The Author. inch which he mentioned was not greatly in excess of the highest value quoted in the Paper, namely 582 lbs., and there did not appear to be any reason to think that the results of the experiments quoted would have been materially altered if the pressure had been increased to the higher value. The Author had aimed at obtaining a value for journal-friction that would represent ordinary railway-practice, and he thought that although in special cases a value of 750 lbs. might be reached, the figure of 600 lbs. per square inch, to which Mr. Bamber's remarks more particularly referred, was a more practical working-limit to adopt, and this was about the limit chosen in the experiments. With reference to the tendency of the weight of the body of a coach to steady the swaying motion of the bogies, there was no doubt that side-bearing bogies swayed less than those with a centre bearing; but although this gave a certain steadying effect, it might be doubted whether it reduced the flange-resistance. The Author's experience was that side-bearing bogies tended to remain hard over in the direction determined by the last curve that had been passed, and that the consequent grinding of the flanges on the rails was, if anything, greater than when centre bearings were used.

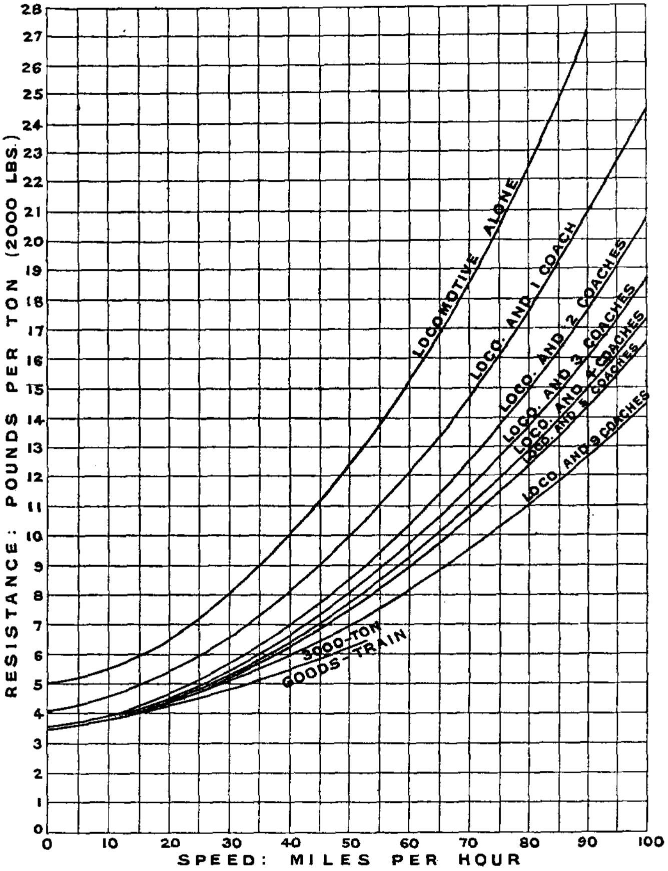
Correspondence.

Mr. Armstrong. Mr. A. H. Armstrong, of Schenectady, offered for consideration various data which had at least the merit of laying the foundation for a workable train-resistance formula. Lest he might be accused of attempting to create a place for a new formula in a field already stocked with such creations, he would state that it was of the three-part type advocated by the Author, which form seemed to be generally accepted as best fulfilling the required conditions. The resistance opposing the forward motion of a moving train depended upon such complex relations between weight and speed, and car-, truck- and track-construction as to render it impossible to accomplish more than general grouping of the several elemental resistances, with the result of producing a formula giving approximate values only. As a formula, to be of any value, must be capable of wide application, it would appear that values of A in the Author's general equation (p. 233) could not be made dependent upon such factors as journal- and wheel-diameters, knowledge of which was not always available. In order to suit A in various weights and aggregations of cars, he had found it convenient to use $A = 50 \sqrt{T}$, where T was the total weight of the car or train in tons (2,000 lbs.). This value was purely arbitrary, but was chosen on account of its close agreement with results obtained from many experimental tests on dissimilar vehicles: not that the term A was determined independently from the tests, but that that term expressed as above and used in conjunction with the rest of the formula to be given later, yielded results closely approximating to test values, over a wide range of types of equipment and of speed. The Author suggested that the well-known difference in tractive effort required to haul a loaded and an empty vehicle might be attributed to B, the coefficient of v in the second term; but by using the expression $A = 50 \sqrt{T}$ this difference in tractive effort between loaded and empty cars was taken care of appropriately in the first term, and might rightfully belong in this term if lubrication of the journal was somewhat more complete than was assumed by the Author in making the statement that the coefficient of journal-friction was constant at all pressures. Mr. Armstrong's experience in determining train-resistance had been limited chiefly to tests in which electrical methods of measurements were made use of: such methods presented a ready means of checking results obtained by coasting. Probably one of the most complete sets of tests taken for the express purpose of determining the resistance to trains of different composition was that undertaken upon the experimental track of the New York Central and Hudson River Railroad during the 50,000-mile endurance-trial of the new 100-ton electric locomotives. These tests, comprising many hundred runs with trains of different composition, under different climatic and atmospheric conditions, had given very valuable data, from which the curves of *Fig. 14* had been plotted. Together with similar tests upon electric motor-cars of several types, they established the fact that the air-resistance constituted the most important element of the total resistance with light trains, and was a considerable factor even in the case of a nine-coach train weighing, complete with locomotive, approximately 550 tons (of 2,000 lbs.). The fact thus established led him to put greater faith in train-resistance measurements where electrical methods were used, and, for reasons given later (p. 287), to place but little reliance on results obtained with either the dynamometer-car or with steam-indicator diagrams, and especially to discredit figures deduced from water- and coal-consumption over an extended run. Most of the records of the electrical runs made in the New York Central and other tests had been obtained by the coasting method, continuous records being kept of speed, time,

Mr. Armstrong.

Mr. Armstrong. distance, etc., but these figures had been checked by meter-readings of voltage and current-input to the motors, also continuously recorded. As the New York Central locomotives were without gears, this troublesome factor was eliminated. The coasting method, checked by the electrical records, also gave opportunity of determining both the constants B and C, by taking advantage of the fact that the coefficient of the second term varied directly as the speed, while the coefficient of the third term varied as the square of the speed. Instead, therefore, of making all tests upon days when there was little or no wind, tests had been purposely made upon days when the velocity of the wind in the direction of the track reached 20 miles

Fig. 14.



Mr. Armstrong.

per hour or more. Thus, a train running at 50 miles per hour with the wind encountered a head wind of 30 miles per hour, while running against the same wind at a speed of 50 miles per hour was equivalent to meeting a head wind of 70 miles per hour. In each case the actual speed of the train in relation to the track was 50 miles per hour, and B remained constant, assuming of course that the wind was directly head on, and did not increase the flange-friction. The difference in train-friction recorded at 50 miles per hour with and against a wind of 20 miles per hour must necessarily correspond with the difference in the third term, and in this case the coefficient C could be obtained directly. In the same way, the coefficient B of the second term could be obtained with a wind-velocity of, say, 20 miles per hour in the direction of the track by running the train at a speed of, say, 70 miles per hour with the wind and 30 miles per hour against the wind, the actual head wind opposing the motion of the train being in each case 50 miles per hour, thus constituting equal values for the third term. By following out this method of making resistance-tests, and taking records on days when the wind was quartering to the track, it was possible to study fully the coefficient B under all conditions, and to determine the value of flange-friction under normal conditions and with a cross wind—a matter of considerable importance. It was unfortunate that lack of time had prevented the full carrying out of the schedule of tests contemplated on these lines, and that only partial results had been finally obtained. Sufficient data had been obtained, however, to convince Mr. Armstrong that the widely scattered points usually observed in the plotted results of train-resistance tests would be brought more nearly in line with the aid of a full knowledge of the atmospheric conditions obtaining (direction of the wind, its intensity, etc.), not only in general during the test, but also as determined by a continuous record made both on the train and at frequent stationary points along the direction of travel. This applied more especially to light trains, in respect of which the air-resistance was of vital importance. Owing to the impossibility of applying train-resistance data obtained from dynamometer-tests to the conditions obtaining with the single car or small aggregation of cars common in electrically propelled trains, he had been forced to build up a formula that would fit the conditions with which an electrical engineer was more especially concerned, with the result that, using the recognized three-part formula advocated by the Author, the following (*Fig. 14*) had been found to apply very closely to working-conditions—

$$R = \frac{50}{\sqrt{T}} + 0 \cdot 03\,V + \frac{0 \cdot 002\,V^2}{T}\,A\left(1 + \frac{N-1}{10}\right).$$

Mr. Armstrong. where R = resistance in pounds per ton,
T = total weight in tons,
V = speed in miles per hour,
A = end cross section in square feet,
N = number of cars,

and $50\sqrt{T}$ was limited to a minimum value of 3·5. The following values were assumed for A and T:—

	A	T
Locomotive	120	100
Passenger coaches	100	50
Goods-wagons	90	40

It was interesting to note the agreement between test results and the formula, as shown by the following figures:—

New York Central Locomotive Alone.

Speed (miles per hour)	20·0	40·0	60·0	80·0
Calculated resistance (lbs. per ton)	6·55	10·0	16·2	22·5
Test resistance (lbs. per ton)	6·7	9·6	14·0	17·5

New York Central Locomotive and Five Cars.

Speed (miles per hour)	20·0	40·0	60·0	80·0
Calculated resistance (lbs. per ton)	4·5	6·31	8·92	12·32
Test resistance (lbs. per ton)	4·2	6·85	9·55	12·40

New York Central Locomotive and Nine Cars.

Speed (miles per hour)	20·0	40·0	60·0	80·0
Calculated resistance (lbs. per ton)	4·44	6·07	8·4	11·4
Test resistance (lbs. per ton)	4·0	6·5	9·1	11·6

Allgemeine Car in Zossen Tests.

Speed (miles per hour)	20·0	60·0	80·0	100·0	120·0
Calculated resistance (lbs. per ton)	5·6	14·7	22·2	32·0	43·3
Test resistance (lbs. per ton)	5·5	14·9	22·8	33·3	46·0

Car No. 5, General Electric Co.

Speed (miles per hour)	10·0	20·0	40·0	60·0
Calculated resistance (lbs. per ton)	8·4	10·6	18·4	31·5
Test resistance (lbs. per ton)	8·3	11·3	19·2	29·2

The suggested formula was not in any way complete, and its several elements could readily be subdivided to advantage; noticeably the first term, $50\sqrt{T}$, should be subdivided into a constant for all trains plus a variable depending upon the composition of the train, its weight, etc., in order to obviate the necessity of limiting $50\sqrt{T}$ to a minimum value of 3·5. The last term also was not accurate, in that it did not differentiate between the head-on wind-friction and that of the sides and top, this error being due to a

Mr. Armstrong.

desire to provide a simple formula. The subdivision of coefficient C into several parts to be calculated separately, as suggested by the Author, was undoubtedly more nearly in accord with actual facts. The formula suggested by Mr. Armstrong depended for its success upon the relations generally existing between the length of a car and its weight as obtaining in the United States, and the formula would not apply with equal accuracy to the much shorter, lighter cars used in England and on the Continent. His grounds for saying that he considered train-resistance data obtained from dynamometer-car or steam-indicator records to be unsatisfactory, were the following:—The tractive effort recorded by the dynamometer-car was the effort required to haul the load trailing behind the locomotive and tender, and with light trains this tractive effort might be perhaps but one-half or even less of the total tractive effort exerted by the locomotive. Both the steam-engineer and the electrical engineer had common interest in determining the total tractive effort required to haul the train as a unit, and while the error introduced was small in the case of goods-trains, the results obtained were entirely misleading in the case of high-speed passenger-trains, even though these were of considerable weight. Further, dynamometer-records taken over extended runs were open to serious criticism, due to the fact that the resistance could not be obtained from such records by deducting or adding the tractive effort due to difference in elevation, as a continuous dynamometer-record included all values of curve-resistance, and acceleration of the train at the original start, as well as after rounding curves where it might be necessary to reduce speed; but more especially it included part of the tractive effort due to the difference in elevation of the two ends of the run, unless the intermediate gradients were so slight that they could be descended without application of the brakes. The energy lost in braking must be replaced, and would be recorded in the dynamometer-readings, so that any continuous test to be of value must be made upon practically straight level track. In addition to the inaccuracies inherent to a continuous run, the method of determining train-resistance by coal- and water-consumption was open to the further criticism that such methods of measurement were, at best, very crude compared with the coasting method or determination of train-resistance by the input to an electrically propelled train. Further, the coal- and water-records included not only the fuel and steam consumed in useful work, but also the losses introduced when the locomotive was coasting downhill and doing no work, but still consuming coal at a rate high enough to invalidate the accuracy of the results. The coal- and water-consumption were of interest

Mr. Armstrong. to the steam-engineer, but they did not afford an accurate means of determining train-resistance when considered from the standpoint of another type of motive power. In general, all tests which had come under his attention had demonstrated the great importance of air-resistance as a factor in the total train-resistance, and while attempts were being made to shape the front end of motor-cars operated singly so as to reduce train-resistance, steps were also being taken to reduce the skin-friction by introducing cars of smooth exterior design, a notable example being the all-steel car designed for the Union Pacific Railroad Company, following the lines of their gasolene motor-car. He desired to compliment the Author upon his clear method of subdividing the train-resistance into its various components, and upon offering the best presentation of the subject in this respect that had yet been published. Mr. Armstrong felt that, his experience having been limited to light trains running at considerable speeds, his remarks should be interpreted as applying more especially to such trains.

Mr. Cardew. Mr. C. E. Cardew remarked that he had done much experimental work with the Timmis bogie-lead on the Burma railways between 1900 and 1905, in communication with the late Mr. I. A. Timmis, M. Inst. C.E., its originator. Mr. Cardew early came to the conclusion that in certain cases its action in steadying a bogie-vehicle by preventing radial oscillation of its trucks around their pivots—generally known as "hunting"—was distinctly beneficial, especially in short bogie-vehicles descending long inclines, when the hunting action of the trucks was always very marked. In the discussion on Mr. Aspinall's Paper[1] Mr. Timmis communicated some of the results of the earlier experiments to The Institution. Besides the radial oscillation of the bogie-trucks, there was a transverse oscillation of the body of the vehicle on the swinging suspension-links of the trucks. To this motion—usually termed "lurching"—the Author did not refer. In many cases it appeared to be indirectly due to the hunting of the bogie-trucks, and in short vehicles it might be very marked and disagreeable to passengers. Under the term "lurch" was not to be included the gentle rhythmic swaying of the vehicle, due to the pendulum action of the swing-links, which in itself was not disagreeable, but only those violent and irregular oscillations which were set up periodically from shocks received by the wheel-flanges impinging on the rails. When the hunting of the bogie-trucks on the track was stopped, the lurching of the body was obviated or at least reduced to a

---

[1] Minutes of Proceedings Inst. C.E., vol. cxlvii, p. 212.

Mr. Cardew.

minimum. Mr. Cardew had found that this desirable result was effected by giving lead to the trucks. To prove the value of the relief thus afforded he had made an experimental arrangement in a car whereby it was possible to apply or remove the lead at pleasure. On running this car at the end of a train without connecting the continuous brake to it, so as to leave the trucks and wheels quite free, it was found that the difference between coasting downhill with and without bogie-lead was very marked. In order to prevent objectionable lurching, it was a common practice to employ side-resistance springs, either in substitution for or in combination with the swing-links; but they were generally more of a palliative than a cure for the trouble. From long experience in the designing, building, and running of rolling stock he was convinced that the longer the vehicle the less the hunting of the bogie-trucks and the lurching of the body, so that generally both were of little or no practical importance in very long vehicles. He was therefore surprised that the Author omitted the length between the centres of bogie-pivots as a factor in the equations he employed in dealing with the question of oscillation. On p. 259, however, this factor was admitted as one to be considered, where the Author treated the action of bogie-lead as equivalent to giving a bogie-vehicle a temporary rigid wheel-base equal to the distance between bogie-pivots, that was, of course, while the vehicle was running on a straight track. Mr. Cardew, however, held that this came about automatically as a function of the length between bogie-pivots, quite independently of any lead given to the trucks. If so, then the equations needed modification to include and express this function. Further, the objectionable oscillations were greatly magnified on poor track and at high speeds; indeed, they could scarcely be studied advantageously except under those unfavourable conditions, and it was exactly under such conditions that the advantage of bogie-lead made itself most felt. He agreed with the Author, however, in condemning excessive side-play between wheel-flanges and rails on straight track. Were that reduced, there would be much less complaint about hunting and lurching, and probably special devices for overcoming these irregular motions would be less necessary. The Author mentioned the tendency of the vehicles in a train to crowd together when "coasting" a descending gradient. He might, however, have added that it was only during this crowding that the objectionable oscillations occurred, that was to say, during periods when the pull on the draw-bars (or central couplers) and on the bogie-pivots ceased, while the transverse stiffness or rigidity of the buffers (or couplers) was the only steadying force on the vehicles composing the train. This

Mr. Cardew. steadying action of the draw-bars and buffers could indeed only be appreciated and studied in perfection on loosely-coupled trains descending long inclines under the control of only engine- and tender-brakes. With tightly coupled trains fitted throughout with continuous brakes the oscillations were largely, though not wholly, extinguished. It would therefore be interesting if the Author could say under what conditions the oscillograms shown on p. 295 had been taken, the state of the track, and whether the gradient was up or down. To thoroughly elucidate mathematically the oscillations of the vehicles of a train crowding each other when coasting downhill, and the flange-resistance to which such oscillations gave rise, it was necessary to regard the train as a long flexible column in unstable equilibrium. The couplings of the vehicles, being then in compression, were so many points at which buckling of the column might take place. In ascending an incline, however, the couplings being all in tension, the column was of course in stable equilibrium, with no tendency to buckle, which greatly reduced oscillation and therefore flange-resistance. Fortunately this occurred just when there was an increased demand for power on the motor hauling the train. On the other hand, though the increased flange-resistance in coasting downhill might make no increased demand on tractive power, yet it certainly conduced to much needless wear of rails, wheels, and other running-gear of vehicles, so that every improvement in design of rolling stock calculated to diminish the mischief was to be welcomed. In order to effect this with certainty, however, it was first necessary to comprehend thoroughly the mathematical conditions of the problem. It was to be hoped, therefore, that the Author might be induced to supplement his admirable Paper by giving for flange-resistance some revised equations, in which might be introduced three new factors, namely (1) Distance between centres of bogie-pivots; (2) Total length over buffers (or central couplers); (3) The coefficient of transverse stiffness or rigidity of couplings while coasting on straight track.

Mr. Collinson. Mr. Arthur Collinson considered that there were several factors which would prevent the general adoption of roller-bearings as at present designed; among them were high first cost, extra weight, liability to get out of order, difficulty and high cost of repair, and, as regarded their adoption for wagons, the necessity for perfect alignment, which was seldom obtained where wagons were built at the rate of one hundred per week, and were anything but a finished piece of engineering work. Roller-gear must be carefully fitted to the wagon, the wagon must be built square and kept square, and the whole vehicle must be made a really good job. The

Spencer roller-bearing had so far been very successful in England, and a large number of high-capacity wagons were running well with this gear, which he understood had lately been further improved. Mr. Collinson.

Mr. Mailloux. Mr. C. O. MAILLOUX, of New York, observed that if the figures and facts which the Author had collected were admitted, discussion must be confined largely to the manner in which these figures and facts were classified and analysed by him. Here there was much room for divergence of opinion, and it need not seem strange, therefore, that Mr. Mailloux should differ radically from the Author in regard to the classification of the elements of train-resistance. While he did not see them, Mr. Mailloux would not deny that the proposed classification might have advantages practically. He was of opinion, however, that this classification could scarcely be considered adequate and satisfactory for the purpose of a scientific analysis, from which a comprehensive formula, or a general equation, was to be deduced. For instance, flange-friction was a particular case of sliding-friction, as was journal-friction. It was usually of the "unlubricated" variety, but, not infrequently at curves when the rails were greased, flange-friction became a case of "lubricated" sliding-friction, precisely like journal-friction. Mr. Mailloux had elsewhere[1] expressed the opinion that a classification of the elements of train-resistance such as that adopted by Mr. Aspinall and other investigators, while perhaps suitable enough for practical purposes, in calculating and comparing train-resistances, was not the most suitable or convenient for the study and analysis of train-resistance, or the determination of its component parts and of the rôle and effect of each. A clearer and more comprehensive idea of these effects was likely to be gained by reference to the fundamental laws of friction. If the components were grouped under the three principal kinds of friction distinguished by the physicist, namely, sliding-friction, rolling-friction, and fluid-friction, the classification on p. 292 was obtained. This complete classification was the most convenient one known to Mr. Mailloux. In devising a formula, it might be convenient or necessary to group together several of these elements; but even then the classification given by Mr. Aspinall was much to be preferred to that given by the Author. The following classification merged the second and third elements of the Author's classification, because they were really different phases of rolling-resistance, and, in the present state of knowledge, could not be separately determined, as the Author admitted. His statement that rolling-friction and track-resistance had not been determined by test,

---

[1] *Harvard Engineering Journal*, vol. ii, No. 4 (1904), p. 239.

Mr. Mailloux.

A.—SLIDING-FRICTION.

(Including two varieties, both involved in train-resistance.)

I. *Lubricated Sliding-Friction:*—
  (1) Rotational friction of axle or journal.
  (2) End-play friction of axle or journal.

II. *Unlubricated Sliding-Friction:*—
  (3) Slipping- or skidding-friction.
  (4) Wheel-flange friction.

B.—ROLLING-FRICTION.

(1) Friction due to mangling or crushing effects.
(2) Friction due to non-yielding inequalities of surface.
(3) Track hysteresis.

A.B.—COMPOSITE FRICTION.

(Combining sliding- and rolling-friction.)

(1) Effects of oscillation and concussion.
(2) Effects of curves.

C.—FLUID FRICTION.

(Including two varieties, both involved in train-resistance.)

(*a*) *Semi-fluid Friction:*—
  (1) Friction of ties, ballast, embankment, earthwork, etc.

(*b*) *True Fluid Friction:*—
  (2) Air friction at head of train.
  (3) ,, ,, rear ,,
  (4) ,, ,, sides ,,
  (5) Wind-friction.

though they were probably small compared with journal-friction, was truer in the first part than in the second, however; for all depended on the conditions. The Author's reference to journal-friction would lead to the inference that it was practically independent of the bearing-pressure and of the rubbing-velocity. The statement that the coefficient remained nearly constant with varying bearing-pressure was in apparent contradiction to experience. The difference in the train-resistance per ton, for trains of loaded and of empty cars running at the same speed, was first noted more than 30 years ago by Mr. A. M. Wellington, and had been noted by all who had made determinations of the train-resistance of loaded and empty cars since that time. Mr. Aspinall had found[1] that the train-resistance per ton was nearly three times as much for empty as for loaded wagons, under conditions otherwise comparable. Mr.

---

[1] Minutes of Proceedings Inst. C.E., vol. cxlvii, p. 199.

Mr. Mailloux.

A. C. Dennis in America had found[1] that, for American goods-wagons, the train-resistance per ton was 80 to 90 per cent. greater for empty than for loaded cars, run at the same speed and under comparable conditions. There was abundant evidence, of equally unquestionable character, that the train-resistance per ton was influenced by the load. It seemed difficult to account for this difference without concluding that the coefficient of friction did vary (inversely) with the bearing-pressure in some such way as it had been found to vary in the laboratory experiments of Messrs. C. J. H. Woodbury, A. M. Wellington, R. H. Thurston, and many others. The Author's conclusion that journal-friction was independent of speed was also in apparent contradiction to practical experience. It had been established, as the result of repeated tests and observations of all kinds, that the train-resistance had a relatively high value, sometimes termed "starting-resistance," ranging between 15 lbs. and 25 lbs. per ton at velocities near zero, and that this value decreased rapidly as the velocity increased, passing through a minimum of, usually, less than 6 lbs. per ton, at a certain critical velocity, which, according to Mr. Wellington, might be as low as 6 miles, and as high as 15 miles per hour, and which, according to the curves published by Mr. Dennis, might occur between speeds of 15 miles and 20 miles per hour. The figures given by the Author, being for speeds above 18·7 miles per hour, might be above the critical speed at which the minimum value occurred. The data cited were, in any case, too meagre to form the basis of any far-reaching or general conclusion, especially when they were flatly contradicted by the results of Mr. Wellington, Professor Thurston, and many others. The Author's conclusion did not seem to Mr. Mailloux to be warranted, especially as it apparently ignored the starting-resistance altogether, or made of it a separate difficult problem. What was needed was a theory of journal-friction which accounted satisfactorily for all the phenomena occurring from start to finish in a car-journal. The theory which he had found most useful, rational, and in harmony with the facts, was one which included the variation of the coefficient of friction with the speed in such a manner that a minimum value must and did occur at a critical speed. According to this theory, lubricated journal-friction might be regarded as made up partly of pure sliding (unlubricated) friction and partly of pure fluid-friction. The characteristics of both of the component frictions had been separately determined and were well known. The characteristics of pure friction

---

[1] Transactions of the American Society of Civil Engineers, vol. 1, p. 3.

Mr. Mailloux. between metal surfaces (unlubricated) had been determined by the aid of data obtained in 1878 and 1879, by the late Sir Douglas Galton.[1] Mr. R. A. Parke, one of the highest authorities on train-braking, had found, as the result of comprehensive study of various brake-friction data, more especially those given in the Papers of Sir Douglas Galton, that the relation between the coefficient of friction and the rubbing-velocity might be represented by curves of hyperbolic type (*Fig. 15*), for which he gave two empirical formulas.[2] In the case of fluids the friction was known to increase as some power higher than the first power of the rubbing-velocity. Therefore, the curve representing the relation between the coefficient of friction and the rubbing-velocity was of parabolic type. If, then, lubricated journal-friction was a composite phenomenon including both of these kinds of friction, the curve showing the values of the coefficient of friction from zero velocity to limiting velocities would be a curve such as could be made by taking, for each ordinate, the sum of the ordinates of a hyperbola and of a parabola. This meant that the curve would be a two-branch curve, having a minimum point at some critical velocity at which the total friction was made up of equal amounts of both kinds of friction. For velocities below the critical velocity the fluid-friction effect decreased, and the sliding-friction effect predominated. At velocities near zero the fluid-friction effect was negligible and the sliding-friction effect was a maximum. For velocities above the critical velocity the reverse took place: and at high velocities, the sliding-friction, while still existing, was small in comparison with the fluid-friction. Professor Thurston had given[3] data of careful determinations of the coefficient of friction for a wide range of rubbing-velocities (from less than 50 feet to as high as 1,200 feet per minute). The curves obtained by plotting these data (*Figs. 16*) indicated clearly the composite character of journal-friction. These curves showed the transition between "starting" friction and "running" friction—a change from a condition where the friction decreased to a condition where it increased with the velocity. As the different curves indicated, the critical velocity varied somewhat

---

[1] "On the Effect of Brakes upon Railway Trains." Proceedings of the Institution of Mechanical Engineers, 1878, pp. 467 and 590; 1879, p. 170.

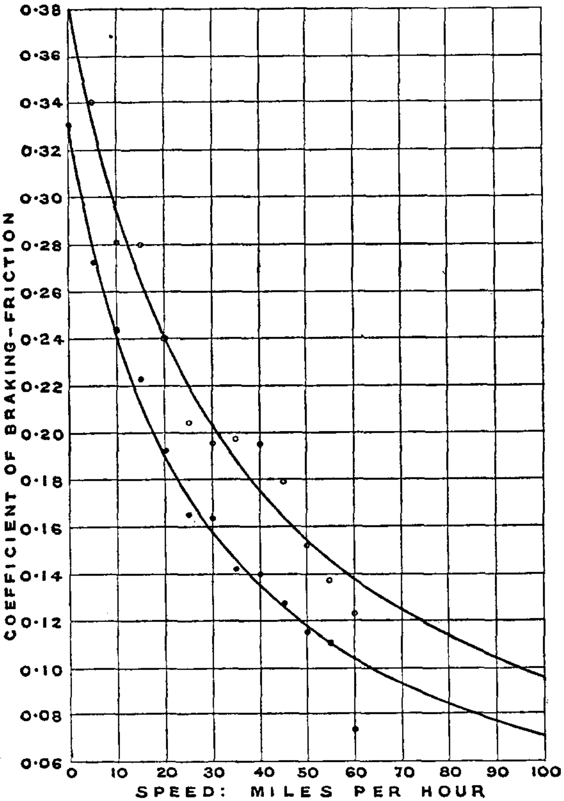
[2] R. A. Parke, "Railroad Car-Braking," Transactions of the American Institute of Electrical Engineers, vol. xx (1904), p. 235. See also "The Friction of Brake-Shoes," *Railroad Gazette*, vol. xxxiii, 1901, p. 405.

[3] "A Treatise on Friction and Lost Work in Machinery and Millwork," New York.

Mr. Mailloux.

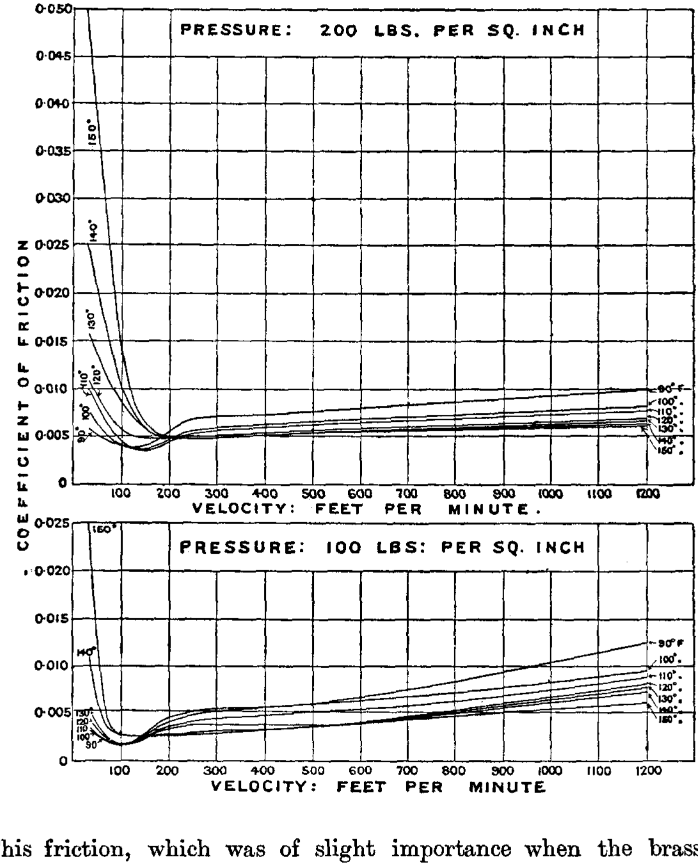
with the pressure and the temperature, and the effects of variations of temperature and pressure were not the same at low and at high rubbing-velocities. This was exactly what might be expected, if lubricated sliding-friction was a composite phenomenon, with components which preponderated at different rubbing-velocities. The portions of the curves which were beyond the critical velocity were

Fig. 15.



more nearly linear, and they represented values of the coefficient of friction which were more nearly constant for high than for low temperatures. Using Professor Thurston's values, Mr. Mailloux had been able to show that the discrepancies in the critical speeds at which the train-resistance was a minimum, as given by Wellington, Mr. Mailloux. Dennis, and others, were due mostly to differences in the diameters of journals and wheels. It was largely a question of determining what train-speed corresponded with a given velocity of rubbing at the journal. There was no disagreement here between theory and practice. Journal-friction was always complicated by a certain amount of friction due to end-play of the journals in the bearings.

Figs. 16.



This friction, which was of slight importance when the brasses and the lubrication were both very good, assumed some importance when the brasses were rough, when the lubrication was inadequate, and when the road was not in first-class condition in regard to alignment and level of the rails at all points. These bad conditions of road and rail were favourable also to increase in wheel-flange

Mr. Mailloux.

friction, and to increase in what the Author called track-resistance. It became very difficult, therefore, under such conditions, to segregate these different elements of train-resistance. The assumption made by the Author, that the journal-friction was constant under all conditions, might facilitate the process of obtaining certain component curves or graphs, which were supposed to represent the constituent elements of train-resistance in a given case. It did not by any means make these curves true or even plausible. On the contrary, in view of the preceding observations concerning the relation between the coefficient of friction and the pressure and rubbing-velocity, it left room for doubt whether any of the component curves were correct. This assertion became all the more significant when it was considered that the two elements which the Author called rolling-friction and track-resistance, being merely phases of the same thing—rolling-resistance—were still more difficult, if not impossible, to segregate. There was much misapprehension and misunderstanding about "rolling-friction." The term itself was a misnomer, and was misleading, being an inheritance from the last century, like the so-called "coefficient of rolling-friction" still mentioned in text-books, although it was not a coefficient at all, since it represented a distance instead of a ratio-factor. These misconceptions of rolling-friction were seemingly the result of an incomplete or imperfect analysis of the energy-reactions involved, and of a consequent failure to note the precise manner in which power was expended and energy was absorbed in doing the work incidental to this kind of "friction." It could be shown that, in every case, the power expended in causing a wheel to roll over any surface was expended in substantially the same manner as if the wheel were ascending a certain equivalent gradient. It made no difference whether the surfaces in rolling contact yielded or not. When they yielded there might be crushing and mangling effects. When they did not yield, there might be shocks. These were merely incidental to the dissipation of the energy abstracted from the moving source which caused the rolling action. When the surfaces in contact were both very smooth, the resistance to rolling, and the force required to overcome it, were both very small. The slightest irregularity in the surfaces in contact obstructed the rolling motion and increased the tractive force required to produce it. Any elevation, however slight, in either surface, would require the rolling object to be lifted bodily in passing over it. Any depression would cause the rolling body to sink to a lower level, from which it must be subsequently lifted, just as it was in passing over an elevation. Hence, every

Mr. Mailloux. irregularity in the surfaces which came in rolling contact, occasioned some lifting action of the rolling object, against its weight and the weight resting upon it. The energy consumed depended, obviously, upon the total weight lifted, the height to which it was lifted, and the number of lifts per unit of time or distance. In a railway-vehicle, every irregularity in the wheel-tread involved a lifting action at each revolution of the wheel, and every irregularity of level in the track involved a lifting action as each wheel passed over it. The hollow caused in the track by the yielding and bending of the rail under each wheel also produced a slight but continuous virtual gradient under the wheel. A sanded or muddy track involved a series of such lifting actions which might follow each other so closely as to constitute a continuous lifting action. The complete analysis of the phenomena[1] showed that, in all cases, rolling-friction might be regarded as something which in its effect was equivalent to a gradient. This view of rolling-friction—more properly rolling-resistance—gave the clearest idea of the manner in which it consumed power, and, at the same time, furnished the most practical way of estimating its amount, as an element of train-resistance, under different conditions. He had found it convenient to consider separately the rolling-resistance due, (*a*) to mangling or crushing effects; (*b*) to non-yielding inequalities of surface; (*c*) to yielding of the track, or "track-hysteresis." The mangling or crushing effects included the increased train-resistance due to sand, mud, ice, snow, etc., which was of special importance in the case of traction over city streets, especially when grooved rails were used. The possibility of accounting for the increase in train-resistance due to these obstructions had proved very useful. Incidentally, it enlarged the scope of many formulas for train-resistance. The equivalent gradient representing the effect of these obstructions might vary from 0·1 to 1·0 per cent. (1 in 1,000 to 1 in 100) or more, corresponding with 2 to 20 lbs. or more per ton (of 2,000 lbs.). Under average street-railway conditions, the journal-friction was always less than the rolling-resistance, instead of more, as stated by the Author. It was seldom less than 5 lbs., commonly 8 to 10 lbs., and at times as high as 15 lbs., per ton. On steam roads, or on electric roads not over public streets or highways, when tee-rails could be used, the track could be kept relatively clear and clean. In the case of deep and drifting snows, however, the rail was often covered again by the snow, immediately after each wheel had passed. In such cases, although the snow yielded readily under the

---

[1] See *Harvard Engineering Journal*, vol. iii, p. 269, and vol. v, p. 39.

pressure of each wheel, and therefore the resistance was very slight, yet, being present at each wheel of the train, and its amount being practically the same at all wheels, it might sometimes cause a material increase in the train-resistance per ton. The non-yielding inequalities were exemplified by open or imperfect rail-joints, the gaps at switch-frogs, and the high and low spots in the rails, due to wear or to original defects or to imperfect laying. These were all obstructions of the intermittent kind. In the case of long trains running at considerable speeds the effects of the obstructions coalesced, the result being virtually the same as if they were replaced by a continuous obstruction, the value of which, as the preceding considerations showed, would obviously increase with the speed of the train. This conclusion was not in harmony with that of the Author, who considered both rolling-friction and track-resistance to be independent of the speed. There was also a certain amount of rolling-resistance due to the wheel-flanges, which also increased with the speed and with the inequalities of the track-alignment. Mr. Mailloux had adopted the term "track-hysteresis," first suggested by Mr. A. Mallock, in the discussion on Mr. Aspinall's Paper, and had used it, with a slightly extended meaning, to designate generally all yielding-effects due to continuous obstructions which were elastic in any degree and which did not involve permanent deformation. In all such cases the (theoretically) recoverable energy stored in the track by compression was not, practically, all recovered. It might be assumed that some of the parts, such as the sleepers, ballast, ground, etc., were compressed somewhat beyond the elastic limit at the expense of additional force over and above what would be required if the limit of elasticity were not exceeded at any point. The energy corresponding with this additional force was non-recoverable, being expended in some form of molecular friction and ultimately converted into heat. A certain amount of set was at the same time produced in the parts compressed beyond the elastic limit. The restoration of these parts to their original condition, after the wheel had passed, involved a second expenditure of energy, substantially equal to the first, and coming obviously from the recoverable energy stored in the parts which had not been compressed beyond the elastic limit. Thus each time that the track was depressed momentarily there occurred a cycle of reactions which caused the abstraction of energy from the moving body and its dissipation by friction, etc., in much the same way as in magnetic hysteresis. While there was a theoretical distinction between the train-resistance due to non-yielding inequalities, and that due to track-hysteresis, it was very difficult, if not

Mr. Mailloux.

Mr. Mailloux. impossible, in the present state of knowledge and facilities for measurement, to separate them quantitatively in practice. For this reason, Mr. Mailloux did not see the logic of separating rolling-friction from track-resistance, as the Author had done in his classification. In connection with the Author's reference to air-resistance Mr. Mailloux was amused to find him seriously proposing to estimate the force on the front and rear of a flat plane by the equation $P = 0 \cdot 00254\ \omega\ V^2$, without any correcting factor. In the discussion of Mr. Aspinall's Paper, this same formula had been put forward by several members, and had been accepted, apparently by all, with the exception of Mr. Aspinall himself, who, in the summing up, had gone a step farther in the wrong direction by proposing to increase the constant to 0·0054, his demonstration and formula being substantially those first given by Newton. Two Italian scientists, Drs. G. Finzi and N. Soldati, by a series of brilliant experiments[1] in which the pressure against bodies moving in air was measured manometrically at every point of the surface, had furnished undeniable proof that the theoretical formula above referred to, which was due originally to Euler, gave the correct pressure for one point only, namely, at the centre, in front of the plane. At all other points the actual value was less than this theoretical value. There was, however, a suction effect at the rear of the plane which was ignored entirely by the formula, and which compensated in part for the excess of front-pressure given by it. In certain cases the theoretical formula could, according to Drs. Finzi and Soldati, be used for planes, with a correcting factor which varied between 0·73 and 0·75. For planes which were not at right angles to the line of motion, the theoretical formula required, of course, still further modification by the introduction of trigonometrical coefficients. For bodies of irregular form, the correcting factor became still more complicated. The perusal of the Paper of these scientists left the impression that the theoretical formula should, in general, be taken with more than a grain of salt, in the form of correcting factors. There were two facts, of importance in connection with the estimation of the effects of air-resistance on moving trains, which ought to be known and appreciated more than they were. The first was the discovery—made in the last century by the French investigator Dubuat, and known in French technical literature as "Dubuat's paradox"—that the pressure against a plane was always less when the plane was moved in a perfectly still fluid than

---

[1] "Esperimenti sulla dinamica dei fluidi." Milan, 1903. (Lecture to the College of Engineers and Architects in Milan.)

Mr. Mailloux.

when the fluid was moved against the plane at the same mean velocity. It had been shown by Professor A. Rateau in an article on the Pitot tube,[1] that this phenomenon was due, primarily, to the circumstance that in a current of air the velocity was never uniform for all parts (elements) of the air-current. Since the pressure produced was proportional to the square of the velocity, the total result was proportional, as Professor Rateau had shown mathematically, not to the mean velocity but to the mean of the sum of the squares of the velocity. Professor Rateau had also proved this by ingenious and conclusive experiments, which incidentally explained many of the anomalous and discrepant results obtained when the velocities of fluids were measured by the Pitot tube. He had also explained fully, in the article in question, the conditions and corrections which were essential to render the Pitot tube a reliable instrument for the measurement of the velocities of fluids. This had also been done by Professor F. E. Nipher in America, and by Messrs. Finzi and Soldati in Italy. The second fact above referred to, discovered by von Lössl several years ago, was that when a body, as it moved forward, also oscillated from side to side, the effect was the same as if its effective area were increased. Both of these facts helped out the theoretical formula a little, by making the correcting factor come a little nearer to unity; but they did not, in the majority of cases, make the formula true. It always needed a correcting factor. Mr. Mailloux was of the opinion that no experiments bearing upon the determination of the air-resistance to trains of any length and form had yet been made which had given more practical and valuable information than those of Professor W. F. M. Goss, at Purdue University, made with small models of railway-coaches placed in a tunnel through which a current of air was passed. These experiments[2] had given for the first time, and apparently with a fair degree of precision, the distribution of the total air-resistance at different portions of a train. It was, true that, previously, the experiments of von Lössl, in Germany, with lighted tapers placed in front of moving bodies, had revealed the existence of a mass of stationary air forming a "cone" in front of the moving body. But von Lössl's ideas regarding the distribution of the resisting force of the air were, as was now known, entirely erroneous. He was led, as so many had been, by the seeming plausibility of the hypothesis of Euler, to place too much

---

[1] "Expériences et théories sur le tube de Pitot et sur le moulinet de Woltmann (hydromètres et anémomètres)." *Annales des Mines*, vol. xiii (1898), p. 331.

[2] *The Engineer*, vol. lxxxvi (1898), p. 164.

Mr. Mailloux. reliance on the theoretical formula. His confidence in it was such as to lead him even to ridicule the suggestion that there might be a force due to back pressure or suction, at the rear of a moving body. The quantitative (dynamometric) results furnished by Professor Goss's experiments had not only settled this point, but they had also given certain ratios which were definite and seemed to be very nearly if not quite constant in all comparable cases. They had shown conclusively that the larger portion of the total air-resistance occurred at the front and rear ends of a train; that the air-resistance was virtually constant for all intermediate coaches of the train, that of each being approximately equal to one-tenth of the air-resistance at the head of the train, and a little less than this at the second coach; and that the ratio of the air-resistances at the front and rear ends was substantially constant, independently of the length of train, the air-pressure at the front end being approximately four times greater than that at the rear end, for coaches of ordinary shapes. Mr. Mailloux had found that train-resistance formulas based upon the results of these experiments had, in practice, given results which compared very favourably in precision with those obtained by more pretentious formulas. It was interesting and significant that the formulas of Mr. W. J. Davis, Jun., used by the Railway Department of the General Electric Company, in America, approached very closely, in their amended form (with smaller constants) the formulas based upon the Goss experiments. Up to the present time the investigations of Messrs. Finzi and Soldati and of Professor Goss were in many respects the most valuable of all contributions to knowledge of the subject of air-resistance. It was obvious from the preceding comments on the theoretical formula used by the Author, that all his calculated values for air-resistance were likely to be inaccurate, being probably too high in most cases. His values for journal-friction were obviously too low, in most cases, at the higher speeds. Consequently, the curves representing the other elements of train-resistance were likely to be wrong, because they represented values which were too small. In conclusion, a word of caution might be said about resistance-equations. The Maclaurin power-series $y = A + Bx + Cx^2$ had always been popular with designers of new train-resistance formulas, and, to do it justice, it had done good service. It was undesirable, however, to dignify this equation with the term general equation until it could be completed by the introduction of a term which would provide for starting-resistance. The complete general equation for train-resistance would be one which gave, at zero velocity not the value 0, but a value equal to the starting (static) resistance.

Mr. Mailloux. In order to do this, there must be introduced a hyperbolic term, or a term in which the velocity appeared with a negative exponent, thus

$$R = A + Bx + Cx^2 + Dx^{-n}.$$

Mr. Mailloux had also found that the train-resistance curve could, in some cases, be expressed symbolically by an equation of the form

$$R = A + \frac{B}{(x + a)^n} + Cx^m,$$

where m was a non-integral exponent, whose value ranged between 1·6, and 2, while n had a value ranging between 1 and 2, and a was a constant. The objection to such formulas was that it was more difficult to determine and to use a non-integral exponent than an integral exponent. Moreover, this exponent might change for the same formula, when the conditions (length and weight of train, etc.) changed. The Maclaurin type of formula was, for this reason, preferable. In using such formulas, he added a virtual-gradient term G, to which different values were assigned according to the quality and the condition of the track.[1] It was obvious that G could be made to include the virtual gradient due to snow, mud, or in general, to any cause affecting the rolling-resistance. In this way, the scope of any train-resistance formula might be greatly increased.

Mr. Mallock. Mr. A. **Mallock** observed that the Author represented the resistance by an expression of the form $A + Bx + Cx^2$. Any curve (with the proper limitations as to the character of the differential coefficients) could be approximated to by such a formula; but the methods by which relative values of the coefficients were obtained was open to objection. He would merely refer to the notes in the Appendix which contained the Author's theory. Note 1 took no account of the resilience of the track and assumed that the whole work done in depressing the track was lost. What was really lost was only the difference between the work done on the track during compression and that given back by the track during expansion. If a weight were rolled over a perfectly resilient track, no work was required, whatever might be the depression caused by the load. Note 2 was unsatisfactory in several ways. He would merely point out that the Author took the flange-action he described to be continuous, but observation showed that where the line was straight the flanges came into play alternately on either side and for considerable intervals of time were not engaged at all; hence it was not

---

[1] See W. C. Gottshall, "Notes on Railway Economics and Preliminary Engineering," 2nd ed., p. 157. New York, 1904.

Mr. Mallock. quite correct to take the mean of the extra tractive force due to flange-friction as equal to its maximum value. Further, the inertia of the mass of the carriage could only increase the lateral pressure between the flange and the rail for a small fraction of the time during which the plane of the wheels was changing in azimuth. What chiefly determined and limited the lateral force on the flange was the resistance of the tread of the wheel to lateral slip. The Author did not notice a source of resistance which generally, Mr. Mallock thought, outweighed the flange-effect, namely, the slipping which must occur between wheel and rail due to the difference of the effective diameters of each pair of wheels when the flanges were not equidistant from the rails. This difference depended of course on the degree to which the tires were coned.

Mr. Sayers. Mr. **Henry M. Sayers** remarked that the principal novelty in the Paper was the Author's analysis of train-resistance into the three parts described, and in particular his attempt to identify that part of the resistance which varied as the first power of the velocity with "flange-action," measured by certain proportions and the mass of the truck or coach. Referring to Note 2 of the Appendix, the first assumption there made was that the truck ran with an average or mean inclination to the rail depending upon the length of the wheel-base and the play or clearance between the flanges and the rails. This meant that the truck was constantly swinging about the pivotal point, and that some two flanges were bearing against the rails throughout the whole of a run on straight track. It also meant that, other things being equal, the resistance due to this action was proportional to the play or clearance. These propositions would not be readily accepted. First, the coning of wheel-treads, and the damping effect of bolster- or pivot-friction, assisted trucks to run for considerable distances on straight tracks without the flanges touching the rails at all. The good running-qualities of different trucks were largely dependent upon the suppression of swinging, especially when the trucks were heavily loaded with motors, and freedom from swinging-propensities should be aimed at in their design. Secondly, while long wheel-bases and small flange-play were no doubt conducive to minimum resistance on straight tracks, this was not so on curves. There the wheel-base and play should be such that no flange-grinding occurred, except that inseparable from the guiding action of the rail upon the truck. The radius of the sharpest curve traversed was therefore the factor which should regulate the ratio of play to wheel-base, and probably this accounted for some of the differences noted in the practice on various railways. Rolling stock must be designed to suit the more difficult rather than the easier circumstances; so that even if the Author's principle were true for straight lines, the increase of wheel-base and diminution of play, while diminishing train-resistance on straight track, might increase it seriously on curves and prove to be, on the whole, a disadvantage. Mr. Sayers.

Mr. W. N. SMITH, of New York, wished that the time at his disposal had been sufficient to examine more experimental data than was possible under the circumstances. The Author's method was so carefully worked out that it would be desirable to compare by means of it all the experimental results obtainable. Train-resistance in general might be regarded in two aspects: first, the general degree in which it entered into railway-working, and secondly, the computation of resistance in detail as shown in the Paper. In the work of predetermining the motive power of electric trains, it was necessary to take account as closely as possible of the actual conditions imposed upon the operation of a given train or trains. The most important things affected by the train-resistance were the capacity of the propelling motors and the speed of the train. On a relatively small system, involving comparatively few motor-coaches for interurban trolley-service, as it was commonly called in the United States, the question of train-resistance was of relatively small moment, and any one of several formulas that had been fairly well verified by practice would answer every purpose in all the predetermination work that was called for by such a problem. Work of this sort constituted the large majority of electric-railway problems commonly met with. But when it became necessary to make careful estimates for very large railway-electrification problems, the amount of equipment involved was so large, and the congestion of traffic and the short headway between trains required such close estimates of speed, that considerable attention must be given to this particular factor. Its importance was greater where the conditions required a relatively large amount of coasting from maximum speed to the point of brake-application, and it was in this connection that a close study of train-resistance might enter into the predetermination of the motor-capacity, especially when it became necessary to estimate very closely the performance that could be expected of some particular motor, which, for reasons of space occupied, or other motives of economy, it was desired to use. Here again, however, it had been usual to employ one of several resistance-formulas that had been fairly well borne out by experiment. Predetermination of the power and equipment for a railway-system was based upon so large a number of variables, many of which had to be assumed in order to arrive at a result, that Mr. Smith.

Mr. Smith. the error introduced by a slightly incorrect assumption of train-resistance eventually became a relatively much smaller source of error in the entire problem. To that extent the exact determination of train-resistance could not justly be regarded as of greater importance than the determination of numerous other factors, any of which were likely to be slightly inaccurate in preliminary estimates. The method proposed by the Author was the best instance known to Mr. Smith in which the conditions entering into the total result had been carefully taken into separate account, and given their due proportion in making up the total result. It had usually been considered that some one formula could be produced, fitting practically all conditions, although the failure of most of the previous attempts in this direction was generally recognized. The Author's method should therefore merit careful consideration from railway-engineers, for he had brought it down to a very reasonable and practical basis. The only check Mr. Smith had been able to make, where all the conditions were known that could be compared in the manner outlined by the Author, was in the case of a test record of a single motor-coach, and this showed that the Author's method agreed fairly closely with actual practice; but neither in this case, nor in two others where such checking was attempted, had the resistance calculated by the Author's method been as high as the actual resistance, nor as close as when calculated by either the Sprague formula, or that proposed by Mr. Smith.[1] The constants calculated by the Author's method did not seem quite large enough for motor-coaches; though checking of still other results might modify this conclusion. Further investigation should be made of the resistances of trucks carrying electric motors, and particularly of the effect of the gear- and pinion-friction in the motor. When a train was coasting the gear- and pinion-friction constituted a resistance that had to be estimated separately. Though usually included in the motor characteristic curves which gave the net tractive effort of the motor (after transmission through gears and pinions), while current was being applied during acceleration and full-speed running, it entered in as an extra resistance after the current was cut off and the train was coasting, and must then be added to the other elements of train-resistance. It seemed exceedingly difficult to estimate at its proper value the flange-wear of the wheels. On a new equipment

---

[1] O. S. Lyford, jun., and W. N. Smith, "Problems of Heavy Electric Traction," *Transactions of the American Institute of Electrical Engineers, vol. xxiii (1905)*, p. 691.

Mr. Smith.

the flange-wear was practically nothing, but within a few years flanges in heavy service frequently wore sharp, sometimes only on one side of the truck, and the wheel-treads assumed noticeably different shapes, which could not but affect the flange-action of the trucks as computed by the Author. It would be interesting to know whether any attempt had been made to compare the flange-action of a new truck with that of an old one in which the treads and flanges have been subjected to considerable wear. The effect of air-resistance on the sides and roofs of the coaches was further complicated by the presence of eddies in the air at the spaces between the coaches. Anyone who had ever walked from one coach to another of an American train in motion knew that there was a violent circulation of air across the platforms at the ends of the cars, and whether this effect might or might not be susceptible of mathematical treatment, it seemed to Mr. Smith that some information about it might be ascertained experimentally. Further tests along the lines followed by the St. Louis Test Commission could profitably be made, with the test-car placed in the middle of a train, as well as at either the front or the rear. It was quite possible that the air-resistance of the second, third, or fourth car in a train of motor-coaches or of other cars might be a different quantity in each case, and the effect of the position of a car in a train was worthy of further study. The large railway-problems of the future involving close estimates of train-resistance were in all probability those wherein electric motive power would be called into play, and for that reason all experimental information bearing upon the subject of electric motor-coach propulsion should be analysed in just such a careful and scientific manner as had been done by the Author, with data from various kinds of coaches, trucks, and types of motive power. In conclusion, he congratulated The Institution and the Author upon the clear, simple, and scientific treatment of this complex subject.

Mr. Taylor.

Mr. H. B. TAYLOR observed that the Paper was of very practical benefit to railway men, and would help towards more efficient working of railways by pointedly illustrating the relative values of the different elements of train-resistance. For instance, the fact that flange-resistance varied as the side play showed the importance of keeping this play small; but Mr. Taylor had met engineers who insisted on the track being laid slack to gauge on the straight. He had known permanent-way inspectors in India who advocated keeping one rail of the track slightly lower than the other, to ensure true steady running. This would be achieved; but if the cant were considerable, side friction would occur from the flange pressing against the lower rail. He would like to hear the

x 2

Mr. Taylor. Author's views on this point. It would be interesting to know what was the side play on the Brompton and Piccadilly tube stock compared with that on the stock on the District Railway. In the tube the running was very smooth, while on the District line, standing at the end of a coach, it was difficult at times to keep one's feet. He would like the Author to go farther and deal with curve-resistance in a similar manner. On curves there arose, in addition to others, a new resistance, buffer-friction, which was very considerable with long bogie-coaches on sharp **S** curves. It was within his own experience, when dealing with construction-trains on the temporary Bolan Railway, that, though on the straight a bogie-truck was reckoned as load equal to two four-wheeled vehicles, on sharp curves the train-load was in favour of four-wheeled vehicles. These curves were usually coupled with the limiting gradient of 1 in 20, and a long coach on such a track no doubt got a twist which added to the train-resistance. He drew attention to this, as bogie-trucks were usually considered advantageous on a railway with sharp curves; but the advantage would be lost on hill-railways if the length of the stock were unduly increased. The Author pointed to the advantage of roller-bearings in the running of goods-stock at low speeds. A point to note in practice was that, through careless shunting and other accidents in yards, breakage of axle-boxes was continually occurring. Such special axle-boxes cost, he believed, £5 more than an ordinary box.

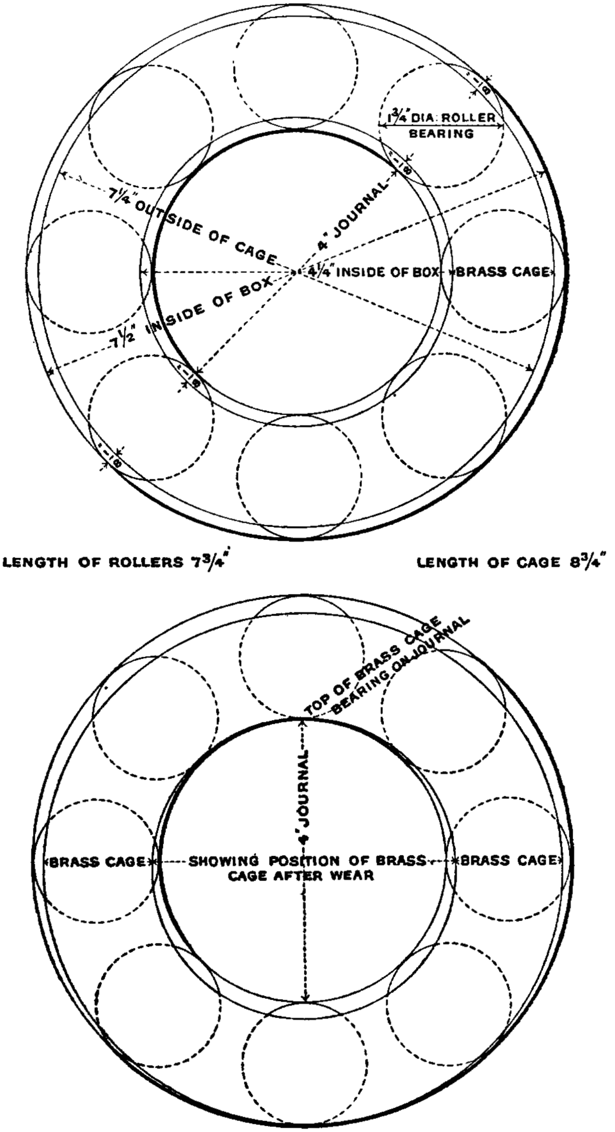
Mr. Williams. Mr. J. P. WILLIAMS, Officiating Locomotive Superintendent of the Eastern Bengal State Railway, forwarded through Mr. W. R. Haughton, the Engineer-in-Chief of that line, eight sun-prints showing how roller-bearings had worn and rolled the journals.[1] In January, 1899, two first- and second-class composite coaches (with wooden underframes and non-vacuum brakes) were fitted with roller-bearings, and each ran about 165,000 miles in 4½ years. The journals had remained parallel and showed very little wear, but the cages had come down as shown in *Figs. 17*, which also showed the same bearing when new. These cages were replaced by others in which the outside diameter was reduced to 7 1/16 inches and the inside diameter increased to 4⅜ inches, thus increasing the clearance between the cage and the journal. These

---

[1] The journals in question were all originally 4 inches in diameter and about 9 inches long. Many of them have worn taper, the reduction of diameter at the smaller end—which in some cases is the inner and in others the outer end of the journals—varies from 1/32 inch to 1 inch. Each journal was fitted with eight rollers (except in one case where twelve rollers were fitted) and the prints show the results of 7½ months' running under bogie-coaches.—SEC. INST. C.E.

Mr. Williams.

Figs. 17.

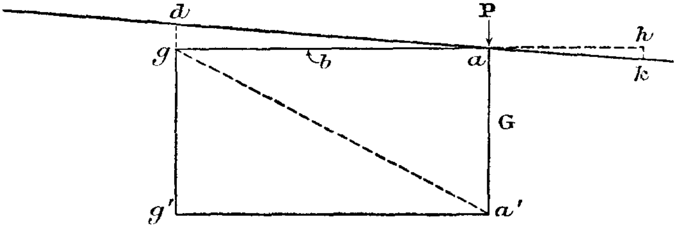


roller-bearings were discarded in September, 1906, the journals having worn taper, and brass bearings were substituted.

Mr. Wolley-Dod.

Mr. F. Wolley-Dod thought that there was an error in the method adopted by the Author in computing the resistance due to "flange-action," the item B*v* in equation 7 on p. 233. On p. 230 it was stated that vehicles continually moved at a slight angle to the rail. It would be more accurate to say that they continually oscillated. During a part of each oscillation the wheels ran free, but diagonally to the rails; during the second part, the leading wheel ran up against the rail, and a horizontal reaction, such as P in *Fig. 12*, on p. 264, was generated, which rotated the vehicle or bogie about a vertical axis, and sent it off again towards the other rail; the third and fourth parts of each oscillation brought a repetition in the reverse direction of the first and second. The magnitude of the reaction P was determined not so much by the inertia of the vehicle or bogie about the axis round which it was rotated, as by the moment about that axis of the frictional resistance of the wheels on the rails. Moreover, the "train-resistance" had little to do with the magnitude of P, as there was very little rubbing between wheel and rail at the point at which the reaction acted, and the work absorbed was of the same nature as rolling-resistance, which was stated on p. 230 to be a negligible quantity. The chief item in the resistance caused by this oscillation was the work done in sliding the wheels during each rotation. The amount of this sliding might be ascertained thus. In *Fig. 18*, which was *Fig. 12* of the Appendix, completed to show the other two wheels, $a\,g = a'\,g'$ = the wheel-base b, and $a\,a' = g\,g'$ = the distance between centre of rails G. If the weight at each of the four points a, a', g, and g' were W/4 and μ were the coefficient of friction, the force necessary to make any one of the wheels slide on the rail was μ W/4. A horizontal force such as P would cause each wheel to slide about g or g' as centre, provided the wheel-base b was more than about $1\frac{1}{4}$ times the distance between the centres of the rails, G. Taking moments about this point

Fig. 18.



$$P \times b = \frac{\mu\,W}{4}\left(b + G + \sqrt{b^2 + G^2}\right).$$

Mr. Wolley-Dod.

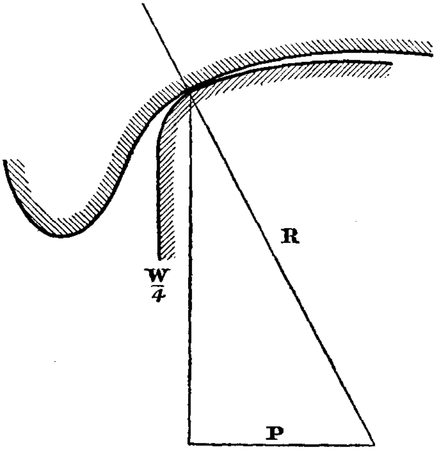
(If b was less than about $1\frac{1}{4}$ times the gauge, the sliding would take place about a vertical axis situated about $1 \cdot 5$ G behind the leading axle, and the numerical value of P would be greater, as all four wheels would slide). Taking the Lancashire and Yorkshire bogie-vehicles, in which $b = 6 \cdot 5$ feet and $G =$ about $4 \cdot 9$ feet, $P = 0 \cdot 75\ \mu\,W$, W being the total weight on all four wheels of the bogie. This value of P applied only to those particular dimensions; if the wheel-base was increased, P was decreased, and vice versa. But for any possible proportions, velocity, and coefficient of friction, the value of P was considerably greater than that required to overcome inertia by the Author's formula on p. 264. During each complete oscillation the vehicle or bogie was deflected, first through twice the angle $d\,a\,g$; it then ran across against the other rail, when it was deflected back again through the same angle, and the work done was equal to the sum of the distances which each wheel slid during the two deflections multiplied by μ W/4, which if all wheels ran on the same diameter was

$$\frac{4 \times \text{length of arc } d\,g}{\text{wheel-base } a\,g} \times \frac{\mu\,W}{4}\left(b + G + \sqrt{b^2 + G^2}\right).$$

Taking $d\,g$ as $\frac{1}{16}$ foot, which it was stated to be in the Lancashire and Yorkshire bogies, this was $0 \cdot 1875\ \mu W$ units of work expended while the vehicle traversed a distance corresponding with one complete oscillation, and the average resistance was this amount of work divided by this distance. Here arose the difficulty of ascertaining the distance, and on examining the details, it was found that both the distance and the possible degree of obliquity depended on a number of items, comparatively small variations in any one of which would affect the result appreciably. The most important were the clearance and the wheel-base; but even with a given wheel-base, and a nominally constant clearance, speed, and all other conditions, the upper line in *Fig. 9* (p. 259) showed considerable variation in both the amplitude and the period of each oscillation. Whatever radius were given originally to the fillet between the flange and the tread of a wheel, and to the shoulders of the rail, the result of wear was such that, when the flange approached the rail, the radius of the flange at the point of mutual contact was greater than that of the shoulder of the rail. The radius of the fillet in new wheels was nearly always made slightly larger than that of the rail-shoulder. The result was that, as the flange approached the rail, the fillet tended to ride on the shoulder of the rail, and to lift the tread clear of the rail-head.

Mr. Wolley-Dod. These conditions were shown in a very exaggerated form in *Fig. 19*. The reaction between wheel and rail took place along R, the vertical component of which was W/4, the weight on the wheel, and the horizontal component was P, the lateral force tending to rotate the vehicle or bogie about a vertical axis. The wheel continued to mount, and the inclination of R to increase, until the horizontal component P became large enough to produce rotation in opposition to the sum of the resistance of the wheels to sliding; and the axis about which this sliding took place was determined by the minimum value of P, the moment of which about that axis was equal to the moment of the resistances. But the wheel would continue to mount and P to increase after the sliding began, because no force however great could change with absolute suddenness the direction in which the wheels were travelling. Further, in order to do work, P must be in excess of the resistance, and it must also overcome the inertia before it could accomplish the work; on the other hand, directly sliding commenced, the coefficient of friction, and consequently the resistance, was reduced. The fact that inertia had to be overcome was one of the chief reasons why oscillation once started would continue; it was also one reason why the moment of inertia of a vehicle or bogie about a vertical axis was a factor affecting the amplitude and violence of oscillation. The wheel which thus ran on its fillet traversed a greater distance in one revolution (or in a given fraction of a revolution)

Fig. 19.



Mr. Wolley-Dod. than the other wheel on the same axle; this reduced the distance which the wheels had to slide, and gave rise to a rotation couple tending to reduce the other forces required to produce rotation. The *trailing* wheels of a vehicle or bogie were less liable to violent oscillation than the leading, and their movements did not necessarily synchronize with them; when the leading wheel was in the position shown, the trailing wheels might be out of centre either towards the opposite rail, or towards the same rail, or in an intermediate position, and this consideration affected the obliquity of the whole vehicle, and consequently the angle through which it was deflected at each oscillation. The speed of the train was unquestionably a factor which affected the violence of oscillation, and consequent resistance, and it was a well-ascertained fact that a given vehicle on a given track frequently ran more smoothly at a high speed than at a low one. An item of train-resistance not noticed by the Author, which was affected by the moment of inertia of a vehicle as compared with that of a bogie-truck, as well as by the speed of the train, was the work lost in producing local depression of the rails, and overcoming friction of springs, axle-boxes, etc., as a result of the pitching of the vehicles. The average resistance due to these was considerably less per gross ton for bogie-vehicles than for four-wheeled stock, but on good track the total resistance from this cause was probably a comparatively small fraction of the total train-resistance. As a consequence of assuming the Cv^2 item of the resistance—in which he included only air-resistance, though probably an appreciable amount of "miscellaneous resistance" varied as v^2—to have been the same per ton for the bogie-vehicles as for the four-wheeled vehicles on the Northern Railway of France, the Author had too large a value for the Bv items. This error was noticeable in *Fig. 4* of the Paper. If C were slightly increased and B slightly reduced, the thick line would correspond more closely with the circles in this diagram.

The Author. The AUTHOR, in reply, observed that Mr. Armstrong was quite right in pointing out that the formula he gave was applicable only to the particular type of coaches tested. The Author had not the necessary data to enable him to work out the resistance of these trains by the method given in the Paper, but Mr. Armstrong could easily satisfy himself of its applicability to the trains in question. The Author had endeavoured to secure the data of the cars tested by Mr. Smith, but was unable to ascertain the most important figure, in his estimation, namely, the actual clearance between the rail and the flange of the wheels, as tested. Unfortunately, this was a dimension rarely noted, and the Author agreed with Mr. Smith in

The Author. his remarks as to the influence of the wear of the flanges on the resistance. He would be much interested to see some confirmation of Mr. Cardew's opinion that the oscillation of the bogies diminished as the length between bogie-pivots increased, and that it became of practically no importance in very long vehicles, as it was contrary to his experience, which went to show that there was nothing in lengthening the coach, *per se*, that tended to steady the bogie. He would of course admit that increased weight on the bogie-pivot helped to steady the bogie, as had been pointed out by Mr. Sayers, but the friction thus produced was quite as likely as not to hold the bogie permanently over in one direction, so that there was no real saving in flange-resistance. The oscillograms given in *Fig. 9* had been taken from a record of a continuous run of about 35 miles on an English main line at speeds ranging from 40 to 50 miles per hour; and they represented very fairly the oscillations of the bogies over the whole of the run, indicating that Mr. Cardew's view that such oscillations only took place under exceptional circumstances required some modification. The classification of the elements of train-resistance proposed by Mr. Mailloux was of interest from an academic standpoint, but it did not appear to add much to a clear comprehension of the relations of these elements from a practical point of view. It might be true that the friction of ties and ballast and the friction of the air on the train were both included under the expression "fluid friction," but it was difficult to see what practical result could be achieved from this grouping. In the same way flange-friction and journal-friction were no doubt both cases of sliding-friction, but beyond that they had nothing whatever to do with one another. Mr. Mailloux seemed to think that the form of equation $R = A + Bv + Cv^2$ adopted in the Paper was accountable for the classification used by the Author, whereas the reverse was the case: the Author had examined the most important elements of resistance and grouped them according to their relation to the speed, and he submitted that this was the only practical method of classification. He would point out, however, that the value of this method of classification lay in the help that it afforded in the interpretation of the results of tests as expressed by curves. Thus, for example, when it was known that the form of every resistance-curve was that given above, an inspection of the curves in *Fig. 2* showed that the difference in the resistance of four-wheeled and bogie coaches was due to that element which varied as the first power of the speed, that was, to flange-action, a result of the greatest practical importance. The Author deprecated the use of formulas in the manner suggested by Mr. Mailloux. The attempt

The Author. to represent by a formula the resistance of a train was justified only so long as it was recognized that the formula could refer to that train and no other, unless it were made up of rolling stock of identical character in all respects. There was of course no difficulty whatever in devising a formula that should represent the results of any given test or set of tests: as Mr. Mailloux had done. But such formulas threw no light upon the problem of the predetermination of train-resistance for trains of a different character. A glance at *Fig. 8* in the Paper was sufficient to show how hopeless was the attempt to obtain a formula that should represent even approximately the resistance of trains such as those there referred to. The Author had endeavoured to trace to their causes the different elements of train-resistance, and to determine from actual tests, not by calculation, in what way these elements depended upon the rolling stock employed, so as to make the results universally applicable. With regard to Mr. Mailloux's objection to the view that journal-friction was independent of the load, the Author had shown that with perfect lubrication the coefficient of friction varied inversely as the pressure, but that with lubrication of the character usually employed on railways this did not hold, and the friction was practically constant within the range of pressures used. This fact was entirely borne out by laboratory tests, when made under the proper lubricating conditions, such as those described by the Author: the tests referred to by Mr. Mailloux had not been made under these conditions and were therefore not applicable. It was impossible to look to this cause as the explanation of the fact that a loaded goods-wagon was easier to haul than the same wagon empty. The Author submitted that the true explanation was to be found in the difference of the flange-action in the two cases. Mr. Mailloux also dissented from the view that journal-friction was independent of the speed, and referred to the admittedly high value of journal-friction at starting in proof of his contention. The Author was quite aware of the high value of what was called "starting-resistance," and had always taken this into account in calculating the acceleration of trains.[1] The Paper, however, dealt only with the resistance of trains running at a uniform speed, and not with the conditions when starting. The high initial value of journal-friction was maintained only for a few revolutions of the axle, and after that all investigators were agreed that journal-friction was practically independent of the speed. It was quite true that, as Mr. Mallock had pointed out, in estimating the amount of track-resistance, the Author had taken no

---

[1] See C. A. Carus-Wilson, "Electrodynamics," p. 176. London, 1898.

The Author. account of the resilience of the track. It was difficult to estimate how much of the work done in compressing the track was given back in consequence of its resilience, although it was certain that an ordinary railway-track was very far from being perfectly resilient, as Mr. Mailloux had observed, and the Author had intentionally assumed that none of the work was returned. He had shown that track-resistance was negligible in the case of long trains, and need only be taken account of with single coaches or motor-coaches with one or two trailers. If the track was actually to any practicable extent resilient, the effect would be still further to reduce the amount of this item of resistance. In discussing rolling-resistance Mr. Mailloux had objected to the Author's use of the term "rolling-friction." The Author would point out, however, that the rolling-resistance dealt with by Mr. Mailloux was not the same thing as the rolling-friction referred to in the Paper. Rolling-friction was defined in the Paper as "the resistance to the motion of a wheel rolling on a clean smooth rail arising from the elastic indentation of the rail," the resistance offered being due to "the consequent friction as the rail rubs over the surface of the wheel in its endeavour to regain its normal level." The term friction was quite accurately attached to this action, and was not a misnomer, or misleading, although it might be attributable to the last century, and was due, as stated in the Paper, to Professor Osborne Reynolds. But Mr. Mailloux had overlooked the limitation involved in the definition—a wheel rolling on "a clean smooth rail," which the Author considered was a fair description of the conditions existing on a main line of railway in first-class order, the conditions, in fact, which were assumed throughout the Paper. Under such conditions, rolling-friction was, as the Author had shown, a negligible quantity when compared with journal-friction. Now the rolling resistance dealt with by Mr. Mailloux was the resistance offered by sand, mud, dust, refuse and dirt as commonly found on a street-railway track, under conditions totally different from those considered in the Paper. The Author was aware that these conditions, coupled with others peculiar to street-railways, resulted in greatly augmenting the tractive resistance, but he had confined his remarks in the Paper exclusively to dealing with ordinary railway conditions, and had made no attempt to take up the question of the resistance to traction on street-railways, which would involve the introduction of entirely new factors, such as the rolling-resistance dealt with by Mr. Mailloux. He could say, however, that rolling-resistance alone was not enough to account for the increased resistance of street-railways, since when the tracks were thoroughly clean and free from

The Author.

dirt and obstruction, the tractive-resistance was still far greater than that on an ordinary railway. The question was admittedly one of great interest and importance, but it should not and need not be mixed up with the question of train-resistance on railways. Mr. Taylor had raised an interesting point when alluding to the practice of some engineers in India, of keeping one rail of the track slightly lower than the other, to ensure steady running. Undoubtedly this would tend to reduce the oscillation, but it would probably produce uneven wear of the flanges, which was undesirable from many points of view. With regard to Mr. Wolley-Dod's remarks, the Author had purposely avoided in the Paper going into the question as to the character of the friction set up by flange-action, as it involved a number of considerations of a complicated nature that were foreign to the main question at issue: he had contented himself with showing that flange-action produced a lateral pressure on the rail, causing a loss of energy in friction. The explanation offered by Mr. Wolley-Dod was, in the Author's opinion, correct in as far as it attributed the frictional loss to a certain sliding that took place, but not as regarded the nature of that sliding, which was much more complicated than Mr. Wolley-Dod supposed, though its precise character did not affect the general conclusions drawn in the Paper as to the effect of flange-action. With regard to observations by Mr. Mallock, Mr. Sayers, and Mr. Wolley-Dod on the subject of flange-action, the Author had made no endeavour to calculate the amount of such action: his object had been to show that the resistance caused by flange-action was proportional to the velocity, to the clearance, and to the mass, and inversely proportional to the wheel-base, and this proportionality was not affected by the length of time during which the flange was inclined to the rail. The actual amount of the action was determined by the results of tests on trains in motion, the continually varying position of the flange with respect to the rail being, as it were, integrated and summed up in the effect on the draw-bar. Mr. Mallock and Mr. Sayers had also referred to the influence of coning the wheels on the resistance. Coning had not so great an influence as was often imagined. For example, in the wheels shown in *Fig. 11*, giving the section used on the Bengal-Nagpur Railway, the tires were coned to an inclination of 1 in 20, while the total clearance between flange and rail was $\frac{5}{8}$ inch. Hence the maximum possible radial difference was 0·0313 inch, or about 0·1 per cent. of the diameter. The mean effective difference would be a small portion of this maximum, making the resistance due to such slipping a negligible quantity. Mr. Mailloux was mistaken in supposing that the Author was not

The Author. fully aware of the real meaning of equation 6 (p. 231) relating to air-resistance, a further reference to which would be found in the Author's reply upon the Discussion. The fact that this equation gave the pressure at the centre of the windward side of a large plate, attributed by Mr. Mailloux to Messrs. Finzi and Soldati, had been well known in England since the publication of Dr. T. E. Stanton's researches.[1] The values given in the Paper by the Author for air-resistance encountered by trains were not, however, in any way derived from this equation, but from the results of actual experiments on trains in motion, namely, the experiments made by the St. Louis Electric Railway Test Commission and by Mr. Barbier on the Northern of France Railway. Mr. Mailloux appeared to disregard these in preference for those conducted by Professor Goss, whose results he had adopted in the train-resistance formulas he himself made use of. The Author had for long been well acquainted with the experiments of Professor Goss. These were made on models of flat-ended box cars, the actual dimensions being $12\frac{1}{16}$ inches long, $3\frac{3}{8}$ inches wide, and $4\frac{1}{2}$ inches high. The results obtained when estimated in pounds per square foot of exposed cross section at 60 miles per hour were:—For the pressure in front, 3·75 lbs., for the suction behind, 0·55 lb., making together 4·30 lbs. The results of the St. Louis tests made on a car in motion showed that the pressure in pounds per square foot of cross section at 60 miles per hour on a leading vestibule with a flat profile was 8·20 lbs., with a suction of 0·50 lb. on the rear vestibule, making a total of 8·70 lbs., or more than double the value obtained by Professor Goss. The results for vestibules of other profiles were given in Table I in the Paper, showing that with an actual car in motion the value 4·30 corresponded with a profile between parabolic and standard shape, or was 27 per cent. less than that due to a vestibule of standard profile. It thus happened that the application of Professor Goss's values to vestibules of standard profile, although obtained from and strictly applicable only to flat profiles, was not attended with more than this amount of error. The Author preferred to use results obtained by testing coaches in motion rather than those deduced from experiments on small stationary models. With regard to side resistance, Professor Goss had ascertained that the force on each intermediate model in pounds was given by the equation $F = 0 \cdot 000010\ V^2$, the speed being in

---

[1] "On the Resistance of Plane Surfaces in a Uniform Current of Air." Minutes of Proceedings Inst. C.E., vol. clvi, p. 78.

The Author.

miles per hour. The exposed area of each model, that was, the roof and two sides, the bottom being screened from the air, amounted to 149 square inches, making the drag due to air-friction equivalent to 35 lbs. per thousand square feet of exposed area. Now Mr. Barbier's tests, as shown in the Paper, indicated a drag equal to 79 lbs. per thousand feet, or more than double that obtained by Professor Goss. The explanation of the difference was obvious. Mr. Barbier's tests were made on ordinary railway-coaches whose sides and tops were covered with irregularities due to fittings, mouldings, etc., resulting in greatly increased friction. The models employed by Professor Goss were made of smooth painted tin, which offered very little frictional resistance, amounting to only 35 lbs. per thousand square feet, a figure only 17 per cent. higher than that observed in the pneumatic-despatch tubes of New York, which had their inside surfaces carefully machined. It was clear that such a low value could not be taken as applicable to ordinary railway-trains. Since with a long passenger-train the side and top friction constituted the most important element of air-resistance, the error in using the values obtained in the experiments with the tin models would be very large.