Mr. Leeming, in introducing his Paper, referred to the derivation of the equation for the length of the transition curve and said that perhaps he should have made it clearer that it had been assumed that there would be a relationship between the radius of the curve at any instant and the superelevation. That was essential to the assumptions made on the theory ; otherwise, of course, the relation for F_p would not hold.

He also wished to mention that this theory had been arrived at as the result of certain tests which Mr. G. T. Bennett, Assoc. M. Inst. C.E., the County Surveyor of Oxfordshire, had asked him to carry out on two curves on which it was felt that something was wrong. In one case, where traffic was observed to swing out to the right at the entrance to a sharpish circular curve, it was found that that was due to the fact that the superelevation had been carried on up to the transition ; that was, the full superelevation was reached at the point of transition of the circular arc itself, with the result that the sideways force due to cant uncompensated by curvature tended to cause drivers to swing to the right at the entrance to the curve. In the case of the other curve, his own first reaction on driving over it had been that something was wrong with the steering of his car ; but it appeared to be all right after he had passed off the curve, and finally he realized that the trouble was due to the fact that a right-hand curve of about 5½ miles radius had been given superelevation, so that the " hands-off " speed was 108 miles per hour, with the result that he was constantly steering to the left on a slight right-hand bend.

Mr. Price, in introducing his Paper, said that it started with the basic formula for the parabola, which was generally stated as $y=Ax^2+Bx+C$, where A, B, and C were constants. He believed that a relationship existed between those constants and the properties of the vertical curve, the chief of which were visibility and riding comfort. The former applied only to summits, whilst the latter applied to both summits and valleys and was taken in the Paper as the comfort in general in respect to smoothness in road riding qualities for the occupants of the car. The existence of those relations had been investigated by him, and his findings were given in the Paper.

Turning briefly to the historical aspect of vertical curve design, the earliest road improvements, as carried out in the first decade or so of the present century, embodied little or no attempt to incorporate the use of vertical curves to connect intersecting gradients, which consequently intersected at points giving rise to sharp changes in the vertical alignment of the road. Whilst the speed of vehicles remained low that did not give rise to any serious troubles, but when speeds began to increase the sharp changes reacted on vehicles with something like the effect of a spring-board, causing the vehicle to leave the road temporarily at particularly sharp intersections. Such conditions gave rise to excessive wear and damage to the vehicle itself, and also to the carriageway. The wear on the road surface due to this cause took the form of numerous pot-holes, undulations, and general disintegration of the surface, and one or more of those forms of wear was very easily discernible before changes of gradient. Constant repair at those points was very noticeable, and the use of the vertical curve at them thus became a necessity in the earliest stages of road design, both from the point of view of road comfort and from that of economics.

The earliest curves were circular arcs and the levels were often either scaled direct from the longitudinal section and set out with a level or set out on the site by eye. Very often they were entirely inadequate to meet the demands of road traffic and gave poor visibility and poor running qualities, besides causing much wear of the road surface, thus necessitating constant repair. For some time those rough and ready methods had given place to more exact methods of calculating the levels and setting them out, whilst bolder vertical curves were also used, giving greatly improved road conditions.

Nowadays the Ministry of Transport recommended a distance of mutual visibility of 500 feet for an eye-level height of 3 feet 9 inches, and that was applied to all roads, regardless of classification. That distance of mutual visibility, he contended, was suitable only for speeds of up to 50 miles per hour. Obviously for higher speeds a greater distance was required, as it was directly related to the braking distance, which for all practical purposes varied directly as the square of the speed. Moreover, the length of the curve required to give that visibility of 500 feet was usually determined by measuring directly from the longitudinal section the distance between the tangent points when a 52-inch radius curve connected the intersecting gradients. For valleys, whatever curve looked right was the one usually adopted.

The trend of development in road vehicles seemed to be a continually increasing speed without a corresponding increase in braking power, having regard to the fact that the increase in the latter should be as the square of the speeds. For a speed of 120 miles per hour 500 feet was a wholly inadequate distance of mutual visibility, as that distance would then be covered in less than 3 seconds. In his Paper he had given an exact method of computing the length of curve required to give up to 2,000 feet of mutual visibility for summits and up to a speed of 120 miles per hour for valleys ; but those figures were not intended to set up an ultimate limit and constants for any figure in excess of them could easily be calculated.

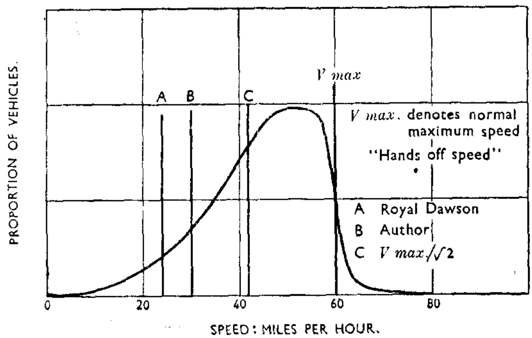
Other and less vital arguments for long and bold vertical curves were, firstly, the aesthetics of road design and, secondly, relief to the driver. Such curves had a pleasing appearance and resulted in an increase in road safety, owing to reduction of the strain on the driver.

Mr. A. J. H. Clayton said that Mr. Leeming had set out very clearly the problem with which his Paper dealt and had put forward a useful basis for calculating the length of transition curves.

With regard to superelevation, it seemed that the first thing that had to be found was some logical basis for the " hands-off " speed, which was an excellent term and much better than " ideal speed " or anything of that kind. From that point of view, there was no doubt that the pull on the steering-wheel was a very important factor. In normal driving on a cambered road there was always a slight pull on the steering-wheel, but to drive exactly at the " hands-off " speed on a curve would be uncomfortable and possibly dangerous. Mr. Clayton thought that people had experienced that when driving on the crown of the road ; they felt that they had no control over the car. Therefore the " hands-off " speed should be lower than the normal speed of driving, but the question was how much less it should be. Mr. Leeming had suggested that it should be half the normal maximum speed, whereas Professor Royal-Dawson thought that the value should be 0·4. Mr. Clayton wished to suggest three bases on which that could be decided. One was to take a fixed proportion of the normal maximum speed, such as Mr. Leeming and Professor Royal-Dawson had suggested. The second, which seemed to have some merit, was to give superelevation sufficient to restrict the unbalanced sideways force to some fixed proportion of the weight of the vehicle—something well below the limit at which the vehicle would skid. That would mean making the superelevation, say, 1 in 10 less than the ideal for the normal maximum speed. The third method, which had been adopted in the past, was to decrease the speed for which the superelevation was designed as the curve became sharper. That was the basis used in the latest circular of the Ministry of Transport on the lay-out and construction of roads.

The curve in *Fig. 1* showing the proportion of vehicles going at various speeds would probably be something like that illustrated, with most of the vehicles travelling at about 40–60 miles per hour and a few at more than the normal maximum speed. If the " hands-off " speed were taken as half the normal maximum speed, that would not provide very well for the most common speed, which was just below the maximum. He would suggest that superelevation should be provided for a value of $1/\sqrt{2}$ of the normal speed ; that would be found to give half the superelevation required for the maximum speed, and then the unbalanced sideways force for the maximum speed and the minimum speed would be the same and would be shared out fairly evenly. Most drivers would still have to pull on their steering-wheels to keep on the road, and that was the normal thing to do.

Fig. 1.



With regard to the length of the transition curve, he would like to refer to what he felt was the basis of the problem. Most people would agree that there must be some transition, because the car itself could not pass from a straight road to an arc of a circle instantaneously. Presumably the length of the transition curve had some relation to the speed and the radius of curvature which was ultimately to be reached, and he felt that it would be advantageous to determine that relation experimentally. He believed that some experiments had been made on the lines of plotting the curve of an ordinary driver taking a bend in the road with reasonable comfort and safety, in order to ascertain the relation between the necessary length of transition curve, the speed, and the radius of the curve. It was very important, he thought, that the transition curve should not be longer than was necessary, because obviously the driver could not follow a very long transition curve ; he could not keep on edging round the curve with fractional movements of the steering-wheel, but should definitely be able to pull the car round into the curve.

Mr. Leeming had made a distinction between British and American practice. Recent American practice seemed to be to use Shortt's basis, namely, the rate of change of acceleration, but to use a much higher value for the rate of change of acceleration, namely, from 2 feet to 7 feet per second per second, 2 feet being used for large-radius curves and the value increasing up to 7 feet as the radius was smaller. Mr. Clayton thought that that was done, not so much because there was any real difference between the large and small radius curves in regard to comfort, as because if the small figure was used for sharp curves ridiculous results were obtained. That brought him to the point that the value of 1 or 2 usually adopted for that constant was much too low for road practice. The value of 1 was adopted for railways, but a railway curve had to be designed for a much higher standard of comfort from that point of view. Therefore he thought that for roads the constant should be determined by experiment, and that its value would probably be found to be very much higher—about 8. He believed that Mr. H. A. Warren, Assoc. M. Inst. C.E., who had made some experiments, had come to that conclusion, and it seemed to fit in with the experience of drivers of the speed at which they went round roundabouts and ordinary street corners. Mr. Leeming's method of determining the length depended not only upon the value of C but also upon an arbitrary choice of the proportion of the maximum speed for which superelevation was to be given, and, in fact, he showed the peculiar situation that if the full superelevation were provided for the speed no transition was required at all. Naturally transition would be required to get up on to the superelevation, and Mr. Clayton thought that it would be necessary to check every case for that condition as well as for the unbalanced sideways force.

He would like to suggest that future research should be mostly experimental, firstly to determine the normal distribution of speeds on main roads and also on town roads. Secondly, it was desirable to determine the maximum reasonable superelevation. That was chosen quite arbitrarily, but it should have some real basis. As much as 1 in 6, he believed, had been used on some motor roads, but in Great Britain it was usually considered that anything more than 1 in 16 was bad. Thirdly, experiments were required to ascertain the limit of unbalanced sideways force for comfort. Mr. Leeming had rightly shown that the adoption of excessively long transition curves led to much too sharp a radius, and, in fact, would lead to an unbalanced sideways force on the ultimate curvature which was much too high for comfort or safety. Fourthly, experiments were required to ascertain the relation between the speed and the transition length, and also the value of the constants involved in that relation.

One matter which might be investigated theoretically was the method of designing the best curve to meet specific requirements with the minimum disturbance, and it might be worth while to look into that question mathematically.

With regard to Mr. Price's Paper, the final result might have been set out in the following simple formulas: for valleys, $L = \frac{(N_1 - N_0)V^2}{46 \cdot 5f}$, and for summits $L = \frac{(N_1 - N_0)d^2}{30}$. Where $(N_1 - N_0) < 6$ per cent., $L < d$. Those formulas could conveniently be put into one's notebook and used for any particular case.

The statement that 500 feet clear view was adequate for only about 50 miles per hour was true for ordinary two-way carriageways; but for a dual carriageway a clear view of 500 feet would be suitable for a much higher speed, as one only had to consider the visibility of a stationary vehicle over the brow of the hill and not that of an approaching vehicle.

Mr. H. C. Platts said that he would like to endorse Mr. Leeming's contention that all the lateral forces present should be taken into consideration in the whole theory of road curvature. He admired the way in which Mr. Leeming had built up that theory, but he felt that in certain respects the assumptions made were not altogether justified.

The whole object of superelevation was to obtain comfort and safety on the road, combined with expeditious movement. For any scientific approach to that ideal it was necessary to have a yard-stick for the degree of safety, or perhaps more usefully, a yard-stick for the degree of danger or inconvenience. It was very important to have that always clearly in mind. Owing to the dynamic effect, on a curve the centrifugal ratio provided that measure, whilst on a straight road the inclination of the camber or cross-fall provided it, and the centrifugal ratio could be reduced to an equivalent cross-fall on a straight road. Therefore if the two were combined a measure was obtained of the inconvenience or danger to which the people in the vehicle were subjected. He considered that the magnitude of that equivalent cross-fall was the major criterion, but he thought that Mr. Leeming attached more importance to avoiding the reversal of direction of the equivalent cross-fall or measure of inconvenience than he attached to its magnitude, and that assumption about the relative importance of those two factors had led Mr. Leeming to the development of many of the conclusions in the earlier part of the Paper. If that assumption were fallacious, as Mr. Platts submitted, the conclusions drawn from it were also fallacious.

With regard to the design of superelevation, there were practical limits to the amount—say from 2 per cent. to 10 per cent. Various ideas existed as to what the higher value should be, but a minimum of 2 per cent., or 1 in 50, for drainage requirements, and a maximum of 10 per cent., or 1 in 10, or even flatter, as governed by the comfort and convenience of slow traffic, were generally accepted. Those limits were really very narrow, and any engineer with discrimination and experience and knowledge of his locality could select the necessary gradient without making any calculation at all and, Mr. Platts submitted, without being very far wrong. It seemed to him that, if one were bounded by those limits, the important point was to consider the effects — the inconvenience and the danger — at the maximum and minimum of the range of speed that was being catered for in the particular circumstances. He thought that was by far the most important criterion, and, if his view were correct, the worst conditions would almost invariably occur at the top speed of the range, and it would therefore be wrong to talk about average speeds as determining the amount of superelevation that should be used. That was an idea which did not seem to him to be founded on any really practical basis. If one considered an average speed one was considering an ideal condition, whereas the degree of danger in the worst conditions should be the criterion.

In conclusion, he wished to make a remark which he hoped the Chairman would not rule out as irrelevant. It had some bearing on the subject under discussion, because Mr. Leeming's conclusions on the right-hand and left-hand curves were affected by the rule of the road. At present all the road signs were down and all manufacture of private motor-cars had ceased; and he wondered whether the war had not presented a unique opportunity for the rule of the road to be changed. That might perhaps be a great boon to British industry after the war.

Mr. G. L. Robb said that, according to Mr. Leeming's statement of his theory, the curve which he proposed to design incorporated, among other factors, the following two assumptions: (1) that the length of transition should be based upon the rate of gain of radial acceleration, and (2) that the length of transition was dependent upon superelevation. Since any one transition could have only one length, it followed, if Mr. Leeming's theory was valid, that superelevation and radial acceleration were interdependent quantities. He would attempt to prove that superelevation had no effect whatever upon the radial acceleration and hence upon the length of transition. It would simplify matters if he dealt in the first place with two cases, namely, the case of the flat road and the case of the road which was tilted to the "hands-off" speed.

When a car travelled around a curve whose carriageway had no superelevation, an external force was exerted by the road surface on the car, namely, the friction force. That friction force was the centripetal or centre-seeking force, expressed by $\frac{WV^2}{gR}$, and it produced an acceleration towards the centre of $\frac{V^2}{R}$. If the road were tilted to the "hands-off" speed, at that degree of tilt the reaction of the road on the car was normal to the road surface and the friction force had therefore disappeared. But, since the car continued to traverse a curved path, there must be some other force present sufficient to produce the centripetal force necessary for motion in a curved path. That second force was the horizontal component of the normal reaction; its value was, as before, $\frac{WV^2}{gR}$, and the radial acceleration which it produced was again $\frac{V^2}{R}$ towards the centre of the curve. Mr. Leeming had expressed it as follows: " The horizontal component of the reaction will, it is true, provide the radial acceleration which is demanded by the theory of circular motion." It could easily be shown, by drawing the force triangles, that the two forces were equal in magnitude and direction. It was also self-evident, because if they were not equal the centripetal force would differ and the car would not describe the same curve. Therefore it was clear that tilting of the road had not affected the centripetal force in any way; and therefore it had not affected the radial acceleration, either in magnitude or in direction. If now it were agreed to adopt Shortt's theory and the value of his contant C, then similar radial acceleration would produce similar lengths of transition. Since, as had been shown, superelevation had not affected the value or direction of the radial acceleration, therefore superelevation had not affected the length of the transition.

He would now consider the intermediate case, where the road was not fully tilted. It had already been shown for the flat road that the friction brought into play supplied the whole of the centripetal force required, and that in the case of the fully-banked road the horizontal component of the normal reaction supplied the whole of the centripetal force required. For the intermediate case the horizontal component supplied part of the whole and friction supplied the balance. Hence for any degree of superelevation considered the centripetal force would remain of the same value and direction as it had for the flat road; hence the radial acceleration would remain the same, and therefore the length of transition would remain the same. In other words, superelevation and radial acceleration were entirely independent factors, and a curve could not logically be designed whose length of transition was dependent on both.

As to the effect on the passengers, Mr. Leeming had said, in his Paper: " If the superelevation is enough to cause the reaction to be normal to the plane, the lateral component becomes zero, and the sole effect on the car and its occupants will be a negligible increase in their apparent weight." He had also stated that: " In this case the function of the tilting of the plane, or superelevation, has been to convert a lateral effect on the car into a purely normal or apparently 'vertical' one, and so to remove the tendency to skid or the feeling of discomfort to the occupants and the disturbance to their balance." The picture presented was one in which the occupants were riding round the curve in a state of perfect balance and complete comfort, and it appeared that some force which had previously existed and caused them distress had been rendered impotent; but neither the magnitude nor the direction of the centripetal force had been altered at all by the mere tilting of the plane. All that had happened was that the passengers had themselves tilted their bodies so as to meet the force fairly and squarely, which was a different matter. Precisely the same effect could be produced on the passengers if the road were left flat and the seats of the car tilted. Indeed, if the mechanism could be arranged so that the tilting of the seats of the car could be steadily increased along the transition, the blind man to whom Mr. Leeming had referred would be quite unable to distinguish a superelevated road from a flat road. No one would design a transition curve based upon the arrangement of the seats in a car.

The fault in Mr. Leeming's chain of reasoning was, however, most apparent in his description of *Fig. 2*, p. 7, *ante* : " It follows that, in analysing the effects on a car or its occupants, F_p must be the force considered, and not F_c." At that point Mr. Leeming appeared to contradict himself, for he went on to say : " Clearly it is wrong, if there are two or more forces acting on a body, to choose one arbitrarily and neglect the effect of the others " ; yet from the forces F_p and F_c he had selected F_p and had ignored F_c. F_c was the centrifugal force and F_p was that part of it which had not been resolved normal to the plane. Mr. Leeming's argument was as follows. A proportion of the total centrifugal force had been resolved normal to the plane, and in that position it had no sideways effect on the car or its occupants ; therefore it might henceforth be ignored. Only the force F_p remained to be considered. F_p was the friction force ; it was only part of the total force and it would contribute only a part of the whole centrifugal force necessary, and hence it would reduce the value of the radial acceleration, thus giving a shorter length of transition, which, of course, was what Mr. Leeming was aiming at. It should be noted particularly that he had to reduce the value of the radial acceleration, $\frac{V^2}{R}$. That value could be reduced only by either reducing the value of V or increasing the value of R. If the value of V was reduced the curve would not have the speed value for which it was designed, whilst if the value of R was increased it would not be the same curve.

Professor Royal-Dawson, in his book " Elements of Curve Design " had stated : " Any formula for length of transition based on hypothetical superelevation is fallacious, because the sole object of transition is to prevent the sudden application of centrifugal force, which is dependent only on speed and radius combined and has nothing to do with superelevation."

The simple parabola described in Mr. Price's Paper appeared to be based on the usually accepted average values for rate of change and vision distance as applied to two-way roads. Mr. Robb had worked out Mr. Price's Example 1 for length of curve, using Tables prepared some years ago by Criswell, and the results agreed within a matter of inches. Since the first publication of his works, however, Criswell had published, or was publishing, a much more comprehensive set of Tables to include one-way and two-way roads, and the cubic parabola as well as the simple parabola. Using these recently prepared tables, Mr. Robb had worked out Mr. Price's

Example 2 for a valley curve on a one-way road, and he had found that the lengths differed considerably. The length given in the Paper was 181 feet 6 inches, whereas the length from Criswell's Tables was 234 feet. The reason was that for one-way roads on summits the vision-distance was no longer the governing factor. The rate of change determined the length in that case, as it did in the case of valley curves, and Criswell had used a rate of change of 1 per cent. for 60 miles per hour, or an acceleration of 0·7744 feet per second per second, in comparison with the acceleration of 1·0 foot per second per second given in the Paper. Would Mr. Price consider that his curve applied to both one-way and two-way roads in that respect? Had he considered the advantages of the cubic parabola over the simple parabola, as being a nearer approach to the transition ideal used for horizontal curves?

With regard to length of curve generally, would Mr. Price use the calculated length as a general rule? In using Criswell's Tables for the design of trunk roads, Mr. Robb rarely adopted the calculated length, since he regarded it as the mathematical minimum rather than a basis for design. One such curve which he had picked out at random had a length of 1,200 feet, in comparison with the tabular length of 340 feet. The reason for that was twofold. Firstly, from the aesthetic point of view the design of trunk roads should be bolder in conception than the design of secondary roads. In plotting the profile, therefore, each variation in the ground-level was not rigidly followed, since that led to numerous changes of gradient connected by vertical curves, which were necessarily short in length, even though they satisfied the mathematical requirements. Whenever possible, long grade lines which averaged out the ground-levels were used, and those were connected by long vertical curves in order to maintain a proper aesthetic balance. The second reason was connected with the economics of the problem. For many years past it had been said that the use of short vertical curves on summits saved earthworks in cuttings, and that the use of similar curves in valleys saved earthworks in embankments; and that was quite true. There were, however, other factors to be considered. Firstly, earthworks were no longer carried out by pick and shovel, and it might reasonably be expected that after the war much greater use would be made of machines for earthworks than had been the case hitherto. Again, there were various ways of regarding the cost of a road. The road which was the cheapest to build was not always the cheapest to use. From the point of view of the road haulier, vertical curves were of some importance. If a road 100 miles long commenced at elevation 100 and finished at elevation 1,100, the least vertical distance through which the load had to be lifted was 1,000 feet. Every vertical curve between the beginning and the end of that road meant an extra use of fuel and additional wear and tear on the brakes and tires of the vehicle; hence the importance on heavily trafficked roads of restricting the number of vertical curves to the minimum. The length of the vertical curve was also of importance in that respect, because the longer the vertical curve, either in sags or on summits, the less was the vertical range of ineffective rise and fall. Another factor which affected the length of vertical curves was the coincidence of horizontal and vertical curves, which unfortunately could not always be avoided. Generally speaking, the horizontal curve would be much longer than the calculated length of the vertical curve, when the formula for L given in the Paper was used, and the distortion of horizontal curves in valleys through foreshortening would be severe. The use of the longest vertical curve consistent with good drainage would do something to remedy matters. The same foreshortening effect was present even when there was no horizontal curvature. With two long intersecting gradients in a valley connected by a short vertical curve, the road had the appearance of a switchback, which was undesirable. When vertical and horizontal curves coincided there might be a case, particularly if the aesthetic value of the road was of prime importance, for treating the horizontal and vertical curves as one entity and designing the horizontal curve, not so that it would be correct in plan according to current practice, but in such a way that it would appear to be correct. That device had been known to architecture since the earliest times.

With regard to formulas, Mr. Robb had found that a ready means of calculating the middle ordinate, and also of finding the position of greatest or least elevation, was of considerable value in designing curves where certain conditions had to be met, such as headroom clearance at bridges. Those could be found in the equations given in Mr. Price's Paper, but not, perhaps, in the most accessible form.

Mr. H. S. Keep thought that, considering how much had been said and written about transition curves and superelevation, Mr. Leeming was certainly to be congratulated on having found a new point of view to discuss. Mr. Keep was in complete agreement with the proposals and conclusions set out in the Paper, but he confessed to some mental confusion in trying to follow the reasoning, given in the section headed " General Theory ", by which Mr. Leeming had reached those conclusions. Taking the third paragraph in that section, it seemed simpler to break up the centripetal force and the force due to gravity into their components than to combine them and find the resultant, and then take the component of that resultant. Mr. Leeming's statement in regard to the effect on the passengers would be true if the passengers were rigidly held and the car was entirely rigid and if the centre of gravity of the passengers was in the plane of the road surface; but that would involve the passengers being in a prone position on a sled. Actually there was tire distortion and spring deflexion, yield in the seats of the car, and the body swing, produced by the couples, due to the fact that the centripetal force and the forces at the road surface which resisted it were not in the same plane. It was true that those forces and couples might be of only secondary importance, but at least it was doubtful whether they were negligible, especially in the case of the blind man who drove entirely by feel; if he was sitting in a car which could yield in that way he would be susceptible to the sway.

In dealing with the force F_p on curves, Mr. Leeming had said: "After the forces have been resolved as described above, those acting on the car will be as shown in *Fig. 3*." That seemed to be rather unfortunately expressed. In considering the forces acting on the car account should be taken of all those forces, including the reactions to the plane, and Mr. Keep suggested that it would be simpler to speak of the forces exerted by the car on the plane than of the forces that the plane exerted on the car.

He did not like the term "car curve" and he thought it was a pity to have so many complications. There were so many different curves: the inner curb line did not have the same radius as the outer curb line, not only owing to the width of the road but also because very often there was widening of the curve, and then the driver often took a different line. Now another curve was introduced, called the "car curve", which did not mean the curve which the car really took, but some other new device. He wondered whether it was worth while to introduce that, and whether it would not have been better to go straight on boldly to the "hands-off" condition. He assumed that Mr. Leeming really meant that on a straight cambered road there was a pull to one side—usually to the left—and that when the car came to a curve it was necessary to take account of that pull, adding it to or subtracting it from the centripetal force. Personally he would prefer to do that without inventing a new curve for the purpose.

With regard to another point, to which reference had already been made in the course of the discussion, was it right to demand that a road should be designed for a constant speed and that the driver of a car should be able to set the governor and go to sleep until he reached his journey's end? Was not it more reasonable to expect him to take some account of the curves and to drive more slowly round sharp curves than he would need to do round curves with a longer radius. That did not mean that the road engineer should not, so far as possible, provide easy curves.

The mathematics in Mr. Price's Paper did not give much scope for discussion, but he thought that persons engaged in setting out vertical curves would be grateful to the Author for giving such clearly worked examples.

At first sight it would appear that the vision-distances on one-way and two-way roads should differ; but there was always the possibility of some obstruction at or near ground-level, and for that reason the sight-distance needed was practically the same on either one-way or two-way roads.

Mr. R. A. Powell observed that although Mr. Price had stated, with regard to Table I of his Paper that: "The figures in the above Table are based on the equation given in Mr. Powell's Paper," they did not seem to agree with those that Mr. Powell had actually given in his Paper[1], wherein

[1] See footnote, p. 31, *ante*.

he had plotted his equation as a curve. Perhaps Mr. Price would be able to explain that.

With regard to the definition of the *curvature of a road*, in Mr. Price's Paper the symbol C, which denoted the visibility or speed-constant, gave very awkward figures. For instance, in Table II, for a distance of mutual visibility of 300 feet the value for C was 0·0003333, and for a distance of mutual visibility of 800 feet it was 0·0000469. Mr. Powell thought that the definition given in his Paper was much simpler to use. He took the symbol c to represent the length of curve for a 1-per cent. change of gradient; it might be 50 feet, or 54 feet, or something like that. That was a simple value which could be kept in one's mind, and all that had to be done to obtain the length of curve required was to multiply it by the percentage change of gradient. He had worked out the following formula from certain experiments that he had made: $c = \frac{V^2}{45 \cdot 6 \times f}$, where f denoted the vertical acceleration. The values he obtained from his experiments ranged between 1·65 and 2·74 feet per second per second, and the mean value was 2·28. Mr. Price had suggested that the lower value should have been taken, and a good deal could be said for that. By adopting the value of 2·28, a very simple formula was obtained, namely, $\frac{V^2}{100}$. With a speed of 40 miles per hour, for instance, $\frac{V^2}{100} = 16$. All that had to be done was to take away the 0 and square the figure remaining. If the value for f, the vertical acceleration, were taken as 1·0, and a value of 50 was taken instead of 45·6—which was quite near enough to what was required—the formula would be $c = \frac{V^2}{50}$. Mr. Price had taken f as 1·0 in his example and had worked out a curve for 1·5 per cent. change of gradient with a speed of 75 miles per hour. The expression thus became $c = \frac{75^2}{50} =$ about 110. When that was multiplied by the change of gradient, 1·5, the product was 165, the length of vertical curve required. Mr. Powell thought that that was much simpler than the method given in the Paper, which involved long rows of ciphers, and there was not very much difference in the results.

Another feature of vertical curves and horizontal curves to which he wished to refer, was the aesthetic point of view. He remembered working out a road—not a trunk road but a district road—with beautiful vertical curves on the section and beautiful horizontal curves on the plan; but the road looked terrible when it was built. It should not have done so, according to the plan and the section, but there was no connexion between the two, and for a road to be satisfactory from the aesthetic point of view that connexion was necessary. The vertical and horizontal curves should begin and finish at the same point; otherwise one worked the opposite way to the other from certain points of view, and, as he had said, the effect was very bad.

He wished to thank Mr. Price for having referred to his old figures, which he had thought to be completely forgotten.

Mr. G. A. Wood said that Mr. Leeming was to be congratulated on a very lucid exposition of a theory of transition as applied to highways, and more particularly on taking that exposition as far as Equation (11). It was, perhaps, important to remember that trunk roads and other high-speed roads were designed by people who paid great attention to theory, and the aim of those people was to keep the radius of the curve as large as possible. A large-radius curve was not so much in need of a transition curve as was a small-radius curve, so the man who mostly dealt with small-radius curves was the very hard-working divisional surveyor of a county authority, or the equivalent officer in a borough, who had many other calls on his time, such as controlling labour and ordering materials, and he could not devote so much attention to theory as the designer of a through route. A fundamental difference existed between the railway engineer and the road engineer in that matter. The former had to provide a running surface, and to guide the vehicle by means of the flange on the wheels, whilst the latter had only to provide the surface. The vehicles on that surface ranged from horse carts travelling at 2 miles per hour, perhaps loaded with hay 16 feet high, to luxurious motor-cars travelling at 80 miles per hour, in which the standard of comfort demanded was very high; there were also motor-cycles, which travelled fast but were easily controllable by their riders, and very heavy lorries, which travelled much slower, but, however light the controls were made, required physical and mental effort on the part of their drivers. All those vehicles had to be borne in mind by the highway surveyor. He provided a road of a certain shape and contour, and then the driver of any of those vehicles came along and chose the path which he found the most convenient. The highway engineer might put in a transition curve, but the driver might like another transition curve better, and would follow it. Therefore all that could be done by the highway surveyor was to provide such conditions as he judged to be the most advantageous, bearing in mind the very varied classes of vehicles and drivers that would use the road. In a large curve the need for a transition was very small indeed, and a driver could make his own transition on the road without getting out of his proper track on the road; but in a sharp curve, if no transition was provided a driver naturally and perforce made a transition, and that led to "cutting" the corner, a proceeding which everyone disliked, but which, with the design of many sharp curves, was nearly forced on the driver. Therefore it was on those sharp curves, which very seldom occurred on through roads, with their highly technical designers, that the most attention to transition should be given, and it was for that reason that Equation (11) was so valuable, because it provided the practical man with a ready basis on which to calculate a length of transition. Until the theoretical man could gain the co-operation of the practical men, who perhaps out-numbered him by fifty to one, he was a voice crying in the wilderness.

One unfortunate feature of all theories of transition curves was that the value of C—an arbitrary value—made a very considerable and, in fact, a direct difference to the length of the transition. The British tendency at the moment seemed to be to regard $C = 1$ as the proper factor. There was no doubt that $C = 1$ would give a perfectly comfortable path to both vehicle and passenger. Various people had carried out experiments and had stated that values as high as $C = 8$ gave quite comfortable results, but those experiments were laboratory experiments: a driver drove round a curve or a selection of curves and observed the results; but in practice what was quite comfortable when experienced once was very different when experienced perhaps two or three hundred times, with reversals of direction, and could then be quite uncomfortable to the human body. Therefore much greater thought should be given to the value of C. He believed that American omnibus companies had carried out experiments and had found that passengers could comfortably be subjected to $C = 2$, but that at $C = 3$ discomfort began to be felt. The essential factor to be borne in mind was the value of C at which passengers on a long run began to experience discomfort.

The limit of the other factor, the unbalanced resultant sideways force, could be determined much more easily, and he thought most people would agree that it should not be greater than about $\frac{W}{10}$. If that value were exceeded, the passenger rolled in his seat. Although the force might be applied gently, the passenger would still roll in his seat when the critical point was passed. If the force were applied suddenly, it could not rise even as high as $\frac{W}{10}$, because the sudden change in force, as in the case of all suddenly-applied force, produced a much greater effect.

He thought that Mr. Leeming would have done better to concentrate on a graphical method of consideration, such as was shown in *Fig. 13*, p. 22, *ante*, rather than on the car curve. Graphs plotted as in *Fig. 13* showed all the essential facts, namely, the force acting sideways on the vehicle and its occupants and, by the slope of the line, the rate at which that force was applied; and for comparing different curves with different superelevations and different transitions, a series of curves such as those shown in *Fig. 13* gave a very clear indication of the resultant forces.

With certain vehicles the "hands-off" speed was a speed of unstable equilibrium. Unless a car was very self-centering, at the "hands-off" speed it might wander across the road and from side to side in the manner which he believed was the bugbear of railway engineers. First it would sway to the right and, as soon as that was corrected, it would go to the left, and so on. Therefore it was perhaps a merit of any road that a constant sideways force to one side or the other should occur.

Perhaps in all matters of superelevation and transition it would be better to concentrate on uniformity rather than on absolute degree, especially on the same road and in the same district. It was very disconcerting, when travelling along a road where one felt quite safe at a high speed, to come suddenly and without warning to a curve on which the basis on which the superelevation was given was quite different from that on earlier curves. Frequently highway engineers, tempted by local circumstances, did not give the same degree of superelevation on a certain curve as had been given on the preceding curves; and that was very dangerous, because it introduced the unexpected. A driver travelling on the limit of safety on the other curves would find himself suddenly confronted with the fact that he was travelling beyond the limit of safety.

Another fault which often occurred was that half-way round a steady curve, for some extraneous reason, the superelevation had been altered. The highway engineer concerned might not realize the danger, because he knew the curve so well, but a stranger might be travelling fast along the road, with his hands fairly lightly on the steering wheel, and, before he could correct the pull on the wheel where the difference in superelevation occurred, an accident would occur.

Another fault which was found was that in a steady curve a change of curvature took place without any difference in the superelevation. That would not matter if due provision were made, either in the close relation of the two curves or by means of a transition, for making a gradual alteration in sideways force; otherwise a very dangerous condition was set up.

Emphasis should be laid on the importance of uniformity of practice, and of adherence to that practice, because lack of uniformity created dangerous conditions.

Mr. D. F. Orchard thought that the value of Mr. Leeming's Paper lay chiefly in the fact that he had demonstrated very clearly the processes which occurred when a car traversed a curve. Whether road engineers adopted his theory or not, he had presented a line of thought which should enable them to study intelligently any cases which departed from the standard. His conception of the car curve was certainly an excellent one, because it combined in one variable the three variables of speed, radius, and cant, but its limitations should not be overlooked. In the first place, it indicated only the state of any particular point on the curve, and was not of much use in assisting in calculations. It also broke down in the case of a car traversing a straight road which was cambered. Mr. Leeming had shown that as the speed increased so, under those conditions, the car radius also increased; in other words, the condition of camber was eased by an increase in the speed. Mr. Orchard did not think that was quite borne out in practice.

In his opinion, the great disadvantage of Mr. Leeming's theory was that a unified rate of turning of the steering wheel had not been adopted for all curves. This was due to the fact that the length of transition was controlled by the " hand " of the curve and the cross-fall on the straight. Mr. Leeming varied the length of transition and kept constant the process by which the change from the state of camber to that of maximum superelevation was attained. The normal practice of road engineers was the exact reverse; they adopted a constant method of obtaining the length of transition and varied the process of changing from camber to superelevation at the centre of the curve. Thus, if they were traversing a left-hand curve they would start with the correct cant on one half of the road, but if they were traversing a right-hand curve they would have an adverse cant on the road at the start and would take that out before they reached the transition. If to the length of transition determined by the normal system was added the length on the straight over which they arrived at a level cross-fall—or level on one side of the road at least—they would get a case parallel to that of Mr. Leeming, but the total length would not be the same as his length, because he had adopted the term F_p, or the force up and down the road, whereas in Shortt's method, which was the more usual, F_c, the centrifugal force, was adopted as the standard. Mr. Orchard thought that it was doubtful whether Mr. Leeming had been correct in taking F_p as the force on which to judge and design the curve. He thought that it would be rather better to keep still to the centrifugal ratio, or, $\frac{F_c}{W}$, because that took account of the weight of the vehicle, as Mr. Leeming desired to do, whilst it was a much simpler expression and had the advantage of having no dimensions; it was merely a ratio.

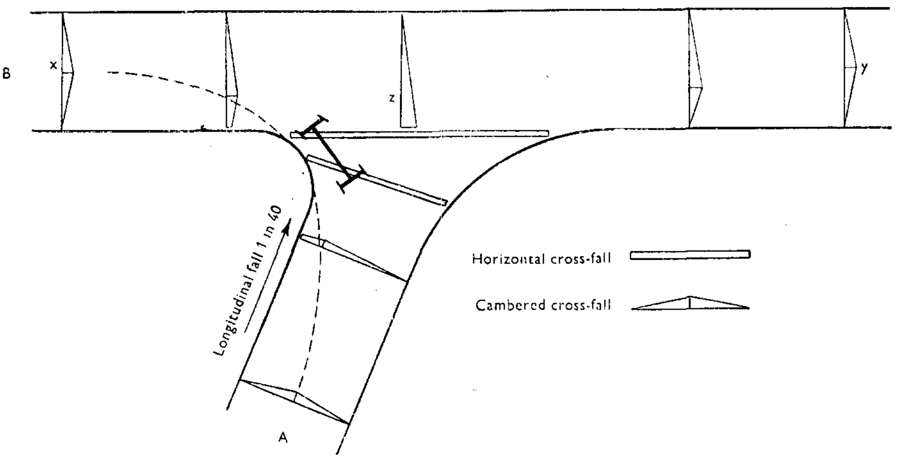
Another disadvantage of Mr. Leeming's line of thought was that, in the case of a left-hand curve, to obtain a constant pull on the steering-wheel it was necessary to superelevate the road at the centre of the curve to a degree far greater than that necessary to balance the sideways force, and that would be quite impossible in practice, as the side slope would reach prohibitively high figures. Mr. Leeming's ideal of a constant " hands-off " speed was impossible, and the change in the pulls on the steering-wheel, according to Mr. Leeming's system, would be much the same as in the normal system.

Mr. Leeming had rightly called attention to the importance of taking careful account of the cross-falls round a corner, but he had ignored the effect of longitudinal gradient. Mr. Orchard had had that effect forcibly demonstrated to him by a certain recently reconstructed curve of small radius which he had to use quite frequently (*Fig. 2*).

Road (A) had a longitudinal fall of about 1 in 40 or 1 in 50; road (B) had a normal cambered cross-fall at (x); and road (C) had a normal cambered cross-fall at (y). The crown of the road was brought round approximately as shown by the dotted line in *Fig. 2*, cutting the corner, and the road at (z) had a constant cross-fall right the way across, this being a continuation of the longitudinal grade of road (A). When a vehicle travelled around the curve from (A) to (B) it would take the attitude shown, and one wheel would be a little down the slope in comparison with the other, so that the condition of adverse superelevation would be considerably exaggerated. Over a period of 2 years he had witnessed at least twelve minor accidents on that curve, which was extremely difficult to negotiate, particularly when snow lay on the ground.

He could not quite agree with Mr. Leeming that it was advantageous to increase the length of the circular portion of a curve because it made the curve easier to set out. He did not think that one should economize in

Fig. 2.



the setting out of a curve, inasmuch as the cost of setting out the curve was a very small proportion of the total cost of the road, and it would be rather like spoiling the ship for a ha'p'orth of tar.

The American system of setting out the edge of a road to a gradient in order to reach the proper degree of superelevation and Shortt's method were really the same. It could be proved quite easily that the gradient of the outer edge of a curve and the gradient of the lower edge of a curve, if the superelevation was introduced by swinging about the centre line, was given by formula : Gradient $= 1$ in $\frac{2vg}{WCk}$.

where v denotes speed, in feet per second.

W „ width of road, in feet.

C „ rate of gain of centripetal acceleration, in feet per sec.^{3}

k „ proportion of centrifugal ratio which was at any instant to be taken up by superelevation.

If a curve were set out by making the rise in gradient of the outside edge and the fall in gradient of the inside edge according to that formula, exactly the same length of transition would be obtained as if Shortt's theory had been adopted.

He had carried out some experiments for determining the value of the rate of change in centrifugal acceleration, which could be used on a road curve, by leaving a trail of water from the under-side of a car and also from a pedal cycle, and subsequently surveying that trail. From the known constant speed of the car, and of the pedal cycle, it was possible, by measuring the radius at different points on the curve, to determine the value of the rate of change of centripetal acceleration. He found that very high instantaneous values of C could be attained ; indeed, he had obtained $C = 37$, but he had dismissed that as being quite useless for design purposes. Although he had obtained those very high instantaneous values of C, if he measured the radius at the beginning and the end of the curve he found that the value of C came down to about 2, and he would not like to recommend a value of C higher than 2 as a basis of design. He had found, however, that a higher value of the centrifugal ratio could be taken than the value of 0·25 usually adopted, and he thought that it could be increased quite reasonably to 0·3, whilst values even higher did not cause acute discomfort.

Mr. J. Taylor Thompson said that in Mr. Leeming's Paper it seemed to be implied that Mr. Shortt and he differed on the subject, whereas they were quite in agreement, having discussed the matter many times at considerable length. He thought it should be made quite clear that Mr. Shortt had been dealing with an uncanted curve. Mr. Shortt and he were railway engineers, and perhaps they did not use quite the same language as Mr. Leeming, but the principles, of course, were similar. Mr. Shortt had been thinking of a level piece of track and considering the rate at which the radial acceleration could be gained. If, however, the curve was canted and there was no unbalanced radial acceleration, then there could be no question of gaining it. Therefore Mr. Shortt's theory did not deal in any way with the maximum speed that could be obtained on a curve, nor, in fact, did it deal with canted curves at all.

In the matter of curves there were two things that one wanted to know. One was the maximum safe or comfortable speed for a circular curve, and the other was the safe or comfortable speed for the approach to that curve. The reason for the approach was partly the gain of the cant and partly that speeds were used in excess of the normal speed for the curve ; the normal speed being the speed when there was no unbalanced force on the passenger.

The problem came into prominence on the London and North Eastern Railway when they began running high-speed trains, such as the "Coronation" and the "Silver Jubilee." They travelled at speeds far in excess of those for which the railways had been constructed, and it had been necessary to examine carefully the existing conditions, in order to decide how fast those very high-speed trains could run with comfort, comfort being taken to cease at the point at which the passengers experienced "that sinking feeling." In order to ascertain that speed it was necessary to determine how much faster a train could run than the speed at which the resultants were at right-angles to the track and the passengers felt nothing at all. He had found, on studying the problem in some detail some years ago, that the old merry-go-round, with the boats on the ropes, gave the best mental picture. When the merry-go-round was level it could go round quite slowly and everybody on it was perfectly happy; there was no appreciable radial acceleration. But, as it went faster and faster, if it remained level ultimately, no matter how correct the rate of change of radial acceleration might be, the passenger would be flung to the ground. That was the point that had been missed, he thought, in all the discussions of the subject hitherto, namely, that rate of change was not enough; one wanted to know when to stop increasing. It was all very well to have a perfectly comfortable rate of increase, but one had to stop increasing some time; otherwise the passengers would be flung out. He had got into touch with some merry-go-round people, and had obtained some interesting information from them, but the most valuable information, he thought, came from braking tests. When one was sitting in a seat and was being swung around a curve, the horizontal force was just like the force that was experienced on a longitudinal seat in an Underground train when the train was braked. That was the basis he had suggested in a Paper[1] some time ago as the most logical and easily determinable one for finding out how much unbalanced radial acceleration the passenger could endure. He thought the value was 2·6. That value having been determined, a simple calculation would show the maximum comfortable speed for the circular part of the curve.

In the case of a canted curve, it was not possible to determine the length on the rate of increase of the unbalanced force, because there was not one; therefore he had suggested adopting the rate of tilt. He had enlisted the help of some fat and thin men and had tilted them in order to ascertain when the average person was just comfortable in the rate of tilt. The passenger was sitting level and he was then tilted until he came to a constant state with no unbalanced force. He was lifted through an angle, and at some particular rate he could be lifted and be unconscious of the fact. That was how he had obtained the expression $L = EV$, which had been quoted in Mr. Leeming's Paper.

With regard to the "hands-off" speed, that term seemed to imply that a road vehicle would travel around a curve by itself at a certain speed. He did not understand that at all, and he thought that any car would run over the top unless the driver steered it around the curve. Cant had

[1] Reference 8 in Bibliography on p. 26, *ante*.

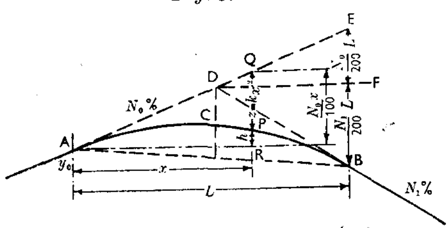
nothing to do with keeping the vehicle in the curve. A train was kept on the line by means of flanges, but a car or other vehicle was kept on the road by the driver turning the steering-wheel.

The Chairman said that it was very refreshing to have the experience of Mr. Thompson as a railway engineer. Road engineers might be inclined to envy railway engineers the absence of many of the complexities relating to curve design which had to be dealt with on the roads by reason of the conditions which obtained there.

Reference had been made to the distance of 500 feet adopted by the Ministry of Transport as a suitable range of visibility; in a Memorandum to be issued shortly that distance would be increased materially.

*** **Dr. L. A. Beaufoy** considered that Mr. Price's Paper was not sufficiently clear in respect of the signs to be used in the equations derived. For example, comparing equations I and II, p. 32, *ante*, a minus sign appeared mysteriously in one, but not in the other, whilst equation 7, p. 10, *ante*,

Fig. 3.



tended to throw suspicion on that minus sign. It was by no means immediately apparent which of those versions of C was correct, and the necessity for applying, in addition, a sign convention (plus/minus for slopes rising/falling from left to right) to values of N tended to add to the confusion. Such a method as that suggested by Mr. Price was clearly intended for more or less mechanical use, and therefore it should be correspondingly foolproof, and free from confusion as to signs.

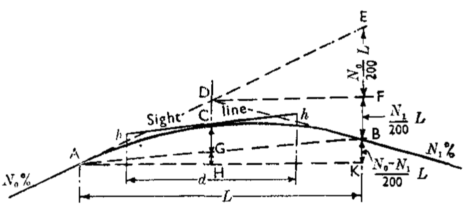
In Dr. Beaufoy's opinion, a further drawback of Mr. Price's method was that it was, to a large extent, divorced from any physical conception, the value for C being simply a mathematical expression resulting from the analysis. It was possible quite easily to introduce the physical conception as well as to dispense with the necessity for a sign convention for slopes. Considering the parabolic summit curve ACB of *Fig. 3* (which had been chosen as having the same characteristics as *Fig. 1*, p. 28, *ante*), if AD were produced to E, vertically above B, then by inspection EF and FB were equal to $N_0L/200$ and $N_1L/200$ respectively. For the skew parabola ACB, the height z for any point P on the curve to Q in the

*** This and the following contributions were submitted in writing.—SEC. INST. C.E.

line of the slope AD was $k \cdot x^2$, where x denoted the horizontal distance of P from A. The value of k could at once be found from the boundary condition $x = L$, where $z = $ EB. $kL^2 = L(N_0 + N_1)/200$, from which $k = (N_0 + N_1)/200L$. Hence the height h of the point P above the starting-point A was given by RQ$-$PQ$=N_0x/100-(N_0+N_1)x^2/200L$. Therefore the reduced level of P $= y_0 + N_0x/100 - (N_0 + N_1)x^2/200L$. That equation should be compared with equation IV, p. 32, *ante*, when it would be seen that Mr. Price's constants A and B were $+ N_0/100$ and $(N_0 + N_1)/200L$ respectively (*cf.* equations III and II). Therefore $C = 2A = (N_0 + N_1)/100L$ (*cf.* equation II). So far as possible Mr. Price's symbols had been used throughout.

Therefore the treatment of any vertical curve, whatever the gradients N_0 and N_1, could be easily derived from a sketch (corresponding to *Fig. 4*), in which most of the quantities could be inserted by inspection, whilst the possibility of error in sign did not arise.

Fig. 4.



The value of C in terms of the distance of mutual visibility (see equation (10), p. 30, *ante*) could be derived equally simply. In *Fig. 4* (chosen to have the same characteristics as *Fig. 2*, p. 30, *ante*), EF and FB, as before, were equal to $N_0L/200$ and $N_1L/200$ respectively.

Hence, $\quad BK = (N_0 - N_1)L/200\,; \;\therefore\; GH = (N_0 - N_1)L/400.$

Thus, $\quad DG = N_0L/200 - (N_0 - N_1)L/400 = (N_0 + N_1)L/400.$

As a property of the parabola, C bisected DG; $\therefore$ CG $= L(N_0 + N_1)/800$. All those values were obtained by inspection. Now when $d/2 = L/2$ (that was, for minimum length of curve),

$$h = L(N_0 + N_1)/800.$$

But $$h = k \cdot L^2/4\,;$$

$$\therefore\; L(N_0 + N_1)/800 = k \cdot L^2/4\,; \;\therefore\; k = (N_0 + N_1)/200L \text{ (as before)}.$$

Thus, $h = \dfrac{N_0 + N_1}{200L} \cdot \dfrac{(d)^2}{(2)} = \dfrac{C}{2} \cdot \dfrac{d^2}{4}\,; \quad \therefore\; C = \dfrac{8h}{d^2}$ (*cf.* equation (10), p. 30, *ante*).

The paragraph immediately preceding equation (7) (p. 29, *ante*), appeared to be based on some misunderstanding, as it implied that $(x_1 - x_0)$ was only *approximately* equal to L. In the notation (p. 27, *ante*) L was given as the horizontal length of the curve, and x as a horizontal ordinate, whilst the primary equation $d^2y/dx^2 = C$ certainly implied x as a horizontal value. Thus it would appear that no question of approximation arose and that $(x_1 - x_0)$ was in fact exactly equal to L. In practice the surveyor, in setting out the curve in the field, was concerned only with horizontal distances between stations and vertical differences of level.

Mr. G. W. M. Boycott considered that Mr. Leeming had done valuable work in drawing attention to the effects of cant and camber upon curvature, and thus showing that they were to some extent interchangeable. More particularly the views expressed by him as to the advantage of the elimination of the camber or the transition seemed particularly sound.

Mr. Leeming had suggested that transition curves were more difficult to set out than circular arcs, but Mr. Boycott, who had in the past set out a large number of railway transition curves, considered that that was not the case and that the method of setting out transitions was not essentially different from that of setting out circular arcs. The calculations took longer, and when they had to be made in the field Tables should be utilized. He was also of opinion that the construction of transitions should be no more difficult than that of circular arcs, although he could not speak from experience. The reason for short transitions should therefore be sought on other grounds.

Mr. Leeming had further stated that long circular arcs with short transitions were better than all transition curves; and although Mr. Boycott could not agree, it was clear from the context that what Mr. Leeming more particularly had in mind was the necessary limitation to the length of the transition as determined by the maximum permissible value of F_p and C, whilst he did not appear to propose any direct maximum value for the superelevation, which was therefore limited only by F_p and the average speed for any given radius. Although he had suggested values for both F_p and C, he had rightly pointed out that those were details which did not affect his main theory. Mr. Boycott considered that in order to obtain the full value of the Paper, and of similar Papers which might be communicated to the Road Engineering Division, tests were required, further to those already made in the past, in order to obtain the maximum permissible and best values for F_p, C, and superelevation.

Mr. R. H. Cunningham observed that since the exterior angle formed by the intersection of the gradients was small, and generally less than 4 degrees, the form of the vertical curve, whether circular or parabolic, was of no consequence and the design of the one could be relied upon to be identical with the other.

In Mr. Price's formula (10), p. 30, *ante*, where $C = \frac{8h}{d^2}$, C was the reciprocal of the radius of the circular curve R, since $R = \frac{d^2}{8h}$:

$$\therefore R = \frac{d^2}{30} \quad \dots \dots \dots \quad (a)$$

Also, where $\frac{1}{M}$ and $\frac{1}{N}$ denoted the gradients,

$$\left\{\frac{1}{M} - \left(\pm \frac{1}{N}\right)\right\}R = d,$$

or

$$\frac{N \pm M}{MN} \cdot R = d \quad \dots \dots \dots \quad (b)$$

whence,

$$d = \frac{30MN}{N \pm M} \quad \dots \dots \dots \quad (c)$$

d in equation (c) was the longest vertical curve which gave the eye-level height of 3 feet 9 inches on summits and, substituting its value in equation (a), R was the flattest curve under the same condition.

Those values of d and R would not be required in practice and, accepting the values of d in Table I, p. 33, *ante*, a Table of values of R might be derived from $R = \frac{d^2}{30}$ for various safe speeds which, substituted in equation (b) above, gave the minimum length of vertical curve required.

A similar procedure might be followed in the case of valley curves.

In Mr. Price's worked examples, the radius of the summit curves was 8,333⅓ feet, and the radius of the valley curve was 12,100 feet for the calculated lengths of vertical curves.

Those radii, if rounded off to 8,400 feet and 12,000 feet respectively, gave the following lengths of curve:

1. $d = \frac{80 + 40}{3,200} \times 8,400 = 315$ feet (Mr. Price's value, 312 feet 6 inches).

2. $d = \frac{200 + 100}{20,000} \times 12,000 = 180$ feet (Mr. Price's value, 181 feet 6 inches).

3. $d = \frac{100 - 25}{2,500} \times 8,400 = 252$ feet (Mr. Price's value, 250 feet 0 inches).

Mr. S. G. Fox observed that the results obtained by Mr. Leeming appeared to form a great advance on any previous theories, for recognition was taken in one formula of all the factors involved. It seemed logical that the criterion for a transition curve should be that the value of F_p (the resultant lateral thrust parallel to the plane of the road surface—not merely horizontal as most earlier writers had considered) should vary uniformly throughout the length of the transition curve. The value of F_p and of its rate of variation affected the comfort of the traveller, the safety of the vehicle (side-slipping), and the actual steering of the vehicle ($F_p = 0$ giving the " hands off " condition). In a road transition curve there was nothing to indicate to a driver (*a*) when the transition curve started; (*b*) its minimum radius; or (*c*) for what speed it had been designed. The only real guide, therefore, which a driver had was the " feel " of the curve or the " handling " of the vehicle, and that was, in effect, transmitted to him by the value of F and its rate of variation.

Unfortunately, Mr. Leeming, after establishing a formula for the length of the transition curve, had given very little information as to the form of road curve which should then be adopted. Therefore Mr. Fox had attempted to carry Mr. Leeming's results a stage farther, and had indicated the form of curve to be adopted according to the method of applying the superelevation. These results were shown in *Figs. 5*, p. 63.

The force F_p at any point on the transition curve, l feet from the commencement, was given by

$$F_p = W\left(\frac{v^2}{gr}\cos\theta - \sin\theta\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

where r denoted the radius of curvature at the point considered, and θ the angle of cant (small enough for $\cos\theta = 1$, $\sin\theta = \theta$).

$\therefore$ Equation (*d*) became

$$F_p = W\left(\frac{v^2}{gr} - \theta\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (e)$$

Now
$$C = \frac{g}{W}\cdot\frac{dF_p}{dt} = \frac{gv}{W}\cdot\frac{dF_p}{dl} \quad . \quad . \quad . \quad . \quad . \quad (f)$$

(assuming v to be constant).

Differentiating equation (*e*) and substituting the value of C from equation (*f*),

$$C = -\frac{v^3}{r^2}\cdot\frac{dr}{dl} - gv\cdot\frac{d\theta}{dl} \quad . \quad . \quad . \quad . \quad . \quad . \quad (g)$$

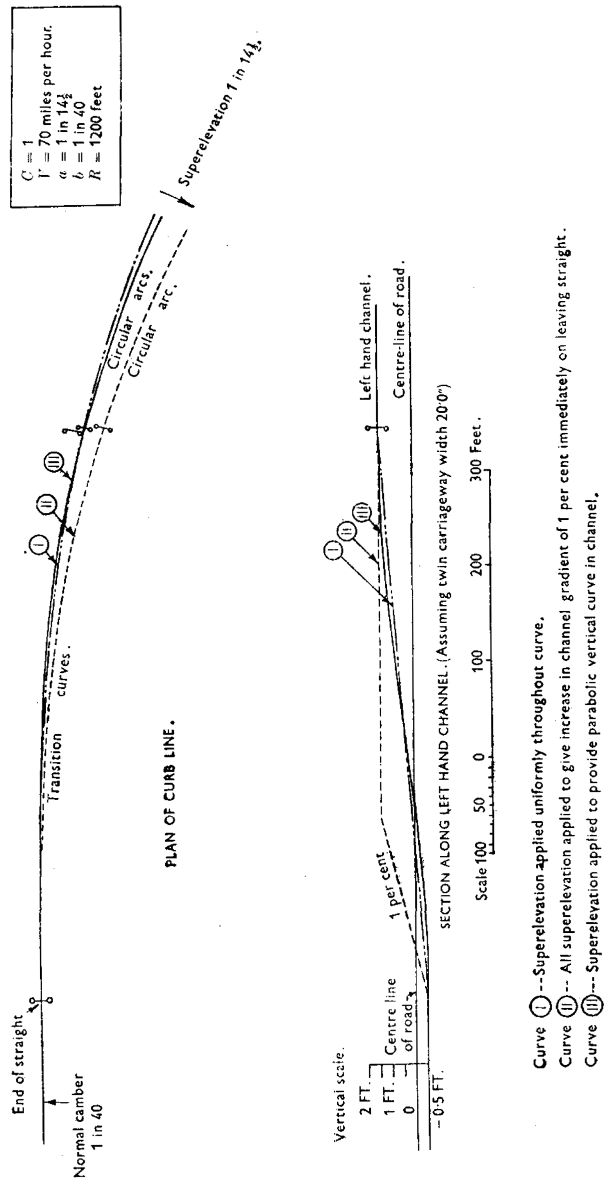
Now $\frac{d\theta}{dl}$ = rate of application of superelevation along the length of the transition curve;

$\frac{dr}{dl}$ = rate of change of the radius of curvature along the length of the curve.

If a lemniscate or spiral transition curve were adopted such that

$r \propto \frac{1}{l}$, then $-\frac{v^3}{r^2} \cdot \frac{dr}{dl}$ was a constant, and if C was to remain constant, it followed from equation (g) that $\frac{d\theta}{dl}$ must also be a constant, that was, the

Figs. 5.



Forms of Transition Curves giving constant Rate of Change of Lateral Force F_p for Different Methods of Applying Superelevation.

In each case length of transition $= \frac{1}{C}\left\{\frac{3{\cdot}5v^3}{R}\cos a - 47{\cdot}2v(\sin a + \sin b)\right\}$.

superelevation should be applied at a *uniform rate throughout the whole transition curve.*

Alternatively, if all the superelevation were applied along a portion only of the transition curve, then the curve would have to consist of parts of two lemniscates, if C was to remain constant. (The total length, however, would remain unaltered, as shown by Mr. Leeming.)

In the example chosen by Mr. Leeming ($R = 1{,}200$ feet, $V = 70$ miles per hour, right-hand curve with camber of 1 in 40 on the straight), let it be assumed that the total superelevation was applied so that the increase, in channel gradient was 1 per cent. (assuming 20-foot wide dual carriage-ways);

then
$$\frac{d\theta}{dl} = \frac{1}{2{,}000} = \frac{(a+b)}{l} = \frac{\left(\frac{1}{14\frac{1}{2}} + \frac{1}{40}\right)}{l} \quad . \quad . \quad . \quad . \quad (h)$$

$\therefore$ from that the length of transition on which the gradient was applied $= l = 188$ feet.

It could be shown from equation (g) that the curve consisted of two parts, as follows:—

when $0 < l < 188$ feet;

$$l = \frac{v^3}{2{\cdot}64} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (i)$$

and when 188 feet $< l < 590$ feet,

$$(l - 188) = \frac{v^3}{r} - \frac{v^3}{2{,}170} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (k)$$

Both of those parts were lemniscates or spirals and the actual curve was shown in *Figs. 5* (Curve (ii)).

An important result was obtained from equation (g) when the super-elevation was applied, not to give a straight channel gradient, but so as to provide vertical parabolic curves to the channels to be lifted.

In that case $\frac{d\theta}{dl}$ was not constant, but was given by the relation $\frac{d\theta}{dl} = 2ml$ for the first half and $\frac{d\theta}{dl} = 2m(L - l)$ for the second half of the transition curve (where m was a constant) $= \frac{2(a \pm b)}{L^2}$.

Equation (g) became, for $o < x < \frac{L}{2}$, $c = -\frac{v^3}{r^2}\frac{dr}{dl} - 2gvml$, or, integrating with respect to l,

$$\frac{1}{r} = \frac{cl}{v^3} + \frac{gml^2}{v^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (l)$$

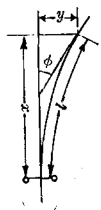
For deviation angles (ϕ) of about 20 degrees or less, it was possible to write x for l (*Fig. 6*).

Furthermore $$\frac{1}{r} = \frac{d\phi}{dl} = \frac{d\phi}{dx}.$$

Making those substitutions, equation (l) became

$$\frac{d\phi}{dx} = \frac{cx}{v^3} + \frac{gm}{v^2} \cdot x^2.$$

Fig. 6.



Integrating,

$$\phi = \frac{c}{v^3} \cdot \frac{x^2}{2} + \frac{gm}{v^2} \cdot \frac{x^3}{3}.$$

Writing $\phi = \dfrac{dy}{dx}$ and integrating again gave

$$y = \frac{c \cdot x^3}{6 \cdot v^3} + \frac{gmx^4}{12v^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (m)$$

(Constant of integration = 0.)

Similarly it could be shown that equation (m) also applied for the second half of the transition (provided that the total length L were obtained as indicated in Mr. Leeming's Paper).

(That result was of a similar nature to that obtained by Mr. H. A. Warren, Assoc. M. Inst. C.E.[1], but instead of the arbitrary constants used by him, the constants in the above formula had been obtained on a more logical basis. The equation was also a lower degree in terms of x.)

Example :

$C = 1$, $V = 70$ miles per hour ($v = 102{\cdot}7$ feet per second) ;
$b = 1$ in 40 ; $a = 1$ in $14\frac{1}{2}$; right-hand curve, radius 1,200 feet.
Length of transition $L = 590$ feet.

Integrating $\dfrac{d\theta}{dl} = 2ml$ gave $\theta = ml^2 +$ Constant . . . . . (n)

[1] "Highway Transition Curves: a New Basis of Design," Journal Inst. C.E., vol. 14 (1939–40), p. 373 (June 1940).

Now when $l = 0$, $\theta = -b$, and when $l = \frac{L}{2}$, $\theta = \frac{a-b}{2}$.

$\therefore$ Substituting in equation (n) gave

$$m = \frac{2(a+b)}{L^2} = \frac{2\left(\frac{1}{14\frac{1}{2}} + \frac{1}{40}\right)}{590^2} = 5{\cdot}4 \times 10^{-7}$$

$$\therefore \frac{gm}{12v^2} = 1{\cdot}37 \times 10^{-10} \text{ and } \frac{C}{6v^3} = 1{\cdot}54 \times 10^{-7}.$$

Equation (m) for the transition curve

became $y = 1{\cdot}54 \times 10^{-7} \times x^3 + 1{\cdot}37 \times 10^{-10} \times x^4$.

The resulting curve (iii) was plotted in *Figs. 5*, p. 63, for comparison with the lemniscate curves (i) and (ii).

Table Showing Offsets and Radii of Curvature of the Three Curves in *Figs. 5*, p. 63.

Distance from end of straight: feet.	Curve (iii).		Curve (ii).		Curve (i).	
	Offset.	Radius: feet	Offset.	Radius: feet.	Offset.	Radius: feet.
0	0	∞	0	∞	0	∞
100	0·2	9,200	0·4	4,080	0·2	7,080
188	—	—	2·7	2,170	—	—
200	1·5	4,000	3·2	2,120	1·9	3,540
295	5·0	2,410	10·1	1,787	6·0	2,400
390	12·3	1,720	22·1	1,545	14·0	1,815
490	26·1	1,355	41·0	1,350	27·7	1,445
590	48·3	1,200	67·2	1,200	48·3	1,200

Mr. B. G. Manton, whilst agreeing that curve design and superelevation should be regarded as inter-connected problems, suggested that Mr. Leeming's treatment of the subject would have been more complete if he had considered the effect of road friction.

The equilibrium of a vehicle on a curve could be represented approximately by the equation $\frac{V^2}{gR} = S + F$, where S denoted the superelevation slope, expressed as a decimal, and F was the friction factor.

Assuming a maximum crossfall of 1 in 12, the maximum value of S was 0·08 and the usually accepted maximum safe value for F was 0·16 up to speeds of 60 miles per hour.

Hence, in the limiting case, $\frac{V^2}{gR} = 0{\cdot}24$, approximating closely to the well-known rule that the centrifugal ratio should not exceed $\frac{1}{4}$.

F denoted the friction coefficient developed between tire and road when the plane of the wheel was not coincident with the direction of motion of the vehicle, but without the occurrence of skidding. That occurred when a lateral force was imposed on the vehicle and the inertia of the vehicle resisted the force. The " slip angle " so produced would rarely be the same for both front and rear wheels and the path which the car tended to describe depended upon their relative values.

If the rear slip angle were greater than the front when the car was on a left-hand curve, the car would tend to turn towards the left, but if the rear slip angle were less than the front, the car would tend to turn towards the right.

The former condition was unsafe, and might be due to the design of the car (" oversteering ") or to an insufficient pressure in the rear tires or to an excessive load on the back of the car.

Mr. Leeming's value of $0{\cdot}025W$ for side pull on a camber of 1 in 40, gave 56 lb. for a car weighing 1 ton. Assuming equal weight-distribution on rear and front wheels, that gave a pull of 28 lb. on the latter. A rough experiment showed that a pull of that magnitude did not perceptibly alter the feel of the steering with the front axle jacked up.

Road friction and the inertia of the rotating wheels would reduce that side pull; and it should be noted that in the case of the average car the weight of each wheel was nearly 10 per cent. of the load it supported.

Professor F. G. Royal-Dawson observed that Mr. Leeming's " new theory " contained (*a*) the assumption that Shortt's formula $C = \frac{v^3}{LR}$ $\left(\text{or } L = \frac{v^3}{CR}\right)$ took no account of superelevation; (*b*) the claim that equation (8), p. 30, *ante*, remedied that supposed defect.

The facts were (*a*) that the experiments from which Shortt's value of C was decided were carried out on normally superelevated railway tracks, and (*b*) that equation (8) contained only the tangential component F_p of the centrifugal force, the normal pressure-component P being entirely omitted. As F_p might be zero, or even negative, according to whether the speed was at or below superelevation standard, whilst P was always positive, increasing with the angle of superelevation; and as, moreover, on railways there was usually a smaller margin between the maximum and superelevation speeds than on roads; it followed that in Shortt's experiments the lateral component must have played a comparatively small part, the chief reaction being P in the form of gradually increasing pressure on the seat. Thus the time-rate at which that pressure was applied automatically constituted the main ingredient of the conditions under which the passage of the transition passed the criterion of being " unnoticed." That was the criterion by which Shortt had derived the value $C = 1$ and its corollary.

Thus not only was the alleged defect in Shortt's formula controverted by facts, but also formula (8) contained the very defect which it purported to remedy.

Mr. Leeming seemed to be vaguely aware of that by admitting that at " hands-off " speed $L = 0$, but he sought to minimize the anomaly by suggesting that certain other facts had to be allowed for, such as the impracticability of putting a step in the road. He had thus fallen into the same trap as Warren, and thereby begged the whole question of a rational method of determining L. To get out of the dilemma he postulated an unduly large and rather haphazard gap between the maximum and " hands-off " speeds, so as to produce positive, but none-the-less artificial, values of L. In his Table of examples he appeared to assume that the lengths of curves resulting from that process complied with Shortt's definition of C (as might be inferred by his use of that symbol in equation (8)). As a matter of fact, they did not comply with any particular definition.

He had introduced a side issue by endeavouring to incorporate normal camber into the problem, but he had produced no convincing argument against the accepted principle of eliminating camber prior to reaching the zero-point of the transition on the side which would form the outer edge of the curve. Equally unconvincing was his argument for differentiating between right- and left-hand curve-lengths. Camber was, dynamically speaking, a defect in road design, justifiable only to the extent that drainage could not be adequately provided by other means. From the user's point of view its objections were obvious, as might be illustrated by the proneness of drivers of all classes to ride on the crown whenever they had a chance. A possible remedy which did not seem to have engaged much attention lay in the use, when practicable, of long flat reverse curves in place of long straights in new projects. The camber was thus replaced by equivalent superelevation, meeting by that means the twofold exigencies of dynamics and drainage, not to mention subsidiary advantages of an aesthetic and psychological nature. Professor Royal-Dawson had dealt already with that question at some length[1].

Mr. Leeming's Paper called for many other criticisms in detail, but only one or two would be mentioned. In *Fig. 11*, p. 20, *ante*, the problem of alignment was usually governed by the available distance from the apex to the midpoint. No intelligent engineer would make his midpoint at A if B were available.

The ever-recurrent controversy as to the value of that much misunderstood symbol C to be adopted in road design, would never be settled by armchair theories, or even by backyard experiments, but only by intelligent and systematic observation of actual traffic movements on the open road.

Mr. Price's method of approaching his subject, by starting with a

[1] " Motorways, Fly-overs, and Mountain Roads " (Spon).

second derivative and proceeding upwards by successive integration, although ingenious, tended rather to obscure the simple geometrical nature of a summit curve. For a given minimum visibility-distance d and a given height of eye h, the vertical curve was essentially a circular arc of radius R, given by the well-known versine formula,

$$h = \frac{d^2}{8R}; \quad \text{whence } R = \frac{d^2}{8h}.$$

By making h a constant 3·75 feet, the formula became $R = \frac{d^2}{30}$.

Hence if $d = 500$ feet, $R = 250{,}000/30 = 8{,}333$ feet.

The required length of curve was then found by the simple process of multiplying the grade angle by $R/100$. Thus, in Mr. Price's example, the grade angle being 3·75 and $d = 500$ feet, the required length of curve was 3·75 (83·33) = 312·5 feet.

Mr. Price had made no mention of a radius in any of his figures, but his symbol C happened to be the *reciprocal* of the radius, a function which might easily pass unrecognized. For instance, in Table II, p. 33, *ante*, against $d = 500$ was found $C = 0{\cdot}00012$, which was the reciprocal of 8,333. By making the length 350 feet the value of R was slightly altered, but it had served its purpose as a stepping-stone, whilst the same remark applied to C.

In Table I, p. 33, *ante*, giving safe speeds and corresponding visibility-distances, Mr. Price had not indicated how the figures were arrived at, beyond quoting Powell's equation. The usual method of determining d was to calculate the stopping-distance required to avoid a collision, assuming 50-per cent. brake efficiency and to add the distance travelled in 1 second to allow for a mental lag on the part of each driver before action was taken after the opposing car was sighted. But as the stopping-distance varied as the square of the speed, it would be found that at speeds of 60 miles per hour and upwards the calculated stopping-distance was sufficient to give the driver a reasonable chance of passing the other car with little or no diminution of speed. That should be the criterion of good design. For lower speeds the distance was so reduced as to give the driver practically no option but to apply his brakes. It could be shown that, assuming both cars to be on the crown of the road when first sighted, they could pass one another in 3 seconds + 1 second for mental lag with reasonable safety without slackening speed up to 60 miles per hour—or 8 seconds in all for the two drivers. Thus for all speeds within that range a simple rule was $d = 8v$, where v denoted the speed, in feet per second. For speeds exceeding 60 miles per hour, owing to the increased difficulty of steering at high speeds, the allowance of 8 seconds should be gradually increased up to, say, 12 seconds for 100 miles per hour. It would be found that the figures arrived at by that means agreed roughly with Table I for speeds of 60 miles per hour and above, but that for lower speeds Powell's values were insufficient. For instance, at 40 miles per hour ($v = 58\cdot7$), d should be $8\ (58\cdot7) = 470$ feet, say 500 feet, whereas Powell's figure was 300 feet, sufficient for only 25 miles per hour.

For valley curves, Mr. Price had adhered to the circular arc form, although the incidence of impact, which was negligible for summit curves, became appreciable and intensified by gravitation in the case of dips. Therefore, there was as much need for transitioning the latter as in the case of horizontal curves. Professor Royal-Dawson's views on the subject had already been expressed in a Paper presented to The Institution [1] and he would not pursue the matter further, except to point out that for a given speed and grade angle the circular arc involved a larger radius than the minimum radius of a corresponding transition giving a safe and comfortable impact factor, especially in the case of large grade angles.

Mr. D. W. M. Smith observed that, in his view, Mr. Price had made the problem appear to be more difficult than was warranted, especially by basing all calculations upon the value of C which was shown by Table II, p. 33, *ante*, to be a difficult figure to utilize.

The equations given should be expressed as functions of, *inter alia*, (*a*) the length of the curve, (*b*) the gradients. The results obtained from both Mr. Price's equation for the curve and that given by Mr. Smith [2] were identical, but the latter was simpler in use.

Mr. Price had overlooked the following very important points when considering summits :—

(1) Formula (10), p. 30, *ante*, was correct only when the length of the sight line was less than that of the curve. The proof given broke down immediately when the sight line became longer than that of the curve, since the " straights " of each end of the curve had to be considered.

(2) The comfort of passengers passing over valleys, *and summits* had to be studied and *not only* valleys. For summits, therefore, the lengths of the curves to satisfy both the sight line and vertical acceleration requirements should be determined and the curve of longer length adopted.

Therefore considering (1), Mr. Smith disagreed entirely with the results of example No. 1, p. 34, *ante*. Taking Mr. Price's figures, since a sight line of 500 feet was to be obtained on a curve of 312 feet 6 inches, one would expect a sight line of approximately 525 feet to be obtained on the designed curve of length 350 feet. That was not so. The sight line on a curve of that length would be found, on studying Table IV, p. 35, *ante*, to be approximately 576 feet long. That discrepancy was due to the

[1] " Highway Vertical Curves : the Impact Factor " (not yet published).

[2] " General Properties of Parabolic Vertical Curves, with Special Reference to Road Design ", Journal Inst. C.E., vol. 15 (1940–41), p. 307 (Feb. 1941).

assumption in formula (10) that a perfect curve existed between the extremities of the sight line, which in that case was not so.

In example No. 3, p. 37, *ante*, it would be found that the sight line passed through the point of intersection of the gradients and therefore, so far as sight line requirements were concerned, no curve was required. Mr. Price had obtained a curve of length 250 feet, again emphasizing the incorrectness of using formula (10) when the sight line was longer than the length of the curve.

Naturally the two gradients could not be allowed to intersect, and in that example it was really the vertical acceleration which was the ruling factor. Therefore, if the speed were taken as 60 miles per hour and the maximum permissible vertical acceleration as 1·5 feet per second per second, the length of the required curve would be 154·88 feet (say 160 feet).

Mr. H. A. Warren observed that Mr. Leeming, by his clear exposition of the effect of camber, by advocating a constant "hands-off" speed, and by taking into account the net lateral force in place of the gross centrifugal force, had contributed much to the science of road engineering. His greatest contribution, however, was his insistence upon superelevation and its effects, as the criterion of design, thus dispelling the quaint notion previously held that there was some value of rate of gain of horizontal curvature which could not be exceeded with impunity.

In his "limitation of the rate of increase of lateral force (p. 16)" he had, however, had to use an arbitrary value based on Shortt's $C = 1$. That was a serious defect, since much higher values of C had been confirmed by experiment[1], and those, when used with the new theory, gave shorter transition lengths, so that other effects such as vertical acceleration, became predominant. Even accepting the values based on $C = 1$ as set out on p. 19, *ante*, it would be seen that all was not well. Referring to the length of 51 feet and superelevation of 40 inches, for the 5,000-foot radius curve, Mr. Leeming had stated that it "*may* have reached the limit at which the rate of application of superelevation *may* require to be considered." To gain 40 inches of superelevation in a distance of 51 feet at 70 miles per hour by a ∽-shaped vertical curve involved a minimum vertical acceleration of 54·2 feet per second per second. Mr. Price, in his Paper, had given 0·5 foot per second per second as the desirable value for trunk roads. As an opposite case, taking the value of 757 feet for the 1,200-foot curve, a time of 7·4 seconds was thus allowed for the turning of the steering-wheel through 2·25 degrees. If the motorist turned the "full" 2·25 degrees in the first 0·5 second, as would usually be the case, the full centrifugal acceleration would be developed about 700 feet in advance of the full superelevation, and Mr. Leeming's careful balancing of the two effects would be entirely upset.

Mr. Warren wished to enter a protest against the inaccurate repre-

[1] Bibliography (p. 26, *ante*), reference 12.

sentation of his own theory. In the sixth line on p. 25, *ante*, the word "vertical" should be "horizontal"; or else the sentence was completely incorrect. That theory did not fail by neglecting one of the forces, since the maximum vertical acceleration occurred only at the beginning and end of the transition, where the centrifugal effects were respectively nil, and opposed to the full vertical acceleration. The approximate value of $C = \frac{v^2}{100}$ was founded upon experiment rather than theory, but since Mr. Warren's Paper [1] set out to discredit C as a basis of design, it seemed purposeless to criticize a value which in fact was not used. C became predominant only at low speeds where "slip" did not occur.

Mr. Leeming, in reply, observed that he would deal firstly with the general principle, in which the whole basis of the theory had been attacked; and secondly with detail, in which matters arising out of the theory had been discussed.

He did not agree with Mr. Robb that it followed from his theory that superelevation and radial acceleration were interdependent quantities. The correct length of transition was a function of the two independent quantities (*a*) rate of gain of radial acceleration, and (*b*) superelevation. It could, therefore, be said to depend on both of them, but that did not imply that they were dependent on each other. Mr. Robb had been at great pains to show that radial acceleration and superelevation were independent; but Mr. Leeming had never attempted to suggest that that was not the case. What he had said was that the superelevation altered the effect of the radial acceleration on the car and its occupants. At the hands-off speed the lateral effect was turned into an apparently vertical effect, and, in fact, experiment showed that "the occupants are riding round the curve in a state of perfect balance and complete comfort", as he had expressed it. Mr. Robb's tilting seats would remove the passengers' discomfort, but would have no effect upon the car's tendency to skid. To remove that, superelevation was needed, and since his seats were not standard fitments on cars, it was needed for the passengers' comfort also.

In his description of "the fault in the Author's chain of reasoning" Mr. Robb seemed to have misunderstood the notation given, and his argument was thereby invalidated. He had stated that "F_p is that part of it (the centrifugal force) which had not been resolved normal to the plane", whereas it was the sum of the components of W and F_c parallel to the plane. It certainly was not an independent force, as Mr. Robb seemed to imply in his statement that "from the forces F_p and F_c, he has selected F_p and neglected F_c." Surely, to resolve a force was not to neglect it. Mr. Robb had himself ignored W throughout his argument, by ignoring its lateral component. He seemed to accept without question Professor Royal-Dawson's statement that the sole object of transition was to prevent

[1] *Ibid.* reference 9.

the sudden application of centrifugal force F_c. One of the Author's main purposes had been to show that the real object of transition was to avoid the sudden application of lateral force F_p. It was F_p which caused discomfort to the passengers, or caused the car to skid, whereas the driver and passengers could not detect the centrifugal force, as such, by any sensation of discomfort. If it were possible to do so in a car, it would also be possible in an aircraft, and the provision of blind flying instruments would be unnecessary. It might be mentioned that in the same book from which Mr. Robb had read[4], Professor Royal-Dawson had quoted with emphatic approval (p. 36) the words of Mr. Shortt cited by Mr. Leeming on p. 23, *ante*. That clearly implied that a curve which was superelevated was less in need of easing—that it could be given a shorter transition for the same standard of comfort or safety.

In reply to Professor Royal-Dawson, Mr. Leeming considered that his assumption (*a*) that Shortt's equation did not take account of superelevation was obvious from an inspection of the equation. Professor Royal-Dawson himself had often made the same assumption, as Mr. Robb's quotation showed. He now seemed to assert that C was in some way dependent on superelevation and also on P. That was very surprising, in view of all the work he had published. As regards point (*b*) both Shortt and Professor Royal-Dawson defined C as the rate of change of radial acceleration, whilst the Author had defined it as the rate of change of lateral acceleration. In either case it was only remotely connected with the pressure component P, which was rigorously perpendicular to the lateral acceleration, and substantially perpendicular to the radial acceleration. Professor Royal-Dawson's statement that "the lateral component must have played a comparatively small part" would not bear detailed examination, even supposing that Shortt had carried out his experiments on superelevated track. Choosing a typical case, that given as case (ii) on p. 29 of his book, the following results were obtained: change in F_p (his P_T), 0·037 W; change in P 0·009 W, or about one-quarter of the change in F_p. However, Mr. Taylor Thompson's statement, from personal knowledge, that Shortt had carried out his experiments on an uncanted track, disposed of the matter completely. Professor Royal-Dawson's statement that equation (8) did not allow for superelevation could be seen to be untrue by inspecting the equation, and his statement that the transition lengths given by Mr. Leeming did not comply "with any particular definition of C" was also untrue; they complied with Mr. Leeming's definition of C.

Professor Royal-Dawson's statement that the introduction of camber was a side issue was unconvincing. Even granted that camber was a defect in road design, it was still present, and had to be taken into account. Mr. Leeming confessed that he had much sympathy with the view expressed by Messrs. Clayton and Wood, that a slight camber was good, in that it provided a small lateral force which took up any backlash in the steering gear. Professor Royal-Dawson's proposal of long reverse curves would not solve the problem of camber, because it would introduce superelevated right-hand curves of long radius. The undesirability of those, and also the necessity for the elimination of camber on the transition curve, were matters of fact, as well as of theoretical reasoning, obtained by " intelligent and systematic observation of actual traffic movements on the open road", examples of which had been given by Mr. Leeming in his introductory remarks.

As regards Professor Royal-Dawson's criticism of *Fig. 11*, it was clearly stated that that figure was drawn for the case in which the tangent distance was restricted, for convenience in reproduction, but an exactly similar argument applied to the case in which the apex distance was restricted.

Both Mr. Platts and Mr. Orchard seemed to think that there was some virtue in the centrifugal ratio. The former had called it a " dynamic effect " ; but on a canted road, or aeroplane, it was a purely imaginary quantity, one of two forces acting and contributing to the reaction, and it was impossible to detect it by any direct and positive means.

Mr. Platts had suggested that the equivalent cross-fall F_p/W should be used, rather than F_p. That was merely a matter of convenience and did not affect the theory. Whilst it was true that the magnitude of that ratio was the important criterion in most cases, Mr. Leeming adhered to his opinion that in certain cases, which he had described in the Paper, and had mentioned in his opening remarks, the change of sign was of great importance. Mr. Platts' statement that the superelevation should be correct for vehicles of the highest speed involved sacrificing the comfort of the many, who drove at normal speeds, to that of the occasional high-speed driver, who would then drive faster still, and ask for more superelevation. Moreover, the provision of excessive superelevation for normal traffic would tend to make vehicles swing out to the middle of the road, which would be more upsetting to high-speed traffic than a lesser amount of superelevation.

In that connexion the very interesting and thoughtful contribution by Mr. Clayton merited more careful consideration than space allowed. The figure of half of the maximum speed, mentioned by him, had been merely taken by Mr. Leeming as an example, to provide figures for working out equation (11). At the same time, he thought that that figure would possibly give a better result than Mr. Clayton's suggestion of $1/\sqrt{2}$. As a general rule for right-hand curves, there could be little doubt that the principle laid down in the Paper was valid ; namely, that for those curves it was better to have too little, rather than too much, superelevation. As a corollary of that, however, there was much to be said for adopting a higher standard for superelevation speed on left-hand curves than on right-hand curves.

The effect of wrong cross-falls had been shown by the extremely interesting example given by Mr. Orchard. For a car going from A to B, the superelevation was removed, and possibly even made negative, just as the car entered the curve, thereby making it hard for the driver to keep it over to the left. If full data were available, it would be possible to plot the car curves for the various traffic lanes. Those curves might be of surprising shape, but they might reveal the causes of accidents, and what changes of level should be made to improve matters.

With regard to the length of transition, perhaps Mr. Leeming had not made himself clear. No advantage in comfort was gained by unduly long transitions—in fact the reverse—and it was wrong to increase the complexity of setting out unless some advantage was to be gained.

In reply to Mr. Keep, Mr. Leeming had not proposed the car curve for the purpose of setting out, and he did not claim any great practical use for it. It was intended more as a method of thought. The whole of the current literature on the subject concentrated entirely on the curve in plan, without any reference to superelevation. In that connexion the context of Mr. Robb's quotation from Royal-Dawson was very relevant. The whole essence of the Paper was that that was wrong, and the car curve was intended to counteract that mental attitude. It was probable that if the designer of the junction illustrated in Mr. Orchard's example had been thinking in terms of the car curve, he would have laid it out quite differently.

In reply to Mr. Manton, the friction force exerted by the road was F_p; Mr. Leeming thought he might fairly claim that he had effectively taken friction into account by concentrating on that force.

Mr. Warren's example was based on a misprint in the Table. The superelevation should have read 1 in 40, not 40 inches, so that the value for vertical acceleration worked out by Mr. Warren would not apply. He wished to apologise to Mr. Warren for the misstatement of his theory.

Several speakers had suggested that practical experiments were needed to determine the values of F_p and C which normal traffic could bear. Mr. Leeming was engaged on such experiments, as far as present conditions allowed. The Paper was intended to clear the ground for an account of those experiments. So far as he was aware, much of the experimental work which had been done on that subject, for example, that of Messrs. Warren and Orchard, involved measurement of the path of the vehicle, and obtaining F_p (or F_c) and C by differentiating twice and thrice respectively. That was a very inaccurate process. He felt that discussion of those points was better deferred until his experimental results were available. That also applied to the very interesting contribution of Mr. Fox. When it was possible to form a clearer idea of the general requirements in terms of F_p and C, it might be found that only very short transitions on the lines of Mr. Warren's theory (or Mr. Fox's modification of it) would be required. But he was inclined to the belief that in the great majority of cases the transition length would be determined by C, rather than by the distance needed for the application of superelevation.

In reply to Mr. Taylor-Thompson, who had doubted the existence of the hands-off speed, he could say that he had found by experiment that it did exist, and that had been confirmed by others. The car did, in fact, steer itself round the curve, and he had not found any instability under those conditions, as one or two other speakers had thought likely.

Mr. Price, in reply, observed that he would deal first with the points more generally raised in the discussion. In the first place, the formulas had been given in the form in which he used them himself, and a reader could adapt them to suit his own convenience should he prefer to do so. The constants had been described as being awkward in use, but that had not been found in practice, as most of the calculations could be performed with sufficient accuracy with a slide rule.

The constant C was actually the inverse of the radius of curvature of the circular curve of equal length, but the curve was parabolic in shape. Actually the two types of vertical curve differed but slightly, but the parabola was more perfectly transitioned and better adapted for calculating the levels.

The distance of mutual visibility for single carriageway roads was based upon the assumption that a complete blockage of the road was mutually visible to the approaching motorists, and they would be able to stop before it was reached. Hence the distance of mutual visibility was twice the braking distance. For dual-carriageway roads only one vehicle had to be considered, and hence the distance of mutual visibility was half that for single-carriageway roads. The Author believed that that erred on the safe side, which was advisable when account was taken of the psychology of certain types of road-user.

The braking distance formula of Mr. Roussel A. Powell as given in his Paper[1] was

$$b = \frac{0{\cdot}0501 V^2}{m \pm 0{\cdot}01p} + \frac{11}{15}V,$$

where b denoted the braking distance, in feet;

m was the coefficient of friction between the tires and the road;

p denoted the percentage gradient of the road; and

v the speed of the vehicle, in miles per hour.

Mr. Powell had taken $\frac{1}{2}$ second as the "reaction-time" giving the last expression in the formula, and $m = 0{\cdot}8$.

Mr. Price had modified the equation slightly by neglecting the effect of the gradient, which was virtually cancelled out when two vehicles approached on the same gradient, reducing m to $0{\cdot}75$ and taking a "reaction-time" of 1 second, thus giving the modified version as

$$b = \frac{0{\cdot}0501 V^2}{m} + \frac{22}{15}V,$$

which reduced finally to

$$b = 0{\cdot}067 V^2 + \frac{22}{15}V.$$

[1] *Loc. cit.*

For a speed of 50 miles per hour the braking distance was approximately 250 feet. However, on the German State roads speeds of 105 miles per hour had been attained and maintained, and hence in designing modern roads it would be advisable to allow for speeds of up to 150 miles per hour, for which speed the braking distance was nearly 1,750 feet by the above formula.

In calculating the length of curve required an odd figure was usually obtained. That was never adopted in practice and the length of curve was extended to fit in with the unit distances adopted in setting out the level pegs.

Mr. Price had considered the cubic parabola which could be obtained by integrating from the third differential, but the results obtained so far did not fall readily into formulas connecting the various properties of the curve. He hoped to obtain some simplification after further work, and would present a further Paper should his work come to fruition.

The formula for the mid-ordinate, which had been unfortunately omitted from the Paper, was:

$$y_m = \pm \frac{L(N_1 - N_0)}{800} + y_0,$$

or

$$y_m = \pm \frac{(N_1 - N_0)}{80{,}000C} + y_0$$

where y_m denoted the level, in feet, at the mid-point of the curve.

Mr. Robb had compared the lengths of the curve obtained by Mr. Criswell's latest method and by Mr. Price, and had stated that for a 1·50-per cent. change of gradient a curve-length of 234 feet was obtained by the former with $f = 0{\cdot}7744$ foot per second per second. If those values were used with Mr. Price's formulas an identical result was obtained, as C now became

$$0{\cdot}0000826 \times 0{\cdot}7744 = 0{\cdot}0000637.$$

Dr. Beaufoy had indicated an error in equation II, which should read

$$C = \frac{N_1 - N_0}{100L}$$

since $N_1 - N_0$ was always negative for summits, giving rise to the confusion in signs.

The approximation of L and $x_1 - x_0$ still held good when consideration was taken of the fact that when a curve was set out the chainages were measured around its perimeter in nearly all cases.

Mr. Smith seemed to have missed the implication of C which, for any given value, stipulated a minimum distance of mutual visibility. Hence if the curve were shorter than that distance, the distance of mutual visibility exceeded that corresponding to the value of C, which was an error on the right side.

Mr. Price had not intended to lay down any hard and fast rules for vertical curve design, but had dealt only with the problem in general; in particular, the examples given were intended only to illustrate the use of the formulas and did not stipulate any conditions to be conformed with for a given class of road. He would welcome any criticisms or suggestions as a means of developing the further use of similar methods of design.