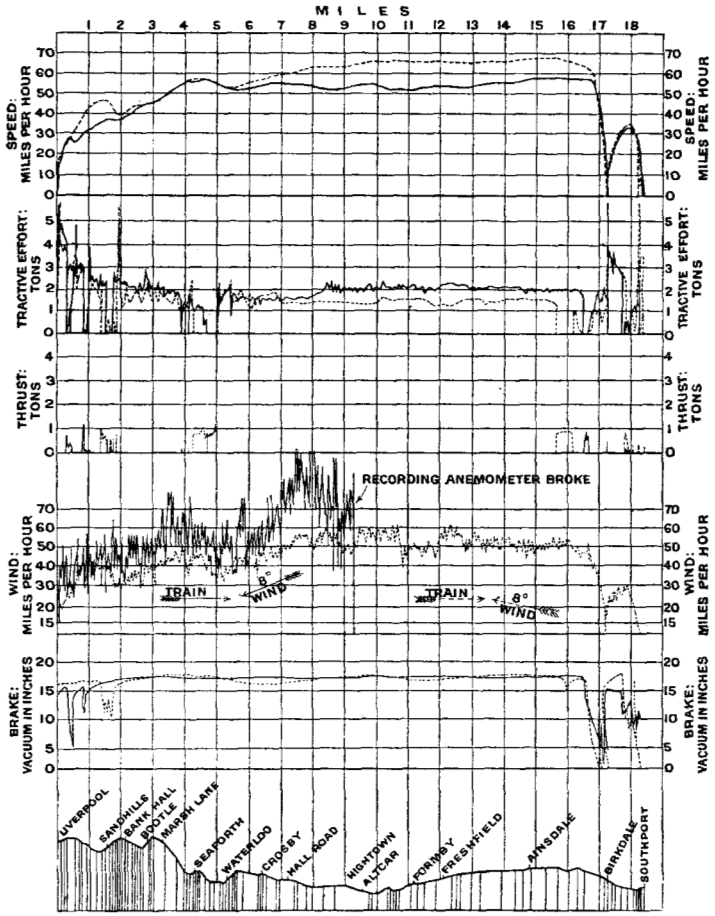
Discussion.

The President. The PRESIDENT, in moving a vote of thanks to the Author for the Paper, observed that the information given was so full that it would afford data to any one who wished to investigate specially any particular point within the scope of the Author's researches.

The Author. The AUTHOR remarked, in reference to the Table of experiments with engine No. 1,392 (p. 185), which gave, in the fourth column, the horse-power from dynamometer-diagrams, that it might be possible to show that the figures did not bear out some of the results given in the diagrams of resistance; but the figures in the Table included acceleration, while those from which the formula had been deduced did not. The figures in the Table were only put forward for the purpose of showing the difference at any given instant between the pull, as registered by the dynamometer, and the horse-power indicated by the ordinary steam-engine indicator, which was not a particularly reliable instrument. At pp. 158 and 162 of the Paper there were references to the method of coupling the engine to the dynamometer-car, that at p. 158 referring to a rigid coupling, and that at p. 162 suggesting that there might be some slack. The slack spoken of was meant to refer only to ordinary trains, and not to the experiments described. He wished to direct special attention to Fig. 38, Plate 3, which showed the influence of the weight of the train; from it could be traced at once the variation of pull upon the draw-bar. The curves in Figs. 12–25, Plate 3, related to coasting-experiments and commenced from the right-hand side of the diagram, most of the other curves in the Paper commencing on the left side. The records taken with the carriages—and sometimes with the engine—were started at the highest speed, and the curves showed the variation in the resistance as the train gradually came to rest. The diagrams illustrated distinctly the great variation in resistance due to the crowding of the vehicles as they pushed one another forward. The effect of the wind was shown by the difference between the resistance indicated by the full line and by the dotted line in each diagram, which lines referred to runs with and against the wind, respectively; and as most people had probably at some time or other ridden a cycle, wind-pressure could be appreciated more now than it had been in the past. *Figs. 42* and *42a* were records of a particular journey between Liverpool and Southport, which

had taken place on the night of the 28th December, 1900, in a tremendous gale, which had blown down one of the stones at Stonehenge. The full lines in the diagram were the records for the night of the gale; the dotted lines were the records of a similar journey on the 31st December, when there had been no The Author.

Fig. 42.



gale: and there was a very great difference between the tractive effort in the two cases. It would be seen that on the 28th December the maximum speed was 57 miles per hour; on the 31st, 67 miles per hour. On the 28th the resistance was 38 lbs. per ton; on the 31st, 21½ lbs. per ton. The average

The Author. horse-power developed on the windy night had been 100 HP. more than on the 31st December. The wind-resistance alone had absorbed 238 HP., and the draw-bar pull had been equal to 4·55 HP. per ton of load, the train weighing 150 tons. Unfortunately the anemometer had broken when the journey was about half over and when the wind was registering something like 100 miles per hour, so that it had not been possible to continue that portion of the experiment. The U tubes fixed in the car, and filled with coloured water, had had all the water blown out of them by tremendous gusts of wind, just after they had been recording wind-pressure of between 33 lbs. and 35 lbs. per square foot. Allowing for the difference in the speed on those two nights, the tractive resistance had been increased 87 per cent. by the wind, which showed what the locomotive must be called upon to respond to in bad weather. A locomotive was not a fair-weather machine only, but must be sufficiently powerful to deal with excessive loads put upon it by wind and bad weather. With regard to the formulas in Appendix II., it was quite possible that the figures, given by some American investigators, although they had hitherto been considered extremely low, were correct for their class of car, because heavy cars proved to need less tractive effort per ton. Mr. Sinclair, an American authority, gave a curve very similar to the one given in the Paper. Mr. Barbier, for a ten-coach train, also showed a similar curve. Mr. Barnes's figures were somewhat lower, and probably they referred to some of the large and heavy American sleeping- or dining-cars. Recently some records of train-resistance obtained on the London and North Western Railway had been

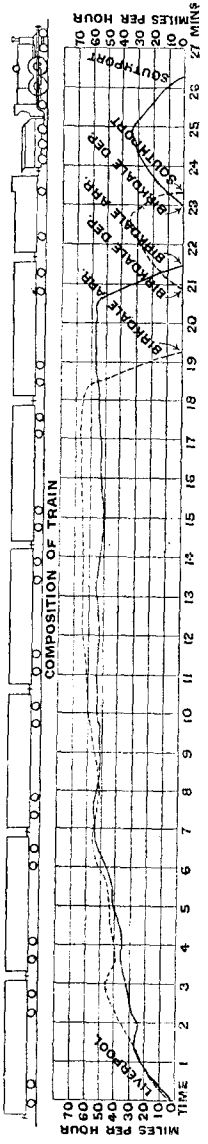


Fig. 42a.

published;[1] but he was not sure that those figures were absolutely correct, although they approximated very closely to his own. No doubt his friend Mr. Webb would be able to say whether the published figures were correct or not. The North-Western train had been run at about 50 miles per hour, and the resistance was given as 11·46 lbs. per ton, whereas, by the formula given in the Paper, it was 11·8½lbs. per ton. The weight of the train had been 339 tons. In order to see how much power the locomotive absorbed as compared with the train, a certain number of experiments had been tried on the Lancashire and Yorkshire Railway (p. 185), and it had been found that the ten-wheeled engine No. 1392 absorbed 34 per cent. of the total horse-power. Mr. W. M. Smith, whose communication to the Institution of Mechanical Engineers was referred to in the Paper, had given the result of his experiments as about 36 per cent. of the total horse-power; and Mr. Druitt Halpin had stated[2] that the Eastern Railway of France had found that the engine absorbed 57 per cent. of the total horse-power developed; while Dr. P. H. Dudley gave it at 55·6 per cent., and Mr. Barbier at 48 per cent. Probably 34½ per cent. or 36 per cent. was about the right percentage, the other figures being much too high; at any rate, the experiments referred to in the Paper rather pointed to that conclusion, though of course the actual figure depended on the load behind the engine. In an endeavour to determine the resistance of engines when running as machines, a certain number of trials had been made with the front cylinder-cover removed and the gland pulled out from the back cylinder-cover, the engine being then hauled for a considerable distance by another engine having a dynamometer-car attached; but he hesitated to give the results. One great difficulty had been that, owing to the back cylinder-covers being cast solid with the cylinders, they could not be taken off, and the compression of air in the cylinder had added to the resistance in a way which made the results somewhat unreliable. With regard to the friction between the tire and the rail, no doubt that was very largely due to the coning of the wheels. If the vehicle was driven over by the wind, so as to force the tire-flanges against the rail on one side, the coned wheels must slip backward on one side or forward on the other; and it had been found, with an experimental coach running at 40 miles per hour, that, in a distance of 2,580 feet, with a "natural" wind blowing at 6·02 miles per hour in a direction

The Author.

[1] *Feilden's Magazine*, vol. 5, p. 111.

[2] Proceedings Inst. Mechanical Engineers, 1889, p. 130.

The Author. making an angle of 60° with the line, the amount of slip was in one case 6 feet 6 inches, and in another case 5 feet 3 inches. In order to observe directly what was taking place while running, a hole had been cut in the floor of the carriage and an observer had sat down close to the wheel; and the wheels had been seen to be continually oscillating from side to side. At low speeds the flanges touched the rails frequently; but at high speeds the intervals between successive contacts became much longer, and he could not help thinking that this lengthening of the interval had a very material effect upon the tractive effort as the speed increased. One of the first observers of the fact that parallel wheels gave less friction than coned wheels, had been Mr. D. M'Dowell, in a Paper [1] read before the Institution of Civil Engineers of Ireland about 20 years ago. No doubt some American engineers who turned their wheels parallel obtained better results than the English engineers who coned them. There was a mechanical reason, however, why it might be better to cone the wheels than to turn them parallel. If a wheel was coned it did not so readily become hollow on the tread, and thus form a second flange; and a second flange, where the wheels were not turned up as often as they ought to be, was a somewhat dangerous thing. As to the slip of wheels due to the coning, experiments had been made with dead engines hauled 16 miles 28 chains, with accurate recording-instruments. In one case, with a radial tank-engine like that illustrated in Fig. 26, Plate 1, having wheels 5 feet 8 inches in diameter, a loss of 85 feet had been shown, and in another case a gain of 140 feet 7 inches. In two experiments with engines similar to that shown in Fig. 37, Plate 1, the loss had been 13 feet in one instance and 143 feet 3 inches in another. He was sorry to be unable to give any definite information about resistance due to curves. It had been noticed that in going round a 20-chain curve at 4 miles per hour the flanges of the wheels did not touch the rail continuously, and in order to verify that observation the whole curve had been whitewashed, and it had been found, after the train had passed, that the whitewash was only removed in places. The recording-apparatus in the dynamometer-car was extremely sensitive, and every little alteration of the regulator was indicated at once. In passing over water-troughs there was a tremendous thrust on the engine, which was recorded as a decreased tractive effort upon the car. It was obvious that that would be so, when it was considered that a water-trough was $\frac{1}{4}$ mile long, and that the

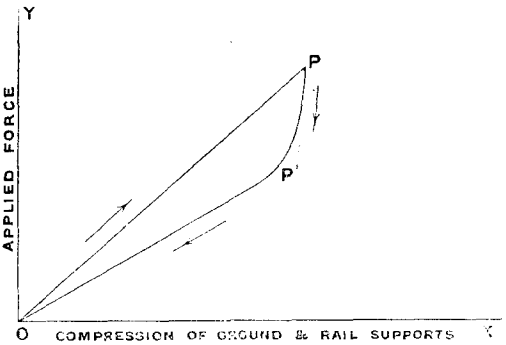
[1] Transactions Inst. Civil Engineers of Ireland, vol. xi. p. 55.

engine picked up 1,000 gallons, or 10,000 lbs., of water. To do that travelling at a speed of 60 miles per hour meant that 5 tons of water were lifted in $\frac{1}{4}$ minute, so that it might be expected that the effect would be fairly distinctly recorded. Not many experiments had been tried with wagons, and although he would mention one or two figures, he did not wish them to be taken as accurate, for he gave them simply as information showing results obtained. He thought it would be necessary to try several hundred experiments in order to determine the resistance with certainty. With a train of empty wagons 1,830 feet long, the resistance had been found to be 18·33 lbs. per ton at a speed of 26 miles per hour; a train of full wagons 1,045 feet long had given 9·12 lbs. per ton, at 29 miles per hour; and another, of the same length, as low a figure as 6·21 lbs. per ton at 28 miles per hour. He trusted, however, that these figures would not be regarded as results which could be finally established without further proof. Fig. 6, Plate 2, was plotted so as to show the number of experiments, from No. 1 to No. 230, so that it recorded the results of 230 different experiments, giving wind-pressure, speed, and tractive effort for each. As would be seen from the footnote at p. 163, all the data from which the diagrams had been prepared were accessible at the Institution, and anybody desirous of going through them would be able to check his figures. He put forward the results of these experiments only as applying to the particular coaches used on the line mentioned, and tried in a certain way; from experiments with other coaches, made in a different way, entirely different results might be obtained. The Author.

Mr. A. MALLOCK, referring to the subject of the ratio of what the Author called "miscellaneous resistances" to the resistances to which he gave a definite name, namely, the axle-friction and the atmospheric resistance, observed that the results in the Paper agreed very closely with what he had ascertained for himself; and evidently the Author had taken every care to note many things which were not ordinarily observed. What accounted for a very considerable percentage of the miscellaneous resistances was what Professor Ewing had called "hysteresis" of the ground on which the wheels ran. When a wheel passed over any particular point in a rail, that point was depressed, and as it was depressed the upward pressure of the rail increased. As the pressure on the rail decreased, the upward pressure diminished, but it did not diminish as quickly as it had increased. Supposing that the line O X (*Fig. 43*) represented the downward depression of the rail, and the line O Y the force required to depress the rail to a certain extent, the force Mr. Mallock.

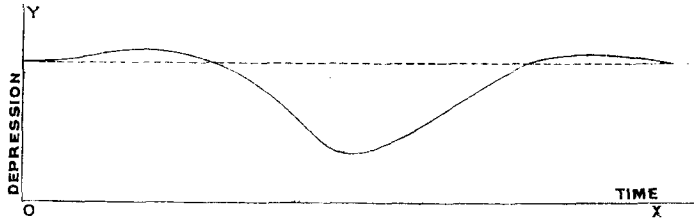
Mr. Mallock. exerted would be represented by a straight line something like O P; that was, the depression would vary as the force on the point depressed; but when the pressure was being removed, what happened was that at first the expansion was small compared with the decrease of pressure, and ultimately the expansion took place along a line such as O P′. The area O P P′ represented work

Fig. 43.



which was expended in depressing the ground or the rail-supports and then letting them expand. *Fig. 44* represented the motion of a point in the rail as a wheel, travelling at a uniform velocity with a certain load, passed over it. As the wheel approached the point the rail was gradually depressed, rising again after the wheel had passed. The rising curve, it would be seen, was not similar

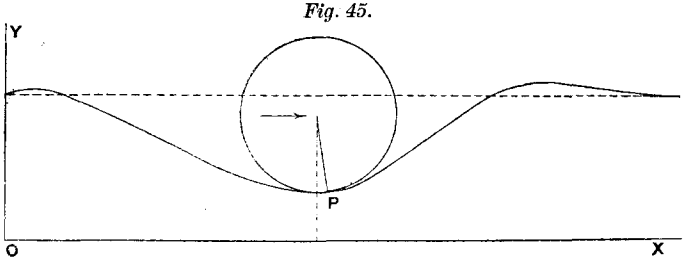
Fig. 44.



to the falling curve. The surface of the rail in the neighbourhood of the wheel, at any instant of time was something like *Fig. 45*; the result being that, owing to the imperfect elasticity of the ground, the wheel was always virtually running up a small hill. In his own experiments that effect had come to something like between 1 in 250 and 1 in 400. It was a very intricate function

of the velocity. The Paper was far too elaborate to be discussed fully at short notice, but he thought the cause he had mentioned Mr. Mallock.

Fig. 45.



would be found to account for certainly more than 50 per cent. of the "miscellaneous resistances."

Mr. Horace Bell remarked that, in spite of the mass of existing literature on the question of train-resistance which had been so carefully and elaborately added to by the Author, his own experience was that the average railway man passed the subject by as merely academical knowledge. He wished to refer to some experiments made by Mr. Desdouits with the dynamometric pendulum, with which the Author was doubtless acquainted. Mr. Desdouits had been engineer-in-chief of the French State railways in 1860, and had made experiments with the pendulum. The French Commission of 1891–2 which had investigated the subject of train-resistance had used his pendulum as well as the dynamometer-car, and had preferred the former. The dynamometric pendulum was a small instrument, very cheap, very portable and very accurate. The result of the experiments made by Mr. Desdouits had been to show that, far from being an academical subject, train-resistance ought to be one of great interest to all who were concerned with the working of railways, not merely to locomotive, carriage, or wagon men, but to the man who was concerned with the net receipts—the traffic-manager. Mr. Desdouits had shown that at high speeds air-resistance was a paramount factor, and that as compared with improvements in the construction of vehicles, in methods of lubrication, or in any other point, the air-resistance was the main thing to be considered; and he had supported his statement by giving examples of the effect of introducing an open platform truck between the tender of an engine and the leading brake-van; showing the increase in coal-consumption per mile to be 40 per cent. at a speed of 40 miles per hour. Other experiments had shown that it was the engine and the Mr. Bell.

Mr. Bell. leading vehicle which at high speeds encountered the resistance, which absorbed two-thirds of the total power. Again, Mr. Desdouits had stated that differences in the heights of vehicles in a train were of great importance, so much so, that, at the same speed of 40 miles per hour, considerable want of uniformity in the heights of the vehicles in a train would cause 7 HP. more to be absorbed than if all the vehicles were of the same size. Then he had instanced what was familiar perhaps to most members who had been abroad, viz., the value of what might be termed "bow" and "stern" plates to a train; and he had stated that an engine furnished with bow plates fitted in front of it and on the tender, at an angle of 45° to 60°, in such a way as to screen the leading vehicle, had shown, after six months' running, a saving in fuel of 10 per cent. to 15 per cent., as compared with three other engines of precisely similar type and doing similar duty: further, that if the engine had been working express trains at high speeds there would have been a saving in fuel of 15 per cent. to 20 per cent. Mr. Bell merely cited those experiments for what they were worth, to show that train-resistance was not a matter for drawing-room experiments; it was a solid fact to which attention had to be paid. The other resistances appeared to be small in comparison with the miscellaneous resistances, and it was very desirable that the causes of the latter should be elucidated. One solution, which he recommended to the attention of locomotive, carriage and wagon superintendents, was that bogies were not only loaded in the centre, but they were driven from the centre. It appeared to him that, while it was necessary to adhere to the practice of loading them in the centre, it would be quite possible to drive them from some point considerably in advance of the centre. Any one who had ridden on the open frame of a bogie-wagon would have seen that the bogie was in constant motion horizontally round the centre, seeking some path which it could never find; whereas if it were driven—like a castor on a chair or a table—from some point in advance of the centre, the frictional resistance would probably be found to be much smaller.

Mr. Upcott. Mr. F. R. UPCOTT considered that it was almost impossible to discriminate between the resistance of the atmosphere and the miscellaneous resistances. The Author did not discriminate between what he called the "natural" wind and the wind caused by displacement, and it was very evident that a side wind on a moving train must influence to a large extent the oscillation and the concussion. He did not think it was really of any use, in

making the experiments, to go beyond the aggregate resistance shown by the formula the Author had arrived at; as no practical result could follow from an endeavour to discriminate between the atmospheric resistance and the miscellaneous resistances. The axle-friction might be taken as practically a constant quantity. He had spent most of his time in India, where trains were subject in the north to blizzards, and in the centre and south to cyclones and dust-storms. About two years ago, in the south of India, a very disastrous effect had been brought about by a gale blowing at 60 miles or 70 miles per hour on a metre-gauge railway-train, which had been running, first against the wind, and then at right angles to it. The train had consisted of thirteen or fourteen coaches, with the engine in front, and a brake-van behind. It had been running at a fairly good speed under shelter, but as it came on to the open plain it had entered a curve, and meeting the wind almost end on, its speed had become slower and slower. By the time it reached the straight again it had come to a dead stand, and thereupon it had been blown over bodily, without anyone being hurt. [Mr. UPCOTT exhibited a photograph of the dismantled train.] That fact was his reason for attaching the greatest importance to wind, as strongly influencing the miscellaneous resistances. Mr. Upcott.

Mr. ALEXANDER SIEMENS thought that, as a very prominent part had been given in the Paper to air-resistance, it would interest the members to have some curves placed before them recording the results of experiments made in connection with the high-speed railway between Marienfelde and Zossen, near Berlin, which had been described in an interesting Paper read at the International Engineering Congress at Glasgow. The experiments[1] had been made on behalf of Messrs. Siemens and Halske by Mr. Walter Reichel, who had found that the ordinary formulas for the amount of power required to move a train would give evidently far too high results if applied to speeds up to 200 kilometres, or 125 English miles, per hour. Mr. Reichel had thought it would be desirable to divide the power required into two portions, the first due to the mechanical friction and the second due to the air-resistance. It was not necessary to go into the details of the calculation by which Mr. Reichel had arrived at the fact that the friction might be taken at about $4\frac{1}{2}$ kilograms (9·9 lbs.) per ton at a speed of 200 kilometres per hour, or 55 metres (180 feet) per second, but in order to determine the air-resistance at those speeds he had constructed Mr. Siemens.

[1] *Elektrotechnische Zeitschrift*, vol. xxii. p. 671.

Mr. Siemens. the apparatus shown in *Fig. 46*. A dynamo was placed with its

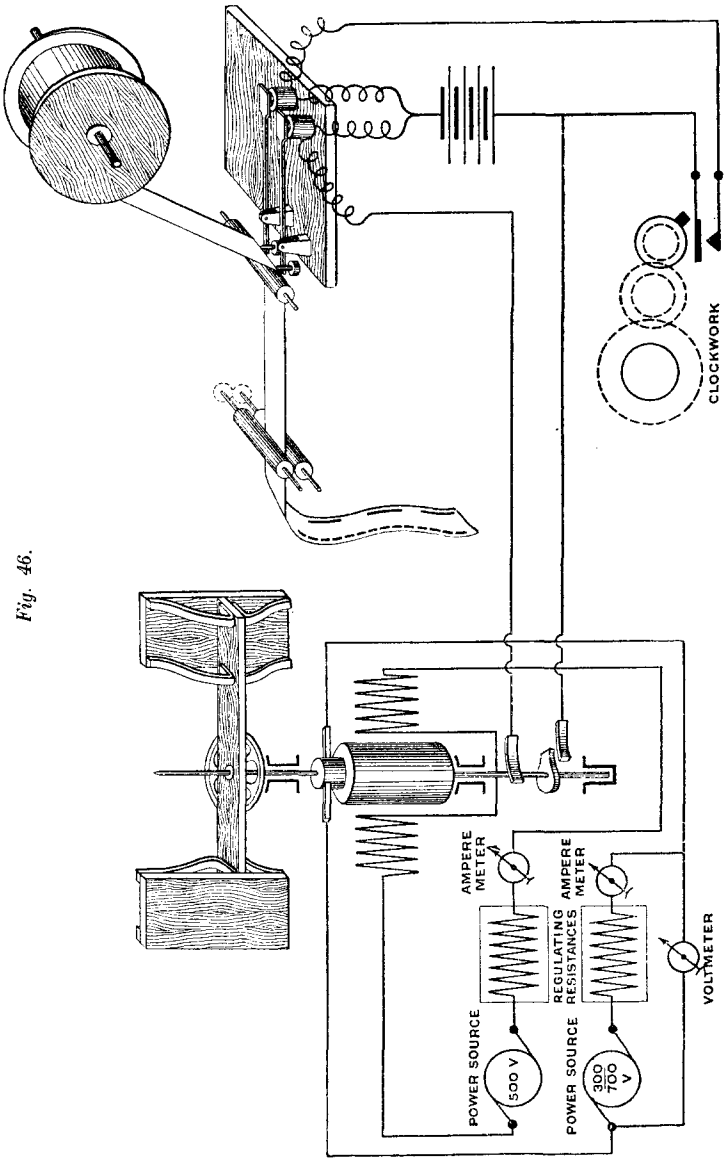


Fig. 46.

ARRANGEMENT OF APPARATUS FOR DETERMINING AIR-RESISTANCE.

spindle vertical, and a tram-wheel was fitted on the top of the spindle. To that wheel a stout plank was fixed, about 20 feet

long, 20 inches wide, and about $3\frac{1}{4}$ inches thick. In the first instance the experiments had been made with two square pieces of wood, about 1 square metre in area, fixed radially. The results had been plotted in *Fig. 47*. Curve No. 1 gave the resistance of the air according to the formula $p = 0{\cdot}00945v^2$, p being the pressure in kilograms per square metre and v the velocity of Mr. Siemens.

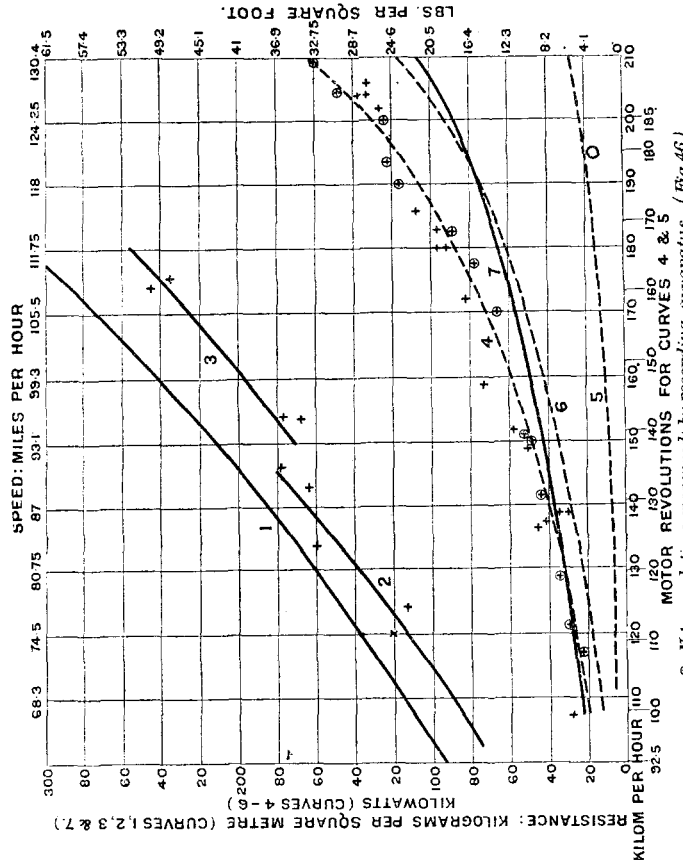
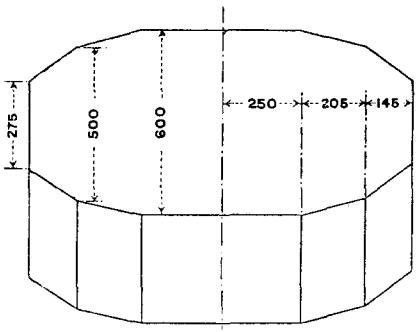


Fig. 47.

the wind in kilometres per hour; curve No. 2 gave the results with the plank about 1 square metre in area, and the next curve (No. 3) the results with a plank about $\frac{2}{3}$ square metre in area. It was quite evident that the shape of the surface facing the wind would have an important influence. For considerations which were obvious it had been thought that the parabolic shape would be the best,

Mr. Siemens. and *Fig. 48* showed the sort of body which had been fixed to the plank, and with which the following experiments had been made. Curve No. 7 (*Fig. 47*) gave the resistance to this parabolic surface actually found in the experiments, at various speeds up to 200 kilometres per hour. In order to insure accuracy in the determination of the power which was necessary, the electricity for the motor had been derived from two sources, one dynamo being for the field-magnets and the other for the armature; and with that arrangement it had been easy to vary the speed, and at the same time to measure accurately the power consumed. To make quite sure of the number of revolutions a further apparatus had been added, a sort of double writer of the Morse construction, which gave time-intervals and counted the revolutions of the motor. The utmost accuracy had therefore been secured in the

Fig. 48.



experiments. The three curves numbered 4, 5 and 6 in *Fig. 47* gave the actual kilowatts, No. 4 being the total amount of power in kilowatts absorbed with parabolic surfaces, No. 5 the kilowatts required to move the apparatus without any body fastened to the end of the plank, and No. 6 the kilowatts for overcoming the air-resistance due to parabolic surfaces. At a speed of 200 kilometres per hour there was a pressure of about 90 kilograms per square metre, equal to about 18 lbs. per square foot. For the car which was actually running,[1] weighing 96 tons, a pull of $4\frac{1}{2} \times 96$ kilograms, or about 450 kilograms, was therefore required to overcome the mechanical friction; and for the air-pressure on the 10 square metres of front, a pull of another 900 kilograms (at 90 kilograms per square metre) was needed. So that the total power would be about 950 HP. at 200 kilometres

[1] See *post*, p. 442.

per hour. He had that day received a letter stating that the car (*Fig. 49*) had been running at speeds up to 100 miles per hour on the

Mr. Siemens.

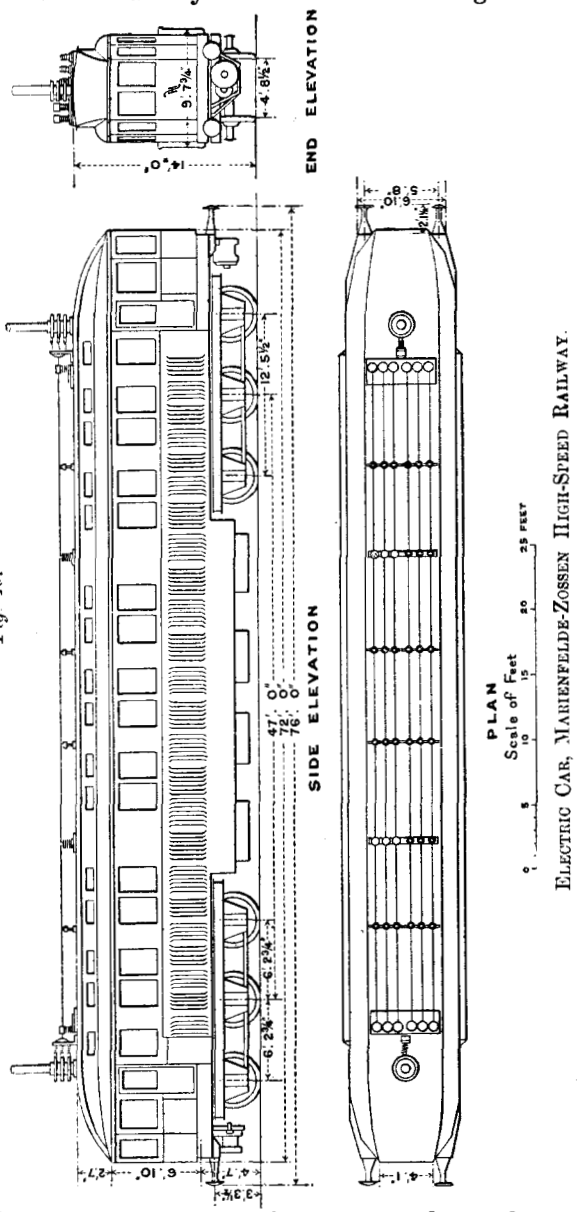


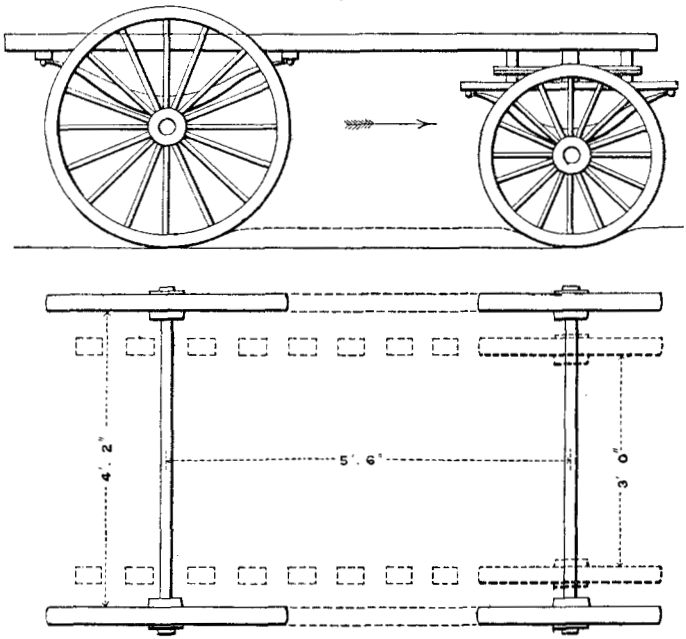
Fig. 49.

ELECTRIC CAR, MARIENFELDE-ZOSSEN HIGH-SPEED RAILWAY.

Mr. Siemens. experimental railway near Berlin. It was stated that the actual measurements of the power required were in close agreement with the experimental results, so that the way in which the experiments had been conducted appeared to have been a reliable one.

Sir Frederick Bramwell. Sir FREDERICK BRAMWELL, Bart., Past-President, observed that the subject was one of great interest to him, and he wished to refer to Mr. Mallock's diagrams illustrating the path of a wheel upon a rail which was not perfectly elastic but had a lag, and showing the wheel to be perpetually running up-hill. No doubt

Figs. 50.



that was quite right, but when Mr. Mallock considered the whole subject he would find that the value of the lag in the resistance of the whole train was very small. The matter of lag, not in the case of a rail having partial inelasticity, but with an ordinary road well pressed down by traffic, and which did not reassert itself, had been well known to carriage-builders for a very long time. *Figs. 50* showed two pairs of wheels, the front and hind wheels of a carriage, the front wheel going up the hill spoken of by Mr. Mallock, and having to crush down the road, while the hind wheel being in the same track, had not to repeat the operation.

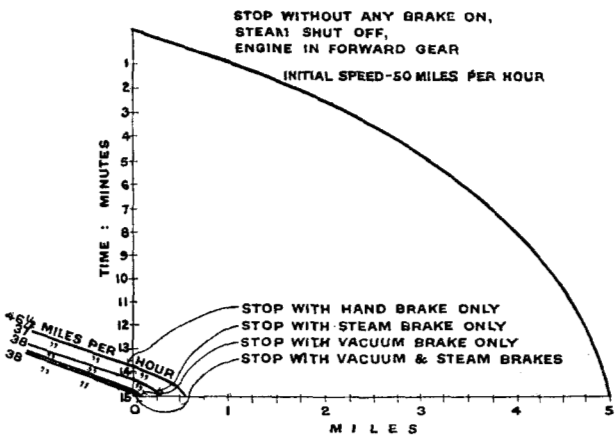
No ordinary carriage-builder would think of making a carriage with wheels as shown by dotted lines in that diagram, 4 feet 2 inches apart for the hind wheels and 3 feet apart for the front wheels, because if he did the wheels would have to surmount two hills instead of one. The same reasoning applied to the imperfectly elastic rail. At a speed of 50 miles per hour, with the driving-wheels of the locomotive 8 feet 7 inches apart, as shown by the Author, $\frac{1}{3}$ second only would elapse between the passage of one driving-wheel and of the other, and he did not believe the rail would have re-asserted itself sufficiently in that time to give a second hill of full size to the following wheel, the rail having been already driven down by the front wheel. That was true, he thought, for the wheels throughout the train. He believed, therefore, that when the whole subject was considered it would be found that although one wheel alone would undoubtedly be influenced, the influence on all the wheels of the train, considering the time-interval, would be found to be very small. Just 20 years ago, in 1881, he had been asked to report, in conjunction with the late Mr. Edward A. Cowper, M. Inst. C.E., on certain brakes in use on the Midland Railway, and experiments had been made with the assistance of stop-watches and similar devices, but without satisfactory results. He had therefore devised a very simple piece of apparatus, an instrument through which paper was drawn uniformly by means of clock-work, and having a pencil which made a transverse movement for every revolution of the leading wheel, that wheel having been selected because there was no brake on it. In that way there was produced on the paper a series of angles, and the closer they were together the higher was the speed of the train. He thought it might interest the members to know the result of what the Author called "coasting," a new word as applied to railways, but which, he gathered, meant the travelling of a train by its own energy, unassisted by a locomotive. In *Fig. 51*, the outer curve showed a train with the steam shut off when it was going exactly 50 miles per hour, the cylinder-cocks being opened, and the train had run 5 miles and 5 yards before it had come to a stop on a perfectly level and straight line. As a matter of interesting comparison, he had shown in the same diagram the distance in which certain trains had come to a stop when the brakes were applied, but that had not to do with the Paper. The value of the apparatus he had designed was that it diminished all personal error and gave with certainty the speed at which the train was running when the brakes were applied and the steam was shut off; and all that it was necessary to do was to

Sir Frederick Bramwell.

Sir Frederick Bramwell.

count the marks on the paper. It gave the distance travelled before the train stopped under the influence of the brake, and it also gave, with the aid of a watch, the exact time; and in that way a good many chances of error were eliminated. *Fig. 52* was a representation of one of the strips made by the indicator. By drawing vertical and horizontal lines a curve was obtained as shown, which was a curve for a stop, with brakes applied, from a speed of 33 miles per hour, effected in less than 144 yards and in $14\frac{3}{4}$ seconds. The train had consisted of ten six-wheeled carriages and two four-wheeled brake-vans, weighing together $122\frac{3}{4}$ tons; the locomotive and tender in working-order had weighed 72 tons, the total being thus 195 tons. The weight acted upon by the

Fig. 51.

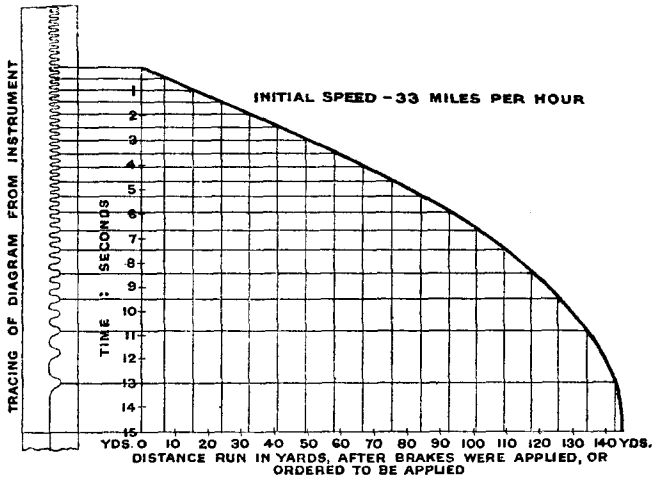


brakes had been 60 tons of the 72 tons of the engine, and $91\frac{1}{2}$ tons of the 122 tons of the train, altogether 151 tons out of 195 tons, or about 77 per cent. He could not help thinking that sufficient credit had not been given to the air-resistance by the Author, whose remark at p. 169 as to the vane being dragged round through 180°, into the direction in which the train was travelling, and staying in that position for 7 seconds or 8 seconds after the train had passed, and the additional statement that this time varied with the velocity and direction of the natural wind and the speed of the train, was a most pregnant one; pointing to the fact that a train carried with it an extensive envelope of air, travelling, if not at the speed of the train, yet with high velocity. What it was necessary to determine if possible was, the weight of air that

was continually being put in motion by the train. It appeared to him that, the train being within the envelope, it was extremely difficult to get any true information as to the action of the air upon the actual skin of the train. In several cases the apparatus had been placed some few feet above the roof of the carriage, but he gathered that the bulk of the instruments for recording air-pressure had really been fed by air which was in contact, and was moving more or less, with the train itself, and it seemed to him that by such a mode of observation the actual resistance of the air had not been measured. He did not know how to measure it more accurately, but if a means could be devised, rendering it possible to see how much air was continuously being

Sir Frederick Bramwell.

Fig. 52.



put into motion by the train, it would be possible to arrive at a better result. When he had been an apprentice, there had been no railway open to London at all, but the Greenwich Railway had been under construction. At the factory in which he had worked, one of Lord Cochrane's rotary engines had been put into a locomotive to be worked on the Greenwich Railway, and the old sailor had said that he was not going to have his engine stopped by the wind, or to have a flat bow, and he had had a sharp boat-bow fitted at each end of his engine. Sir Frederick was sorry he could not state the practical effect of this, for the designer had forgotten to make any provision to keep the case of the engine from turning round except the steam-pipe, and when the steam had been turned into the engine the case of the engine had worked

Sir Frederick Bramwell. round and broken the steam-pipe, instead of driving the locomotive. The information which might have been obtained from Lord Cochrane's device had, therefore, not been secured.

Mr. Timmis. Mr. I. A. TIMMIS remarked that the interest with which he had read the Paper had been considerably enhanced by the fact that the question of train-resistance had not received, in the past, the attention it ought to have received. He was grateful to the Author for having brought the matter forward, because discussion of the subject was sure to lead to good results. With regard to flange-resistance and oscillation he had had occasion to carry out some experiments during the past three years, and he hoped that the results of these experiments might not be altogether without interest. In dealing with bogies, whether they were running under carriages or goods-wagons, most railway men knew that there was a great tendency to oscillation when a certain speed was reached. No rule could be laid down for any number of vehicles, each vehicle having its individual speed at which it began to oscillate on a given road. He did not mean to say that all vehicles oscillated, but nearly all did. He had tried to give a lead to the bogie. As was well known, the bogie was driven—pulled or pushed—from its centre, but after a great deal of trouble he had managed to devise an arrangement by which the point from which the bogie was driven was a considerable distance ahead of its centre. He had had a number of these bogies running in different places, and he was at liberty to give the following information. In Burma Mr. C. E. Cardew, M. Inst. C.E., the chief mechanical engineer to the Burma Railways Company, had fitted two inspection-cars with bogies, and had reported that the cars ran very badly, so much so that it had been necessary to adopt some means of improving their running, the engineers travelling on them having been unable to sleep or write. Mr. Cardew had fitted a bogie-lead to them, driving them from a point about midway between the centre and the axles, with the result that the engineers had found that they could sleep and write with perfect ease. The practical point was that this expedient entirely eliminated the lateral oscillation, and if that was eliminated a very serious amount of frictional resistance was done away with; and he hoped the Author would bear that point in mind in any future experiments he might carry out. It seemed that the question of the resistance of four-wheeled cars, as compared with the resistance of bogie-vehicles, was one which ought to be solved, and would be easily solved, by dynamometric experiments. The resistance

to trains running on lines in the United Kingdom was not comparable with that of trains running in countries where the roads were very bad, but the question of frictional resistance, apart from wind-pressure, was a serious one, and ought to be solved ere long by proper experiments. Mr. Timmis.

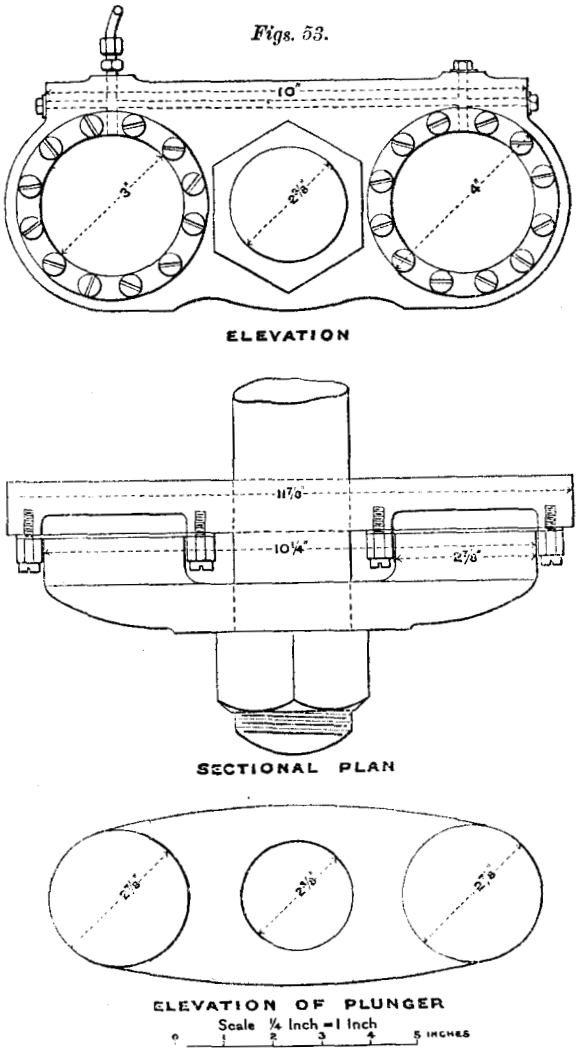
Mr. P. V. McMahon mentioned that in 1897 several tests and experiments, of which he had already given some account,[1] had been made on the City and South London Railway, in the actual running of locomotives, with a view to design some new locomotives to haul heavier trains at a higher speed; and a large number of tests had been made with the dynamometer which he exhibited. After several trials with a dynamometer in which a stuffing-box had been employed, a small dynamometer had been made which was fixed to the draw-bar of the train. On account of the small amount of clearance between the bottom of the bogie and the rail, a dynamometer large enough could not be used. Therefore the one he exhibited (*Figs. 53*[2]) had been placed in the buffer-beam of the locomotive. It consisted of two recesses in a brass casting connected by a small passage, the recesses being covered by an india-rubber diaphragm pressed upon by a sort of plunger. The dynamometer was fixed on the draw-bar, and the pressure-gauge was inside the locomotive. The pressure-gauge was calibrated to show the pull on the draw-bar in pounds. Five observers were employed, one giving the time at intervals of 5 seconds, another reading the pressure on the gauge, a third the amperes, a fourth the volts, and the fifth noting the speed. The results of the tests were given in the diagram (*Fig. 54*), which was representative of about one hundred curves taken on the line, that shown having been taken between the Oval and Stockwell stations. It would be noticed how closely the draw-bar curve followed the ampere curve. The curve showed the variation in the draw-bar pull, its high value at the start, about 40 lbs. per ton, dropping to the minimum of 9 lbs. per ton. Taking the draw-bar pull and allowing for the effect of gravity and also for acceleration, the second curve (*Fig. 55*) had been plotted, showing the resistance in pounds per ton for speeds up to 26 miles per hour. The most striking feature of it was the high resistance at starting, and in order to test that an experiment had been made with the train in a siding, a spring-balance being used to start it very slowly; the resistance in that case had been found to be 20 lbs. to 25 lbs. per ton. A Mr. McMahon.

[1] Minutes of Proceedings Inst. C.E., vol. cxxxix. p. 152.

[2] See also Journal Inst. Electrical Engineers, vol. xxviii. p. 513.

Mr. McMahon. similar experiment on a locomotive had shown the resistance to be 25 lbs. to 30 lbs. per ton, but the first test when the locomo-

Figs. 53.

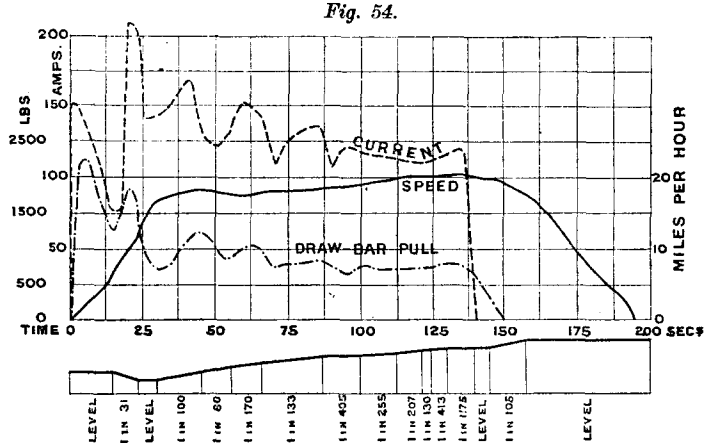


HYDRAULIC DYNAMOMETER.

tive was standing had always given a higher result, and that seemed to be due to the squeezing out of the oil. The diagram connecting the tractive resistance per ton and the speed was the

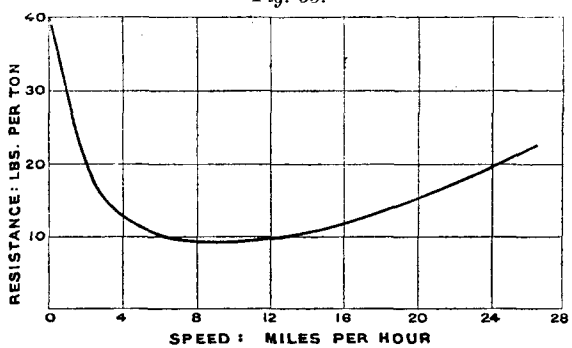
result of between five hundred and six hundred tests. In order to test the method and check the dynamometer-readings, some coasting-experiments had been made when running on falling gradients, the current being shut off at a speed of about 18 miles per hour, and the Mr. McMahon.

Fig. 54.



train being allowed to coast. The loss of kinetic energy and the reduction of speed, worked out in a similar way to the dynamometric results, had given other points on the same curve. Experiments had been made with two-coach trains and three-coach trains, and

Fig. 55.



TRACTIVE RESISTANCE: CITY AND SOUTH LONDON RAILWAY.

in every case it had been found that the resistance per ton of a three-coach train was less than that of a two-coach train. An attempt had also been made to determine the effect of curves on the tractive resistance, but it had been found to be very difficult to do so on account of the curves on the line being so short. The

Mr. McMahon. results of three tests on a curve having a radius of 390 feet gave 27·91 lbs. per ton at 16·45 miles per hour, whereas on the level road the result was $12\frac{3}{4}$ lbs. per ton, leaving about 15 lbs. per ton due to the curve alone. Six experiments had been made on a curve of 540 feet radius, giving 22·6 lbs. per ton at $13\frac{1}{2}$ miles per hour, whereas on the level it was 11·3 lbs. per ton, leaving 11·3 lbs. per ton due to the curve. The results of the experiments described in the Paper showed that the steam-locomotive had an efficiency of about 65 per cent., and that result was somewhat borne out by the results of tests on North Eastern Railway engines. The results of sixteen tests on four electric locomotives showed an efficiency ranging between 86 per cent. and 88 per cent. measured in a similar way, that was, as the ratio of the electrical to the useful horse-power at the draw-bar. Several attempts had been made on the City and South London Railway to measure air-resistance, but without success. In one experiment a metal frame, covered with canvas, and operating three levers on a diaphragm in a similar manner to the dynamometer, had been placed in front of the locomotive, but owing to vibration and other disturbances, no satisfactory results had been obtained. It had been observed that the locomotive always pushed a large quantity of air in front of it, and that when it came to a cross passage between two tunnels the pressure dropped, rising again to the maximum after passing such a passage. The wheels of the train were only 2 feet in diameter, and that might in some way account for the high tractive resistance.

Mr. Woods. Mr. EDWARD WOODS, Past-President, considered that the Institution was greatly indebted to the Author for his careful report and for the accuracy of his observations. The subject was not quite a new one, for 66 years ago he had had occasion to take part in some experiments having the same object. About the year 1834 the French Government had sent over to England the Comte de Pambour, and being then engaged on the Liverpool and Manchester Railway, Mr. Woods had been deputed by the Board of that Company to attend the Count and to render whatever assistance he could. Therefore he had been cognisant of all the investigations made by the Comte de Pambour, who had spent a long time upon the Liverpool and Manchester Railway making various experiments with the trains, his principal object being to ascertain the resistances which they had to overcome. The account of those experiments had been published in 1836. Later, the matter had been taken up by Dr. Lardner, who had gone to Liverpool and made a series of experiments with the same

Mr. Woods.

object, viz., to get facts to enable him to estimate the various resistances which the trains encountered on that railway. He had had the privilege also of assisting Dr. Lardner, who had reported to the British Association, which had published his report in the year 1838. The subject had then been taken up by several engineers and had been discussed at a meeting of the British Association in 1839, when a committee had been appointed, with Dr. Lardner as chairman. The other members had included Mr. (afterwards Sir) Hardman Earle, a very active director of the Liverpool and Manchester Railway at that time, who had taken great interest in the matter. Mr. Woods had also been appointed a member of the Committee, and a long series of experiments had been carried out upon the Liverpool and Manchester and the Grand Junction Railways, whose engines and trains had been placed at the disposal of the committee. The results of the experiments were recorded in the Report of the British Association.[1] Dr. Lardner had been the reporter of the committee, but unfortunately he had been called away to America, and in his absence Mr. Woods had written the report. Dr. Lardner had subsequently sent in a report based upon the same observations. The report recorded fully what had passed on those occasions and the conclusions come to on the subject. Since then the matter had been taken up carefully by various engineers, amongst others by the late Mr. Wyndham Harding, M. Inst. C.E., who had written on the subject in the year 1846, his conclusions being not very different from those come to by the Committee. The late Mr. Thomas Hawksley, Past-President Inst. C.E., and father of the present President, had taken part in such investigations and had made experiments and reports; and the subject had also been pursued by Mr. Bidder and others, as mentioned by Mr. Harding in the "Proceedings."[2] In mentioning these facts he was going back almost to prehistoric times, and therefore his observations must be taken with the qualification necessary in such cases; but reference to the writers he had named would, he thought, substantiate his remarks. These earlier experimenters had made some observations on the conditions incidental to motion through the air and to wind-pressure. No special instruments had been provided to estimate the effects, and thanks were due to the

[1] Report of the British Association, 1841, p. 405.

[2] Minutes of Proceedings Inst. C.E., vol. v. p. 369.

Mr. Woods. Author for the careful manner in which he had devised and arranged his apparatus for registering the effects of wind and of friction, and for ascertaining the real facts in connection with resistance. The effect of wind was certainly the most disturbing element among the resistances encountered by trains in motion, and must be regarded as a variable one. On the other hand, in the case of well-appointed trains, with properly constructed engines, the other elements of resistance might be fairly agreed. The effect due to gravitation on inclined planes, for instance, was perfectly determinate, as were also the effects of the friction of axle-bearings and of the machinery of the engines. Those resistances were readily ascertainable, and had been well considered by the Author in his arrangements for recording them. Again, the resistance due to flange-friction in passing round curves was difficult to estimate. It depended very much upon the amount of lateral pressure of the wind and upon the speed at which the train was moving, which caused variations in the pressure of the flanges upon the rails. It seemed to him that a large margin of allowance must be made in engine-power to meet the ordinary changes of weather. The earlier experiments had pretty well established the fact that on level lines in calm weather the resistance to the train, including the engine, at speeds up to about 12 miles per hour did not exceed 8 lbs. per ton gross; while at a speed of about 25 miles per hour that was increased by a little over 50 per cent., and at higher speeds would of course be increased still more. At that time the ordinary speed of passenger trains had been much lower than the speed now attained with more powerful engines. Under the circumstances, the results he had indicated might, he thought, be considered as fairly representative of the facts of the case.

Dr. Chree. Dr. CHARLES CHREE thought that people who had not had experience in the measurement of ordinary wind-velocities had comparatively little idea of the extraordinary variations which took place. It was extremely difficult to give any correct idea of how exceedingly variable wind-velocity was. An ordinary anemometer, say a Dines pressure-tube, which was comparatively dead beat, would be found to be in continual oscillation, and the velocity of the wind might be varying between 10 miles and 30 miles per hour within a few seconds. The consequence was that when measuring wind-velocity on trains, it was extremely difficult to know what was the velocity which had to be dealt with. A train going with the wind was something like a ship travelling with the waves. Running with the wind, a comparatively small number of air-waves was

Dr. Chree.

encountered by a train and there was much less variation in the pressure; but on the other hand, in running against the wind a very large number of waves was encountered in a comparatively short time, the consequence being large fluctuations in the resistance which had to be overcome. The Author had noticed that when he had run his train against the wind the irregularities had been much greater than when running with the wind; and Dr. Chree thought the fact he had mentioned was probably the explanation of the phenomenon observed. It added greatly to the importance of the experiments to which Mr. Siemens had referred, that in them velocities had been dealt with which were so exceedingly high, compared with ordinary wind-velocities, that the effects of fluctuations in the velocity of the wind were relatively small. Consequently, he thought that experiments at very high speeds were much more likely to lead to certain results—to results showing smaller fluctuations—than experiments at low speeds. Again, when dealing with wind-pressure on a building it was found that the building itself acted as a sort of obstacle to the wind. It brought to rest air immediately in front of it, and the consequence was that an attempt to measure wind-velocity close to the ridge of a flat roof gave a measurement which was much less than the true wind-velocity. On the other hand, if measurements were made 5 feet, 10 feet, or 20 feet higher, according to the height of the building, a considerably higher velocity of the wind might be obtained than the true one. The lines followed by the wind close to the building were given an upward tendency, and the wind might be in a measure concentrated in spaces some little distances above the ridge. That happened in a marked degree in passing under a bridge or through a tunnel where there was an obstacle above the train. The air had to find its way through somehow, and in such a case there might be a very great difference indeed between the velocity near to the surface of the train-roof and the velocity at some little distance above. With regard to the measurement of the velocity at different points of the railway line, it would be found that, owing to the extreme variation in the velocity, even at a given place, a very imperfect idea was obtained of what the wind-pressure and velocity had been in the course of even 15 minutes, unless a continuous record was taken. The observations recorded by the Author referred in some instances to such intervals, but he thought it would be advisable to make observations on a continuously-recording instrument and at a large number of points along the line on which the train was run.

Mr. Webb. Mr. F. W. WEBB remarked that on the London and North Western Railway the dynamometer-car in use had six wheels, the middle pair, which actuated the recording instrument, being turned parallel and carrying very little load. The car was drawn for some distance by an engine so as to determine the actual value of the periphery of the wheel. There being but little load upon the wheels, there was no wear and tear and no liability to the error which might occur by using the ordinary coned wheels carrying the car. One thing in the Paper that had struck him was the serious difference in the power required to work the train under apparently almost identical conditions. In one experiment (p. 185), with the train running at 60 miles per hour, the pull on the dynamometer was 32 cwt., in the next two experiments given in the same Table it was 26 cwt. and 17 cwt. respectively at the same speed. The indicated horse-power, however, did not vary so much as the dynamometer-readings; and therefore he could not help thinking that some error must have crept in in the springs used for the dynamometer. He would be glad if the Author would put a column in the Table at p. 185 showing the direction of the wind at the time of making each of those experiments. He admitted that it was very difficult to get a good spring for a dynamometer, and difficult to know exactly what power was required to work a train of given weight. Some managers—he hoped the Author was not one of them—thought it was only necessary, with a certain number of carriages, to use two engines, without any reference to the state of the weather, the direction in which the wind was blowing, or the gradient over which the train ran. That was what managers were coming to in this country, and he thought it was quite time they learned a little about the effects of wind and of gravity in working the heavy trains of the present day. He could give instances of two engines being used to haul a train where the second engine was doing no useful work, and was absolutely in the way. With regard to the remarks made by Sir Frederick Bramwell, he well recollected the late Mr. Patrick Stirling saying one day, when he had been discussing with him how he managed to pull the heavy trains on the Great Northern Railway with a single driving-wheel: "Mon, I have the weight on the bogie, and it lays the road down for the single wheel to get hold of it." That was the principle which Sir Frederick Bramwell had illustrated so graphically by his diagram. With regard to the time taken for a train to come to rest when coasting, it was well known that, unless very large cylinder-cocks were used, when steam was shut off the cylinders became air-

pumps, and greatly retarded the engine. For some years he had had all locomotives on the London and North Western Railway fitted with large vacuum-valves to avoid that result, and he believed the same thing was done largely in the United States and had a marked effect. It enabled the driver of a train to shut off steam about 200 yards in advance of the point at which he otherwise would do so, and the train would run into the station, with a great saving of steam. Another good effect of this practice was that dirt was not drawn out of the smoke-box into the cylinders. With regard to air-resistance, he thought the engineers on the Paris, Lyons and Mediterranean Railway had tried some extensive experiments, and if engineers could always insure the wind being in the same direction and the line being perfectly straight, some of the things done on that line might be of use. He had seen some engines in the last Paris Exhibition which absolutely had the dome and the front of the engine made wedge-shaped, the designers having forgotten that when the engine ran round a curve or was exposed to a side wind the evil they tried to cure was intensified. Mr. Webb.

Mr. DRUITT HALPIN mentioned that he had discussed with the late Mr. Belpaire, shortly before his death, the subject of the deflection which took place in the road on the passage of a train, to which Mr. Mallock had referred; and Mr. Belpaire had stated that the Belgian Government were carrying out an extensive series of experiments on the matter with very complete apparatus. These had been carried out under the direction of Mr. Flamache; and, the results having been published, any one interested in the subject could get a great deal of information from them. The Paper under consideration certainly gave a large amount of fresh information applying to the particular coaches used and the road upon which the experiments had been made; but there was a point which he hoped the Author would amplify, although he had done so to some extent in his remarks at the opening of the discussion—namely, the resistance of the engine and tender. That absorbed a very large portion of the whole amount of work done. It appeared that the majority of the experiments described had been made with the smaller train, consisting of the dynamometer-car and five coaches, and the weight of those coaches and the van was 115 tons, the weight of the engine being 70 tons, so that the total amount of the weight not dealt with was 38 tons. The importance of that part of the subject was evidently quite clear to the Author, because he mentioned (p. 168) that the resistance of the engine and tender was Mr. Halpin.

Mr. Halpin. higher than the resistance of the coaches, which was, of course, to be expected, because there was a flexibility in coaches which was not permissible in engines. Messrs. Vuillemin, Guebhard and Dieudonné, engineers of the Eastern Railway of France, had made the most complete series of experiments on the subject that had ever been made,[1] and they had given the resistance of engines and tenders, considered as vehicles, including the friction of the machinery at different temperatures. One of the engines used had been similar to those referred to by the Author, having four wheels coupled; but, as the experiments had been made about 35 years ago, the engines had been much lighter. The total weight of the engine and tender had been 50 tons, and the tender had had grease-lubricated axle-boxes. The maximum speed had also been low—36 miles per hour. At that speed, and under those conditions, the experimenters had obtained a tractive resistance of 13 lbs. per ton. Taking the Author's figures for the oil-lubricated axle-boxes, read from the diagrams and not from the formula, the resistance under those conditions would be about $7\frac{1}{2}$ lbs. per ton. With oil-lubricated axle-boxes the French engineers had experimented at $24\frac{1}{2}$ miles per hour with a six-coupled goods engine, making both dynamometric and coasting tests. With a mean speed of 13·6 miles per hour they had obtained a tractive resistance of 20·95 lbs. per ton when using grease in the tender axle-boxes, and less when using oil. The differences between the dynamometric and the indicated horse-power in the Author's Table (p. 185) was easily accounted for by the fact that the whole of the resistances to traction of the engine and tender as vehicles, and of the working parts of the engine, were not taken into account. He believed that one great cause of the error in locomotive indicator-diagrams—in which he had very little faith—was the long pipes used, usually of small diameter, and the high piston-speeds at which the engines ran. He had had a case some years ago [2] in which an engine of his own design would not give the calculated power. His friend, Mr. Schönheyder, had inspected it, and then the cause had been apparent. There had been the ordinary arrangement of long pipes, with one cock in the middle, and the indicator above it; and when the arrangement had been disconnected, and the indicator had been put in the right place on the engine-cylinders, the increase of power had been about 35 per cent. That had

[1] Mémoires de la Société des Ingenieurs Civils, 1867, p. 701; and *post*, p. 256.
[2] Proceedings Inst. Mechanical Engineers, 1886, p. 364.

happened with an engine running at a piston-speed of only 330 feet per minute, whereas the piston-speed of locomotives was 800 feet to 1,000 feet per minute. As the quantity to be measured was a function of the piston-speed, the error in the diagrams must, in his opinion, be very serious. With regard to the three figures in the Table at p. 185 to which Mr. Webb had drawn attention, referring to a speed of 60 miles per hour, he found that the mean difference between the indicated and the dynamometric horse-power shown in that Table amounted to 36 per cent., and he thought it was accounted for legitimately in the way he had mentioned. If that dynamometer-pull and speed was taken into the weight of the engine and the tender, that would bring it to 15·4 lbs., including tractive resistance and engine-resistances. But, of course, another possible source of serious inaccuracy in the results was the acceleration, and, unless that was actually known, no accurate deduction could be made as to how the differences between the dynamometric and the indicated horse-power arose. Mr. Halpin.

Professor C. A. CARUS-WILSON remarked that, while he did not suppose the point had escaped the Author's observation, it might be worth mentioning that the relation between p, the pressure of air in lbs. per square foot, and v the speed in feet per second was capable of being shown by an expression of the form $p = \frac{G}{2g} v^2$, which depended upon G, the weight of a cubic foot of air in pounds, and held good under every circumstance. This relation could be written $p = 0{\cdot}00254\ V^2$, V being the velocity in miles per hour. It was interesting to know that the results of the Author's experiments did not differ from this by more than 20 per cent. The difference was no doubt accounted for by the way in which the air-pressure had been measured. If the opening in the instrument with which air-pressure was measured differed considerably from the small aperture of an ordinary Pitot tube, or was of the nature of a bell-shaped diaphragm, the result was increased. Taking into account the difficulties of making the observation, the Author's result approximated closely to the theoretical relation. But the value of the Author's deductions from an observation of that kind was doubtful. He understood that the Author had taken the pressure in pounds per square foot—which was simply the pressure due to the velocity—in the middle of a current of air absolutely unobstructed by anything else, and had multiplied that pressure by the cross-sectional area of the car, which, according to the Paper, was 70 square feet; the result, expressed as pressure in pounds, being Professor Carus-Wilson.

Professor Carus-Wilson.

$P = 0{\cdot}021\ V^2$, or, for a train weighing 115·2 tons, $P = 0{\cdot}00183\ V^2$ lbs. per ton. The question arose, whether it was admissible to do that, and whether the pressure in pounds per square foot observed at the side of the car could rightly be multiplied by the cross-sectional area of the car, and the retarding force in pounds due to the air-resistance be thus obtained. It would have been difficult to answer a question of that kind had not elaborate experiments been made in order to determine the exact relation between the pressure obtained in that way, and the actual force retarding the train. Those experiments had been made by Professor Goss, to whom the Author had alluded: they were of extreme interest, but the time was too short to do more than mention that the method was to take a long wooden tube 20 inches by 20 inches in cross section, in the middle of which was placed a model of a train, and to measure the pressure on the model due to air blown through the tube. Professor Goss had measured the pressure of the air by the Pitot tube, and had deduced the velocity from the pressure: he had then measured the actual force in pounds on each model, and had been able to test the pressure on models of coaches up to twenty in number, and to find out how it was distributed—how much came on the front car, how much on the last, and how much on the intermediate cars. These experiments had given some very interesting results. Briefly, in a train of the length which the Author had taken, a six-coach train, the actual force displacing the whole train was about 63 per cent. of the product of the pressure in pounds per square foot multiplied by the cross-sectional area. That being so, it was possible to find out exactly what the retarding effect of the air would be on a train made up of cars of the dimensions used by the Author. He had worked it out and had reduced it to pounds per ton. He did not think that was at all a satisfactory way of stating the result, because the pressure of the air on a train had nothing whatever to do with the weight of the train; but he had adopted it in order to make the result comparable with other results given in the Paper. Dividing Professor Goss's results by 115·2 tons, the weight given by the Author for the train consisting of one dynamometer-car and five coaches, and correcting for the length of the car used by the Author, which was, in proportion, twice that of the car used by Professor Goss, that relation in pounds per ton was $P = 0{\cdot}00116\ V^2$, V being in miles per hour. That was considerably less than the Author's results, and taking a special case of 80 miles per hour the pressure would be equivalent to a retarding force of 840 lbs. giving 180 HP. and a resistance of about 7½ lbs. per ton. He had

heard it questioned whether the results obtained by Professor Goss were applicable when, instead of the air passing against a stationary train, the train passed through otherwise stationary air. That might have been a fair criticism, and he had not felt satisfied that in every respect there was a true analogy between the two cases until the experiments referred to by Mr. Siemens had been made. Those experiments confirmed in a remarkable way the experiments made by Professor Goss, for they showed that at a speed of 124 miles per hour the pressure of the air on a single car involved an expenditure of 650 HP.; and, taking the results of Professor Goss's experiments and making the necessary corrections for the proportions of the car, the figure was 600 HP.; so that, all things considered, the approximation was very close. The Author had attempted to divide the total resistance to motion into its constituent parts. Taking, for example, the resistance of the five-coach train at a speed of 80 miles per hour, the retarding force, according to the Author's results, was 28 lbs. per ton, and he had no reason to doubt the accuracy of that figure. The air-resistance, as given by Professor Goss, represented only 26 per cent. of that total resistance. The resistance due to axle-friction was taken by the Author as 2½ lbs. per ton, and he could not help thinking that that was rather a small allowance, and might perhaps be found to be below the mark. Assuming 4 lbs. per ton for axle-friction, the remarkable result was arrived at that the miscellaneous resistance was as much as 60 per cent. of the whole. That resistance should certainly be accounted for. It appeared to him that one of its chief elements was the force which had to be applied to a train to steer it—not on a curve, but on the straight. At first sight it appeared an anomaly to say that it required any considerable force to steer a train on the straight, but he thought a little consideration would show that when bogies were being pulled along at a considerable pace they took up an angular position. The bogie was turned slightly to the left or to the right, or, more probably, was continually oscillating sideways, and one flange or the other was always bearing against a rail. The practical outcome of this was that the bogie was virtually always running on a curve, because the effect of the angle which the direction of the bogie made with that of the rail was equivalent to placing the bogie on a curve; and, for a given amount of play, the radius of that curve varied as the square of the length of the wheel-base. That showed the great importance of the length of the wheel-base in determining the resistance, because even when the train as a whole was travelling along the straight, the

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individual bogies were moving along curves the radii of which varied as the squares of the corresponding wheel-bases. If the wheel-base were doubled, the force required to steer the train would be reduced to one-fourth. For that reason he thought that any records of tests made to determine the resistance of trains should always state the wheel-base of the vehicles used; and one explanation of the extraordinary discrepancies in *Fig. 41* might be found in the fact that the wheel-bases were all different, and consequently widely different values had been given to the force required to steer the train on the straight. Comparing, for instance, the wheel-base used by the Author, namely, 6 feet 6 inches, with the 5-foot wheel-base used on the coaches referred to by Mr. McMahon, and allowing $\frac{3}{8}$ inch as the difference between the wheel-gauge and the rail-gauge, it would appear that the effort required to haul the bogies and to keep them in the straight was 70 per cent. greater in the case of Mr. McMahon's coaches than in the case of the Author's coaches. But there was another important point about the effort required to steer a bogie along the straight, and that was, that the force required to be applied at the flange, being a centrifugal force, depended upon the weight of the bogie. That statement might appear to be rather unorthodox, but he thought it was justified when it was remembered that it was not the weight of the car as a whole which was being considered in the question of steering, but the weight of that portion of the train which had to be steered. It was simply a question of steering a bogie, and incidentally thereto the train was steered as a whole. It therefore appeared to him that the force which had to be applied depended not upon the weight of the cars but upon the weight of the bogies. If that was so, there followed the important result that that portion of the force required to haul a train along the straight which was due to the effort required to steer it did not depend upon the weight of the train but upon the weight of the bogies. Comparing again the Author's coaches and the coaches quoted by Mr. McMahon, it would be seen that the Author's coaches ran on two bogies and weighed about 20 tons, while Mr. McMahon's coaches also ran on two bogies but weighed about 7 tons, or about one-third of the weight of the coaches used by the Author. If what he had said was correct, and the force required to steer the train depended on the weight of the bogie and not on the weight of the coach, then the effort in pounds per ton, required to steer the whole train would be three times as much in coaches weighing 7 tons as it would be in coaches weighing 20 tons. That was one reason why it was so economical to have

long and heavy coaches on bogies. Further, if it was true that the force required to steer a train along a straight line depended not upon the weight of the train but on the weight of the bogie, then it was important that the bogies should be as light as possible. In an electrically driven train, heavy motors had to be put on the axles of the bogies. That would have an important influence upon the tractive resistance. In a case he had under his notice the bogies weighed 3½ tons and the motor on each bogie weighed 2 tons. Perhaps the high tractive resistance obtained in some cases of electrically driven trains was due to the large increase of weight of the bogies caused by the addition of the motor. In any discussion relating to train-resistance, the wheel-base and the weight of the bogies should always be given, to enable one test to be compared with another. Professor Carus-Wilson.

Professor W. E. DALBY desired to ask the Author a few questions with regard to the Table of experiments with engine No. 1,392. In the columns for the dynamometric horse-power there were several figures, among them 766 HP. and 758 HP. at the draw-bar, which appeared to be high for an economical rate of combustion. He would like to know whether those rates of 766 HP. and 758 HP. could be maintained with a steady steam-pressure in the boiler; or, to put the question in another way, what was the maximum indicated horse-power that the engines could maintain with a steady steam-pressure in the boiler? He would also like to know the grate-area of the engines used for the experiments, the size of the blast-pipe, and the maximum number of pounds of coal per square foot of grate that the Author considered could be economically consumed in express passenger engines. With reference to the question of engine-efficiency, the Author had remarked that the figures were not corrected for acceleration, and that it was consequently impossible to obtain the engine-efficiency from the Table (p. 185), but he had also stated that he considered it to be about 64 per cent. He did not know whether the Author was aware of the fact that the average of all the efficiencies worked out at 64 per cent.—practically the figure stated as derived from other experience. It might be of interest to compare that result with the results of experiments by Messrs. Adams and Pettigrew,[1] from which he had taken the following facts. In those experiments an engine and tender weighing 80 tons had taken vehicles weighing 217 tons up an incline of 1 in 314, and in that case the acceleration had been Professor Dalby.

[1] Minutes of Proceedings Inst. C.E., vol. cxxv. p. 282.

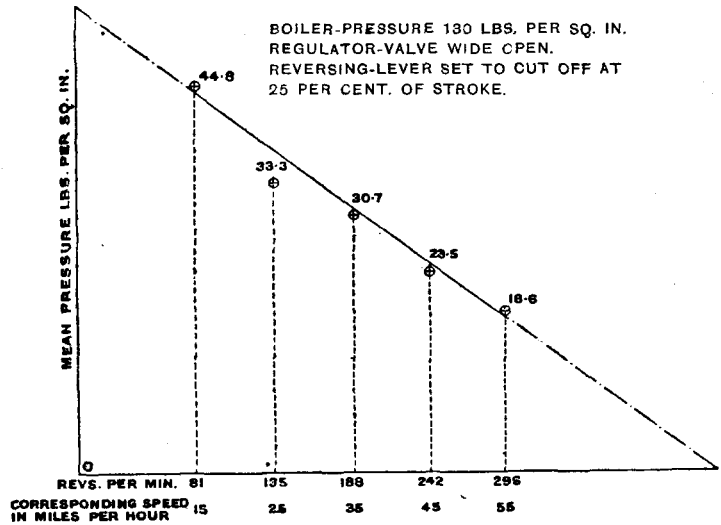
Professor Dalby.

practically nothing, because the curves given in the Paper, so far as could be judged, showed a uniform velocity of 40 miles per hour for some considerable time. Taking the resistance of the train from the old experiments of Sir Daniel Gooch, modified for oil-lubricated axle-boxes, at 10 lbs. per ton, and taking the engine-resistance from Gooch's experiments at 27 lbs. per ton, the following remarkable coincidence was found. The power required to maintain the engine and tender at 40 miles per hour against the resistance of 27 lbs. per ton worked out to 230 HP.; that required to maintain the vehicles at 40 miles per hour against a resistance of 10 lbs. per ton was 231 HP.; and that required to work against the gradient (1 in 314), including engine and tender, came to 226 HP., making a total of 687 HP. Messrs. Adams and Pettigrew's experiments showed the indicated horse-power to be 684 HP., so that there was a remarkable agreement between the indicated and the draw-bar horse-power when the acceleration was eliminated. In this case it would be seen that the engine-efficiency was 66 per cent., a figure in close agreement with the Author's experience of 64 per cent. The indicator-cards referring to the Author's experiments were not given in the Paper, and therefore he would like to ask whether the Author could give the relation between the mean pressure and the speed for a given uniform boiler-pressure and a given position of the reversing-lever, the regulator being wide open. It was mentioned in the Paper that a large number of the experiments had been made with the regulator wide open, so that probably the Author would be able to give the information from the indicator-diagrams. To illustrate exactly what he meant, he thought it might be interesting to give the record of an experiment which had been made on the experimental locomotive at Purdue University, and of which Professor Goss had sent him the indicator-cards. The driving-wheels of this engine were supported on wheels mounted in a pit with the top points of their circumferences at rail-level.[1] The shafts carrying the supporting wheels carried also Alden brakes, so that the resistance to the turning of these wheels could be adjusted to any degree. It had been possible in those experiments to maintain the boiler-pressure constant and to adjust the draw-bar load to any given speed, and experiments had been made at different speeds with the regulator wide open and the reversing-lever in one particular notch

[1] "An Experimental Locomotive." W. F. M. Goss. Transactions American Society Mechanical Engineers, vol. xiii. p. 427.

throughout, namely, at 25 per cent. cut-off. The locomotive had been stationary during the experiments. It was well known to locomotive men that, with a given position of the reversing-lever, the indicator-diagrams became thinner and thinner as the speed increased. He did not think he had ever seen a set of diagrams taken from a British engine at speeds varying gradually between 15 miles and 55 miles per hour, the other conditions being all maintained constant meanwhile: it was so difficult to make the experiment. At Purdue the load could be altered quickly to allow the change in speed. On the road such an alteration was of course not possible without considerably complicating the experiments. Professor Dalby.

Fig. 56.



In the diagram (*Fig. 56*) the mean pressure at various speeds between 35 miles and 55 miles per hour was plotted, the abscissas representing speed in miles per hour, and the ordinates mean pressure in pounds per square inch. The cut-off was at 25 per cent. of the stroke, and the steam-pressure, 130 lbs. per square inch, had been maintained uniform throughout the whole of the experiments. The points plotted in the diagram lay practically on a straight line. Assuming the line to be straight, the law of mean pressure was somewhat as follows: $p = 55 - 0{\cdot}125n$, p being the mean pressure in pounds per square inch and n the number of revolutions per minute. The consequence of this was that, the horse-power being proportional to the product of the mean

Professor Dalby. pressure and the number of revolutions, and the mean pressure being a function of the number of revolutions, there was one particular speed at which the engine was working at its maximum rate; and without going into the calculation he might state that that occurred at 220 revolutions per minute, with a piston-speed of 880 feet per minute. For that particular engine, therefore, with the reversing-lever set to cut off at 25 per cent. of the stroke, and with the regulator wide open, 880 feet per minute and a boiler-pressure of 130 lbs. per square inch was the most economical piston-speed; and above or below that speed the horse-power was less. The information he wished to obtain for modern engines was the nature of the curve connecting the mean pressure and the speed, not only for the 25-per cent. cut-off, but for the different cut-offs at which steam could be maintained. It would be possible then, for a given proportion of steam-port to cylinder-area, to say which was the most economical speed at which to run the engine for a given cut-off and boiler-pressure. The ports in the case he had mentioned were 16 inches deep by $1\frac{1}{4}$ inch wide, being 0·088 of the cylinder-area, and that was very nearly the proportion which British locomotive-engineers adopted. Therefore he thought the results might be taken as indicating approximately what took place in express engines in this country; and that there was a particular speed at which they were doing their best work. He hoped the Author could furnish enough data from his experiments to give some idea of the nature of the curves for the different cut-offs in the cylinders.

Professor Smith. Professor R. H. SMITH, referring to the Table at p. 185, remarked that the efficiencies ranged from $89\frac{1}{2}$ per cent. down to 40 per cent., and it was not possible to judge what the differences meant without any data as to acceleration. He wished to know whether there was any tendency in one direction or the other in the experiments, towards acceleration or retardation. The number of experiments in the Table was large, and if they were all indifferent in respect to acceleration there would be some probability of much of that effect cancelling out from the average of 64 per cent. mentioned by Professor Dalby. The large oscillations of the indicated train-resistance at starting, which the Author explained as being due to the relative acceleration between the locomotive and the train, might be damped by enclosing the dynamometer-spring in a cataract cylinder filled with oil or a mixture of alcohol and glycerine. The arrangement would involve only one stuffing-box, the friction of which would not materially affect the accuracy of the measurement. The period of oscillation

Professor Smith.

was not the natural period of the dynamometer-springs taken by themselves, but the period of oscillation of those springs attached to the very large mass of the train, which meant a very slow oscillation. Mr. McMahon's dynamometer, from that point of view, seemed to be a better instrument, and he believed it had been used in America for some years past. The effect of the want of uniformity in the driving-effort of the locomotive ought to be confined as far as possible to acceleration of the mass of the locomotive alone, and not be transmitted through the draw-bar. The mass of the train was so large, and the period of oscillation produced by the variation of crank-effort was so very small, that the acceleration due to a considerable excess of tractive effort did not become visible or measurable at all in any velocity-diagram that could be obtained, autographically or otherwise. Those effects might be fairly well restricted to the mass of the locomotive by interpolating between the dynamometer-springs and the draw-bar a long flexible spring or its equivalent, such, for instance, as a fairly large volume of air in an air-vessel, the air being compressed on its two sides, so to speak, by two columns of liquid, through which the drawbar-effort was transmitted. With regard to the velocity of the air past the train, the Author had overlooked the backward displacement currents, temporary air-currents over the earth due to the passage of the train through a perfectly still atmosphere. The currents ran backwards in curved lines outside the dead air which was carried forward by the skin-friction of the train. They would be usually masked by the natural wind and by what the Author called the " theoretical " wind, but they might be sufficient to account for apparent discrepancies such as the Author referred to when he mentioned that the leeward wind was sometimes stronger than the windward wind when the angle of impingement was small, and where he said that running with the wind the wind-velocity recorded by the anemometer was greater than the theoretical wind-velocity, or that when running against the wind the anemometer at 1 foot above the roof showed less, but at 3 feet above the roof considerably greater, wind-velocity than what he called the theoretical velocity. The Author's theoretical wind-velocity did not take account of those displacement currents, which had long been well known in pneumatic and hydraulic theory. The curve for five coaches (Fig. 7, Plate 2) agreed very well with the numerous points plotted on the diagram. The curve started at the initial height of $2\frac{1}{2}$ lbs. per ton and represented $V^{\frac{1}{3}}$. Curves for ten, fifteen, and twenty coaches were drawn with those two constants, $2\frac{1}{2}$ and $1\frac{2}{3}$, obtained from the experiment

Professor Smith.

with five coaches, the multiplier of $V^{\frac{3}{2}}$ alone being altered to suit the experimental points on the three latter diagrams; but in these diagrams there were not sufficient experimental points to justify the application of the formula, and the results of these three sets of experiments could be as easily satisfied by other curves with other constants. Again, the shape of the general formula was not deducible from any of those diagrams. It had been selected in accordance with the Author's notion of what it ought to be, and the constants had been worked in from the experiments. The Author mentioned that in one set of tests the same wagons empty and fully loaded had shown a resistance of 18·3 lbs. and 6·2 lbs. per ton, respectively. It had been well known for at least half a century that the resistance did not increase uniformly with the load. It therefore seemed to him a pity, when so important a subject was brought forward for discussion, to allow the unmeaning custom of reckoning the resistance in pounds per ton to survive. No doubt a portion of the resistance did increase in that way, but only a small portion. From the Author's experimental results he had worked out coefficients for another formula which appeared to allow for the influence of length and of weight of a train in a more reasonable manner. The total resistance in pounds was given by—

$$\text{Total R} = 2\tfrac{1}{2}\,\text{W} + \left\{2 + 0{\cdot}0035\,\text{L} - \frac{200}{100 + \text{W}}\right\}\text{V}^{\frac{3}{2}},$$

W being the weight of the train in tons, L its length in feet, and V the speed in miles per hour. That formula gave results nearly identical with the Author's for five, ten, fifteen, and twenty coaches. The coefficient within brackets became zero when L and W were zero. For trains 100 feet, 300 feet, 500 feet, and 1,000 feet in length it became 1, $2\frac{1}{4}$, $3\frac{1}{4}$, and $5\frac{1}{4}$ respectively, with normal loads. His object was simply to suggest a coefficient for the part of the resistance which varied with the speed, as well as to make a simple rational allowance for the effects of length and weight of train. He had worked out the following Table of the values of the coefficient for various lengths and weights of trains. The general effect of the formula was to make the resistance increase slightly with the loading of long trains, but largely with the loading of short trains. For instance, with trains 1,000 feet in length, weighing 200 tons, 400 tons, or 600 tons, the coefficient was 4·83, 5·10, and 5·32 respectively, the 600 tons giving only about 6 per cent. more than the 200 tons; but for trains 50 feet in length, weighing 10 tons, 20 tons, or 30 tons, the coefficients

Professor Smith.

L (Feet).	W (Tons).			$\left\{2 + 0{\cdot}0035\,L - \frac{200}{100 + W}\right\}$		
	1	2	3	1	2	3
50	10	20	30	0·36	0·51	0·635
100	20	40	60	0·68	0·92	1·10
150	30	60	90	0·98	1·275	1·47
200	40	80	120	1·37	1·59	1·79
250	50	100	150	1 54	1·875	2·075
300	60	120	180	1·80	2·14	2·34
400	80	160	240	2·29	2·63	2·81
500	100	200	300	2·75	3·08	3·25
750	150	300	450	3·825	4·125	4·26
1,000	200	400	600	4·83	5·10	5·32

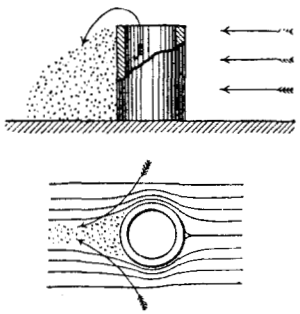
were 0·36, 0·51, and 0·63 respectively, in which cases the loads varied in the same ratios as in the other cases, while for the heaviest of these trains the coefficient was more than one-and-a-half times that for the lightest. With regard to variations in speed, he wished to point out, as specially worthy of note, formula No. 31 in Appendix II. That formula, given on the authority of Mr. West, made the resistance decrease slightly as the velocity increased from zero up to $8\frac{1}{4}$ miles per hour. Just after the start it was 5 lbs. per ton; it diminished to $4\frac{3}{4}$ lbs. per ton at $8\frac{1}{4}$ miles per hour; and beyond that speed it increased slowly. He thought it was a mistake altogether to consider that the only part of train-resistance that varied with the speed was the air-resistance. No doubt it was by far the most important component, but there was the vibration-resistance—the amount of energy represented by all the noise made and the trembling left in the earth as the train passed—and there was also the resistance due to journal-friction, the coefficient of which certainly varied with the speed, decreasing at first with increase of speed and then increasing.

Mr. C. H. Wingfield.

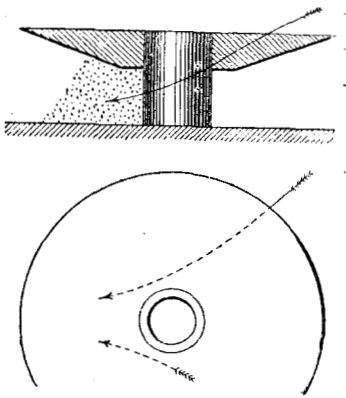
Mr. C. H. WINGFIELD remarked that he had found experimentally that with very high air-velocities the vanes of anemometers of the screw-propeller form were liable to become permanently distorted. He presumed the precaution had been taken to have the accuracy of the Hall anemometers checked after as well as before the trials. It seemed a pity that the car selected for experiment had not been of uniform height, as strong eddies must

Mr. Wingfield. have been formed in the rear of the raised part of the roof; and although it was mentioned in the Paper that the readings of the recording anemometer had been calibrated from those of the Hall anemometers, taken in nearly the same position, it seemed likely that the current of air meeting all three instruments had been affected by this cause. The details of the wind-gauges shown in *Figs. 28* and *29* were not given, but he gathered that they were simply small tubes bent so as to face ahead, and that some had cups on the ends, one being contracted so as to form a Pitot tube. Provided the flow was directly along the tube these would give a measure of the kinetic energy of the moving current of air, but not necessarily of the resultant pressure on the car. At the ends especially, *Fig. 30* (B) showed that the currents moved in great measure across many of the gauges, and in that case they would give erroneous readings. *Figs. 57* represented a tube projecting from the end of a carriage, and exposed to a cross current in the direction of the three arrows on the right of the figure. To the left of the tube a space of low pressure was formed, as indicated by dots. Air tended to flow into this space from all points, and the air which tended to come from the tube reduced the pressure in the latter below the true pressure of the air in the main body of the current. To overcome that difficulty, in a special case, Professor Unwin had devised the wooden flanges shown in *Figs. 58*, which were quite successful so long as the direction of the current was exactly parallel to their faces, but not otherwise. It would be seen that there was still a neighbourhood of low pressure, but that the end of the tube was screened from it, the result being that a current at right angles to the tube did not affect

Figs. 57.



Figs. 58.



the pressure in the latter. If the tubes ended flush with the surface of the car, instead of as shown in *Figs. 57* the effect of the cross currents, if they were exactly at right angles to the axis of the tube, would be eliminated. By far the best arrangement, however, with which he was acquainted for recording accurately the static pressure near surfaces where air was thrown into eddies was an instrument,[1] consisting of a sandwich made of three or four disks of wire-gauze, $3\frac{1}{2}$ inches in diameter, between two parallel metal plates, $2\frac{1}{2}$ inches in diameter, the centre of one of the latter being drilled and fitted with a pipe leading to a water-gauge (*Fig. 59*). To test the accuracy of these instruments the Illinois Central Railway had fitted a car with a vertical board 3 feet by 4 feet, mounted on ball-bearings, and held in position by a spring-balance. That board had been divided into one hundred and eight 4-inch squares, each provided with a pair of these disks, one in front and one behind, from which rubber tubes had led to gauges inside the car. A remarkable agreement had been found between their readings and the actual pressure on the board, as shown by the spring-balance. The observations had extended over several weeks, and had included speeds between 20 miles and 50 miles per hour, the experiments being made on a fast goods train. The accuracy of the readings had been found to be entirely unaffected by the direction and speed of the air-current. A method of magnifying the readings of U gauges devised by Mr. Macfarlane Gray would, he thought, interest the Author. The top of each limb of the tube was enlarged (*Fig. 60*), and two fluids of nearly the same density, one of them coloured so as to show the surface of contact, were used, the lower half of the U tube being filled with the denser liquid, and the upper portion of each branch and the vessel at the top

Mr. Wingfield.

Fig. 59.

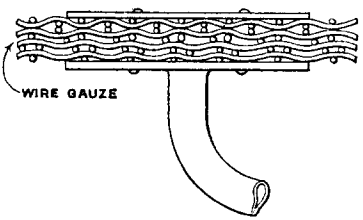
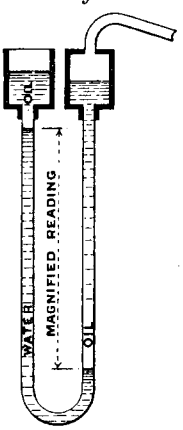


Fig. 60.



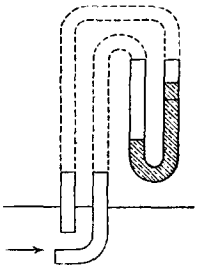
[1] For a full description see *Engineering*, vol. lxv. p. 376.

Mr. Wingfield. with the less dense liquid. Greatly magnified readings were thus obtained, depending principally on the relative sectional areas of the tube and the enlarged portion of it. If the fluids were of equal density, the readings would be greater than those of a plain U tube, in the ratio of the sectional areas of the tanks and tube. If, as had to be the case in practice, there was a perceptible difference between their densities the magnification was reduced accordingly. Oil and water were the most convenient fluids as a rule. He presumed that the words "just sufficient to overcome the friction of the draw-bar in its bearings" (p. 158) were not meant to suggest that it was possible to eliminate the friction of the draw-bar by compressing two opposed springs so that either taken by itself would overcome the friction. Unless the figures in the Table at p. 170 had been taken simultaneously, or the wind had been perfectly steady, they would not prove anything, he thought, as to the relative velocity at different distances from the surface. Such as they were, they did not seem to bear out the statement three lines lower, that the surface affected only those parts of the air-current close to it. No doubt the "other observations" referred to would have been more conclusive. At p. 178, however, it was stated that the velocity of the current, as shown by the collectors, was affected up to a distance of at least 4 feet 4 inches. The Robinson cups appeared, by scaling the drawing, to be 3 inches above the raised part of the car. It might be expected, therefore, that the air in their neighbourhood would be affected considerably. The reference to the continued flow of air after the train had passed (p. 169) was of interest as illustrating the inertia of air. A somewhat similar effect was noticeable on the Central London Railway. The doors throughout the long corridor trains were thrown open as they reached the stations, and for some seconds a draught from the rear of the train was experienced by the passengers, due to the inertia of the following mass of air, which did not come to rest so soon as the train. He had always understood that, other things being equal, the pull on the draw-bar diminished as the speed increased, and the Table at p. 185 appeared to bear this out, as did also the curves in *Figs. 5*. The "tractive effort," on the other hand, was shown by the formulas and diagrams to increase with the speed. It was apparently, therefore, something distinct from the pull on the draw-bar, and he was not clear as to the exact meaning of the term. No attempt seemed to have been made to determine the head resistance of the locomotive, which he thought would be very much greater than

that of the following carriages. Sir Hiram Maxim, in a lecture before the Aeronautical Society on the 3rd December, 1901, had referred to experiments with flying-machines at speeds of more than 40 miles per hour which had led him to the conclusion that skin-friction on smooth surfaces was negligible. That would point to the great advantage of doing away, as far as possible, with all roughnesses on the sides of trains in which eddies could be formed—such as the sunk window-frames, spaces between coaches, etc. It was difficult to over-estimate the value of careful experiments such as those brought forward in the Paper, and his object in making those remarks was to give the Author the opportunity of further elucidating the few points to which he had called attention. Mr. Wingfield.

Mr. W. GILBERT pointed out that the value of the atmospheric resistance given by Professor Goss as the result of his experiments on small models of railway carriages, was considerably less than the results obtained by the Author; but he thought a good deal of the discrepancy might be accounted for. Professor Goss had used a tube about 20 inches square and 60 feet long, along which air had been blown by means of a fan. Small models of carriages had been placed in the tube and a spring dynamometer had been arranged to record the pressure on each carriage. To obtain the velocity of air in the tube Professor Goss had used a gauge of the kind shown in *Fig. 61*, and he had made no allowance for the inductive action of the air-stream flowing across the end of the tube placed at right angles to it. Experiments in which Mr. Gilbert had been concerned 4 or 5 years ago had shown that the effect of that inductive action was to augment the reading of the gauge above the true reading due to velocity by as much as 42 per cent.; which, in the circumstances of Professor Goss's experiments, would mean that, for a given velocity, his value for the atmospheric resistance was 42 per cent. too low. On adding this amount to the curve for Professor Goss's results shown in *Fig. 34*, the result obtained would not be very different from that given by the Author. The Author, however, did not state clearly how he had obtained his curve for atmospheric resistance, though it could be inferred from the Paper that the formula $R_w = 0{\cdot}003\ V^2$ had been established by means of independent experiments with anemometers and water-gauges, and that the Author had then estimated the atmospheric resistance at a given velocity by taking the corresponding pressure in pounds per square foot from the formula, and multiplying Mr. Gilbert.

Fig. 61.



Mr. Gilbert. by 70 square feet, the area of the cross section of the train. If that was so, the curve was also subject to a slight correction, because careful experiments showed that the real value was $R_w = 0{\cdot}0026\ V^2$. He did not agree, however, with Professor Carus-Wilson that this result could be demonstrated theoretically for the ordinary Pitot tube. From the results of Professor Goss and the Author, it appeared that for a five-coach train at a speed of 60 miles per hour the effective horse-power required to overcome atmospheric resistance would be about 90 HP., and of course four times that amount would be required if there was a head wind blowing also at the rate of 60 miles per hour. As the Author could command all that was necessary for experimenting, it might be worth while to fit a goods wagon with suitable curved shapes and push this in front of the engine as a kind of atmospheric plough, so as to enable the saving effected to be judged. At p. 172 the Author described some experiments made with water-gauges to determine the pressure on the sides and ends of the dynamometer-car. It would be an advantage to have further details of the form of the tubes projecting from the sides and ends of the car. To measure the actual pressure against the sides and ends of the car it would probably be best to use a plain tube, simply pushed through and finished off flush with the outside of the car, as that would tend to obviate the effects of any inductive action.

Mr. Beaumont. Mr. W. Worby Beaumont asked for a reference to the experiments which Mr. Gilbert had mentioned, as giving $R_w = 0{\cdot}0026\ V^2$. He had carried out experiments with various apparatus and had used $R_w = 0{\cdot}0025\ V^2$, deducing that relation from a number of results obtained by others, and from the laws of falling bodies.

Mr. Gilbert. Mr. Gilbert said they were to be found in a Paper on "Centrifugal Fans," communicated to the Institution by Messrs. Heenan and Gilbert.[1] Of course the value of the coefficient of V^2 varied slightly, according to the height of the barometer and the temperature of the air.

Mr. Inglis. Mr. J. C. Inglis remarked that he had been struck by the serious amount of the total resistance which was put down to oscillation, concussion, flange-friction, and rolling friction, classified in the Paper as miscellaneous resistances, and amounting to 60 per cent. of the total resistance. He wished to know whether it was a common practice after a locomotive was built to suspend it and to drive the machinery at the speed at which the locomotive was intended to run, so as to see whether the

[1] Minutes of Proceedings Inst. C.E., vol. cxxiii. p. 272.

engine was properly balanced. He had chiefly to do with the road on which the locomotive ran, and his criticism referred rather to the disposition of engines to oscillate more or less at given speeds. The variation in engines in that respect was something alarming, and anything which would tend to reduce the oscillation, which was due to a number of causes, would minimize greatly the total resistance, because it could be easily proved that the flange-friction and the other resistances might amount to a great deal. He also wished to know whether, before the connecting-rods were put on an engine, the locomotive was tested on a long straight length of road to see that the turning of the pins was synchronous, that was, that there was no internal friction arising from a slight difference in the diameters. He had made experiments in that way, and had found that the synchronism was not quite perfect. Those who had to do with the maintenance of an express road knew how very important it was to eliminate the element of oscillation. A great deal depended on the locomotive, but much could be done on the road, and in that respect he had had considerable experience. It was the practice to lay roads $\frac{1}{4}$ inch wide of gauge, but he had given instructions now to lay them precisely to gauge, and that fact alone, coupled with extreme care in regard to uniformity of the gauge, had had the effect of reducing oscillation very materially, and thus diminishing the total resistance to the train. A point which the Author evidently considered to be an important one, was the question of conical tires *versus* cylindrical tires. He hoped the Author would adhere to the tires which were at present in use. There were quite enough parallel tires at the present time, because, notwithstanding the conical shape with which they started, they quickly wore down. He was sure that any practice which would make it easier to get a hollow tire would be ruinous to the fittings of the line. Nothing did more damage to fittings on express lines than hollow tires, and if the tires were cylindrical in the first instance they would have a shorter life before they became dangerously hollow. Mr. Inglis.

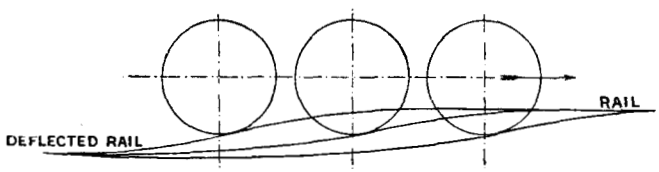
Mr. Bryan Donkin mentioned, as the discussion had turned somewhat on the question of the Pitot tube, that he had had considerable experience in measuring the velocity of air by means of it, and had found the results very satisfactory and exact, particularly in measuring the velocity of air issuing from large pipes. The gauge referred to by Mr. Wingfield, with two liquids of different densities, was much used in the United States; but the difficulty he had found was that after a certain time the two Mr. Donkin.

Mr. Donkin. liquids mixed together, so that no reading could be obtained. An inclined water-gauge $\frac{1}{2}$ inch in diameter and several feet in length was convenient for giving very small pressures, and could be read to about $\frac{1}{50}$ inch.

Mr. Wingfield. Mr. WINGFIELD observed that he had used a double-fluid gauge very successfully.

The Author. The AUTHOR, in reply, remarked that to ascertain the extent of the lag mentioned by Mr. Mallock would require some very delicate apparatus. No doubt there was a certain amount of lag, which would result in the point of maximum deflection not being at the point where the wheel was in contact with the rail, but somewhat behind that point. It was evident that, when the wheels of a train passed over a rail, work was done in consolidating the earth and in deflecting the rail. He did not agree with the theory that the train was always travelling up-hill; for it was evident that if that were so the train would be constantly overcoming the resistance due to gravity. Whereas if the position of the wheel

Fig. 62.



on a deflected rail at any instant of time were considered (*Fig. 62*), then theoretically the wheel was always in the same position on the incline and never rose any higher, but was being pushed forward, kneading the rail down as it went along; and the resistance it overcame was not due to gravity at all, but was work done in deflecting the rail, and the largest part of this resistance was encountered by the first pair of wheels. He had experimented in this direction and had found that when an engine was moving slowly, at about 2 miles per hour, the inclination of the rail in front of the first pair of wheels was equal to about 1 in 720, the deflection of the rail being 0·2 inch; but when the engine was run at about 45 miles per hour, the deflection of the rail was increased to 0·25 inch; that was, the deflection increased as the speed increased. Of course the deflection would vary according to the nature of the earth, etc. Further, he had found that the rail lifted slightly between each wheel of a train. No doubt this work accounted for a considerable amount of the miscellaneous resistance.

Dr. P. H. Dudley had stated[1] that when he substituted an 80-lb. rail for a worn 65-lb. rail it made a difference of 75 HP. to 100 HP., and he estimated that a 105-lb. rail saved 200 HP. as compared with the 60-lb. or 65-lb. rail. The late Mr. A. M. Wellington, M. Inst. C.E., who had made a great many experiments in America, had stated[2] that the work done by the front wheels in bending and compressing the rail was not given out to the last pair of wheels; and had pointed out that work was done in giving the rail velocity, and owing to its being somewhat stiff it did not fly back with sufficient quickness to give up all the energy imparted to it. The Author.

The experiments of Mr. Desdouits on atmospheric resistance, mentioned by Mr. Horace Bell, had been made with flat boards suspended laterally from a train, movable on axis, and counterweighted. That experimenter had found that on a board 3·94 inches square the pressure of the air was at the rate of 10·6 lbs. per square foot at a speed of 44½ miles per hour, which was almost twice that given by the Author, viz., $0{\cdot}003\ V^2 = 5{\cdot}94$ lbs. per square foot, at the speed in question. He had found that apparatus similar to that used by Mr. Desdouits was not suitable for use in experiments of this description. He had used such apparatus in the earliest experiments, but had discarded it as it had been found that the readings obtained were not applicable to trains in motion. With regard to fitting prows to locomotives, he believed that the advantages claimed for these fittings, even when running on a straight line with a dead head wind, were somewhat imaginary, for it was a well-known fact that, when an engine was running at a fair speed there was carried along in front of the smoke-box a volume of air somewhat pyramidal in form, although very shallow from apex to base. Fitting a prow to the front of a locomotive would no doubt be more effectual if the air struck the front of the smoke-box horizontally and then escaped at right angles to its original path; but this did not occur in practice. As Mr. Webb had pointed out, the engineers who fitted these appurtenances had evidently forgotten that when the engine was passing round curves or when there was a side wind—which there nearly always was to some extent—the evil they were trying to overcome was intensified.

It would be interesting to know whether the train mentioned by Mr. Upcott as having been blown over in a gale in India had

[1] Transactions Am. Inst. Electrical Engineers, vol. viii. p. 83.

[2] "Economic Theory of the Location of Railways," 1887, p. 516.

The Author. been travelling on an elevated bank, as the wind striking it would then act in a direction approaching the direction of the resultant force required to overturn the train. During the run between Liverpool and Southport, on the 28th December, 1900, to which he had already alluded, the wind had acquired a much higher velocity (in gusts) than that mentioned by Mr. Upcott, but in this case the country was flat, the surroundings being on the same level as the railway line, and further, there were hedges alongside, which protected somewhat the lower portion of the train.

It was very doubtful whether the whirling-experiments described by Mr. Siemens, even at a 10-foot radius, were applicable to railway trains, owing to the eddies created by the apparatus when in motion. Mr. O. T. Crosby, who had carried out a great many experiments [1] in America with whirling-apparatus, had arrived at the conclusion that the resistance in all cases and at all speeds varied directly as the velocity, and not as the square of the velocity. That experimenter had also found that a parabolic wedge, with base and height equal, constituted a form of head of least resistance, and that the resistance to such a body was about half that offered to a normal plane surface, which somewhat verified the remarks of Mr. Siemens. It therefore followed that the experiments described by that gentleman were intended to ascertain the resistance due to a body offering the least resistance to the air, and it would be interesting to know whether the train mentioned was fitted with a parabolic head. If so, the results could hardly be applied to the Author's experiments, as these had been made with a train having a flat front surface. Mr. Siemens had stated that at 124 miles per hour the resistance was 18 lbs. per square foot; this, it should be noted, was only 47 per cent. of the theoretical pressure, and 39 per cent. of the pressure due to such velocity according to the Paper, viz., $0{\cdot}003 \times (124)^2 = 46{\cdot}2$ lbs. per square foot. It was evident, therefore, that this low pressure was due to the parabolic shape of the body exposed to the action of the wind.

Sir Frederick Bramwell was approximately correct in stating that, at a speed of 50 miles per hour, the driving-wheels of a locomotive being 8 feet 7 inches apart, about $\frac{1}{9}$ second would elapse between the passing of the one wheel and the other over the same point. He had found, however, that at a speed of 45 miles per hour the rail did reassert itself slightly, but it did

[1] "An Experimental Study of Atmospheric Resistance," *Engineering*, vol. xlix. p. 663.

not present to the following wheel a hill of the same height as that presented to the first wheel; however, this would be increased when bogie-coaches were tried, as the distance between the bogies was greater. The apparatus used by Sir Frederick Bramwell was very similar to, but less elaborate than, some apparatus which he had used in his dynamometer-car. Where Sir Frederick Bramwell obtained a curve for each revolution of the wheel, he obtained a single mark, due to a hammer striking the pencil-holder; it had been absolutely necessary to do this in his experiments, owing to the high speeds at which the train had run. With regard to Sir Frederick's remarks about the wind-vane being dragged round through 180°, into the direction in which the train was travelling, it was a very difficult matter to ascertain the amount of air set in motion by moving trains. As mentioned in the Paper, he had made experiments by means of a collector and small strips of paper fed from the front of the train, but it had been found that to ascertain anything definite would require an enormous number of experiments. The envelope of air carried by a train gradually increased in thickness with the length of the train—a fact which had been verified by Professor Nipher, who was an authority on wind-currents. The instruments used for ascertaining the behaviour of the wind-currents had been placed as far as possible from the body of the train. The distance had been limited by telegraph- and signal-wires, bridges, etc.; but they had been placed in the front vehicle where the envelope of air would not be thick as at the rear, and consequently the readings had been but little affected. The Author.

The idea suggested by Mr. Horace Bell and Mr. Timmis, with regard to the position of the pivot from which the bogie should be driven, had been applied to locomotive bogies over 40 years ago, and it was in common use at the present time in the four-wheeled Bissell bogie. The Americans used the same principle in the pony-truck. It was evident that the running of bogie-coaches would be improved by giving a lead to the bogie-centre, as the wheels would not oscillate in the same way, nor tend to jamb between the rails. With regard to the resistance of four-wheeled coaches compared with that of bogie-stock, he might mention that this matter had received considerable attention from Mr. Barbier, who had conducted a great many experiments on the Northern Railway of France. Even if the road was not as good as an English standard road the results were comparable. Mr. Barbier had found that the resistance of four-wheeled vehicles was 20 per cent. to 30 per cent. higher than that of bogie-coaches.

The Author. He, however, had made his experiments with bogie-stock, because all modern vehicles were constructed with bogies.

The dynamometer used by Mr. McMahon on the City and South London Railway was very similar to the one used in his earlier experiments, which had been discarded owing to its working unsatisfactorily. He would imagine that Mr. McMahon would have difficulty with the diaphragm, and that the instrument would be sluggish in its action at low pressures; further, the diaphragm would be subjected to very high pressures, as a pull of 1 ton would subject the fluid to a pressure of 160 lbs. per square inch. It might, however, be suitable on that line, as the loads hauled were very light. The resistances shown in *Fig. 55* were considerably higher than those given in the Paper, and were no doubt due to the construction of the vehicles and the conditions under which they ran. It was interesting to note that the results of Mr. McMahon's coasting-experiments agreed very closely with those obtained with the dynamometer; this might be due to his train being so short and closely coupled, and not exposed to side winds. The difference in resistance per ton between a two-coach and a three-coach train was to be expected, when the large head-resistance which occurred in a tunnel was taken into account. As the resistance had been found to decrease considerably at the cross passages on the City and South London Railway, it might be advantageous to increase the number of these, or to arrange for a larger section of tunnel when building a similar line. The question of resistance on curves was one which, as he had already pointed out, would necessitate the use of very elaborate apparatus in order to obtain anything like accurate results.

It would no doubt be interesting to examine the results of Mr. Flamache's experiments on the deflection of rails. With regard to the resistance of the engine and tender, it was pointed out in the Paper (p. 168) that in the coasting-experiments with the engine and train, the resistance of the locomotive and tender was considerably higher than that of the carriages, which was an obvious result, seeing that the engine, when coasting, was compressing air in the cylinder, the whole of which could not possibly pass through the cylinder drain-cocks. Hence, the conditions were not the same as when a locomotive was hauling a train. From a large number of indicator-diagrams taken during these experiments, he estimated that the resistance of an engine was, approximately, the same per ton as that of a train, where the weight of the train was 115 tons and that of the engine 70 tons. The engine would absorb about 38 per cent. of the

The Author.

total power. The difference between the dynamometric and the indicated horse-power might be accounted for by the fact that, while an indicator-card was being taken, the stress on the draw-bar varied considerably, owing to the varying rotative effort at the rails and to other causes; consequently the mass of the engine was undergoing slight positive and negative acceleration, which was transmitted to the draw-bar. In order to ascertain the relation between the power developed in the cylinder and that shown at the draw-bar, the variation in stress on the draw-bar should be recorded at the exact instant when the card was taken, which would require suitable apparatus. The figures in the Paper were quoted to illustrate the large variation in the difference between the two; they were not intended to show the resistance of the engine and train, as in some of them no allowance had been made for the acceleration. The figures quoted by Mr. Halpin from the experiments carried out on the Eastern Railway of France were not applicable to the experiments described in the Paper, as the French experimenters had deduced a formula from a few figures only. Nevertheless, these experiments, though carried out by Messrs. Vuillemin, Guebhard and Dieudonné about 35 years ago, gave a great deal of information as to the various components of train-resistance, and were, he thought, of sufficient interest to justify him in adding a summary of the results[1] to his reply. There was no doubt that the indicator was a very unreliable instrument, and the question of the effect of long pipes had frequently been investigated, especially by Professor Goss, who had made some elaborate experiments.[2]

Referring to the remarks of Mr. Webb, the car used in the experiments had only four wheels, but great care had been taken to ascertain the effect of the wear of the tires, which was very slight indeed. The wheels were run over a measured track periodically, and the necessary correction was made, which would be needed with any car; the only difference being that with the four-wheeled car this would have to be done more often. The variations noticed by Mr. Webb in the figures for the dynamometric horse-power were due to acceleration, gravity, etc., and the same with the indicated horse-power; they were not intended to be compared with each other in terms of the speed, but each dynamometric horse-power was meant to be compared with the corresponding indicated horse-power. Mr. Webb was quite right in regard to the fitting of prows to engines.

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[1] *Post*, p. 256. [2] Transactions Am. Soc. Mech. Engineers, vol. xvii. p. 398.

The Author. The matter of the theoretical relation between the velocity and the pressure of air had not escaped his notice. He agreed with Mr. Gilbert's remarks regarding the inductive action of the air-stream flowing across the end of the Pitot tube, such action tending to influence the readings from the correct value. He also wished to emphasize the same speaker's remark that the equation $R_w = 0{\cdot}0026\ V^2$ could not be demonstrated theoretically for the ordinary Pitot tube, and to point out that there was evidently some error in the equation, given by Professor Carus-Wilson and by Mr. Worby Beaumont, for the pressure of air impinging on a plate. The constant given in formulas for the pressure of air in motion was arrived at by experiment and varied with the height of the barometer and the temperature of the air. It could be demonstrated theoretically[1] that $R_w = 0{\cdot}0054\ V^2$, where V was the velocity of the wind in miles per hour. When the velocity was expressed in feet per second (v), this became $R_w = 0{\cdot}0025\ v^2$. But he would like to draw attention, in reference to the remarks of Professor Carus-Wilson and Dr. Chree, to the fact—mentioned by many authorities—that if the theoretical formula was applied to calculate the atmospheric resistance to a surface of given area, the result did not correspond with the actual resistance obtained in practice, which varied according to the shape of the body. Professor Goss had pointed out that the resistance due to the direct pressure on the front of a coach was less than that which resulted from the impinging stream of air on the point of a gauge at the front of a vehicle, and had also found that the ratio of the effect upon each of the models of coaches forming his miniature train, measured in pressure per unit area, as given by the indications of the Pitot

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[1] Let v = velocity of wind in feet per second.
W = weight of air delivered per square foot per second in pounds.
P = pressure of wind in pounds per square foot on exposed area.
m = mass of air delivered per square foot per second.

Then $$P = m\,v$$

therefore $$P = \frac{W\,v}{g}.$$

But W = weight of 1 cubic foot of air (w) × velocity of the air in feet per second (v),

therefore $W = w\,v$

and $$P = \frac{w\,v^2}{g}$$

Or, expressed in miles per hour (V), and taking the weight of 1 cubic foot of air as 0·0807 lb.,

$$P = \frac{0{\cdot}0807 \times (1{\cdot}466\ V)^2}{32}$$

$$P = 0{\cdot}0054\ V^2.$$

gauge was, approximately: For the Pitot gauge, 100 per cent.; for first coach of model train, 40 per cent.; for last coach of model train, 10 per cent.; for second coach of model train, 3·2 per cent.; for any other coach, 4 per cent.; for locomotive and tender at head of model train, 44 per cent. Mr. Irminger, who had also carried out elaborate experiments, stated that the pressure exerted by a current of air upon the face of a cube was less than that exerted upon a thin plate of the same area, and also that the suction at the rear of the plate amounted to 55 per cent. of the total resistance.[1] The Author.

The Paper showed that the pressure of the wind on the front of a train was counteracted somewhat by the reaction of the air-currents on the following vehicles. It was obvious, then, that the pressure, per unit of area, acting on the front coach of a train, as shown by the gauge, was greater than the actual resistance to the motion of the train. From the Table at p. 176 it would be seen that the velocity of the wind in the centre of the back of the first vehicle, blowing in the direction in which the train was travelling, amounted to 40 per cent. of that acting on the front of the car (*Figs. 30*, A). Sufficient data had not been obtained to show how far this action took place on each coach of the train, but the results pointed to the fact, previously mentioned, that the pressure of the wind on the front of a coach was counteracted somewhat by the reaction of the wind on the following vehicles. The slight vacuum, as would be seen in *Fig. 22* and the Table at pp. 174 and 175, existed only close to the edge of the car-body, and was due to local eddies. The behaviour of the wind-current, however, would depend on the velocity of the wind, and the speed of the train. In the analysis of the formula, illustrated by *Figs. 34* and *36*, the atmospheric resistance was taken at that indicated by the formula $R_w = 0{\cdot}003V^2$. This formula had been arrived at after a great many experiments, as being a simple formula applicable to trains in motion. It had therefore been used to determine the pressure acting on an area of 70 square feet for a five-coach train, and 88 square feet for a twenty-coach train. The effective area of the end of the first coach was 64 square feet, and 1·2 square foot on each coach had been taken as being exposed to the direct action of a dead head wind; and in order that some common velocity might be taken as a standard, V had been assumed to be equal and opposite in direction to the velocity of the train. Professor Carus-Wilson's statement that, according to Professor Goss, the actual atmospheric resistance retarding a train of five bogie-coaches and the dynamometer-car

[1] *Journal of Gas Lighting*, vol. lxxiii. p. 1203; and *post*, p. 276.

The Author. was only 63 per cent. of the theoretical pressure, as obtained by a Pitot gauge, was approximately correct, the actual amount calculated from the results mentioned above being 65·2 per cent. Applying this to the formula given in the Paper, Professor Carus-Wilson had obtained the equation—Atmospheric resistance in lbs. per ton for five-coach train $= \frac{0{\cdot}003 \times 70 \times 63}{115 \times 100} V^2 = 0{\cdot}00116\,V^2$; and had stated, further, that, according to the Paper, the retarding force at 80 miles per hour was about 28 lbs. per ton, and the atmospheric resistance, according to Professor Goss, only represented 7½ lbs. per ton, or about 26 per cent. of that total: also, that the miscellaneous resistances were thus equal to 60 per cent. of the total resistance. Professor Carus-Wilson had evidently misapprehended the formula given in the Paper, viz., $R_w = 0{\cdot}003V^2$, and had assumed that the ratio between the actual and the theoretical resistance given by Professor Goss, viz., 63 per cent., could be applied to that formula, and had reduced it accordingly; that was, Professor Carus-Wilson had taken only 63 per cent. of the atmospheric resistance indicated by the formula, instead of its full value. The formula $R_w = 0{\cdot}003V^2$, however, should be applied to the area exposed to the resistance, which gave a different light to Professor Carus-Wilson's remarks. Then, according to this formula and that of Professor Goss for atmospheric resistance at 80 miles per hour, the following figures were obtained:—

Speed, 80 miles per hour.	Aspinall. Lbs. per ton.	Goss. Lbs. per ton.
Total resistance	27·8	27·8
Atmospheric resistance	11·7	6·4
Miscellaneous resistances	14·1	19·4
Axle-friction	2·0	2·0

From these figures it would be seen that at a speed of 80 miles per hour his miscellaneous resistances amounted to only 50 per cent. of the total resistance, whereas if Professor Goss's formula was applied, the miscellaneous resistances would amount to 70 per cent. of the total resistance. This explanation would doubtless make the matter clear. It did not follow that, because Professor Goss had found that only 63 per cent. of the pressure was effective in a particular case, this ratio must be applied to every other formula. The formula for atmospheric resistance given in the Paper was to be applied without any deduction, as it included the whole of the atmospheric resistance, viz., that due to the pressure on the front coach, and on a certain portion of each following vehicle, the rarefaction at the rear of the train, and the frictional resistance of the carriage-bodies, etc. In the

upper part of *Fig. 34* the miscellaneous resistances were plotted for a five-coach train, from which it would be seen that at 80 miles per hour the miscellaneous resistances indicated by the several formulas amounted to:—Smeaton, 22 per cent.; Aspinall, 50 per cent.; Nipher, 58 per cent.; Goss, 70 per cent. From this it was clear that, according to Professor Goss, the atmospheric resistance for this particular train was only a very small portion of the total resistance. He estimated the various resistances at a speed of 80 miles per hour as, axle-friction, 7½ per cent. of total resistance, atmospheric resistance 50 per cent., and miscellaneous resistances 42½ per cent., which was a very fair proportion. It would also be well to mention that Professor Goss's figures were applied to the train, where the first coach had been taken as the first vehicle of the train and not the third. He concluded that, having an engine and tender in front of the coaches, the figures applied to the first vehicle were more applicable. Although the experiments carried out by Professor Goss might be taken as illustrating the relative effect of the wind on the different vehicles, it was a question whether the formula given could be applied to a full-sized railway train. It certainly seemed to give a very low result for a five-coach train. Sir Benjamin Baker, who had carried out a number of experiments on wind-pressure at the Forth Bridge, had found that with two wind-gauges, one 300 square feet in area, and the other 1½ square foot, the larger gauge registered a pressure 38·7 per cent. less per square foot than the smaller one. Professor Goss's experiments had been made with stationary models in a current of air—conditions just the opposite to those of a train running against the wind. No reliable information had yet been obtained as to whether the pressure between fluid and solid was greater in the case of movement of the one or of the other; but Mr. Crosby had found, in the tests he had made in America, some experimental indication that motion of the fluid gave higher pressures. Owing to the variations in the atmospheric resistance, the total tractive resistance given in the Paper might vary to the extent of 25 per cent. of the total, on either side of the curve; for instance, at 80 miles per hour the mean resistance given by the formula was 27·8 lbs. per ton for a five-coach train, which figure might vary between 20·9 lbs. and 34·7 lbs. per ton, depending on the wind, etc.

The Author.

Professor Carus-Wilson appeared to consider the force required to steer the bogie to be a very large factor in the miscellaneous resistances, and his theory about the bogie tending to get crossed

The Author. between the rails was perfectly true. If it were assumed that a bogie with a wheel-base of 6 feet 6 inches was travelling on a perfectly straight line, and that the flange of one of the leading wheels was $\frac{1}{4}$ inch from its normal position, then the bogie-although running on a straight line, was really trying to run round a curve, but was kept in a straight line by means of the rails. In that case the bogie was running in an arc whose radius

$$R = \frac{V^2 + C^2}{2\,V} = \frac{\frac{1}{4}^2 + 78^2}{2 \times \frac{1}{4}} \text{ inches} = 1014 \text{ feet};$$

where V was the deviation of the flanges of the leading wheels from the normal position, and C the wheel-base, in inches. But if the wheel-base were doubled and the bogie were run under the same conditions, it would be travelling round a curve of radius

$$R = \frac{V^2 + (2C)^2}{2\,V} = \frac{\frac{1}{4}^2 + 156^2}{2 \times \frac{1}{4}} \text{ inches} = 4056 \text{ feet}.$$

So that by doubling the wheel-base the radius of the curve on which the bogie virtually ran was increased practically four times. That was the theoretical side of the question; in practice it was found that the flanges of the leading wheels were never in contact with the rail for long, but that the wheels were constantly oscillating from side to side. There was, however, an advantage to be gained by increasing the wheel-base of the bogies, and that was that the interval between successive oscillations was much longer, consequently there were fewer oscillations in a given time and therefore less resistance.

The dynamometric horse-powers given in the Table at p. 185 were undoubtedly high for economical combustion, but it should be understood that they were not maintained throughout a journey, but were only occasionally developed—for instance, when going up an incline. Such dynamometric horse-powers as these could not be maintained for any length of time with a steady steam-pressure, as a little consideration would show. The maximum dynamometric horse-power maintained with a fairly constant steam-pressure had been recorded on the 28th December, 1900, during the run between Liverpool and Southport in a gale, previously referred to. On this trial there had been an average draw-bar pull of 4,480 lbs. at a mean speed of about 52 miles per hour, which was equal to 620 HP. on the draw-bar. This had been exerted over a distance of 8 miles. It was about as much as the boiler could maintain, and was rather more than could

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be expected for a longer journey. If the engine-efficiency were taken as 0·64 (which meant that the resistance of the engine per ton was the same as that of the train per ton), the engine must have been exerting something like 970 HP. Indicator-experiments had not been made on a journey of any length to ascertain the maximum horse-power that could be maintained, but, from the dynamometer-records, 500 HP. might be taken as a high average that could be maintained at the draw-bar with an engine of the class of No. 1392. If the engine-efficiency were estimated at 0·64, the relative horse-power might be stated as:—engine, 280 HP.; train, 500 HP.; total, 780 HP. Then, according to the above figures and the formula given in the Paper, the highest speed which could be maintained on a level road with a train whose coaches weighed 115 tons would be about 71 miles per hour. The grate-area of the engines in question was 26 square feet, with a heating-surface of 2052·8 square feet, the diameter of the blast-pipe-nozzle being 5 inches. Therefore, if the steam-consumption were taken at 25 lbs. per I.HP.-hour, the quantity of water evaporated per square foot of heating-surface per hour would be 9·5 lbs. To ascertain what Professor Dalby required, viz., "the relation between the mean pressure and the speed for a given uniform boiler-pressure, and a given position of the reversing-lever, the regulator being wide open," would require a great many experiments; in fact it would form a Paper of itself. The subject of the present Paper was "train-resistance," and not the relative efficiency of locomotives. However, since Professor Dalby had mentioned Professor Goss's experiments it would perhaps be advisable to give the following results obtained. These experiments had been made with a locomotive having cylinders 17 inches in diameter and 24 inches stroke, the steam-pressure being 130 lbs. per square inch, and the regulator wide open. Professor Dalby's statement that with cut-off at 25 per cent. of the stroke the most economical speed of this engine was 220 revolutions per minute, was evidently approximately correct. It should be observed that indicator-cards had not been taken on every trip with the dynamometer in the Author's experiments. The difficulty of measuring the relation between the dynamometric and the indicated horse-power, owing to the variation in stress on the draw-bar while the indicator-card was being taken, would undoubtedly account for the large difference between the readings. The oscillations mentioned by Professor Smith were large only at the start, the period becoming longer, and the amplitude becoming less, as the speed increased. They were absorbed by the springs

The Author.

Speed. Miles per Hour.	Revolutions per Minute.	Indicated Horse-Power.		
		Cut-off. 6 Inches.	Cut-off. 8 Inches.	Cut-off. 10 Inches.
15	81	190	270	..
25	135	223	368	455
35	188	298	431	501
45	242	302	437	..
55	296	292	438	..

MEAN EFFECTIVE PRESSURE AT VARIOUS CUT-OFFS.

Speed. Miles per Hour.	Revolutions per Minute.	Mean Effective Pressure.		
		Cut-off. 6 Inches.	Cut-off. 8 Inches.	Cut-off. 10 Inches.
15	81	43·5	61·9	..
25	135	30·5	51·2	63·3
35	188	29·6	42·4	48·0
45	242	23·2	33·2	..
55	296	18·3	27·4	..

STEAM-CONSUMPTION PER I.HP.-HOUR.

Speed. Miles per Hour.	Revolutions per Minute.	Cut-off in Inches of Stroke.		
		6 Inches.	8 Inches.	10 Inches.
15	81	28·93	27·66	..
25	135	28·06	26·60	28·6
35	188	26·93	26·28	30·1
45	242	28·60	28·45	..
55	296	30·64	32·00	..

on each vehicle and they decreased gradually throughout the length of the train. He did not agree with the remarks made by Professor Smith in reference to Mr. McMahon's dynamometer, as the action was just the reverse of what Professor Smith had stated, for the simple reason that there was little flexibility in this instrument, and consequently the oscillations were transmitted more readily to the mass of the train. Although that speaker had remarked that, while the curve for the five-coach train agreed very well with the numerous points plotted on the diagram, he did not consider there were sufficient points on the diagrams for the ten-, fifteen- and twenty-coach trains to justify the alteration of the coefficient of $V^{\frac{1}{3}}$ to suit each individual case, he had proceeded, after making this assertion, to deduce a formula to actually suit, as nearly as possible, the points which he considered insufficient

in number. It would be seen from the following Table that Professor Smith's formula gave slightly higher results:— The Author.

5-Coach Train; Weight, 115·2 Tons.			20-Coach Train; Weight, 429·4 Tons.		
Speed.	Resistance.		Speed.	Resistance.	
	Smith.	Aspinall.		Smith.	Aspinall.
Miles per Hour.	Lbs. per Ton.	Lbs. per Ton.	Miles per Hour.	Lbs. per Ton.	Lbs. per Ton.
10	3·3	3·3	10	3·07	3·1
20	5·1	5·0	20	4·32	4·3
30	7·7	7·4	30	6·09	6·1
40	10·9	10·5	40	8·29	8·3
50	14·7	14·0	50	10·9	11·0
60	19	18·1	60	13·9	14·0
70	23·9	22·8	70	17·2	17·4
80	29·2	27·8	80	20·9	21·1
90	35·2	33·3	90	24·9	25·1
100	41	39·2	100	29·2	29·4

He had endeavoured, from the result of the experiments described in the Paper, to deduce a simple formula satisfying the curves obtained. Although by far the greater number of experiments had been made with a five-coach train, the data obtained with the ten-, fifteen- and twenty-coach trains afforded sufficient evidence to show that the resistance per ton decreased in a certain ratio as the length of the train increased; and the curves drawn on the diagrams were as near to the mean as it was possible to draw them. Very many equations had been tried, and it had been found that the formula given in the Paper coincided more closely with these curves than any other. The experiments had been made with empty trains; to have tried a similar set of experiments with the same trains loaded would have caused a considerable amount of inconvenience on a busy line. Efforts had been made, however, to compare the effect of loaded and of empty trains by attaching the dynamometer-car to booked passenger trains. From the data obtained from these experiments with both loaded and empty trains, he had come to the conclusion that the length of the train had much more effect than the weight, in varying the resistance. Within the range of the trains experimented with, viz., ordinary express trains, there did not appear to be any evidence of a material difference between the resistance of empty and of loaded trains. Whatever difference there might be, a large number of experiments would be required to determine the law it followed, and the trains would have to be loaded so as to get as much weight upon the axles as

The Author. possible; for with ordinary passenger trains it was difficult to detect the small difference in the total weight of a loaded and an unloaded train. Therefore, under these conditions, he considered that, since the total load was directly proportional to the length of the train, and the length affected the resistance more than the weight, the expression for the resistance should have a denominator varying with the length of the train only. For instance, taking an empty five-coach train weighing 115 tons and hauled at a speed of 50 miles per hour, the total resistance would be 1,610 lbs. or 14 lbs. per ton; and if this train were loaded with passengers its total weight was increased to 140 tons, and according to the formula the total resistance for a train of that weight at the same speed was 1,960 lbs., or 350 lbs. more. That was, the full resistance had been allowed, viz., 14 lbs. per ton, for the additional weight, which was quite reasonable, because both the axle-friction and the miscellaneous resistances, which formed the major portion of the total resistance, varied directly with the load. The only item which warranted a reduction in the resistance was the atmospheric resistance, which would be the same whether the train was loaded or unloaded; but this would be very small on the extra weight compared with the other resistances, and it would be difficult to detect the difference. The following example would show the difference between his formula and that suggested by Professor Smith. If a five-coach train, having a length of 285 feet, and weighing when empty 115 tons, and when loaded 140 tons, were run at a speed of 50 miles per hour the resistances were as follows:—For the empty train: Aspinall, 1,610 lbs., or 14 lbs. per ton; Smith, 1,691 lbs., or 14·7 lbs. per ton. For the loaded trains: Aspinall, 1,960 lbs., or 14 lb. per ton; Smith, 1,818 lbs., or 13 lbs. per ton. This showed that, according to the formula in the Paper, it required a pull of 350 lbs. more to haul the train loaded than it did to haul it empty; the resistance per ton was the same in the two cases. According to Professor Smith it needed a pull of 127 lbs. more to haul the train loaded than to haul it empty; the resistance per ton being 1·7 lb. less when the train was loaded. Now increasing, for the sake of comparison, the length of each coach of the empty train to make it equal in weight to the loaded train (viz., 140 tons), its length would then be about 347 feet, and the resistances of this longer train would be:—Aspinall, 1,918 lbs., or 13·71 lbs. per ton; Smith, 1,965 lbs., or 14·04 lbs. per ton. From this reasoning it would be seen that, according to the Paper, it took 42 lbs. less to haul the longer train than it did to haul the shorter, both being of the same weight; according to Professor Smith it took

147 lbs. more to haul the longer train than it did to haul the shorter, both being of the same weight. To put the matter in a different way, his own formula showed that, with a train of constant length, the resistance per ton was constant; whereas, according to Professor Smith's formula, it decreased as the weight of the train increased. Again, with a train of constant weight, his formula indicated a decrease in the resistance as the length of the train increased; whereas Professor Smith's formula indicated an increase in the resistance under similar circumstances. In fact, the latter gave results exactly the reverse of those obtained from the formula given in the Paper. With two trains of the same weight, his formula showed that as the length increased each ton weight of the train offered less resistance, which was quite consistent; for, since the formula was only constructed to include a direct head wind, it followed that the wind would have less effect on the rear coaches as the length of the train increased. He had made experiments to ascertain the velocity of the wind in a train's length, and he had found that the velocity of air past the first coach was much greater than its velocity past the last coach. With a train 260 feet long, travelling at a speed of 40 miles per hour, the velocity of the air 20 inches from the carriage-body with a dead head wind was 42 per cent. greater past the side of the first coach than it was past the side of the last vehicle. This might be accounted for by the fact that the train carried a certain volume of air with it, and that volume was collected gradually along the train's length, being of small dimensions at the front and increasing towards the rear. Therefore the relative velocity of the air and the first coach must be greater than that of the last coach and the air in its immediate neighbourhood, because this latter air would have had motion imparted to it relative to the train. Since the velocity of the air relative to the train was greater at the front than at the back, it followed that the atmospheric resistance must decrease as the length of the train increased. Consequently, the difference in weight between a loaded and an unloaded passenger train being so small compared with the total weight, and this difference varying directly with the major portion of the resistances, it followed that the resistance per ton would be almost the same with either the loaded or the unloaded train when the extra weight was considered. Also, since the atmosphere had more effect on the bulk of the train than on its weight, the resistance per ton would be more affected by the length than by the weight. He had therefore included only the length of the

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The Author. train in the denominator of the formula, which formula, as previously stated, was intended to be applied only to rolling stock similar to that experimented upon. A formula for trains composed of goods wagons would have a denominator varying with the weight, and would necessarily differ from that obtained for passenger trains, because the difference in weight between loaded and empty wagons was considerable when compared with the difference in the case of passenger trains. For this reason a formula obtained with a particular class of vehicle did not apply to every train. With regard to the foregoing remarks on the difference in the velocity of the wind in a train's length, this variation showed that Professor Goss's experiments on small models were not applicable to a full-sized train. Mr. West's formula (No. 31, Appendix II.), referred to by Professor Smith, was obtained from Mr. Deeley's figures. Mr. Deeley had given[1] figures for speeds between 36 miles and 72 miles per hour, from which Mr. West had derived a formula for speeds from rest, and had estimated the speed corresponding to least resistance at about $8\frac{1}{4}$ miles per hour. This was pure assumption, since the result had not been obtained experimentally, but it just happened to be least at that speed according to the formula given. No doubt the resistance did decrease at first, but it attained its minimum at a speed of less than $8\frac{1}{4}$ miles per hour. He had tried several forms of wind-gauge, including one similar in principle to Mr. Macfarlane Gray's, but the upper vessel had been constructed with a series of cells to avoid the splashing of the liquid when the train was in motion. It had been found unsuitable, however, as the liquids had become mixed, as pointed out by Mr. Bryan Donkin. Such apparatus was satisfactory when used in boiler-tests, but the heavy gusts of wind in rapid succession which it had to indicate when on a railway train very soon disturbed the liquid.

Summary of Results of Experiments on Train-Resistance Carried out by Messrs. Vuillemin, Guebhard and Dieudonné.

These experiments were carried out on the Eastern Railway of France during the years 1862–1867, and were recorded in a Paper read before the "Société des Ingénieurs Civils de France."[2] Experiments were tried with carriages and locomotives by both coasting and hauling methods.

Resistance of a Single Carriage (Dynamometer-Car).—The carriage was a four-wheeled coach, oil-lubricated. Its weight was 5·4 tons. The readings obtained by each method were as follows :—

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[1] *The Engineer*, vol. lxxxv. p. 276.

[2] Mémoires de la Société des Ingénieurs Civils, 1867, p. 701.

The Author.

Speed.	Resistance.	
	Coasting Method.	Hauling Method.
Miles per Hour.	Lbs. per Ton.	Lbs. per Ton.
0	19·5	..
0·6 to 3·1	4·48	..
3·1 „ 6·2	5·6	..
6·2 „ 9·3	7·6	..
9·3 „ 12·4	9·6	..
12·4 „ 15·5	12·1	10·17
15·5 „ 18·6	14·1	..
21·7	17·0	..
31	..	20·38

The resistance to starting, 19·5 lbs. per ton, was afterwards verified by a small dynamometer with a spring, by which the hauling power was gently applied.

Resistances of Locomotives.—The locomotives experimented upon were in road-worthy condition, and they were accompanied by their tenders, which carried their ordinary loads. Two classes of engines were tried, viz.:—

Mixed engines with four wheels coupled, 6 feet 1 inch in diameter, weighing 49 tons. Cylinders 16½ inches in diameter.

Goods engines with six coupled wheels, 4 feet 3½ inches in diameter, weighing 47 tons. Cylinders 16½ inches in diameter.

The values for the coefficient of mean resistance were as follows:—

COASTING METHOD.

——	Speed.	Resistance.
	Miles per Hour.	Lbs. per Ton.
Mixed Engines	12·4 to 18·0	7·17
	18·6 „ 24·2	8·96
	24·8 „ 30·4	9·74
	31·0 „ 37·2	12·76
Goods Engines	12·4 „ 15·5	11·9
	15·5 „ 21·7	14·4
	21·7 „ 24·8	17·8

HAULING METHOD.

——	Speed.	Resistance.
	Miles per Hour.	Lbs. per Ton.
Goods engines. Six wheels coupled, weight 44·6 tons	14·8	21·3
Goods engines. Six wheels coupled, weight 47·8 tons	16·12	22·9
Mixed engines. Four wheels coupled, 6 feet 1 inch in diameter	27·9	14·3
Mixed engines. Four wheels coupled, single drivers	27·9	12·3

These latter figures are naturally higher than those obtained by the first method, because the speeds were higher and the lubrication of the cylinders and slide-valves was less perfect.

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STARTING RESISTANCES OF LOCOMOTIVES AND TENDERS.

Mixed engines . . . . . 35·6 lbs. per ton.
Goods engines (six wheels coupled) . . 44·13 „ „

RESISTANCE OF TENDER ALONE.

Speed, miles per hour . 18·6 Resistance, lbs. per ton . 11·6
„ „ „ . 27·9 „ „ „ . 15·7

RESISTANCE OF GOODS TRAINS.

Speed, miles per hour . 10·5 to 16·1 Resistance, lbs. per ton . . 7·05
„ „ „ 16·1 to 19·8 „ „ „ . . 8·84

RESISTANCE OF PASSENGER TRAINS.

——	Speed.	Resistance.
	Miles per Hour.	Lbs. per Ton.
Short trains . .	28·12	16·15
	35·96	21·44
	47·12	32·57
Long trains . .	27·9	13·39
	32·6	14·63
	37·2	18·03

FRICTION OF AXLE-BOXES.

Coefficient of friction at speeds between 0·62 mile and 3·1 miles per hour and at a temperature of 68° F.

Oil-lubricated axle-boxes . . . . . 0·018
Grease-lubricated axle-boxes . . . . . 0·032
Engine axle-boxes, oil-lubricated, at speeds of 15·5 miles to 18·6 miles per hour . . . . . 0·052

INFLUENCE OF TEMPERATURE ON THE RESISTANCE.

Grease-lubricated axle-boxes on passenger trains.

Low temperatures (26·6° F. to 32° F.) = 11·7 lbs. per ton.
High „ (59·0° F. to 68° F.) = 7·77 „ „

With oil-lubricated axle-boxes the influence of temperature was not observable.

Influence of Length of Train.—It was found that in passenger trains the resistance per ton became less as the length of the train was increased. The reason for this is that the atmosphere exercises a greater resistance to the first carriage than to any of the others, and the average resistance per carriage decreases as the number of the latter is increased.

In goods trains the length is generally much greater whilst the speed is less. The resistance of the air is then of less importance, whilst, on the other hand, the length has the effect of rendering the line of traction oblique on curves and

thus increasing the frictional resistance. In consequence it was found that in the case of goods trains the resistance per ton was in general augmented by an increase in the length of the train. The Author.

It was found that on a section of the line where there were numerous curves of 3,280 feet radius a train of forty-two wagons required 2·24 lbs. per ton more than a train of twenty-eight wagons over the same length.

Influence of Curves.—With passenger trains on a curve of 4,920 feet radius there was no increase of resistance if the speed did not exceed 31 miles per hour, but the resistance was increased by 5 per cent. when the speed was increased to 34·7 miles per hour with a train of twelve carriages.

With goods trains of forty wagons, running at a speed of 16·12 miles per hour, on a curve of 3,280 feet radius, the resistance was increased by 2·24 lbs. per ton, and by 3·36 lbs. per ton on a curve of 2,624 feet radius.

Influence of Wind.—It was found that with a wind blowing at the rate of 18·7 miles per hour a difference of 30 per cent. was made in the resistance of a passenger train according to whether it blew in the same or in the contrary direction to that in which the train was running.

The experimenters also met with many examples which prove that, without speaking of exceptional atmospheric disturbances, the resistance of trains is sometimes doubled merely by the action of the wind. The same train was found to present very variable coefficients on account of the winding of the line which caused the wind to cut the direction of motion at very different angles.

Relation between HP. developed by Engine and that exerted on Draw-bar.—In the case of a goods engine having six wheels coupled, 4 feet 6 inches in diameter, it was found that in one particular case 175 HP. was exerted on the draw-bar, 34 HP. was absorbed by the engine, or $\frac{192}{226} \times 100 = 85$ per cent. of the work exerted by the engine was transmitted to the draw-bar.