Discussion.

Mr. Wells Owen. Mr. G. WELLS OWEN was reminded by the Paper of the story of the man who was put in the stocks, and whose friends kept on saying, "But they cannot do it." Whilst his oft-repeated cry was, "But they did do it." Theoretically, according to the Paper, every six-wheeled engine ought to run off the rails; but the fact remained that they did not do so. There were very many circumstances in practice which prevented machinery and materials acting in the way in which they were theoretically supposed to do. He did not intend to enter into the mathematical part of the question, but merely to say that he thought it would be interesting if those who took part in the discussion would enter as much as possible into the causes which prevented engines acting as they should do according to the theory set out by the Author.

Mr. Walter Browne. Mr. WALTER R. BROWNE said the Author had stated that "The tendency which a wheel has to mount the rail on a curve is evidently caused by the adhesion or friction between the rail and the flange of the wheel." He was taught many years ago at Cambridge that whenever one saw the words, "it is evident," or, "it is easily seen," in a proof, particular attention should be paid to that sentence, because one might be pretty sure that there lay the difficulty of the whole; and the sentence he had just read seemed to him no exception to the rule. Generally when a man said, "It is evident," what he really meant was, that it was not quite evident to his own mind, but he hoped it would be so to the minds of other people. He did not at all say that was the case with the Author; but he wished, for the sake of those who had not studied the subject so deeply, that he would, in his reply, explain the grounds on which he asserted that the tendency to mount the rail was caused by adhesion or friction between the rail and the flange of the wheel. To him it was difficult to see how friction could cause any such mounting. Friction tended to prevent action in almost all cases, and it certainly seemed a paradox to state that it tended to produce that particular action. Further on, the Author stated that if there was no friction, an engine would never run off the rails. The reverse of that appeared to be, he would not say evidently, but demonstrably true. Taking any one of the tire-sections shown by the Author, and supposing that there was absolutely no friction whatever between the wheel and the rail, then, if the wheel ran round a curve, it followed that the

centrifugal force, as it was generally called, acting outwards on the wheel, would have a component at right angles to the rail, and that component would have another along the tangent between the wheel and the rail-head; and as there was no friction, by hypothesis, to stop it, it was evident that the wheel would move outwards upon the rail-head (he assumed that there was no weight to keep it down), and since the flange was nowhere vertical, would continue to rise until it mounted completely over the rail. He could see no possible way out of that difficulty; therefore, if there was no friction, an engine would always run off the rails at a curve; and therefore it was at least difficult to see how the existence of friction could tend to make an engine mount the rails. There was a sentence (p. 5) which might possibly be intended for the Author's explanation of his meaning: "The point of contact between the outer leading-wheel flange and the rail is in advance of a perpendicular from the wheel's axis, so that the motion of the wheel at that point is downward, imparting a downward pressure to the rail, and an upward pressure to the wheel. Thus, when the adhesion between the flange and the rail is greater than the weight upon the wheel, the flange would rise and mount the rail." That did not appear to him to be happily expressed, but he presumed that what the Author meant was, that the flange being in contact with the rail, there was a tendency for the wheel to turn, not about the point of contact between the tread and the rail, but about the point of contact between the flange and the rail. It was not that the flange itself would rise at this point—which seemed to him impossible; but that the wheel itself would rise, as a whole, over that point, and in so doing would mount to that extent upon the rail. But if it was considered that the whole weight of the car was resting on the tread, and not on the flange, and that if that action occurred the tread must leave the rail altogether, it seemed impossible to suppose that this tendency, whatever it might be, had in practice any real effect.

There was, however, a cause for the derailment of engines which the Author seemed to have ignored, and which appeared to be one of those practical points to which Mr. Owen had called attention. He had often watched an express train going at full speed through a station-yard where there were many crossing points, and many things to make the line uneven; and had seen the wheels lunging and swinging and banging from side to side as they crossed the points, so that the engine seemed at one time coming straight towards him, and at another going 50 feet to the right or the left of him. That was enough to show how an engine might leave

Mr. Walter
Browne.

Mr. Walter Browne. the line, independently of any question of friction. Whenever there was any inequality in the side of the rail, which the flange of the wheel struck, it gave the whole engine an impulse inwards; it then struck against the opposite rail and flew back again, and that oscillation went on for some little time. If this lateral oscillation was combined with a vertical jolt, as for instance if one rail at a joint was lower than the other, then derailment was certain to occur, and he contended that derailment was in all cases due to some such cause as this, and not to friction at all. That effect seemed to be fully recognised by practical men, as, indeed, might be expected. It was stated, with regard to the particular instance that the Author had chosen as the basis of his elaborate calculations as to the exact amount of adhesion necessary to make an engine mount, that the inspector reported: "The road did not gauge very evenly . . . being in some places as much as $\frac{3}{16}$ inch slack, and in others exact to gauge." The inspector went on to say, "I am of opinion that this accident was due to the want of a proper check-rail at this curve of under 10 chains radius, and to the somewhat slack state of the permanent way." It was obvious that if the gauge differed so much within so short a distance, that in itself constituted a considerable obstacle tending to throw the engine sideways; and it was a cause nearly independent of the speed at which the engine was going, accounting therefore for cases of derailment at low speed, which the Author thought it so difficult to explain, because the centrifugal force was not sufficient.

He would conclude by drawing the attention to a discussion that had recently taken place in America upon an important part of the question which the Author had hardly touched upon, namely, the question of coning the wheel tread. All the sections shown were coned, and he supposed that every wheel running on an English railway was coned: nevertheless the question whether it was a right thing to do could not be considered as altogether settled. Mr. Crampton, a high authority, held that the coning of wheels was a mistake. Mr. Haswell, at Vienna, had the whole of his rolling-stock fitted with cylindrical wheels. In America a very large number of instances were quoted in which cylindrical wheels had worked perfectly well, and even in which the taking off of the cone and turning down the tire to a cylinder had proved beneficial. The Master Car-builders' Association had discussed the question, and the discussion was contained in their last report. He might be permitted to read a few words that had fallen from a gentleman who had paid as much attention to railways as almost any one—Mr. M. N.

Forney, of New York. "Any one who will undertake to look into this subject and examine it analytically will find that it is one of the most complicated things relating to railroad construction. I have made more graphical diagrams, and more drawings of sections, and tangents, and rails, and various other things, in order to get the matter clearly before my own mind, than, I presume, any of the members here would suppose it was possible to devote to so simple a subject. The precise relation which the flange bears to the rail, in going round a curve, is a thing which it is extremely difficult to understand clearly in your own mind." With those words he fully agreed, and he therefore hailed the Paper under discussion as showing the subject was attracting attention in England; but he hoped the Author would give it still further consideration, and then perhaps he might see cause to modify some of the conclusions at which he had arrived.

Mr. J. W. GROVER said there was a sentence in Appendix I. to the Paper which seemed to call for special attention. It was stated in the Board of Trade Returns for 1880 that more deaths had resulted from passenger trains, or parts of passenger trains, leaving the rails than from any other cause. The Paper was very suggestive, but any one looking at the diagrams would see that there were many matters still unsolved which it was desirable to have explained. The whole subject was one of much importance, but it had been greatly neglected. About the year 1853 the Emperor Napoleon III. instituted a Commission for investigating the question, and there was an elaborate series of reports on it in the "Annales des Ponts et Chaussées," third series. Fourteen years ago he endeavoured to bring the reports before the Institution, but was unsuccessful. Perhaps the subject was not then ripe; it was certainly more ripe for discussion at the present time than it was then. With the increased size of rolling-stock and greater length of wheel-base there had been a tendency to reduce the radii of the curves; consequently the two poles were approaching each other. The question was one which had not been thoroughly considered. There was an element of safety shown by the diagrams, and in all carriages and wagons which were used on railways, viz., the fact that the wheels were keyed on to the same axle. In 1871 he had an engagement in Woolwich Arsenal, and he made a series of experiments there, which were published in "Engineering," March 24th, 1871, showing the remarkable manner in which the derailment of a truck was facilitated by the unkeying of the wheel upon the axle. Why that should be it was difficult to say, but the simple fact remained, and it was one

Mr. Walter Browne.

Mr. Grover.

Mr. Grover. which it was important to bear in mind, that while two wheels were firmly keyed upon one axle there seemed to be some law which prevented the one from mounting and climbing over the outside rail in passing round a curve. He found it was possible to keep the trucks upon the line by making the axles radiate, if he made the wheels loose; but he did not find it possible to keep them on at all without radiation if he gave the outer wheel the independent motion which it had by unkeying it off the axle. He thought it could be explained thus: that the moment the outer wheel began to mount the rail it travelled on a much larger circumference than the inner one. There was a partnership between the two wheels so long as they were keyed on the same axle; and the inside partner, on the smaller diameter, objected strongly to the outer partner travelling faster than itself; consequently it gave a retarding movement through the axle, and prevented the outer wheel from mounting the rail. He thought that element more than anything else contrived to keep the carriages on the line. But, going carefully through the Paper, a difficulty was apparent which seemed almost insurmountable. There was, for example, the fact that an engine did go off on a 640-foot curve; but then, again, there was the still more startling fact, which might be assumed, although it was not stated, that the engine during a great part of its life must have been constantly going round curves in station yards of very much smaller radius than that of the curve where it ultimately went off. Of course the answer was that it did go off at that particular curve; but it was astonishing that it should not have gone off on the sharpest curve that it went round, where the tendency to derailment must have been greater. With regard to the coefficient of friction which had been assumed in the Paper, he had looked at Rennie's experiments in the "Philosophical Transactions,"¹ and found that he gave a coefficient as high as 0.409, with 560 lbs. pressure to the square inch; and with a less pressure the coefficient was less. He therefore thought that the Author, in assuming that 0.37 was possible and practicable, was certainly not exceeding the bounds of possibility. With regard to the fact of the inner wheel skidding if the outer wheel passed round a curve, there was no doubt that to a large extent there was in that an element of friction. He had examined the matter mathematically, and found that as the wheel-base was to the gauge so

¹ "Experiments on the Friction and Abrasion of the Surfaces of Solids," by George Rennie, Phil. Trans. 1829, p. 143.

was the resistance caused by the friction of the flange being at Mr. Grover. the angle to that caused by the skidding of the inner wheel. Thus, with a wheel-base of 20 feet, and a gauge of about 4 feet $8\frac{1}{2}$ inches, there would practically be four times as much tractional resistance from the flange friction as from the skidding of the tire inside. Therefore, for practical purposes, that skidding of the wheel might be neglected. The serious and vital thing was the tendency which the wheel had, in consequence of its obliquity, in passing round a curve, to run off. There was a remark in the Paper "That the danger increases when the radius of the curve decreases." "The danger of leaving the rails diminishes when the length of wheel-base increases in proportion to the width of gauge; but when the length of wheel-base increases in proportion to the radius of the curve, the danger increases; so that there is a certain length of wheel-base giving a minimum of danger: but, within the limits of practice, the longer the wheel-base the less the danger of leaving the rails." He thought that the Author's formula and the whole argument in the Paper were directly contrary to that experience. The wheel-base was a chord drawn across any point of a curve, and the larger the wheel-base the greater was the versed-sine, and the greater was the angle made by the exterior wheel in passing round the rail. Considering the wheel-base as equal to twice the radius, the condition then was of a wheel standing square to the rail, and having a tendency to go straight over. Now conceiving the wheel-base to be infinitely small, then the wheel would be perfectly plane and parallel to the rail. Between those limits there was, no doubt, a proportion beyond which it was evident that, with an increased wheel-base, the danger of running off the curve was increased. The Author had clearly shown that as the wheel-base was increased, so also was the tractional resistance round the curve. It was absurd to say that the danger was not at the same time increased. It was well known to practical men that with sharp curves the rigid wheel-base must be reduced, while with good curves a long flat wheel-base was possible. He desired to call attention to the great danger of the present practice of enormously increasing the rigid wheel-base under railway carriages. In the reports of railway accidents it would be found that there were an immense number of cases in which carriages had gone off at railway stations and at crossings. The reason was that those long carriages, many of them with 22-foot wheel-base, going round curves of 300 or 400-foot radius, had a great tendency to mount the outer rail. The wheel ground the outer rail until

Mr. Grover. it got hold of something, and the check-rail, being a little slack, allowed it to do so, and off it went. There was a place at Leeds where two trains went off one week after the other. It was a question demanding serious consideration, whether by radial axles a long safe wheel-base could be had. It was certainly worth some effort to obtain a radial axle. He had lately seen a terrible accident at Birkenhead on a tram-road, resulting from a fixed wheel-base and a rigid axle. Such axles could not relieve themselves, or get adjusted to the curves; they had to grind round the curves as best they could. At the place to which he referred there was a somewhat steep gradient, and at the bottom a sharp curve, and in going round that curve the car ran off, fell over, and sent the passengers on to the pavement, the result being that twenty or thirty persons were killed or wounded. A similar accident shortly afterwards took place at Blackburn. Between thirty and forty persons were wounded and two killed from similar causes within three weeks. He thought that public attention ought to be directed to the subject.

Mr. Langley. Mr. A. A. LANGLEY agreed that a long wheel-base increased the difficulty of an engine getting round curves. A short time ago some ten-wheeled engines were put on the Great Eastern Railway. They went round sharp curves very well so long as the permanent way was old; but when the permanent way was relaid with new and heavier materials, and the sleepers were firmly bedded in the ballast, the engines went off the line. The first time this occurred he thought there had been some neglect on the part of the man in charge of the relaying, but he found that the road had been relaid properly, with the super-elevation of the outside rail, and with the extra width of gauge for the curve. On investigating one case, it appeared that one of these engines flattened the curve in its progress, making a 5-chain curve into a 7-chain curve along its wheel-base; the sleepers moved out of their place backwards and forwards as much as $\frac{1}{2}$ inch in the ballast. When, however, the sleepers were firmly secured, and the 5-chain curve was maintained, the engine mounted the rails. This kind of action he believed to be going on in many cases where least expected. He always keyed the rails outside, as he was of opinion that this plan tended to keep the engines on the line. He had lately been taken over a line of railway which had the same description of rails, chairs, and other materials throughout its length; but portions of it were keyed outside, and portions inside. Any one travelling on this line could tell which portions were keyed inside, as the metal to metal on the outside caused a rigidity that did not exist with

the outside keying. An objection had been made to the outside Mr. Langley. keying, that a number of keys might drop out, and so liberate the rail and cause an accident; but he kept the ballast boxed up on the outside, nearly to the level of the rails. The keys did not then shrink and expand so much with dryness and wet, and kept in their place better. In the 4-foot way, on the contrary, he kept the ballast down to the level of the top of the sleepers, so that the fastenings and condition of the sleepers could be seen, and the sand and fine particles of ballast were not then blown so much into the working parts of passing trains.

Mr. W. STROUDLEY considered the Author had done good service Mr. Stroudley. in drawing the attention of railway men to the risks incurred in using parallel axles in engines passing round sharp curves. The subject was at present only imperfectly understood. It would, however, have been better if the Author had been able to show the condition of the roads and the forms of the curves, as the wear of the wheel and rail were greatly dependent upon each other. If the curves on railways were so laid out that the clearance allowed between the wheels and the rails was taken up, by a slight divergence of the rail from a right line before entering upon the curve proper, the engine would pass round it without shock or oscillation. Curves were frequently made as parts of circles, and an engine, owing to the clearance between the wheels and the rails, would run forward from a straight line into a curve for a considerable distance before it would take the outer rail, and consequently would strike it with great force, causing oscillation and some risk of leaving the rails. He had found by experience that when a curve was so arranged that the wheel was brought gradually up to the outer or guiding rail, and the whole of the slack and elasticity of the bearings, frames, &c., were taken up, the train passed over it at high speed without oscillation or disturbance, provided the super-elevation of the outer rail was not too great. It had been the practice to elevate the outer rail much in excess of what was either good or safe. He believed that if the outer rail was kept somewhat above the level, say 2 inches, it was sufficient; he had known cases of as much as $7\frac{1}{2}$ inches super-elevation. With regard to the wheel-base of the engine, he thought, with the Author, that an extension of its length would make an engine safer to travel round a curve than with a short one, it being understood that the short wheel-base was such as would be represented by the bogie-wheels of the engine or carriage, or the distance between the leading and driving wheels as meant by the Author. The Author had stated that when the

Mr. Stroudley. inner driving-wheel flange was allowed to press against the inner rail, the trailing end of the engine acted as a lever, and forced the leading flange against the outer rail, with a greater amount of force than if the flange was taken off the driving wheel altogether. In practice Mr. Stroudley had cut away the flanges of the driving wheels, until they did not touch the rail. It would be a mistake to cut away the flange altogether from the driving wheels, because if the engine was de-railed it would be liable to turn round across the road, as the four-wheeled engines, and the engines made years ago without driving-wheel flanges, were found to do. With regard to the force exercised by the wheel on the inner side of the curve, urging forward that side of the engine, and so twisting it in a direction opposite to that of the curve, one thing had been lost sight of, namely, that the greater the amount of super-elevation of the outer rail, the greater the weight would be on the inner wheel; and the greater the amount of weight on the inner wheel, the greater the liability of the engine to leave the outer rail. That was his reason for recommending a moderate amount of elevation of the outer rail. He considered it safest for an engine to have a high centre of gravity, as in that case it would run round a curve with safety at great speed, because the centrifugal force threw over the engine, and put the greatest amount of weight on the outer wheels; in fact, the engine would run round a properly arranged curve at a high speed, with even less liability to leave the rails than at a low speed. He considered it an important matter, with a view to safety, to have the heaviest weight on the leading wheels. The case selected by the Author was somewhat extraordinary, the curve being of less radius than usual, and the engine much lighter, and it had at the trailing end a greater weight than at the leading end. It was not, therefore, a fair example of ordinary practice. He thought the form of flange, also, was well worth considering. For many years flanges were made with semicircular edges. He had found from experience that switches, supposed to be closed, were sometimes not quite close to the rail; and if the wheel had a semicircular edge, being pretty nearly square to the end of the partly-open switch, it would mount upon the point of the switch. If the flange was made with a more pointed form, as in the London, Brighton, and South Coast Railway Company's section, that would close the switch instead of mounting it. Since he had altered the form of flange, fewer engines had mounted the rails during shunting operations, and the same thing that was useful in shunting operations would give additional safety to an engine travelling at

high speed. He thought the Paper would prove useful in drawing attention to a matter which had not been sufficiently considered. He believed that the highest speed could be obtained by the greatest amount of simplicity, and that every engine should be designed to suit the peculiar character of the work which it had to do. Where there were curves of 5-, 6-, or 7-chains radius to be worked regularly, special machines should be devised for them; but with curves of 20- or 30-chains radius, ordinary six-wheeled engines, with a rigid wheel-base of a moderate amount, say 16 feet, would suffice. He had known an engine run 110,000 miles with one set of wheels, with a rigid wheel-base of 15 feet 6 inches, without the wheels being removed to have the tires turned; the engine weighed 40 tons. When wheels on parallel axles would do work in that manner, he thought they could not cause much damage to the road, and there could not be much danger in employing such an engine.

Mr. J. TOMLINSON, Jun., said so few cases of derailment of Mr. Tomlinson's engines had occurred within his thirty years' experience, that he thought there could not be much wrong after all. Theoretically, perhaps the Author was right, but in practice the difficulties and dangers indicated were not manifested, and therefore he considered the practice better than the theory, and that it would be safe to continue the existing method, and not to take the possibilities of danger too much to heart. If the dangers were to be weighed too carefully, the high-speed services of the country could not be carried on. It seemed to him that the engine, quoted by the Author from the Board of Trade Returns as having left the road, was of an exceptional character for these days, and such as could hardly be found now on a main line of railway, it being, as appeared by the distribution of weight, on a centre pivot, and ready at the slightest provocation to leave the rails, however good its working state might have been. The Metropolitan Railway, with which he had been connected for the last ten and a half years, was, he believed, one of the most difficult to work of any, so far as wheels and rails were concerned, owing to the frequency of curves of 10-chains radius often running into each other. But during the time he had been engaged upon it—and he believed since it was opened in 1863—no case of derailment of an engine had occurred which could be charged to the difficulties and dangers of which the Paper treated. In shunting, or in working through facing-points of some cross-over-road seldom used, this had occurred. The wear of wheels had been solely one of flanges, and scarcely, if at all, of the treads; but

Mr. Tomlinson. how this was to be overcome he could not say. It could not be solved by super-elevation, as, owing to the varying speeds the engines were by circumstances compelled to be worked at, no super-elevation, such as supplied by theory, would be available. It was of daily occurrence to have to stop the trains, or to run them slowly, when by rights the greatest speed should be attained; and thus, if stopped, great difficulty and loss of time would take place in starting again. He had therefore laid down on all curves of 10-chains radius and under, a check rail, and reduced the super-elevation to 2 or $2\frac{1}{2}$ inches, which he considered safer than attempting to regulate the amount of super-elevation. In practice with a long wheel-based stock, when working at a low speed, the tendency of the trailing wheel was always to hug the low rail almost as much as the leading wheels did the high rail, and hence to mount the low rail. On the Metropolitan Railway, curves of 6-chains radius were daily passed over by hundreds of passenger-trains, with perfect safety, at speeds of 15 miles per hour. He agreed with Mr. Stroudley, that it was a good plan not to make the nose of the flanges too bluff; and also with Mr. Langley, that frequently an engine did, when the road was loose, actually form its own curve; he had often observed the bending of the rails in such cases. He also agreed that it was better to key the rail on the outside, as it made the road easier to work round, and he had adopted this plan when he changed the permanent way from the Vignoles section of rail to the bull-headed section.

Mr. Macnair. Mr. A. H. MACNAIR said that the Author had stated in Appendix II.: "A carriage with four, six, or more wheels on parallel axles, does not absolutely require flanges on any except the leading wheels, which being thus guided, the other wheels will follow them, and, if wide enough, will keep on the rails. Thus, when there is clearance enough, or the curve is easy, the whole of the flange friction comes on the outer leading wheel." That showed the great advantage there was in using a bogie, instead of leading wheels on a parallel axle under the engine, because it shortened the wheel-base and so reduced the side-sliding. It also showed the still greater advantage attending radial axles, getting rid of the side-sliding altogether, and leaving no sliding except backwards and forwards, which was inseparable from the case of two wheels of equal diameter running over rails of different lengths. This might be corrected, of course, by coning, but he agreed that coning was probably a mistake. It was recommended as a means of helping the vehicle round a curve, but clearly it was not of much assistance to the leading wheels, and in the case of the

other wheels it would be of use if they were coned in an opposite direction; that, however, was impossible, because the cant of the rails could only be in one direction. Coning would be useful if the road were all straight, and it would be useful on curves also, if the axles were radial; but with roads not straight, and with parallel axles, he thought the wheels would be better without coning. The Author's reasoning, of course, applied to all vehicles with parallel axles. Every vehicle in a train was more or less influenced. The leading end of each vehicle was running outwards, but the trailing end of the one in front of it was running inwards. No advantage, or, at any rate, the least possible advantage, was taken of this fact in the usual mode of coupling. He could not help thinking that if the couplings were crossed, so that the action of each vehicle might help the other, it would be to the advantage of both in getting round a curve. In regard to the super-elevation of the rail, he thought that was generally left as an entirely practical matter. If either rail seemed to be too much rubbed, the super-elevation had to be raised or lowered accordingly. The Author had stated in Appendix II. that if the flanges were removed from all the wheels, the carriage would run forward at a tangent to the curve. This was so far true; but if the carriage were at liberty it would also have a rotary motion round an axis of its own. On a straight road there was no such rotary motion; it was generated by the curve. Therefore the severest strain and the greatest danger were just when this rotary motion was generated all at once. That was at the commencement of the curve. There was no way to meet this danger but by a compound curve, so as to generate the rotary motion gradually. As Mr. Stroudley had remarked, a good deal depended on the nature of the curves. Professor Rankine had described a method of laying out curves for this purpose; it was to make the circular curve a little easier near the tangent by having it a little tighter at some other place. He was not aware that this method was generally adopted. He thought engineers required something that could be set out with the theodolite at one operation, like circular curves by the tangential method, for which he believed they were indebted to Professor Rankine. He had long thought that, as a circle was set out by equal angles, some method might be invented to set out the curves required by unequal angles. He had found no other method than that of making the necessary calculations, and had never had an opportunity in practice, but he believed that much good might be done in this way.

Mr. W. W. BEAUMONT considered the members were indebted to Mr. Beaumont.

Mr. Beaumont. the Author for bringing forward an old subject in a new way and throwing a new light upon it. He was, he believed, the first to show in the way he had done the relation existing between the coefficient of adhesion and the pressure against the flanges of the leading wheels. In dealing with that question, and with the analogous one of the form of flanges, it had been usually assumed that velocity was a controlling element; and, in order to arrive at the force that had to be counteracted by the flanges, a maximum speed had been usually assumed. Wöhler had recently dwelt at some length upon the subject, and his calculations were based on that idea. The Author had been led chiefly to two points: first, that derailment was as likely at a low speed as at a high speed; and, secondly, that the danger of derailment did not necessarily increase in the inverse ratio of the radius of the curve. It appeared that the relation which he had shown to exist between adhesion and flange-friction was new to him, as thus stated: "In the case of the wheel most likely to mount the rail, namely the outer leading wheel, this side pressure, at low speeds, is principally caused by the resistance which the treads of the wheels oppose to the sliding motion which takes place in running round a curve;" an idea that he had developed in other parts of the Paper. Elsewhere he stated that "It will be assumed that, at moderate velocities, the flange-friction bears the same proportion to the pressure as does the adhesion of the tread, and that the two vary together when the state of the rails varies. This being so, with a double coefficient of friction the side pressure on the leading-wheel flange, required to make the other wheels slide, is doubled: at the same time the proportion which the flange's own adhesion bears to that side pressure is also doubled, so that the adhesion of the flange is fourfold." In applying that to the example he had given, he assumed a certain mean angle of flange surface; and in Fig. 4 he took that angle as 15° . Referring to the accident reported on by the Board of Trade officials, he arrived at the conclusion that it "might have been caused by the rails being in such a condition that the adhesion was 37 per cent. of the weight, assuming that it happened at a place where the gauge was sufficiently tight to allow the flange of the inner driving wheel to come into contact with the rail before that of the inner trailing wheel." Further on he increased the angle by 50 per cent. "The angle of the flange was just as likely to have been $22\frac{1}{2}^\circ$ as 15° ." And taking it at that, he said: "If the springs of the leading wheels had been very flexible, so that the flange would rise beyond the point of maximum stability without notice-

ably increasing the load, the flange would have mounted the rail when the adhesion increased to 30 per cent." It was thus clear that an increase of the flange-angle, for instance, to the greater angle more commonly used, would materially decrease the coefficient of adhesion necessary to bring about derailment; and if that angle, instead of being 15° or $22\frac{1}{2}^\circ$, was say 30° , it might be easily seen that derailment ought to take place much more frequently than it did: supposing the other conditions assumed by the Author to be also true. There were several points which might be questioned tending to alter the conclusions to which the Author had come, but only in degree, not in principle. In the flanges on the diagrams, the angle was generally more than the Author had assumed; and in one case, on the Great Eastern Railway, the angle was very large and very much rounded between the tread and the flange, and on the outside of the flange; and as far as he knew that railway had not always been remarkable for derailment, though it was so during the year 1882. There were far more accidents from unexplained derailment in America than in England. That could hardly be said to be on account of long wheel-base, or because the permanent way was always so bad, since the accident occurred on some railways that had very good permanent ways. It appeared quite possible that the velocity at which trains were running had more effect on their derailment than might be supposed from the Paper, especially in the case of the long and heavy cars used in America, the inertia-motion of which might push the bogies off the line when entering curves. That was to say that the heavy cars refused to change their course as rapidly as the light short wheel-based bogies. As to the question of coning, it was rather doubtful whether that also did not come into play in preventing slipping, thus making a discrepancy between the Author's conclusions as to the number of derailments there ought to be and the number there actually were. No doubt there was a certain amount of agreement between the different wheels and axles of an engine as they went round a curve, managing the business amongst themselves. It was not altogether a slipping forward of the outer wheels, or a slipping backward of the inner ones, but a little of each, and that might to a considerable extent upset the Author's calculations. As to the question of the best size for leading wheels, different locomotive engineers had different opinions, the larger wheel being held to be more likely to mount the rail under some conditions than the smaller wheel with the same form of flange. It was quite evident that the experience of Mr. Langley and of the Mr. Beaumont.

Mr. Beaumont. Author pointed to the necessity of using radial axles whenever it was possible. Perhaps a system had not yet been brought out suitable for engines, but no doubt there were very good systems for vehicles. It was certainly curious that vehicles should still be used with wheels fixed on parallel axles to run round small curves.

Mr. Woods. Mr. E. Woods, Vice-President, thought that practice was a safer guide than theory in matters of that kind. He should be sorry to base the design of an engine simply upon mathematical considerations as to the amount of adhesion between the wheel and the rail, or between the flange and the rail. He agreed with Mr. Stroudley that the construction of a railway had a good deal to do with the facility with which engines could pass round curves, but he thought the construction of the engine itself, in the sense of specially adapting it to traverse curves, was also a matter of importance. The difficulty was felt very early on the American lines, many of which were laid not only with sharp curves, but, like a contractor's railway, the rails not laid on chairs, but simply flat-footed rails spiked on to the sleepers, and at first hardly fish-jointed; consequently where curves occurred the tendency of the carriages and engine to go off the line was greater than it would be on the better-constructed railways of this country. The difficulty was remedied in America by the introduction of the bogie-truck. Almost all the early American engines rested on a four-wheeled bogie-truck at the leading end. At first the trucks were pivoted on a fixed centre; latterly they had a transverse motion, as in Adams's bogie-truck. The effect was to apply a steady lateral pressure to the leading end of the engine, and to cause the engine to traverse the curves in an easy and smooth manner. The introduction of that truck enabled the wheel-base to be considerably reduced, because even for a four-wheeled engine the flanges of the leading driving-wheels could be dispensed with, and so the wheel-base be reduced from 10, or 12, or 14 feet to not more than from 6 to 8 feet. He had for many years worked with engines of that kind round curves of 5- and even 4-chains radius. Most of the lines on the south coast of America were laid on that principle, and were worked with engines of the class he had mentioned; and he was not aware that any accident had happened from derailment except in cases of very steep inclines, where a train had got loose and was running at a speed of 50 or 60 miles an hour. The ordinary speed was from 25 to 30 miles an hour. The subject was an interesting one, and he hoped that more

attention would be given to it than had hitherto been bestowed Mr. Woods. on it.

Mr. H. S. RIDINGS said that in the case of lines with sharp Mr. Ridings. curves combined with steep gradients, as on the west coast of South America, several elements of danger had not been touched upon. Sometimes, when there was a tendency, the rails being wet, for the train to break loose, the brakemen skidded the wheels, in which case the engine or wagon would mount the rail much more readily than before. He well remembered one accident of that kind of which the results were very deplorable. The locomotive was one of Fairlie's double-bogie engines, which if properly treated would have easily gone round the curves. The train was a long one, and it was too heavy, the rails were wet, and it was night time. The brakeman was, as usual, on the opposite side to the engine-driver, and was not under his control. The driver, perceiving that he could not hold the train tried to reverse the engine, while the brakeman was putting his brake hard on and skidding the wheels. The engine went off against the side of a cliff, the boiler burst, and there was a great loss of life. Reversing an engine was a source of danger, because the wheels would mount the rails more readily. Sharp curves on lines such as those to which he had referred, where there were reverse curves of 350-feet radius, on gradients of 1 in 25, were sometimes a source of safety. Occasionally, owing to the small number of engines, it was found necessary to bring down a very long train, and the effect of the sharp curves had been to retard it and prevent it from breaking loose. That, he believed, had often prevented terrible accidents. On a straight road, or one with easy curves, such long trains could not have been brought down, because they would have inevitably broken loose.

Mr. E. A. COWPER said that some years ago he made some Mr. Cowper. experiments as to the adhesion of dry wheels upon dry rails. It often ran up to 25 per cent., and, in a few cases, to 33 per cent., of the weight of the engine. He had tried it with a pair of 7-feet wheels, and he dragged them on the line without rolling, so that he was certain that he had got the exact result. When an engine was running round a sharp curve, no doubt there was considerable pressure against the outside rail; but notwithstanding this, if the engine and the line were in good order, no accident would occur; but if the flange were worn square, it might catch and ride on a bad or projecting joint in the rails on a curve. A number of accidents happened, through some fault in the rails, in going round curves. He had been round very sharp curves, at

Mr. Cowper. very high speed, without any evil consequences, and without any very bad motion; but if the speed was high, the line bad, and the motion at all irregular, there was, no doubt, a good deal of danger.

Mr. Mackenzie. Mr. JOHN MACKENZIE, in reply, observed that he did not agree with the remark of Mr. Owen, that "theoretically, according to the Paper, every six-wheeled engine ought to run off the rails," as it was probably very seldom that adhesion was as high as 37 per cent.; and, though liable to happen at any time, it might not happen during the lifetime of an engine that this high rate of adhesion should exist along with the other circumstances necessary to cause a well-proportioned engine to leave the rails. Indeed, the Paper was open to criticism quite the reverse of that of Mr. Owen. It might have been urged, that the Paper was an attempt to treat in a loose and popular way a subject more fit for a strict mathematical and physical investigation; and that, even allowing it all to be correct, it treated of a danger which had always been recognised, and which was not very great unless under circumstances which could hardly occur, and with proportions which nobody would adopt; and such criticism could not have been easily met. Mr. Owen went on to say the fact remained that engines did not run off the rails: but temporary immunity from accident was no certain proof of the absence of danger: for instance, the Tay Bridge had been shown to have been unsafe from the beginning, but it stood unsuspected for some time, and, but for one unfortunate gale, it might have been standing now, and still have been pointed to as an example of boldness of design and a monument of engineering genius. Mr. Browne said: "It was obvious that if the gauge differed so much within so short a distance, that in itself constituted a considerable obstacle tending to throw the engine sideways; and it was a cause nearly independent of the speed at which the engine was going, accounting therefore for cases of derailment at low speed," &c. Now when a wheel, travelling slowly on a horizontal plane, came into contact with an obstacle, it must either stop, or rise over the obstacle, or be turned aside by it; the circumstances determining the conduct of the wheel being the angles which the surfaces at the point of contact formed with the plane of the wheel, and with the horizontal plane, as well as the proportion of friction to pressure, and the direction of the force, or resultant of forces, by which the wheel was pressed against the obstacle. The flange of an engine-wheel in this acted much as any ordinary wheel would do, the side of the rail constituting an obstacle lying at a

very obtuse angle to the plane of the wheel; so that generally the wheel was turned aside, but in exceptional cases it rose over the obstacle, and in the absence of any defect in the rail it was friction alone which gave the flange the hold on the rail which enabled it to rise. Referring to the sections of tires exhibited, Mr. Browne "supposed that every wheel running on an English railway was coned." It should be explained that the sections alluded to all showed tires of leading wheels of engines, and that the Great Eastern and the London, Brighton, and South Coast Railway Companies made the tires of driving wheels cylindrical on the tread, and with very thin flanges. Probably the same might be the practice of some other companies.

A remarkable case had been mentioned by Mr. Grover, in which the wheels of a locomotive kept on the rails when keyed on the axles, but ran off when made loose. A possible explanation might be that the side pressure on the flange was less than the load on both wheels, but greater than the load on its own wheel. Thus, when keyed on, both wheels would revolve at the velocity due to the treads, or to one of them, and the flange at its point of contact with the rail would move at a higher velocity, sliding backwards on the rail, so that the friction between them would be the friction of a skidded wheel, and consequently less in proportion to the pressure than the adhesion of the treads. When the flanges were loose, the pressure on the flange being, by hypothesis, greater than that of the one tread, the wheel would revolve at the velocity due to the point of contact of the flange, and the friction of the flange would change into adhesion, while that of the tread would be the smaller friction of a skidded wheel. In short, with fixed wheels the flange friction was the friction of motion, while with loose wheels it was the friction of rest. From this it would appear that, although the tractional resistance and the wear would be greater, the danger would be less when there were two points of contact between the wheel and the rail, than when there is only one (Appendix IV.). In the latter case, when the outer leading wheel moved towards the rail on a curve, there must be one position in which the velocity of the outer wheel at the point of contact would be exactly that due to the tread of the inner wheel, so that neither wheel would slide, adhesion would be complete, and this adhesion would continue in the case of the outer wheel, and when its flange rose over the rail it would, by its greater pressure and adhesion, cause the tread of the inner wheel to slide. This, however, would not be the case if the leading wheels were coupled to the driving wheels. The above indicated that, when there was one point of

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Mr. Mackenzie. contact and the leading wheels were not coupled, adhesion of the flange might take place, not only at low speeds, but also at higher velocities. On the other hand, assuming that in Mr. Grover's experiments there were two points of contact, these experiments seemed to show that, in proportion to pressure, flange friction was less than tread friction, not only at high speeds, but also when moving slowly, as no doubt was the case with the wheels in question. This being so, the mode of calculating the example quoted from the returns of railway accidents, would apply solely to cases where there was only one point of contact between the wheel and the rail. Where there were two points of contact, flange adhesion would be much less likely to occur; but if, by any slight inequality of the rail, or by oscillation, adhesion of the flange once set in, it would continue, unless when the leading wheels were coupled.

In quoting the words "within the limits of practice," Mr. Grover certainly did not keep within those limits when he asked the Meeting to "consider the wheel-base as equal to twice the radius." When, however, after stating the extreme limits, he went on to say that "between those limits there was, no doubt, a proportion beyond which it was evident that, with an increased wheel-base, the danger of running off the curve was increased," he was quite in accordance with the Paper, although there was a difference of opinion as to what that proportion was, and Mr. Grover's view of the matter was not altogether upheld by subsequent speakers. Mr. Grover further said that, when the tractional resistance was increased, "it was absurd to say that the danger was not at the same time increased." But here he was wrong, as the tractional resistance depended on the distance which the wheels slid in travelling a certain length along the rails, as well as on the weights on the wheels; while the danger depended principally upon the proportion of weight which held down the leading wheel, and the leverage with which this wheel acted on the others. Thus the tractional resistance and the danger did not depend upon the same circumstances; and although they might often increase and diminish together, they did not always, nor necessarily, do so. A small tractional resistance in going round a curve was not invariably a proof of safety from running off the rails. It might, on the contrary, be a symptom of extreme danger. The best possible proof of this was the case quoted by Mr. Grover, where wheels were made loose on the axles, no doubt with the express intention of lessening the tractional resistance, with the result that they ran off the rails. A carriage with an extremely short wheel-base would

run very easily round a curve; but it would be in considerable danger of running off the rails owing to the short leverage with which the outer leading flange resisted the tendency of the inner wheels to outrun the outer ones, and twist round so as to face the outer rail. Mr. Grover remarked that "the reason was that those long carriages, many of them with 22-foot wheel-base, going round curves of 300 or 400-foot radius, had a great tendency to mount the outer rail. The wheel ground the outer rail until it got hold of something, and the check-rail, being a little slack, allowed it to do so, and off it went." This last sentence might be taken as a complete brief abstract of the Paper; the only difference of opinion probably being as to what the "something" was. In the Paper it was supposed to be the side of the rail, the adhesion being sufficient to allow the wheel to get hold of it, the precise spot being probably determined by some slight indentation or irregularity in either the rail or the wheel. If Mr. Grover meant by "something" a loose or imperfect joint, he was not at variance with anything stated or implied in the Paper, unless perhaps he might demur to the extreme smallness of the defect which, according to the Paper, would, in addition to the adhesion, be sufficient to make the flange mount the rail under certain circumstances. Mr. Grover's description of the accident to a tramcar was, he thought, hardly to the point; but it might serve as an excuse for saying that these vehicles afforded a strong confirmation of the opinion expressed in the Paper, namely, that only a sufficient side-pressure, and that not extremely great, was required to cause flanged wheels to mount the rail. When two trams met on a single line, the driver of one turned his horses to one side, so as to pull at an angle, and the wheels left the rails, although in this case there were two leading-wheel flanges to mount out of their grooves. As settling a somewhat difficult point, particular notice should be taken of the statement of Mr. Langley, that an engine he described "flattened the curve in its progress," confirmed as these words were by Mr. Tomlinson when he said that he agreed "that frequently an engine did, when the road was loose, actually form its own curve." Thus the springing of the rails would maintain the tightness through some little range, probably sometimes great enough to enable the flange to climb to the top of the rail. This springing of the rails was evidence of considerable side pressure.

The remarks of Mr. Stroudley were most instructive. As regarded the flanges on driving-wheels, as long as they did not touch the rail they could do no harm; but it seemed questionable whether it was advisable to contribute, even in the most remote

Mr. Mackenzie. degree, to the risk of an accident on a very sharp curve for the sake of lessening its effects. Prevention was better than cure. The remark as to the safety of a high centre of gravity was most important where high speeds were concerned. It was a point omitted in the Paper—and indeed, at the low speeds treated of, it hardly came into play. In the case quoted in the Paper, the centrifugal force was more than balanced by the cant, so that raising the centre of gravity would have lessened, instead of increased, the weight on the outer wheels.

It had been said by Mr. Beaumont that “It was not altogether a slipping forward of the outer wheels, or a slipping backward of the inner ones, but a little of each;” and Mr. Cowper had considered the case of a carriage revolving on the rails about its centre. Now this being thought a very doubtful point, some experiments had been made, and it was found that a model would not revolve about any imaginary centre, however the weights were arranged, but that it always revolved about some one actual point of contact, all the others sliding. When it could not move from one position to another by revolving about one centre, it first revolved round one point of contact, and then round another, until, by a succession of small movements, it had taken up its new position. From the extremely rough way in which the experiments were made, no great reliance was placed upon them; but they seemed sufficient to justify the views expressed in the Paper. This point did not greatly affect the result. It was not meant by the above that, when not coupled, all the outer wheels slid forwards, or all inner ones backwards; probably it was often otherwise. Mr. Beaumont further said that “It appeared quite possible that the velocity at which trains were running had more effect on their derailment than might be supposed from the Paper, especially in the case of the long and heavy cars used in America, the inertia-motion of which might push the bogies off the line when entering curves.” In this case Mr. Beaumont was probably correct. The remarks in the Paper applied only to six-wheeled engines or vehicles. In a four-wheeled carriage, the outer leading-wheel flange had to resist half the unbalanced centrifugal force; so that such a carriage, though less likely than a six-wheeled one to leave the rails at slow speeds, might be more dangerous when the velocity was increased. A bogie in this resembled any other four-wheeled carriage with a very short wheel-base, and indeed was more dangerous, being pushed along by a pin some distance behind the front axle, so that any slight deviation had a tendency to increase. Indeed a long carriage on two bogies was a most insecure-looking

contrivance, whose character would probably upon investigation Mr. Mackenzie be found to correspond with its appearance. In the "Engineer" of the 4th of May, 1883, there appeared a list of accidents on American railways during February, numbering in all one hundred and eighty-four, of which one hundred and thirteen were from "derailment," and thirty-six of the hundred and thirteen were "unexplained," being the only unexplained accidents on the list. Although not claimed by him, it was due to Mr. Macnair to state that some part of the Paper had been suggested by remarks made by him in a discussion on a Paper on "Train-Resistance on Railways."¹

With regard to a few general observations as to the theoretical and mathematical nature of the Paper, it could only be replied that it was inevitable that this should be the case if the subject was to be treated at all. An unwillingness to adopt novelties whose safety or economy were unproved, was a conservatism of the most valuable kind; but an opposition to an inquiry into a cause of accidents, merely because it was theoretical, was as much an element of danger as was resistance on railway curves.

Correspondence.

Mr. T. C. FIDLER remarked that the numerous sources which Mr. Fidler contributed to the increased resistance of railway vehicles upon sharp curves had been frequently investigated, and among these there could be no doubt that the most important were to be found in the conditions which produced the skidding motion of the wheels, and by which the leading and trailing wheels were continually dragged across the rail in a diagonal direction, as shown in Fig. 1. Neglecting the effect of any spreading of the gauge, this diagonal skidding motion might be resolved into two components, due respectively to two distinct causes, namely, first a transverse slip in the direction of the axle, due to the fixed parallelism of the axles, and proportional to the length of wheel-base; and, secondly, a longitudinal slip at right angles to the above, due to the fixity of the wheels upon the axle, and proportional to the width of gauge. These two component motions were indicated by the Author, but their relative values, which were in the ratio of length of wheel-base to width of gauge, did

¹ Minutes of Proceedings Inst. C.E., vol. xxxi., p. 403.