so that the advantage of a gradient of 4 feet per mile, over a gradient of 10 feet per mile (as estimated in 1838), was made to appear 160 per cent. more than the formula would show it to be. The formula, be it observed, appearing to agree with experiment, within the limits of this case.

Again: the saving of friction of 1 lb. per ton, supposing, for the sake of argument, that such a saving would be attained by increasing the diameter of the wheels, instead of being a saving of 12½ per cent. of the whole resistances on a level, at 40 miles per hour (as it would be, were the resistances constant at all velocities, and therefore the same at 1 mile per hour as at 10 miles per hour), would in practice be a saving of only 4 per cent. of such resistances, since such resistances (in a train of 80 tons, as assumed by Mr. Brunel) would have increased at 40 miles per hour to 25 lbs. per ton.

In giving these examples, it is wished that it should be understood, that the author does not give any opinion in the present place, as to the relative advantages of different diameters of wheels, or of different gradients or gauges. These arguments, by which particular arrangements were defended, are selected in order to show the application of the law of increase of resistances with the velocities, to the questions discussed, merely because they were the best practical examples of the importance of determining the true value of resistances to trains at various velocities which occurred to the author.

A glance indeed at a few of the applications of this important question of the variations of resistances, will satisfy every engineer, that it is no mere theoretical question, but one which is of the most direct practical bearing.

This Paper is illustrated by two sheets of diagrams, I. and II.

Mr. Harding reiterated the explanation of the diagrams given in the paper, and showed, that the resistance per ton to a passenger train of about 30 tons, at a velocity of 60 miles per hour, would appear to be upwards of 40 lbs. per ton, instead of 18 lbs. per ton, as had been stated as the results of other experiments, whose correctness might, he contended, be reasonably questioned.

Mr. Scott Russell said, he had been engaged in making a series of experiments on the Sheffield and Manchester, South Western, South Eastern, and other railways, for the purpose of testing the amount of resistance offered to the passage of trains at different velocities. The results only of those experiments had already been partially made public, and he was obliged to Mr. Harding for again bringing the subject before the Institution. They would now have greater weight, from the more recent researches of that gentleman, and it was gratifying to find their truth corroborated. So far as he understood the diagrams, he conceived, that the experiments they illustrated, would be found to agree with those of both Mr. N. Wood and Mr. Stephenson, and that the results of his mode of making the experiments coincided with the results obtained from five different sources, which circumstance gave him more confidence than he before had in it. In discussing the experiments with which he had been engaged, he had begun in this manner. In analysing the experiments made on the Liverpool and Manchester Railway, it appeared to have been proved, that the friction of a train amounted to a resistance of from 6 lbs. to 10 lbs. per ton. In the best class of carriages it would not exceed 6 lbs. per ton. He believed the law of resistance was composed of several elements, the first of which was the friction of the wheels and axles, which was constant at all velocities; another was the frontage resistance. But there were other elements of resistance. In comparing the tables given by Smeaton, he considered, that the resistances therein stated were much exaggerated. These experiments had been made upon thin discs only, and erroneous results had been obtained; if the experiments had been made upon bodies shaped like a long box, they would have been more analogous to the case of a railway train; the lateral resistance had not been considered, nor had the effect arising from the air rushing into the space, formed by the passage of the train, been taken into account. It was, however, at the present moment, unnecessary to go into these considerations. In various experiments, which Mr. Russell had made with Pitot's tube, he had found the resistance about half that which was given by Smeaton's tables. There was also another element in this formula; the first was, the 6 lbs. represented as the amount of axle friction; the second was the resistance of the air, and he had, in his calculations, taken the height of motion due to the velocity, and had then taken the weight of a corresponding column of air, and by multiplying it into the area of the frontage, he had got a series of results which represented the amount of resistance due to frontage. When he had done this, he deducted it along with the 6 lbs. constant, from the new total of the actual resistance by experiments, and found there was a large remainder. Now, what was that remainder? It appeared to be proved that the remaining resistance was due to the weight of the trains, and did not increase as the squares of the velocity, but directly as the velocity, and that the amount of the third element of resistance to a train. moving at the rate of 10 miles per hour, was = 3 lbs., at 20 miles per hour = 6 lbs., and at 30 miles = 9 lbs. per ton, and went on as the weight of the train; in every instance he found, that it increased as the velocity, but not as the squares of the velocity. There were many other elements acting on trains moving at high velocities, which, at low velocities had no effect.

Mr. Russell said, that the word concussion, used by Mr. Harding, might perhaps be objected to by some engineers; it was to a certain extent, however, explanatory of his meaning, although he found the difficulty of finding a term to express, exactly, the action. He had found that when standing near the atmospheric railway, when a train was passing at the rate of 60 miles per hour, an agitation was felt which shook the earth, like the action of piling engines. That agitation, or concussion, must arise from a certain expenditure of power, and that expenditure must be a loss of power, varying as the velocities.

There was still another element of resistance, arising from the flexure of the rails, although partially provided for by the springs; that resistance was also as the velocity. There was yet another element to be considered. Every swing of a train produced an amount of friction, from the abrasion of the impinging flanges of the wheel against the inside edges of the rails; that, also, acted in proportion to the increase of the velocity itself, but not as the squares. All these made up the elements comprising the resistance to trains; the influence of the weight, and the wearing away the rails, must also form a portion of the elements of resistance, and all these elements increased as the velocity, but not as the square of the velocity.

Assuming that the consideration of these elements was comprised in the formula, he was glad to find, that the representation of the formula agreed with the results obtained by Mr. Harding.

Mr. E. Woods bore testimony to the general accuracy of the tables, and to the correctness of the facts brought under the notice of the Institution. Mr. Harding had mentioned three methods of determining the resistance suffered by trains. The first was from the force of gravity on inclined planes. The second was arrived at from the experiments upon the atmospheric railway. The third was by the analysis of the power of the locomotive engine. Mr. Woods considered the two first of these to be unexceptionable, as the force of gravity could be arrived at with accuracy; the amount of resistance which Mr. Harding had attributed as arising from that source might be received as correct, particularly as he had been careful to select experiments where an uniform velocity had been attained.

The second method might be considered even less liable to variation; but as to the third method of deducing the resistance from the effect of the steam, due to the quantity of water evaporated, he could not entirely accord with Mr. Harding's views. It was not correct to assume that all the expansion took place in the cylinder.

He was of opinion that it commenced at the regulator, where it was throttled, and that it passed along to the cylinders at a greatly attenuated pressure; for that and other reasons he could not place so much dependence on the third method.

It was of importance to the question, to ascertain whether Mr. Harding had considered only the amount of frontage resistance, or had taken into his calculation the friction, arising from the body of air, which he conceived must be carried along with the train in its passage. Judging from the analogy of the mass of water carried along by vessels of different forms at various velocities, this must form an important element of resistance, which should be considered; in his opinion it would account for a great part of the remainder sought by Mr. Russell.

The meeting must feel indebted to Mr. Harding for bringing so many valuable results together, and he was gratified to find, that his own opinions were generally in accordance with the results deducible from the experiments.

Mr. Samuda said, if he understood Mr. Harding's position correctly, he could not agree with him in his deductions. He regretted that he had not with him the papers containing the results of some experiments he had made, and which differed widely from those spoken of by Mr. Harding, both as to the amount of friction and the amount of duty performed on the atmospheric railway. He would ask Mr. Harding to state what, according to his formula, would be the resistance of a train weighing 100 tons, travelling at a speed of 43 miles per hour on a level, with the atmospheric system, the piston carriage being in front?

Mr. Harding replied, after making the calculation, that the resistance to such a train would be, according to Mr. Scott Russell's formula, 2,300 lbs., or 23 lbs. per ton.

Mr. Samuda said, he had carried a train weighing 100 tons, at a velocity of 43 miles per hour, with a vacuum of 20 inches, at a resistance of 17 lbs. per ton, under conditions not very favourable to the train; as, if the line had been longer, the speed would have still augmented, for, at the end of the fourth mile, the mercury in the barometer indicated 20·5 inches. The train was then travelling towards Forest Hill.

Mr. Harding reminded Mr. Samuda, that in approaching Forest Hill, there was a descending gradient which reduced the resistance 3·3 lbs. per ton.

Mr. Samuda resumed, and stated that the results of many of the experiments should be modified by the circumstances under which they were made, for it had been generally remarked, especially on railways like the Great Western, where considerable speed was attained, that a period of at least 12 or 15 minutes elapsed before the regular speed was arrived at, and during all that period there was a constant acceleration of velocity, which would have misled any experimenter. He did not either coincide with the method of trying the experiment upon trains, when descending inclined planes. He had made some experiments which led to very different practical results, and they had been tried upon an inclined plane of sufficient length to have allowed of an uniform velocity being attained. He found, that when the loaded carriages were at the latter part of the train, and the lighter carriages in front, a less speed was attained when descending an incline by their own gravity; and there was more resistance, the hindermost carriages appearing to jostle those in front, and to throw the flanches against the rails, causing a large amount of lateral friction; but when the heavy carriages were placed in front, the train acquired a greater velocity, and travelled much more steadily. These circumstances had doubtless been observed by others, and he only mentioned them in order to show, that unless all proper precautions were taken to secure fair trials, erroneous deductions might be drawn from apparently well conducted experiments. The difference of results obtained by him on an incline of 1 in 100, allowing the trains to start at the top, from a state of rest, and descend by their own gravity, was 29 miles per hour in the former case, and 37 miles per hour under the latter conditions.

In some experiments, upon a train weighing 80 tons, at a velocity of 50 miles per hour, he found the resistance to be about 23 lbs. per ton. These experiments were, however, made under somewhat disadvantageous circumstances, and he could not entirely rely upon the results.

Mr. Harding said, he had adduced the third method, as he had distinctly stated in his paper, not as a means for accurately measuring the resistance, but chiefly as a check on the other modes adopted. He was well aware, that although it was easy to arrive at the amount of pressure in the boiler, what was going on in the cylinder was much less evident; however, quite enough was known to induce doubts in his mind, as to the correctness of the results arrived at with the indicator, by Mr. D. Gooch, and he was of opinion that in the experiments in question the error had arisen, from all the circumstances attending the experiment not having been taken into consideration.

Sir John Rennie, President, said, as it might assist members in the discussion, the Secretary should read some extracts from a letter received from Mr. D. Gooch, embodying some observations which he intended to have made, had he been able to have attended the meeting.

The following extracts from a letter, dated June 9th, 1846, were then read:—

"As the discussion on the resistance of trains on railways, is to be renewed at the meeting of the Institution to-night, I regret I am unable to attend, for the purpose of explaining an error Mr. Harding has fallen into, in stating it is my opinion, that at 60 miles per hour the resistance on a train is 18 lbs. per ton. It is true that in a table I made for the purpose of showing the comparative power of several engines, I there made use of 18 lbs. as the resistance per ton, at 60 miles per hour, measured from an indicator card, taken from one of the Great Western engines. This experiment, however, as I explained to the Gauge Commissioners, was not made for the purpose of ascertaining such resistance; but at the time was the best result I could obtain, and which under similar circumstances, is not very far from the correct amount, and as above stated, for the purpose I used it; it mattered not whether I took 18 lbs. or 30 lbs., as the resistance in each case. As a comparison between the power of the engines, the result will still remain the same, and this was all the table was intended to show. It is therefore not my opinion that 18 lbs. is the exact resistance of a passenger train, at 60 miles per hour, and I have never so stated it. I however think Mr. Harding's figures, as I recollect them, give amounts at least one-third more than the resistance experienced on the broad gauge. I beg to enclose you a table of experiments made by the Gauge Commissioners, and the results of which Mr. Harding referred to as supporting his views.

"You will find I have here calculated the resistance from the consumption of water, and I have given all the data necessary for checking the results. The loss from back pressure on the piston of the 'Ixion,' when working at 50 miles per hour, is about 8 lbs. per square inch; and at 60 miles per hour, about 10 lbs. I do not recollect the figure given by Mr. Harding as the resistance of the third broad gauge experiments; but he ought to have explained, that, in that case the water was no measure of the power, particularly in the down journey; as he was aware that the engine slipped so badly for upwards of 12 miles during the trip, that her consumption of water was very much beyond what it otherwise would have been, and that from small rain falling, the rails, engines and carriages, were in such a dirty state as to give no measure of the resistance on a favourable day. These facts ought in fairness to have been stated by Mr. Harding. The first and second experiments were made with a strong side wind blowing. They all therefore show a resistance, much greater than would exist, if the day was calm and hot, as on the day that 18 lbs. was taken as the result. The figures, as given in the table, may be taken as follows: that during bad weather the resistance of a passenger train, including the weight and friction of the engine, was—

	Miles.	lbs.	
At an average speed of	52·8	27·4	per ton
„ „	55·2	23·2	„
„ „	52·3	29·7	„
„ „	57·5	26·1	„

"Within the last ten days I have made two experiments, with an engine with 18 inch cylinders, 24 inch stroke, and 8 feet wheels.

"A gross load of 184 tons, including the engine, was taken at an average speed of 55 miles per hour, with an evaporation of water equal to 5·2 cube feet per mile, down a gradient averaging 4 feet per mile; the steam being cut off at $\frac{15}{24}$th of the stroke. The power exerted in this case, including the gravity, was 24·8 lbs. per ton. Several of the carriages had hot axle-boxes.

"On a second experiment with the same engine, cutting off as before, at $\frac{15}{24}$th, a gross load of 94 tons was carried at a speed of 67 miles per hour, with a consumption of water equal to 4·5 cube feet per mile, or a resistance of 34 lbs. per ton; deducting 2 lbs. per ton for gravity, as it was on an ascending gradient.

"As I am making preparations for a series of experiments on this matter on a large scale, I offer no opinion on these facts, and shall be glad if they are of any use in the discussion on the subject."

Mr. Bidder remarked upon the letter, that it had certainly been the general opinion, that the amount of resistance of a train at 60 miles per hour, had been stated at 18 lbs. per ton, without the qualifying circumstances now brought forward. He begged to state that he differed from Mr. Gooch in thinking that the weather, on the 17th December, when the third broad-gauge experiment which is quoted by Mr. Harding was made, was such as to affect the amount of resistance; he was present and considered the day a very favourable one for this purpose.

Mr. Harding stated, in reference to the letter of Mr. D. Gooch, whom he regretted was not present, that he would read the heading of Mr. D. Gooch's table, laid before the Gauge Commissioners, which induced him to take it for granted, that Mr. Gooch did hold the resistances to ordinary passenger trains, at 60 miles an hour, to be 18 lbs. per ton. The heading was as follows:—"Table showing the dimensions and power by experiment of the passenger engines on the following railways, with their calculated power." "In the above table the calculated powers of the engines are taken all at a speed of 60 miles an hour, and the pressure of the steam is obtained from the evaporative power of the boiler, assuming each square foot of fire-box surface to evaporate two cubic feet of water, which is the quantity shown by the experiments. The resistance per ton of load at that speed is taken at 18 lbs., which is the result of the experiment shown by indicator card, No. 4. No. 1 is the only exception to this, which is taken at a speed of 65 miles per hour, and the resistances at 20 lbs. to the ton." These are the words from which he, Mr. Harding, certainly had concluded that Mr. Gooch did consider the resistances to a passenger train to be experimentally 18 lbs. per ton at 60 miles per hour.

With great respect for Mr. D. Gooch, he must also demur to the statement, that it mattered not, as far as the purpose of the table in question was concerned, whether the resistance per ton was taken at 18 lbs. or 30 lbs. per ton. It must make a difference in all calculations respecting the power of engines; in the present instance it did make a material difference. The result arrived at in the table for taking the resistances at 18 lbs. per ton was, that a certain engine (which happened to be a narrow gauge engine) could take 1 ton at 60 miles per hour, while another engine (which happened to be a broad gauge engine) could take 120 tons at 60 miles per hour, a result which Mr. Harding disputed at the time as being entirely fallacious. As Mr. Gooch was absent, he would merely remark, that if one of the principal elements on which the table in question was based, namely, the resistances being 18 lbs. per ton at 60 miles, was withdrawn by Mr. Gooch as incorrect, the table at any rate fell to the ground, and the results it exhibits must be regarded as admittedly erroneous.

He had selected the experiment on the broad gauge, made under the sanction of the Commissioners, of the 17th December, 1845, solely because it was much the calmest day on which any of the experiments had been made, and a day, he believed, on which the resistances under favourable circumstances might be measured.

Mr. R. STEPHENSON said, he should regard with doubt, any results arrived at merely from a comparison of the powers of boilers, assumed from the relative areas of their fire-boxes. The evaporating power depended rather upon the superficial area of the tubes, and upon this consideration the long boilers have been introduced; indeed he did not think, that the proper length for evaporation had yet been arrived at, and it was evident how much more improvement was required, when in the short boiler locomotives, the temperature of the air escaping at the chimney, after passing through the tubes, would melt lead (600°) and even, at times, would sublime zinc (730°). By the improvements already effected, one superficial foot of fire-box was now doing as much duty as two feet did with short tubes.

As regarded the resistance of trains and the method of measuring the amount, he considered the quantity of water evaporated, should only be considered as a means of corroborating the results otherwise arrived at; it must be clear, as an absolute measure of resistance it was liable to error. For instance, a boiler capable of evaporating very freely, might be attached to an engine which, either from its original defective construction, or being out of order, would use the steam wastefully; or again, when travelling up an incline, a much higher pressure of steam might be used, than upon a level or in descending an incline, and in the latter cases the steam would be used more expansively, and the amount of resistance deduced from the quantity of water evaporated would be incorrectly valued.

With respect to the experiments on resistance instituted by the British Association, Mr. Stephenson had examined the results obtained, very minutely, and had compared them with his own and other experiments, and he was bound to state his great reliance upon their correctness, indeed, he thought that none exceeded them; on that account he had used them with the utmost confidence in his examination of the Atmospheric system.

For practical purposes it was essential to ascertain, not the resistance of five or six carriages, but that of long trains under all circumstances, and in the examination and in the statement of the results, all these circumstances should be given minutely, or else it would be impossible to arrive at correct deductions. Take, for instance, the case of a strong wind blowing directly against the front of the leading carriage, and the engine; the frontage resistance would be easily appreciable from the measure of the area of frontage; but suppose the case of a long train and a side wind; the resistance would be materially varied whether such a train was crossing an exposed plain, or an elevated embankment, or was sheltered in a deep cutting. He had noticed cases of trains of ten or twelve carriages, on the London and Birmingham railway, which could only travel under these circumstances at a speed of about 20 miles per hour, on an embankment, increase their velocity to 35 miles per hour, as soon as they entered a cutting. It was evident, that the experimenters for the British Association were aware of this, as they endeavoured to make all their trials on calm days, or with little wind right a-head.

Mr. Rennie, in reply to questions from the President stated, that as far as his recollection served, the experiments of the committee named by the British Association, had first for object to ascertain whether inclined planes made much difference. They accordingly ran trains weighing 80 tons backwards and forwards along the line, and the result was, that whether travelling up or down inclines, or on a level, the consumption of fuel and water was much the same. The gradients varied, he believed, between 1 in 96 and 1 in 180. Secondly, trains were permitted to descend inclined planes by their own gravity, and it was observed, that at about 26 miles per hour, the velocity became uniform. The general results were however very complicated and were difficult to analyse. It appeared, that the front surface was not of the greatest importance in causing retardation, and that the shape of the front of the carriage did not make any material difference.

Mr. R. STEPHENSON said it was clear, that if the engine had been used as a dynamometer, in the cases just quoted by Mr. Rennie, an erroneous result must have been arrived at, as it was evident the resistance must have been greater in ascending than in descending the inclines. The resistance must have differed, although the consumption of fuel remained the same.

He thought no better method could have been devised, than that of allowing the trains to descend by their own gravity and observing when the velocity became uniform.

The results obtained from the experiments on the Atmospheric railway were not so satisfactory, in his opinion, as other elements entered into the consideration, and erroneous conclusions had been drawn.

Mr. HARDING reiterated his explanation of the diagrams exhibited. The first sixteen experiments were those of the British Association, and of these it was necessary to know all the attendant circumstances, in order to be convinced of their accuracy, for as Mr. Samuda had stated, material changes might be effected by the different positions in which the heaviest carriages had been placed during the experiments and in other ways. He (Mr. Harding) had carefully selected all the experiments made during calm days, and in which uniform velocities had been attained by retardation from higher, rather than by acceleration from lower speeds, as the safest results were thus arrived at. In these experiments a speed of 37 miles per hour was reached. In those tried by him on the broad gauge, 45 miles per hour was attained; and when he required to go as far as 61 miles per hour, he was obliged to have recourse to the Atmospheric system. In these high velocities the discrepancy between his results and those of Mr. D. Gooch became more evident, as he showed at least twice the resistance Mr. D. Gooch contended for. He thought, however, the results shown were entitled to credit, as there was great uniformity in them, they appeared to demonstrate, that there was a rapid augmentation of resistance in proportion to the increase of velocity.

Mr. Bidder said, he believed the process, by which some of the results commented upon, had been obtained, was, by taking an engine, the boiler of which had from 98 feet to 100 feet of evaporating surface, multiplying that by 2, which gave 200 feet. Then assuming the largest fire-box of the long boiler engine to have an evaporating power of 100, and that it could convey a gross load of 50 tons, at a velocity of 60 miles per hour, and deducting from the gross load the weight of the engine and tender, the remainder showed the amount of useful load conveyed. Then in comparing the case of the 'Ixion,' which evaporated 2 cubic feet of water per superficial foot of area of the fire-box; it was found, that in a long boiler engine 3 cubic feet of water were evaporated per superficial foot of fire-box; taking that alone into consideration and omitting the surface of the tubes, which however was the true standard of power. Then as to resistance, it had been shown, that a gross load of 430 tons could be conveyed over 1 mile, at a speed of 30 miles per hour, by an expenditure of 7 cubic feet of water; whilst to convey a gross load of 80 tons, at a velocity of 50 miles per hour, 4 cubic feet of water were required. Hence five times the load only required about twice the quantity of water in the shape of steam; the difference of speed being only $\frac{2}{5}$ths. In these trials the dimensions of the fire-boxes and the length of the tubes, were identical in both engines; the only difference being that in the high speed, the wheels were not coupled, whilst in the other case they were coupled; but this difference was more than met, by the fact of the steam being at a higher pressure in the boiler and being cut off at a portion of the stroke in the one engine, whilst it was not cut off in the other. In his opinion, the test of the consumption of water as steam, for a dynamometer, was very liable to disturbing causes, and to lead to erroneous conclusions.

The experiments upon the Atmospheric system were much more likely to set the question at rest; and in examining the results already obtained, it must be remarked, that the resistances observed at Dalkey should have an allowance made, on account of the peculiar locality of that railway, the trains travelling through numerous bridges and in a confined space between high walls, where other elements than that of simple resistance of the air, might have come into play and have disturbed the results.

Mr. Scott Russell drew Mr. Stephenson's particular attention to the classification of resistances assumed in the formula, and wished to have his views as to the constitution of the remainder, which Mr. Russell had endeavoured to show was in great part accounted for, by the power absorbed in what had been termed concussion. His observations induced a belief, that in the rapid transit of a train, a wave of motion was communicated to the comparatively inelastic earth, and a proportionate expenditure of power occurred. This element of resistance increased simply as the velocity, and not as the square. To this should also be added all that resistance causing noise, a vibration of the rails, motion or vibration of the parts of the engine and carriages, wear and tear, and flexure of light rails and insecure fastenings. It would therefore be evident, how very essential it became, in attaining high velocities, to avoid flexure, by having stiff heavy rails, and having the chairs and sleepers of proportionate dimensions, securely attached to each other, and firmly embedded in the ballast. These were practical points, but they involved some of the principal considerations of the theory of railway resistances.

Mr. R. STEPHENSON had frequently discussed this point with Mr. Scott Russell, and he confessed, that some months ago he had differed with him as to the amount of resistance arising from the concussions of the impinging of the wheels against the rails and from the motion of the carriages, from flexure of the rails, and even from the movements of the passengers on their seats. He now thought it was evident, that whatever created motion must absorb some power, but he was not prepared or the statement of Mr. Scott Russell, that this amounted to one-third of the velocity in lbs. per ton. He should, however, say, that within the last few months, on consideration of the subject, he had become, to some extent, a convert to his views. He now was satisfied that there was greater loss from these causes than had been hitherto admitted; but he was pet uncertain to what extent that resistance applied in retarding trains. The lateral motion of the train in a side wind or by vibration, must absorb great power, and even the apparently smooth motion of the wheels upon the rails produced a loss, which formed a portion of the remainder sought.

Mr. HOMERSHAM suggested, whether in the case of a side wind acting upon a long train of carriages, considerable absorption of power might not arise from the conical form of the tire of the wheels, which being keyed fast upon the axles, must necessarily revolve simultaneously, although the smaller diameter was bearing upon one rail, and the larger diameter on the other side.

Mr. F. BRAITHWAITE referred to the statement of the simple resistance offered by the air to the bulk of the engine and train passing through it. In the recorded experiments upon the resistance to bodies passing through fluids, it was found, in the case of a boat travelling along a canal, that the speed was reduced in proportion to the decreased width of the water way, and that the resistance increased in proportion to the friction of the water against the sides and bottom of the channel. Was it not probable, that the same action influenced, to some extent, the results obtained in experiments on the resistance to bodies moving through air, which was only a fluid of less density than water? It appeared there was a considerable difference in the conclusions of Mr. Harding and Mr. Samuda, as to the allowance for the resistance offered by the front area of the carriage, and the lateral area of surface of the train. Now, it had been stated in evidence, by Mr. Bury, that in a heavy gale of wind acting upon the side of a long train, it became necessary to employ seven locomotive engines, on account of the friction of the flanches of the wheels against the rails. Mr. Braithwaite was of opinion, the whole question resolved itself into the amount of resistance offered by the superficial area presented by the train, under circumstances which were of so varying a nature, and combined so many elements, that it would be very difficult to lay down any formula sufficiently comprehensive to meet all cases.

Mr. SAMUDA thought the third method, of measuring resistance by the quantity of water evaporated in a locomotive engine under known circumstances, as used by Mr. Harding, was liable to mislead observers, and, indeed, the assumed state of things, appeared to be doubted by some, and disputed by others, it would, therefore, be safer to rely more upon the two first methods of test.

He then alluded to the method of obtaining the results quoted in Mr. Gooch's letter, the particulars of which he corroborated, and stated that, under identical circumstances, a resistance of 18lbs. per ton had been shown, as had been quoted by Mr. Gooch to the Gauge Commissioners, for the sake of argument, and adopted as an arbitrary datum, when urging another position entirely foreign to the question of resistance.

Now, the difference in the results arrived at by Mr. Harding, as compared with those given by Mr. Samuda, amounted to nearly 100 per cent. It was necessary, therefore, for the sake of the elucidation of the truth, to ascertain where the discrepancy existed, and with all deference to the great authorities on locomotives, he must record his dissent from the results as to the measurement of resistance hitherto obtained from them.

The same might almost be said with respect to the process adopted by the committee of the British Association. In his experiments on the New Cross incline he found, that with the same train, and on the same morning, without any change of weather or other disturbing cause, he succeeded in obtaining a difference of 20 per cent., by merely altering the position of the weighted carriages in the train.

He thought the atmospheric railway offered the best opportunity of arriving at correct results, but it was difficult to arrive at them, inasmuch as a considerable distance was required at such high speeds for arriving at an uniform velocity, as there was a constantly accumulating speed for a longer period than was permitted by the distance between the stations. It was, therefore, difficult to draw any correct deductions from the results already obtained. He was, however, convinced, that a less amount of power was exerted than Mr. Harding had supposed, and he was confident, that upon a longer length of railway, where the atmospheric system could be correctly tested, very much more advantageous results would be arrived at.

Mr. Samuda proceeded to examine Experiment XXIII. of Mr. Harding's paper, and observed, that although the number of carriages and the general particulars were given correctly, the assumed weight of the train was wrong; instead of 21·5 tons, as given in the table, it should have been 23 tons, as the carriages, with passengers, weighed on an average, 4 tons each, and the piston and heater carriages weighed together 12 tons. This discrepancy, however, did not cause any error sufficiently important to disturb the general result. He had constructed a diagram representing the gradients, taken opposite the several posts, $\frac{1}{8}$th of a mile apart, along the atmospheric line between the Dartmouth Arms and Croydon. They were, with some slight corrections, taken from Mr. Harding's paper; and upon the same diagram, by another line representing the speed of the train at the same points, it was shown, that the maximum velocity was not balanced by the pressure at the point calculated upon in the paper. Mr. Samuda was of opinion, from a careful examination of the diagram, with the figures of the experiment No. 23, it would be shown, that if the retardation occasioned by ascending one side of the steep viaduct, and the acceleration consequent on descending the other side, had been taken into account, different results would have been arrived at. His conclusions were, that 25 lbs. per ton, was the utmost resistance shown by that experiment. Other experiments, made upon trains running in the opposite direction, confirmed these views, as in these latter cases, the resistance at 60 miles per hour was 22 lbs. per ton.

Mr. Harding had quoted the steam used in the locomotive as a measure of resistance, because it was an interesting problem, well deserving the attention of the Institution, and also because it had been stated, that by Mr. Gooch's indicator experiments, the resistance of only 18 lbs. per ton had been arrived at, at 60 miles per hour. Now, in the experiments made by him in conjunction with

Mr. Gooch, under known conditions, agreed upon between them, by applying the test in question, and deducting arbitrarily, as much as 20 per cent. for loss of power, double that amount of resistance was shown; he therefore was warranted in arguing as he had done, and doubting Mr. Gooch's former indicator experiment; because if the opposite views were entertained, a deduction of 60 per cent. must be made from the theoretical power of the locomotive. It appeared, however, from Mr. Gooch's letter, that 18 lbs. per ton was only an assumed resistance, therefore it was useless to reason upon it, and he should not have done so, but for the heading of the table at page 153 of the Minutes of Evidence before the Gauge Commission and the fact that much of Mr. D. Gooch's evidence was based on the results of that table.

In the recent experiments on the Great Western line by Mr. D. Gooch, in the letter read this evening, it would appear, that the resistance at 55 miles per hour was 24·8 lbs. per ton, and at 67 miles per hour was 34 lbs. per ton, which results approximated to those contended for by him (Mr. Harding), and totally differed from the former amount of resistance understood to be contended for by Mr. D. Gooch.

He then contended, that by analyzing the groups of experiment No. 23, the resistance was shown, and might be fairly considered to be not less than between 40 lbs. to 50 lbs. per ton, an uniform velocity being arrived at. The measurement of the resistance was effected with greater facility on the atmospheric railway, than on a locomotive line, and there could not be any doubt, he thought, that the former system was the best known dynamometer for the purpose; for by observing the degree of exhaustion in front of the piston, shown by the barometer, supposing the air behind it to be at the atmospheric pressure, and allowing 100 lbs. for the friction of the piston itself, a very accurate estimate of the resistance for propelling a given weight must be arrived at.

Mr. Bidder thought the greatest difficulty arose from discrepancies as to matters of fact in the observations, which might be perhaps attributed to want of uniformity of system in making them. It had been stated, that the 'Ixion' locomotive had propelled a train weighing 100 tons, with a consumption of water which indicated a resistance of 18 lbs. per ton. Now he must accord completely with the hesitation expressed by other members, in receiving that method of testing the resistance from the quantity of water evaporated; it involved several difficulties and was so mixed up with the consideration of the point where the steam was cut off, and the amount due to back pressure, &c., that he was inclined to reject the evidence in that case, as quite inapplicable.

The load was stated to have weighed 80 tons; on the journey to Didcot, although it was alleged there was a strong adverse side wind in one direction, but favourable on the return, the difference of elevation being 360 feet, and the difference of time between the journeys being only 4 minutes, which was equivalent at the rate maintained to $3\frac{1}{2}$ miles; then 18 lbs. per ton would be equal to 150 feet only, yet there was an elevation of 360 feet to be accounted for, and a discrepancy must exist somewhere, he must therefore contend, that many of the facts were left undetermined, and upon them no reasoning could be based unless the premises were agreed. As to the experiments on the atmospheric system, he did not hesitate to state, there could be no better occasion than the present, for setting the question at rest, as that system offered the best means for testing the resistance to all kinds of traction. He had the returns of some experiments made for Mr. Brunel, and he found, that with a load of six carriages, reckoning 12 tons for the piston carriage, making in all 25 tons, with a pressure of 20 inches, on starting from Croydon, the maximum velocity was 45 miles per hour; and from that point, the pressure began to decline to $16\frac{3}{8}$ inches. That indicated, at 40 miles per hour, a resistance of 60 lbs., a result entirely differing from the others which had been quoted. The question of acceleration and of the power consumed and given out, admitted of the most rigid investigation. Mr. Harding had stated, that on the day he made a series of experiments, the vacuum was maintained with much uniformity, but there might be a side wind occasioning a resistance of 45 lbs. to 50 lbs., and other circumstances, which would interfere with these deductions. Now if these experiments were examined, they might indicate a more favourable result, as it was just possible that a side wind might have affected the trains, yet a resistance of 40 lbs. to 45 lbs. per ton, was indicated at a velocity of 45 miles per hour. In all such experiments, the evidence required to be most accurate, and the amount of all the circumstances which might have arisen, should be given, or else the statements became useless, and practical differences must be stated in order to correct theoretical deductions.

Mr. R. Stephenson agreed in the opinion of the want of accurate records of all the circumstances, under which the experiments on resistance to traction had been made, and without questioning the good faith of the records of the experiments, he might be permitted to doubt, whether all the attendant circumstances and conditions had been noticed. He was also of opinion, that the atmospheric system afforded a good practical mode of testing dynamometrically the amount of resistance, and he was so convinced of the importance of attempting to set the question at rest, that he would suggest to the Institution to make a requisition to the Council relative to the advantage of applying to the Croydon and Epsom Railway Company for permission to make a series of experiments on the atmospheric railway, for the purpose of ascertaining the amount of resistance to trains of carriages on railways at different velocities. In making this suggestion he would have it distinctly understood, that he had no intention of entering upon the comparative value of the atmospheric and locomotive systems of traction, or the commercial value of the two plans; but would restrict the investigation rigidly to testing the amount of resistance of trains of different weights at various velocities, which the system was capable of showing so accurately, by merely recording the height of the barometer, the weight of the train and the uniform velocity attained, with the necessary allowance for the friction of the piston in the tube. If the funds of the Institution were not legally applicable for the purpose, and a subscription opened among the members was not adequately responded to, Mr. Stephenson would be ready to expend any reasonable sum to attain results so desirable for the scientific world generally; but more especially for Civil Engineers, and he thought it would be more fittingly done through the instrumentality of the Institution, than by any other means.

The length of 5 miles of pipe now completed and at work on the Epsom line, was eminently adapted for the experiments, and the objection would be annulled, which had been raised as to the inaccuracy of the experiments on the Dalkey line, arising from the shortness of the line and the consequent difficulty of arriving at an uniform velocity. He admitted that a small error might have occurred in those experiments, in consequence of that circumstance, but the length of the Epsom line was sufficient to arrive at the most accurate results, and he trusted every exertion would be used to obtain the requisite permission.

Mr. F. Braithwaite hoped that Mr. Samuda would give every assistance, both in using his influence with the Railway Company, to grant the permission for trying the experiments and affording his aid in their ultimate prosecution.

Mr. Samuda replied, that he should be happy to give every assistance in his power; but as the matter was entirely in the hands of the Company, he thought it would be better for him to stand aloof from participating, even in the request for the permission to try the experiments; but, as far as he was concerned, whether the Company granted the permission or not, he would recommend that every facility for observation, and all information should be given, that might be required for the purposes of the Institution.

Mr. Hawkesley said, that since he had presented to the Institution the formula relative to the friction of fluids in tubes, circumstances had occurred which induced him to pay attention to the resistances to trains passing through the atmosphere. In the course of these investigations he had occasion to refer to the experiments of the British Association, and found that his formula corresponded completely with those experiments. Mr. Hawksley then referred to his formula, and stated that if

L were made to represent the length of the train in yards,
D „ „ the diameter of the train in inches,
V being the velocity of the train in yards per second;

then H would represent the height in inches, of a column of water pressing against the head of the train, the weight of which would be in equilibrio with the resistance arising from the friction of the atmosphere; to this must be added the frontage resistance, which might be taken from Rouse's tables, and the resistance of the wheels and axles. From this must be deducted the value of the diminished resistance, arising from the two circumstances, first, that the air had not to be gathered into the mouth of the tube, as in the case of air moving withinside a pipe; and, secondly, that less resistance existed in air moving freely outside a pipe, than when it was moving within it, subject to the friction against its internal periphery. He had just made a rough calculation, and assumed that the weight of a train might be taken at one ton per yard of its length; to such a train the resistance at 60 miles per hour would be 5000 lbs., presuming the train weighed 100 tons, and extended 100 yards. Then taking the resistance due to displacement at 13 $\frac{95}{100}$ lbs. per ton, which must be added, as also 8 lbs. per ton for resistance from the wheels and axles, a total resistance of 72 lbs. per ton would be given. In this rough calculation, which was only given in illustration of a principle, the two cases of the value of the diminished resistance of the air were not taken into consideration, and therefore it must not be considered that 72 lbs. per ton was the actual resistance. He might have taken imperfect data, and he only made the calculation to show the applicability of the formula, and to demonstrate that a speed of 60 miles per hour could not be maintained without great expense.

Mr. CRAMPTON stated that, having tested the diagrams made on the Great Western Railway engines, three or four years ago, and having found the quantity of water calculated from the diagram itself invariably agree with that measured as consumed, to within 5 to 8 per cent.; he was induced to try the diagram given in the Gauge Report, and found that 212 feet of water would be required at 60 miles per hour, which agreed with the evaporative power of the boiler, and he considered the diagram a good practical test of the tractive power of the engine, when the indicator had been proved to be correct. He had also made a diagram, taking practice for his guide, and found the broad gauge experiment, as calculated by Mr. Harding, did not agree with practice. Mr. Harding's result was 33 lbs. per ton traction, after deducting 20 per cent. for loss from the theoretical result. Mr. Harding did not appear to have taken into account the point of cutting off the steam, but had assumed the expansion to have commenced at the pressure noticed in the boiler and ended at 35 lbs. on the inch, giving a cylinder full of steam at 35 lbs. pressure, which was that due to the measured amount of water evaporated. Mr. Crampton's diagram was for the purpose of showing that the pressure in the boiler should not be taken for the pressure in the cylinder, as that was due to the quantity of steam in the cylinder at the portion of the stroke at which it was cut off, and that, taking these elements into account, with the other known losses, the water evaporated in the boiler would give 24·5 lbs. per ton traction, at 55 miles per hour, instead of 33 lbs. as stated by Mr. Harding; and that, if the weight of the train was taken at 98 tons, instead of 93 tons, the weight of the engine and tender being supposed to exceed the stated weight by 5 tons, the tractive power would then be 23·3 lbs. per ton. It was very remarkable how closely the quantity of water coincided in this experiment with that contained in Mr. Gooch's diagram at 60 miles per hour, it being 212 cubic feet in each instance.

Mr. Bidder read a totally different result from the diagram; he calculated that the quantity of water must be compared with the volume of steam, estimated, in reference to the point in the cylinder, where it was cut off, and, unless this was done, it would be impossible to arrive at a correct result. Now, if the power derivable from the quantity of water converted into steam were advantageously applied, there would be an effective pressure of 76 lbs. instead of 56 lbs.

Mr. Stephenson thought an error had been committed in assuming that steam, under these circumstances, was a permanently elastic fluid, and followed an unvarying law of expansion. Now, from the moment of its being cut off, condensation commenced, and the temperature decreased; a certain portion of the water, therefore, ceased to exist in a state of steam. This property of steam had been very cleverly investigated by Dr. Lardner, in his paper read before the Royal Society, December 20, 1832. He contended, "that steam, raised from water, under any pressure whatever, admitted of indefinite compression and expansion, without returning to the liquid state. The effect of its compression would be to evolve heat and raise the temperature, that of its expansion, to absorb heat and lower the temperature; but in every state of density it would possess exactly that temperature which it would have were it immediately raised from water under the pressure which it had acquired by compression or expansion. The only cause of the restoration of vapour to the liquid form, was the abstraction of heat, and this cause would be equally operative, whatever might be the state of the vapour with respect to density." Now, as a given temperature was only compatible with a given density of steam, if that position was changed, the whole tenor of the observations being deduced from it, the arguments would be based upon erroneous premises.

It was acknowledged that the investigation of an indicator diagram from a locomotive engine, was an operation of the most complicated character, and as such was liable to all kinds of error.

Mr. Stephenson had, in the course of a former discussion, suggested to Mr. Scott Russell, the propriety of using the marine engine, as a dynamometer for ascertaining the resistance to bodies moving in water, but, by a little consideration of the matter with Mr. Russell, he became convinced that the marine engine could not be so used; and yet, the low pressure marine engine was much better understood than the locomotive engine, which his experience convinced him was as yet very imperfectly comprehended, especially as to the varied action of the steam entering the cylinder at the velocity and pressure now used.

Mr. Crampton stated, with all deference to Mr. Stephenson's superior knowledge on these matters, he was under an impression, from the examination of all the good diagrams he had taken from condensing and non-condensing engines by the best makers, that the loss by condensation was not practically felt, and he had never seen a diagram which showed a diminished pressure at the end of the stroke, beyond what theory would give; but the pressure shown generally, in practice, exceeded that given by theory; this he thought might be accounted for by the valves not being quite tight, consequently the leakage through them would increase the pressure in the cylinder, and in the event of that loss taking place, which was ascribed to condensation, the result would be less than the amount he had stated.

Mr. Crampton must record his conviction that the indicator was a better test than was generally believed, for arriving at the maximum effect given out by the engine, under all ordinary circumstances.

Mr. Harding said, that Mr. Crampton had entirely misapprehended the mode of calculating the effect of the steam in the experiment in question adopted by him (Mr. Harding), he begged to refer Mr. Crampton to the paper itself, where he would see that he (Mr. Harding) had in the first place calculated the effect of the steam according to the bare theoretical effect a certain weight of steam generated under a certain known pressure in the boiler, expanded into a certain known bulk, in filling the cylinder the necessary number of times. Mr. Crampton would find that he fully admitted, in the passage in question, that a large deduction must be made from this for the loss of power in the mode of applying the steam, such as in wire drawing and in other ways ; he then showed the amount of resistance per ton of the train, which was indicated, if 20 per cent. was arbitrarily deducted for loss of power, stating at the same time, and reiterating that statement in other parts of the paper, that any such mode of estimating the effect of steam must be regarded as a mere approximation, and that the measure of resistance so afforded must not for a moment be supposed to approach to the trustworthiness of the direct measures afforded either by the atmospheric apparatus or the effect of gravity. The result of the calculation to which he wished to draw attention was, that if Mr. D. Gooch's estimate of resistance of 18 lbs. per ton at 60 miles per hour had been applied to the case of the experiment in question made by Mr. Gooch, in conjunction with others, the total loss of power in applying the steam in a locomotive must have been more than 60 per cent., a result which he (Mr. Harding) was inclined to think could not be correct.

In the paper there were thirty-three resistances shown, as measured by the more direct methods, and only two as approximately indicated, by deducting 20 per cent. from the theoretical effect of the steam. The valuable discussion which had arisen on this point he thought indicated the interest attached to the question of the loss of power in applying the steam in locomotive engines, to which question, by giving the results in question, he wished to draw attention.

Before the discussion was closed he would beg to draw attention to the facts of the experiments on the atmospheric railway, and stated from the paper, that it would be seen, the velocities were taken in each case, at the point where they became uniform, which was also the highest velocity attained during the trip: the point at which the pressure on the travelling piston and the resistances at that velocity, might be considered to balance each other, was, it appeared to him, indicated by the mean pressure on the travelling piston, during the part of the journey passed over at such maximum velocity.

Mr. Samuda allowed, that some discrepancies might naturally have been detected in the recorded results of the experiments, and he contended, that they were inseparable from investigations of such a nature. In the arguments he had used, he had sought to point out those experiments which enabled clear deductions to be drawn, whether they were good or bad. These facts being taken, and allowance being made for adverse or favourable circumstances, if they existed, he must contend, that fair results might be obtained, and upon such statements he had arrived at a conviction, that at a velocity of 45 miles per hour the resistance did not exceed 20 lbs. per ton.

It was curious to observe how closely the series of results of the experiments approximated throughout one entire day and how widely they differed on another occasion, not only from the former series, but among each other, although apparently tried under identical conditions, and with similar precautions. He had before alluded to the difference produced by the position of the loaded carriages in the train, and there were numerous conditions which would equally affect the results.

Mr. R. Stephenson said, with respect to the experiments tried by the British Association, the difference said to be caused by placing the weighted carriages before or behind, in descending inclined planes, had been noticed to them, but it was not especially attended to by them, because it was considered that with ordinary trains the carriages were not all equally loaded, but a certain amount of uniformity of lateral motion was attained, by screwing the buffers tight up against each other.

Sir John Rennie, President, said the members must be fully sensible of the importance of the proposition made by Mr. Stephenson, when it was a subject of discussion whether the resistance was 18 lbs. or 72 lbs. per ton, for such was the actual difference of opinion; therefore although it was irregular to make the proposition at that meeting, for addressing the request of the Croydon Company for permission to make the experiments, he would promise on behalf of the Council the best attention to the subject, and that it should be laid before the next meeting.

Mr. Harding said, all the experiments from which he had drawn his deductions, were made with the greatest care, on calm days, on a perfectly straight part of the road, with the carriages in good order, and with a general determination to avoid all disturbing causes or errors in the deductions, and all the circumstances attending the experiments had been carefully noted in the report which was drawn up.

Mr. Harding must record his dissent from the result of the application of Mr. Hawkesley's formula, and he believed some serious error existed in stating the case.

Mr. R. Stephenson said, Mr. Hawkesley's figures were not given as statements of the positive resistance, but as a hypothetical case, of which the data might not have been clearly defined.