Mr. M'Clean inquired what mode was proposed for overcoming the mechanical difficulty of starting the train with a single-cylinder engine.

Mr. Makinson replied, that he had devised a means of doing this; but he had not prepared either a model or a drawing of it, as he had proposed in the Paper merely to define the limits of the pressures and forces necessary for the starting of the engine, and so prepare and clear the way for the mechanical solution of the problem. The plan which had occurred to him as a feasible one was as follows:—Two parallel friction discs, having their working faces inclined to each other, capable of being thrown in and out of gear, were pressed upon and gripped between them the two sides of the tyre of the driving-wheel. Pressure applied by the driver to a hand-wheel, and multiplied by proper gearing, turned the friction discs, and with them the driving-wheel.

Mr. Kitson thought the members were indebted to the Author, for his elaborate demonstrations of the disturbing forces which, in his opinion, operated upon the locomotive engine; but as a practical man, Mr. Kitson was not disposed to admit, generally, the correctness of all the data. In the first place, it was difficult to apply theory to the working of the locomotive engine; and he could hardly understand how it was possible to apply all the tests that would be necessary, to enable such carefully tabulated results to be prepared. The chief reason for the introduction of the double-cylinder locomotive was the practical difficulty of starting a train with a single-cylinder engine, a difficulty which he at present regarded as insurmountable, and which was of itself sufficient to cause the idea of a single-cylinder locomotive to be at once dismissed. Another difficulty seemed to have been overlooked by the Author,—that was, that the centre of gravity would have to be raised so high, with a cylinder in the centre, as to create a serious objection, which would counterbalance any presumed advantage of a single-cylinder engine. Nor was he disposed to admit, that the locomotive engine deserved all the blame attributed to it for the lateral motion of the train, as he thought that defects in the permanent way had a great deal more to do with it. With a light locomotive engine it was possible that many of these forces might have a serious effect, in increasing the unsteady motion; but with locomotives of the present weight, the influence of the cylinders upon the large mass was much less than he was prepared to expect, as was shown by the results now submitted. The tendency to produce an oscillating motion by the engine was to a great extent neutralised by its weight, and was not communicated from the engine to the train; but was principally owing to the difficulty of keeping the road in perfect order. Although great pains had been taken in demonstrating, theoretically, some of the results, yet practically he considered the object sought would not be obtained by the reintroduction of the single-cylinder engine.

Mr. T. E. Harrison said, the point suggested, of equalizing the motion of the engine by having a single-cylinder, was tried in another way by the late Mr. Robert Stephenson. An engine was built by him with three cylinders, a large one in the centre and two smaller ones at the sides, and was tested for many years on the North-Eastern Railway. The main object was to avoid the lateral motion. He had constantly ridden upon that engine, and having been in the habit of riding upon other descriptions of engines at the highest velocities, he confessed he had never found any advantage in that respect in the engine to which he alluded over other well-balanced engines. There was no doubt a great difference in the balancing of engines; and one class, known as the Rocking Horse of Kent, with outside cylinders, badly balanced, had caused more accidents from running off the line than any other engines with which he was acquainted. Much depended upon the state of the permanent way, and that could not always be kept in perfect order, subject as it was to variations in the temperature, to changes in the weather, and to other influences. Two things required special attention; first, the permanent way should be maintained in as good order as possible, and in the next place, the balancing of the engine should be rigidly enforced; and here he would remark, that whatever might be professed, he believed engines were not sufficiently put through the test of the weighing machine. When turned out of the factory, the engines were put upon the machine, and the springs were properly adjusted; but when an engine was tested, which had been at work some time, it was often found that, for some reason or other, it was not properly balanced. That was a question simply as regarded the adjustment of the springs, but in the design of the engine he believed sufficient attention was not always paid to the balancing. He had noticed a great difference in engines in respect of their motion, and it undoubtedly was a difficult question, but he could not agree, that by the plan suggested, of using one cylinder only, the steadiness of the engine would be improved; because in the three-cylinder engine to which he had referred, the necessity of raising the boiler, to get in the middle cylinder, caused the centre of gravity to be so high, that all the expected advantage in steadiness was lost, and if a single cylinder was employed the difficulty would be increased.

Mr. Makinson said, in reply to questions, that in Diagram No. 6, the tendency to produce sinuous motion was that due to the pressure of the steam only, without any allowance being made for the momentum of the parts; and that throughout the series of diagrams, the pressure of steam per square inch of piston, referred to the path of the crank pin, was given.

Mr. F. J. BRAMWELL said, the opinion appeared to be entertained. that in this Paper theory and practice were at variance with each other. Now he believed it was a truism, that theory and practice could never be at variance, when the theory itself was perfect and sufficient, *i. e.* worked out so as to embrace all points, and when the practice had been carefully observed and properly reported. In this case there could be but little doubt, that the practice had been carefully reported, for observations had been made by too many men of experience to leave that in dispute. If, therefore, there were in this instance discord between theory and practice, it must arise from inaccuracy in the theory, and not from imperfect appreciation of the practice. He would briefly state what he considered the inaccuracies were in the present Paper. The disturbing forces had been divided by the Author into two classes,—those which arose from the pressure of the steam in the cylinder propelling the engine,—and those which arose from the momentum of certain parts,—and it would be convenient to keep to that classification. With regard to the former, and assuming the parts to be without weight and without momentum, he understood the Author put forward this position, that there were working pressures on the piston, divided by the ratio of the crank to the radius of the wheel, acting alternately on the two sides of the engine, and that these tended to produce sinuous motion. This, in Mr. Bramwell's view of the case, was an error; for when two wheels were keyed fast upon a shaft, of such strength as not to yield appreciably under torsion, there could be no difference of pressure (of this kind) between the two sides of the engine; and that was true, wholly irrespective of the number of cylinders, or the places where they were put. In order to show this he would refer to diagrams, Figs. 1, 2, and 3 (p. 86), for the elementary character of which he apologised. He had supposed, for convenience, as the Author had done, that the cylinder, one wheel, and the bearing were infinitely thin, so that they might all be considered as lying in one plane, and therefore there would be no occasion to take into account the places where the pressures bore upon the shaft. Fig. 1 assumed a wheel 6 feet diameter, and a crank 1 foot in length, and that the piston in its forward stroke pressed with a force of 3 tons. The black arrows indicated the pressures due to the piston, and the dotted arrows the opposite pressures, or those due to the cylinder cover. It was clear if there were a pressure of 3 tons driving the piston to the right, the tendency to drive the crank shaft to the right was as 4 : 3, as the crank and the radius of the wheel together would be a length of 4, while the radius of the wheel would be only 3. But that was in part counteracted by the pressure of 3 tons tending to drive the cylinder and framing to the left hand, leaving a balance of 1 ton for the propelling pressure of the engine, or the pressure on the piston divided by the multiple that the radius of the driving-wheel was of the crank.

Fig. 1.

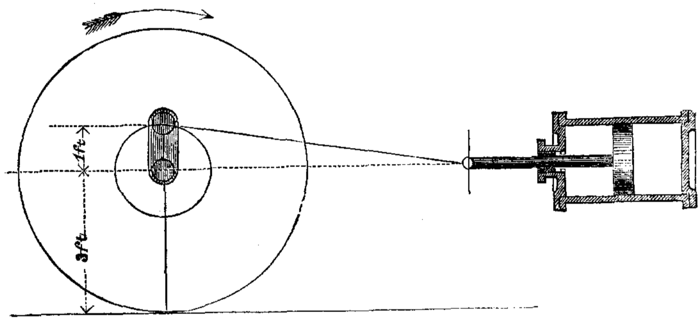


Fig. 2.

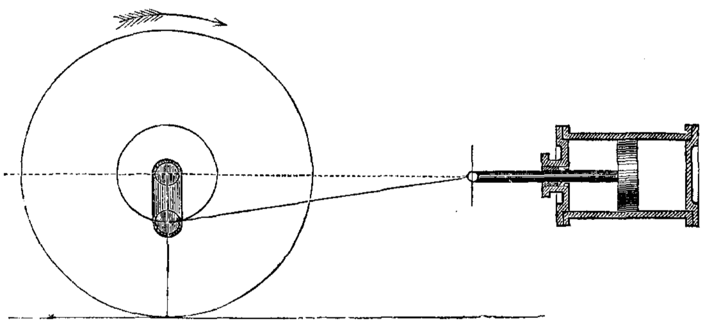
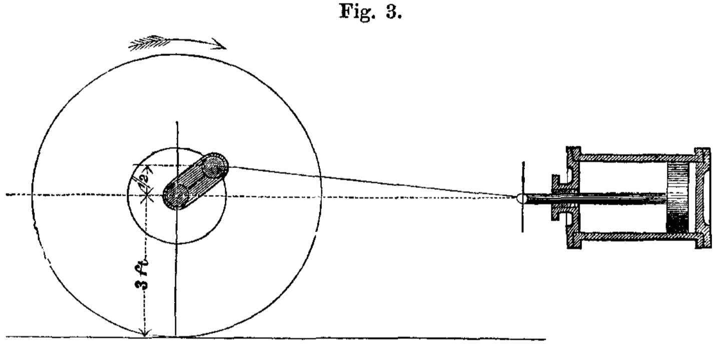


Fig. 3.



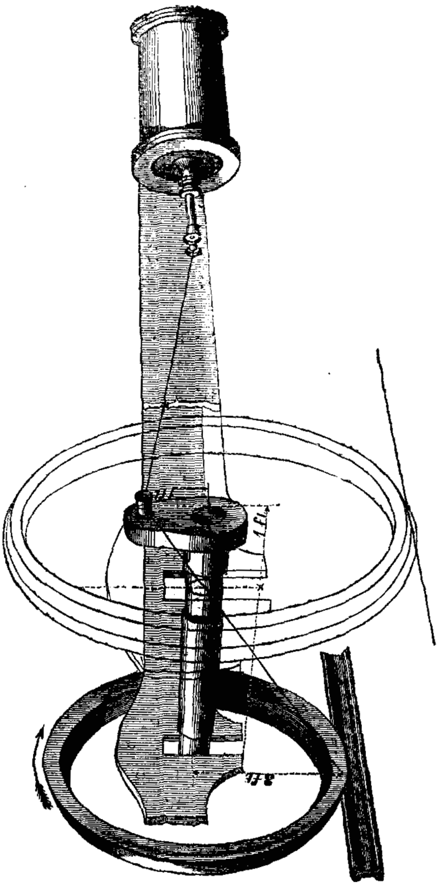
In Fig. 2 the piston was shown making the backward stroke. In this case the crank pin, instead of being above, was below the shaft. Therefore 3 tons pressure on the piston produced only 2 tons pressure upon the crank shaft; but 3 tons on the cylinder-cover produced 3 tons upon the frame and shaft, and the difference between 2 and 3, was the 1 ton for the driving pressure. In Fig. 3, the piston was shown as making its forward stroke with the crank at 30° from the horizontal. Following out the reasoning used when alluding to Fig. 1, it would be seen that the pressure now tending to propel the engine was only $\frac{1}{2}$ ton; therefore it was clear the unbalanced pressure upon a bearing at any time was that due to the force driving the engine by that particular bearing, and that it could be nothing else.

Having, he thought, established that point, he would refer to a diagram (Fig. 4, p. 88), which, in order to test the principle, was illustrative of an extreme case; an engine, being assumed with one (the shaded) wheel only, and one cylinder, the piston being supposed to be in the act of making a forward stroke. The result of that would be to put a pressure of 3 tons upon the crank pin to the right,[1] driving the crank in that direction, but it would be reduced by the pressure from the cylinder-cover. Taking an imaginary line from the crank pin to the driving-wheel, and another through the centre of the shaft from the bearing to the centre of the wheel, then as the piston put on a pressure of 4 tons in the right-hand direction, at the point where these two lines crossed, this was balanced at that point by an equal pressure obtained from the cylinder-cover acting in the left-hand direction; and it would be seen, by tracing the two imaginary lines to the parts where they touched the driving-wheel, that the piston-pressure resulted in an effort to turn the wheel upon the rail, with a pressure to the left-hand of 1 ton (which might be called the abutment pressure), while the cylinder-cover put a pressure of 1 ton to the right-hand on the bearing close to the wheel, which was, in truth, the driving-pressure. Such an arrangement of one wheel would no doubt tend to produce sinuous motion, with a force proportioned to the distance of that wheel from the centre of gravity of the engine. But if the other wheel were taken into consideration, as being fixed on the shaft, then it would be seen that an engine which was capable of producing 1 ton of tractive force, must distribute it over the two bearings, and that there would therefore be $\frac{1}{2}$ ton on each bearing; though the engine were driven by one cylinder, it was manifest it could not put a pressure

[1] Mr. Bramwell wished to correct an error in the diagram (Fig. 4), viz.—the bearing on the right hand, instead of being drawn close to the crank, was shown away from it, and it had been put in such a position that unhappily the imaginary line indicating the transmission of the force from the crank-pin to the point where the driving-wheel touched the rail, appeared to cross it. He would request the meeting therefore to dismiss from their minds the position of the bearing, and suppose it to be in the plane of the cylinder.

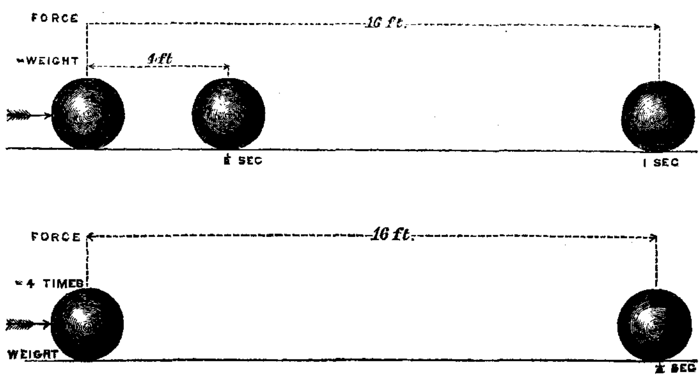
upon one side of that engine, which was not equalled by that on the other side at the same moment. He trusted that he had made it apparent, that there could not possibly arise any sinuous motion from the driving pressures. If not true, then he would ask what would happen. Take the case of a paddle-steamer,

Fig. 4.



with a pair of wheels, and, as had occurred in his own practice, with the driving-engine on one side. Now, if the position of the cylinder in relation to the wheels it worked, whether those of a locomotive, or the paddles of a steam-boat, had anything to do with the amount of the driving-pressure on either side, that vessel ought to have carried helm on the side nearest to which the engine was. Again, in the case of a screw not fully immersed, and therefore producing a sideway pressure upon the bearing, such pressure ought to be greater, or less, according as the engines were placed aft, or nearly midships. Now Engineers knew, that no such result could happen in the case of the paddle-wheel, or of the screw, and he trusted his observations would lead to the conviction, that it was equally impossible for it to happen in the case of a locomotive. With respect to the disturbances arising from the momentum of the parts, no doubt there was such disturbance, but he could not help thinking that the Author had given it an undue importance, and that he had been led to do so by a neglect of this fact, that though the force of the parts in motion increased as the speed got higher, the times through which they acted decreased. The space through which these forces were capable of moving an engine must be equal for all speeds. Referring to another elementary diagram (Fig. 5), in the upper portion a ball was shown, which was assumed

Fig. 5.



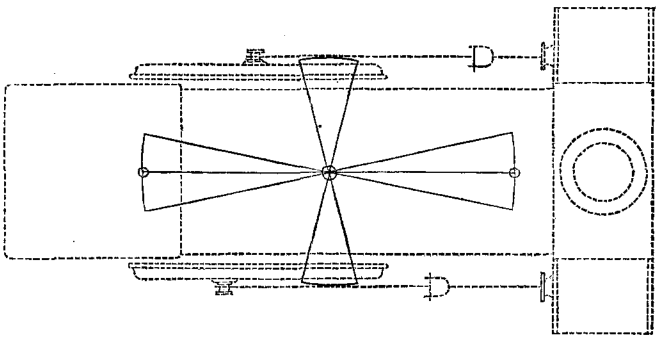
as being moved towards the right by a force equal to its own weight; therefore at the end of a second of time it would have moved through 16 feet, and in half a second it would have moved through 4 feet. In the lower part of the diagram a ball was shown which was assumed to be impelled by a force of four times its own weight. The result would be, that in half a second it would be moved four times as far as the 4 feet motion of the other ball, or 16 feet.

The consideration of this diagram (Fig. 5) showed, that if there were two forces, one of which was four times as great as the other, but acted for only half its time, this greater force would produce the same amount of motion as the first force. He did not say, however, the terminal velocities of the balls would be the same in the two cases, as no doubt, in the instances cited, the ball impelled by the greater force through the same distance would have a terminal velocity equal to twice that of the other. Applying these considerations to the question under discussion, if a locomotive had a velocity of one mile an hour, the force due to the momentum of the parts might be called 'one,' and the time during which it acted also 'one.' If the velocity were increased to 2 miles, the force would be increased as the square of the velocity (or 4), but it would only act for half the time. Similarly, if the velocity were increased to 10 miles, the force would be increased 100 times, but would only act for a tenth part of the time. Thus it would be seen, that whatever the speed of the engine, the distance through which the forces moved it must be the same for all velocities. The next point to which he wished to direct attention was the distance which the forces alluded to by the Author ought to move such an engine as he had described. Assuming, as the Author had done, the weights of the moving parts belonging to each cylinder to be 492 lbs., or in round numbers 500 lbs., and that 300 revolutions per minute were made, giving 63 miles per hour, or sufficient to fulfil all the conditions of a railway, then 500 lbs. and 300 revolutions per minute, at a radius of 1 foot, which was the length of the crank of the engine under consideration, gave, in round numbers, a maximum alternately of momentum and inertia of about thirty times the 500 lbs.

Therefore, each set of moving parts would give a pressure of 15,000 lbs. at the velocity he had stated. He thought it was in accordance with the Author's diagram to say, that the forces could not be taken as being of more value than was equal to their maximum through two-thirds of the whole revolution; and as 300 revolutions per minute had been assumed, which gave $\frac{1}{5}$th of a second for a revolution, it followed that the force of the moving part—viz., the 15,000 lbs.—could only act through $\frac{2}{3}$ of $\frac{1}{5}$ second, or $\frac{2}{15}$ths of a second. But it must be borne in mind, that even of this time, short as it was, only the latter half could be employed in producing motion, because it was clear the first half must be occupied by destroying the motion left by the immediately preceding impulse in a contrary direction, and that, therefore, in effect $\frac{1}{15}$th of a second was the time during which the momentum of the parts would operate to produce motion.

Having ascertained the time, the next point for consideration was the distance of the force from the centre line of the engine, for which purpose the weight of the engine must be taken into account, and a position be fixed for the centre of gyration of that weight. Diagram Fig. 6 was prepared to illustrate these points. He had assumed the centre of the cylinder, *i. e.*, the centre of the moving parts, to be 3 feet from the centre line of

Fig. 6.



the engine; that the engine weighed 60,000 lbs., or from 27 tons to 28 tons; that the weight was uniformly distributed over a length of about only 16 feet, giving the centre of gyration at 4 feet 6 inches on each side of the centre of the length. These proportions would reduce the 15,000 lbs. force of the moving parts to 10,000 lbs. when applied in the centre of gyration; and as this 10,000 lbs. would be equal to only $\frac{1}{6}$th of the whole weight of the engine, the effect would clearly be to move the engine only $\frac{1}{6}$th part of the distance through which it would be moved by the action of gravity operating for the same time. As this force would act for the $\frac{1}{15}$th of a second, and as the distances moved through were as the squares of the times, it would follow that the space in this instance would be only

$$\tfrac{1}{6} \text{ of } \sqrt{\tfrac{1}{15}} = \tfrac{1}{6} \text{ of } \tfrac{1}{225} = \tfrac{1}{1350},$$

and the $\frac{1}{1350}$th part of 16 feet, through which a body would fall in a second of time, was equal to about $\frac{14}{100}$ths of an inch. That being so, he trusted he had shown, that with a locomotive, the crank shaft of which was making 300 revolutions a minute, with a crank 1 foot in length, the moving parts of each engine weighing 500 lbs., and the weight of the locomotive being from 27 tons to 28 tons, there would be an amount of motion equal to only $\frac{14}{100}$ths of an inch; and it must be remembered, that while this would be the extent of each motion, the number of such motions, as they occurred at each revolution of the crank shaft, could not increase for a given distance travelled over, although from the greater number of revolutions per minute when the velocity was high, there would be a greater number of movements in a given time.

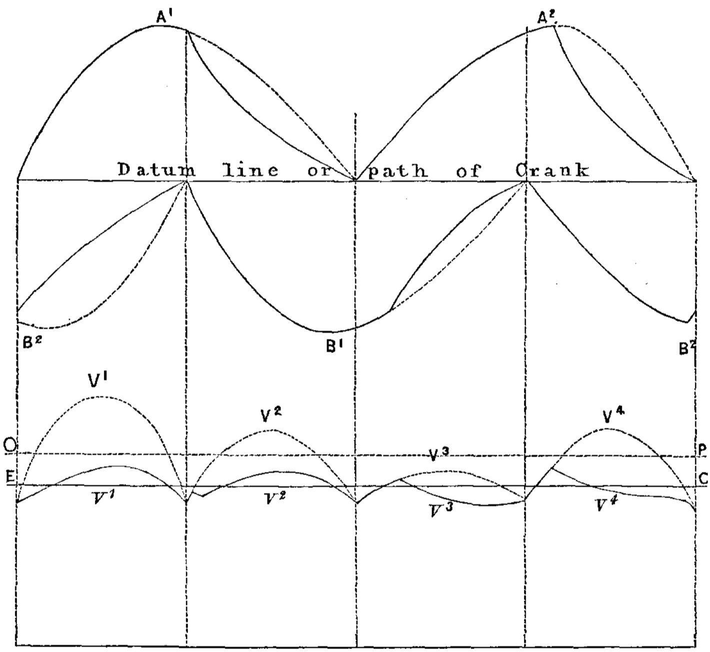
If these conclusions were correct, he thought it must be admitted, that the disturbance from the momentum of the parts was not so great as the Author imagined, and that the corrections proposed would make theory accord with practice. But even if these disturbances were so grave, then Mr. Bramwell would not seek a remedy by the construction of single-cylinder engines, to which there were great practical objections, while there were many better contrivances. There was the plan introduced by Mr. Bodmer, as early as the year 1834, of having two pistons in one cylinder, with the piston-rods issuing at opposite ends of the cylinder, and provided with suitable connecting-rods to the crank shaft. In 1841 Mr. Bodmer modified this plan, by having the two piston-rods to issue at one end of the cylinder, the one rod being made hollow to allow of the passage of the other. An engine of that kind he saw at work many years ago. Further, there was the 'Duplex' engine in the International Exhibition of 1862; also drawings by French Engineers, who proposed to employ four cylinders, two for giving a forward, and the others an aft motion. To his mind any one of these plans was superior to that which had been proposed by the Author. Supposing, however, a single-cylinder engine were to be used, he believed it would not be impossible to scheme an implement to enable the driver to start the engine. He had introduced into some heavy reciprocating machinery, a starting screw, which worked the machine up to the time when it was in a position to move without such assistance, and then this starting screw was simply thrown out of gear, by the mere act of the machine; but he hoped no one would ever be called upon to devise a similar plan to be employed upon locomotive engines.

Mr. J. Scott Russell said, although he had pursued a somewhat different mode of investigation to that adopted by Mr. Bramwell, yet it was curious that he should have arrived at almost identical conclusions, but for another size of engine; that was to say, taking the forces exactly as the Author had done, he found the total lateral translation in the mass of an engine by those forces would be in one case $\frac{1}{5}$th of an inch, and in another $\frac{1}{10}$th of an inch. He considered the valuable part of this Paper was the careful analysis which had been made of the forces as actually measured from the diagrams of Mr. D. K. Clark's observations, and the manner in which the various effects were recorded; all of which must be regarded as tending to eliminate the effects produced by the momentum of the parts. At all events, as he understood the matter, the diagrams truly represented the resolved transverse forces tending to displace the engine, horizontally, off the rails;

and to those, therefore, as the results of an investigation, which he had every reason to believe had been carefully carried out, he would apply himself. As far as he had followed this investigation, the practical effect of these disturbing forces would be this:—That if the engine wheels had no flanges, and the engine was going forward at the velocities stated, on a perfectly smooth surface, it would perform this vibratory oscillation, first on one side and then on the other. That being so, and taking the analyses, as given by the Author, of the times during which those forces acted at a speed of 35 miles an hour, he observed that each of those was acting during about $\frac{4}{16}$ths of a revolution, and as at this velocity a revolution was made twice in a second, it followed that each of the diagrams represented forces acting during the $\frac{1}{8}$th part of a second. The effect of that, compared with the effect of gravity, gave 3 inches as the total displacement which would take place, supposing those forces to act during an instant of time. In the next place, those forces were, as nearly as he could make it, the $\frac{1}{15}$th part of the mass which they moved; and it followed therefore that the lateral translation produced by the force in one of the diagrams was $\frac{1}{5}$th part of an inch to the right or to the left, as a maximum when the force was uniform. Taking the diagram as composed of two straight lines, one-half this amount, or the $\frac{1}{10}$th part of an inch, would represent the horizontal translation on either side. It appeared to him, that the inference to be drawn from this inquiry, as regarded the light to be thrown by theory upon a practical question, was that it was most desirable a theoretical investigation of this kind should have put in a visible form a series of tendencies, which were very small, but which certainly existed, and that every practical man should know those tendencies, and where they lay. The Author had, in his opinion, done good service in bringing forward the subject so plainly and so clearly, for when it was known where a bad tendency existed, however slight it might be, practical precautions could be taken to prevent its increasing and to diminish it. But it seemed to him that the subject should have been carried further, so as to embrace the measure of the effects of those tendencies; for it was not worth the trouble of inventing cures for tendencies, until the strict and rigid measures of the effects which those tendencies produced had been ascertained. If the Author would do this, before the Paper was printed, for the actual engine of which he had all the measures, then the communication would be more complete, and by that means he believed theory and practice would be thoroughly reconciled. In the early days of railways comparatively little attention was paid to the perfect balancing of the revolving and reciprocating masses in the locomotive engine; but now that this was done, railway travellers were not conscious of those serious jolts in every revolution, which the appearance of the diagrams would lead them to apprehend. The Paper gave rise to one valuable suggestion, the importance of allowing considerable play between the flanges of the engine and the rails. These should not fit each other too closely; and it appeared that, in a good engine, a lateral free play of more than $\frac{1}{5}$th of an inch on each side of the centre line did take place, and considerably more must be allowed. It was much more comfortable to stand on the foot-plate of an engine when going round a curve, than when running on a perfectly straight line, as in the latter case there was a series of oscillations, and all the road did on the curve was to guide that side, and prevent the engine going towards the other side. It was important that attempts should not be made to remedy small evil tendencies, by serious and formidable measures. He thought, if Mr. Bramwell and he were right in their measures of the horizontal translation, that this amount of oscillation should not lead to the conversion of the double-cylinder engine into a single-cylinder engine, with all the mechanical inconveniences which attached to the latter. He was sure that no engine-driver, who had limped home with only one cylinder at work, or who had one or two inclines to surmount, would wish to 'improve' the double-cylinder engine by making it a single-cylinder engine. He was glad that justice had been done to Mr. Bodmer's plan of balancing the engine. He had noticed substantially the same plan as Mr. Bodmer's in an Austrian engine in the International Exhibition of 1862, yet if it turned out that the measures of the effects given were near the truth, he thought no one would recommend a considerable deviation from the established form, to get rid of tendencies, the measure of whose effects was really so small.

Mr. COWPER could not agree that there was any necessity for having resort to a kind of Cyclops engine with one cylinder, in order to remedy the defects mentioned, in the balancing of locomotive engines. Indeed it hardly seemed to require an argument to prove, that it was unwise so to construct an engine, as to produce the greatest possible amount of snatching at the drag chains, or irregularity in the tractive power; to say nothing of putting a crank with twice the ordinary strain upon it, in the centre of the axle, where it was weakest, and where in fact there was no room for it to go round under the boiler. The result of concentrating the whole power in one cylinder would be, that in place of a somewhat distributed rotative force, there would be two portions of each revolution during which the engine would not even have power enough to drive itself, and must therefore be propelled by the pressure of the train behind. This would be a great evil; but a still more formidable objection would arise from the power being excessive at two other portions of each revolution, nearly twice what it ever was, with an ordinary engine having two cylinders. Practical locomotive engineers knew that the weight on the driving wheels had to be adjusted so as to give sufficient adhesion to the rails; and that often this weight was quite as much as it could be made with safety. Now if one cylinder only were used, nearly double this weight would have to be applied. In ordinary heavy engines, and particularly those for luggage trains, it was generally absolutely necessary to couple at least two pairs of wheels, in order to obtain sufficient adhesion; and with a single-cylinder engine, of the same power, it would be impossible to get the extra adhesion required to make the wheels 'hold' the rails. It was unnecessary further to allude to the starting of the train, as that was so great and self-evident a difficulty, that any proposition for a single-cylinder engine was incomplete, without the means being defined by which the engine was to be started. With regard to the plus and minus pressure caused by the momentum of those parts of an engine which moved horizontally, in a straight line, backwards and forwards, he was well aware that it was a matter worthy of serious consideration; for although it had received some attention, much more was required, in the way of direct trials of improvements, and alterations in the weights and position of the parts, before the best result could be attained. He had designed, many years ago, an arrangement for balancing a quick-working machine, moving a weight by means of a crank. A second crank was placed precisely opposite to the first, to move another similar weight, so that the machine was completely balanced. Mr. Heaton, of Birmingham, had proposed the same plan for a locomotive, and had constructed guides under the foot-plate of an engine, on the London and Birmingham Railway, and placed an outside crank on the axle to work a dummy weight in the guides, so as exactly to imitate the piston, &c., the cranks being opposite. This made the engine run very steadily indeed, and was so far successful, though the complication was of course objected to. Recently the doffers of carding engines had been balanced with second cranks and dummy weights, with great advantage to their quick and smooth working. With regard to the effect produced by the reciprocating parts of an engine when left unbalanced, he could not agree that the $\frac{1}{5}$th of an inch was practically anything like the real measure of the sinuous motion of most engines; indeed, from his own observation, he felt convinced that the motion was much more like 2 inches, at the ends of the frame, even with a good road, and more if the road were bad. Nor did he think that a small measure of motion expressed the evil in its true form, as it was, or might be at any instant that the flange was in contact with the rail, a question of the amount of force that was exerted against the rail, tending to bend the rail or break the chair. He believed for this reason that high speeds were far more dangerous, when the ballast was frozen hard, than when the road was in an ordinary condition. The plus and minus pressure under ordinary circumstances was upwards of 8,000 lbs. for each cylinder. With respect to the uniformity of the rotative power, irrespective of the plus and minus pressure, it would be seen from the diagrams, that one cylinder with full steam was the most irregular, next came two cylinders at right angles with full steam, whilst the most uniform rotative power was obtained by using two cylinders, and cutting off the steam at a little before, and a little after half-stroke. The advantage of cutting off steam at half-stroke had been first pointed out, in 1849, by the Author of a Paper read at the Society of Arts. Fig. 7 showed the variation

Fig. 7.



in the rotative power obtained by using full steam throughout the stroke in two cylinders, having cranks at right angles, and also the variation when cutting off at certain definite points. The full lines A^1, A^2, represented the rotative power obtained from the two strokes of one cylinder, as developed on the path of the crank, whilst the full lines B^1, B^2, represented the power obtained from the other cylinder, also developed on the path of the crank, when using full steam in both cylinders. The dotted lines V^1, V^2, V^3, V^4, showed the combined rotative power of the two cylinders, and a variation from the average line OP of 31 per cent. It should be stated that, if expansion were carried to any extent in each of the cylinders separately, this variation would be much greater. The full lines A^1, A^2, and B^1, B^2, showed the rotative power when cutting off at the points indicated, and the lines v^1, v^2, v^3, v^4, showed the combined rotative power of the two cylinders, working as just stated, giving a variation from the average line E C of only 14 per cent., which in practice amounted to perfect uniformity in effect. The length of the connecting rod had been taken into account in Fig. 7. It should also be noticed, that when using full steam, the variations in power were not only large, but long continued; for instance, in the first quarter of the revolution, the power was greatly in excess of the average, whereas in the third quarter it was greatly below the average. When cutting off at the particular points before noticed, each quarter had nearly the same power.

In conclusion, he would beg once more to urge locomotive engine builders, in common with marine engineers, to use steam-jackets both to the cylinders and the cylinder covers, as he felt it was an improvement that had been far too long neglected. He was bound, however, to say, that some marine engineers were now beginning to appreciate steam-jacketing, though the Government neglected it still.

Mr. PHIPPS said, with regard to the supposed advantage of one cylinder over two cylinders, he was willing to concede, that as all the reciprocating parts would be situated in the middle of the single-cylinder engine, it would be free from the sinuous motion, which in the double-cylinder engine was occasioned by the resistance to the change of velocity in those parts, acting on the mass of the engine, with a determinate leverage. As regarded tractive power, however, he could not agree that there would be anything like the same equality of pull in the single-cylinder as in the double-cylinder engine. If the tractive power of the two classes of engines were examined, simply with relation to the pressure of steam on the pistons, and the effective leverages of the cranks, it was easy to show, that whilst the maximum and minimum rotatory power of the double-cylinder engine varied in each quarter revolution nearly in the proportion of 14 to 10, in the single-cylinder the variation was as much as 20 to 0. That was, for the double-cylinder, the area of each being 6, when the cranks were at A and C (Fig. 8, page 98), if the power were taken as 10, at B and D, it would be as 14 nearly; while for the single cylinder, whose area was 2, when the crank was at A, the power would be as 0, whilst at B it would be as 20. As, however, the Author in his Paper stated, that in estimating the rotatory power of the engine, it was necessary to incorporate with the pressure of steam on the pistons, the effect of the inertia of the reciprocating parts, it could be shown that, in the cases illustrated by these figures, the inertia was totally inoperative, and therefore that the proportionate variation of power remained unaltered. For example, whenever the cranks were at any of the positions A, C, E,

Fig. 8.

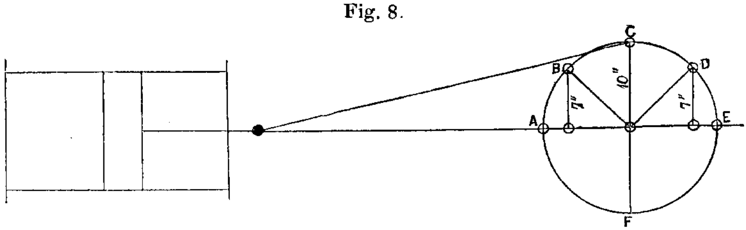
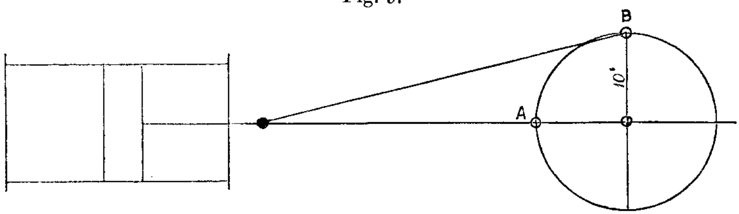


Fig. 9.



or F (Fig. 8), the reciprocating parts attached to them were undergoing neither acceleration nor retardation, and could not therefore be either absorbing power from the steam, or giving out power. Also when the cranks were at B and D (Fig. 8), the parts attached to B were being accelerated, precisely in the same degree that those at D were retarded, and hence the original power of the steam was not interfered with. These results were calculated on the supposition of full steam being used throughout the stroke. If cut off at half-stroke, the friction connected with the crank at D (Fig 8) would only have upon it $\frac{35}{60}$ths of the pressure on the other piston connected with B, whence the total rotative power would then be as 11·08 in the position of B and D, compared with 10, as before, when at A and C.

Mr. Sinclair said, he had had considerable experience in the practical working of the locomotive engine, and was surprised to hear that the sinuosities of its motion had been reduced, by calculation, to $\frac{1}{5}$th or $\frac{1}{10}$th of an inch. In all his experiments with these machines, he had never found one whose irregularity of motion was not much greater than anything which had yet been stated in the course of this discussion. He believed it had been the universal practice to apply weights to the wheels, for the purpose of balancing the irregularity of motion, and that by that means the irregularity had been reduced to a minimum: but he had never met with one whose minimum amount of irregularity was not greater than had been mentioned. These balance weights might be applied until the irregularity was reduced to a minimum, but beyond a certain amount the more the weight was increased the greater would become the motion. He had always entertained the belief, that irregularity was due to the varying position of the several reciprocating parts of the engine; that beyond a certain point it was next to impossible to alleviate it; and that however perfectly the engines might be designed, so long as there were two cylinders, a certain amount of sinuosity or oscillation must result; but that the measure of it, according to his experience, was considerably greater than had been pointed out. Then, unfortunately, any defect in the original design was aggravated as the engine got out of order, and whenever the way upon which it travelled was not in a good state; and neither could be assumed to be so perfect, under all circumstances, as to prevent this increased injurious effect taking place. He believed that if a single-cylinder engine could be got to go at all, it would neutralize, to a great extent, these sinuosities and irregularities. He had for many years had that notion; and a long time ago he took an engine, which had become deteriorated by long use as a ballast engine, and which, at a slow speed of 15 miles an hour, was very uncomfortable to travel upon, and although he did not actually convert it into a single-cylinder engine, yet the two cranks were placed in the same direction, so that for all practical purposes it was a single-cylinder engine. That engine—when it could be got to go—that was when the difficulty of getting over the centre was overcome—travelled 60 miles or 70 miles an hour with perfect steadiness. Therefore he was led to think, if that inconvenience could be overcome, a simple and a steady engine might be obtained. Although he had applied himself assiduously to the subject, he regretted to say that both his ingenuity and patience had been at fault, for he had not yet been able so to arrange such an engine as to give facility for starting or reversing, without which it was of course useless for practical purposes.

Mr. GRAVATT remembered that twenty-five years ago, when driving wheels of 10 feet in diameter were in use, he investigated this question. The result then arrived at was, that if there were no torsion, there would be no sinuosity. The whole thing depended upon the torsion of the crank shaft. If the shaft was indefinitely stiff, and the wheels were all going at the same speed, there could be no sinuosity.

Mr. KERSHAW thought there could be no question, that an action of a side character took place in an engine, but not to so dangerous an extent as the diagrams indicated. The speed at which the whole of the parts ran had a tendency, he believed, to counteract any evil effect, and there was no reason to apprehend danger from diagrams of the nature of those exhibited. The single-cylinder engine appeared to be considered as wholly impracticable, therefore it was not necessary to enter upon that question.

Mr. MOLESWORTH said, the theory of the disturbing forces had been so fully gone into, that there was little left to add upon that point. Although agreeing, to a certain extent, with the remarks which had been made on that subject, he yet held the opinion that, practically, the measure of motion very greatly exceeded what was indicated by the theory which had been propounded. He ascribed the discrepancy between theory and practice to the fact, that the theory was based upon the supposition that the wheels were without flanges, whereas, in his opinion, the flange was a very important element in the question. When the direction of the course of an engine at a high velocity was suddenly altered by any disturbing force, the momentum of the engine brought the flanges into violent contact with the rails, and the elasticity of the rails caused the engine to rebound, or rather glance off, against the opposite rails. When the rebound happened to be simultaneous with a disturbing force acting in the same direction, the maximum oscillation was caused; but the rebound was in some degree neutralized, when the disturbing force acted simultaneously in the opposite direction. In support of this view it was adduced, that the oscillations of an engine were not coincident with its pulsations, as they would be in the theoretical case of an engine without flanges; but that they were very irregular, sometimes extending over a period of eight or ten pulsations.

A single-cylinder engine he considered to be impracticable. Even if means could be devised for starting the train under ordinary circumstances, that was not the only difficulty; as it would be much increased in frosty weather, a drawback which could only be fully appreciated by one who had driven an engine, and had experienced the difficulty of starting a heavy train in slippery weather. He presumed that the apparatus for starting would be worked by hand, and thrown into gear so as to move the engine off its dead centre; after which it would be thrown out of gear, and the steam would carry on the stroke. When the rails were slippery, the wheels, if moved off their dead centre, would rotate a few times without biting, and would probably stop on one of the dead centres again, as the steam would have the least power at those points to make the wheel rotate; the hand apparatus would then have to be thrown into gear again, and the engine worked off its dead centre, probably with the same result as before.

Mr. VIGNOLES said it appeared to him, that the object the Author had in view was to ascertain how far the engine itself was at fault, and how far blame might be laid upon the permanent way. It had been satisfactorily demonstrated theoretically and practically, that the sinuous motion of the engine was but small, and that it was not so injurious as had been supposed; and he believed in the earlier experiments upon railways, it had been proved that, where the railway was in perfect order the flange of the locomotive might be almost dispensed with. Therefore the remedy for the evils described must be found in the perfection of the permanent way. When the subject was first broached he thought too much blame had been thrown upon the deficiencies of the permanent way, and too little assigned to the locomotive engine: but he now considered that, when the problem was the revolution of the engine upon a theoretically perfect line, and as the maximum amount of sinuosity was only one-fifth of an inch, if an accident arose, it must be from other causes than defects in the construction of a well-balanced modern locomotive, and that safety must be sought in the perfection of the road. So small a lateral deviation as the one-fifth of an inch might sometimes arise from the imperfection of a chair, and he would ask, why should chairs be used at all, since the most perfect way was also the simplest, and chairs were merely a complication and only uselessly increased the number of parts? Defects in the way comprised the want of perfect gauge, of perfect gradient, and of perfect smoothness of the whole of the parts. The perfection of gauge was of course insured by the transverse sleepers to keep the rails in place; the perfection of the vertical position of the rails by attention to the ballasting and by packing; while as regarded smoothness, the junction of the rails, by means of fish plates, enabled one even continuous line to be obtained, which was so important to the easy action of the engine travelling over it.

Mr. G. W. HEMANS, without controverting the scientific inquiry into the action of the disturbing forces in the locomotive, and the measure of their results, would record his opinion, for what it was worth, that these sinuous movements were very serious. He had had occasion to observe the movements of engines on one line in particular, where a great proportion of the stock consisted of inside-cylinder engines, there being only six engines with outside cylinders, and he found that the sinuous motion with outside cylinders was greater than with inside cylinders,—so much so indeed as to be dangerous,—and the result of the investigation showed the movement to be much more than the one-fifth of an inch indicated as the limit in theory. He believed this irregularity arose from imperfection in the machinery; that there was a movement in the axles of the engine, which did actually bend and suffer some amount of torsion, which in outside-cylinder engines with light axles might be serious. If, as he thought was the case, outside-cylinder engines had more sinuous motion than those with inside cylinders, it was clear that the sinuous motion must arise from a want of proper balancing. The adoption of a single-cylinder engine, which was the remedy proposed by the Author, was not, in his opinion, one which would cure the defect, or remove one present source of safety. When an engine with two cylinders was partially disabled, it could be brought home with one cylinder working; whereas the single cylinder would take away that important chance of safety, and on that account would be objectionable.

Mr. G. P. BIDDER,—Past President,—thought it desirable to come back to the essence of the Paper, which undertook theoretically to point out the sources of danger in engines, and to suggest a practical remedy; and here he would remark, that the observations, on which the investigation was based, were, in his opinion, as unpractical as the remedy proposed for overcoming the evils that had been described. The Author alluded to the fact, that the side pressure, arising from the position of the cranks, was a source of great increase of resistance; and confirmed that position, by reference to the effects of a very strong side wind upon the train. Now it had been satisfactorily established, that the resistance of a strong side wind was not due to side pressure, but to end pressure; and that strong side winds became diagonal, when combined with the speed of the train, and would thus be distributed partially over the ends of all the carriages; whilst a direct end wind operated on the end of one carriage only. It frequently happened, especially in regard to railways, that remedies designed to overcome one evil, themselves introduced other sources of danger. The Author had suggested a single-cylinder engine; but that, he thought, had already been satisfactorily disposed of as a most unpractical idea. If it were desired to get rid of this sinuous action, or tendency to sinuous action, it could be done with two cylinders, by putting both the cranks in the same position, instead of at right angles to each other; and that would be a more practical method than the one proposed. This had been accomplished by the late Mr. Robert Stephenson, in a three-cylinder engine, having one large cylinder in the middle and two smaller ones at the sides. That engine worked with remarkable steadiness, but it was costly, and that, together with the complication of the parts, prevented its adoption. In 1836 Mr. Weallens, of Newcastle-on-Tyne, brought out an engine with four cylinders; and it was a curious fact that there was an engine upon that plan in the late International Exhibition of 1862. But although by the use of a single-cylinder engine, or a double-cylinder engine with the cranks moving together, the sinuosity might be avoided, another element of danger was introduced, inasmuch as there must be a varying pressure between the back and the forward motion of the engine, which would produce a jolting action that was likely to give rise to accidents from the breaking of the connections of the train. Therefore, practically, all these suggestions failed, and he thought they would hardly have occurred to any one conversant with, or experienced in, the working of railways. He concurred in a great measure in the remarks which had been made, as to the extent to which the oscillation of the engine was occasioned by the varying operations of the steam in the cylinders. But there was this fault in the deductions, that the whole mass of the engine had been taken into account. If that were done, there could not be a side motion without causing the wheels to grind literally upon the rails; and as the friction was one-sixth of the weight, it would be found, if the whole mass of the engine was taken, that the pressures given by the Author could produce no sinuous action at all. But the fact was, that part of the mass of the engine should be taken which was upon the springs, and this was much less than the whole; therefore the motion communicated to this portion of the bulk of the engine would be in excess of the results arrived at by Mr. Bramwell and Mr. Scott Russell. But even then the amount of oscillation which was known to exist would not be reached; and so long as there were wheels and cranks, and engines were put on railways with curves, so certainly would the element of oscillation be introduced. With a perfect road, and an engine perfectly adjusted, it was not in the power of man to place that engine perfectly straight upon that railway, and then, at once, the element of oscillation came into play. Besides, an engine that was put on the road perfect on one day would not be so on the following one. The eye failed to detect those defects in a railway which occasioned oscillation; and not only the eye, but no instruments could do it. Neither could those errors in the construction of the engine which occasioned oscillation be detected. If any one of the wheels were out of truth the one-hundredth part of an inch, that might occasion serious oscillation. There were no means of investigating the extent to which these defects prevailed, because they might be acting either together or in opposition to each other, and thus aid or retard the oscillation, or the tendency to oscillation, which the backward and forward pressure of the steam in the cylinder must occasion to the boiler and other parts connected with it. Therefore it was found in practice, that engines, more or less imperfect, ran at high velocity for a long period without accident; whilst a new engine, or, what was better, one which had run a few journeys, might be put upon as perfect a piece of road as man could make, and an accident might forthwith ensue. The investigation of this subject, so as to embrace all the elements which tended to affect the action of the engine, demanded very high mathematical powers, combined with great daily experience in the working of railways. If that combination could be brought about, data would be afforded of great practical value to locomotive managers, and to those who were so much, and so painfully, interested in the results of railway accidents.

Mr. REYNOLDS thought one point had been overlooked in considering these disturbing forces, and that that was the only one of practical importance, because, though it must be admitted that many imperfect engines were running, still the ordinary knowledge of engineers, if properly applied, was sufficient to enable them to neutralize the evils arising from the sources of disturbance which had been discussed. The effect of the inertia of the pistons, &c., was well known ; but he could not conceive how the combined action of the two sets of parts making strokes alternating with each other in the usual way, and having, therefore, in the course of each revolution, four periods of acceleration and of retardation at (neglecting the influence of length of connecting rod) equal intervals, could possibly produce diagrams like those shown in Plate 2, Diagrams No. 5. It surely must be more like that exhibited in Fig. 7, page 34. It was only fair to say, that in Mr. D. K. Clark's treatise the question of sinuous motion arising from the inertia of the moving parts had been carefully, and as it appeared to him, accurately gone into. The effect of such inertia could be, and in practice frequently was, completely neutralized by balance weights in the wheels. It was true that if there was sufficient balance in the wheel to counteract the horizontal action of the piston, &c., a new tendency was introduced, which, however, at the speeds attained in practice, did not practically affect the motion of the engine. He meant that as the balancing weight must be greater than that necessary to counteract the centrifugal force of the revolving parts, a greater pressure was caused upon the rail at one period than another, and thereby a slight inequality in the wear of the wheel tires was in practice produced ; but as the amount of unequal pressure was not sufficient to lift the wheel from the rail, the horizontal action of the balance weights alone came into operation, so far as regarded the motion of the engine. As had been previously stated, he believed the amount of displacement of engines was generally much greater than had been calculated by some of the speakers; but he thought it might be accounted for, by considering that the engine had as much tendency to turn itself round the driving axle, as to turn the axle in its bearings. If this tendency was uniform, it would merely amount to a transference of weight from the leading to the driving and trailing axles, and if it was centralized, as in Mr. Robert Stephenson's three-cylinder engine, it would not induce, or affect, lateral motion, but in practice the action took place alternately at opposite corners of the engine, and the amount of the tendency might be taken for the present purpose to be represented by the upward pressure of the slide blocks upon the slide bars. The "White Horse of Kent" had been referred to. A number of engines of this class, which had come under his observation, had 15 inch cylinders, 2 feet stroke, and 6 feet driving wheels, and the connecting rods were about $5\frac{1}{3}$ times the length of the crank. Speaking from memory, the usual weight was about 21 tons, disposed thus:—leading wheels $5\frac{1}{2}$ tons, driving wheels 9 tons, and trailing wheels $6\frac{1}{2}$ tons. Hence, if the engine had 100 lbs. per square inch on the pistons, the total pressure on each piston would be 17,600 lbs., and this divided by $5\frac{1}{3}$ would give something like $1\frac{1}{2}$ ton lift on the slide bars; and as the whole weight on the rails at the leading wheels was only $5\frac{1}{2}$ tons, of which the wheels and axle would weigh more than $\frac{1}{2}$ ton, leaving only 5 tons, or $2\frac{1}{2}$ tons upon each leading spring, and as these springs were only 4 feet between centres, whilst the slide bars were 6 feet 1 inch between centres, it was easy to understand how considerable motion might be produced. He once had occasion to make a number of small engines, with 14 inch cylinders, 18 inches stroke, and 4 feet wheels, only 6 feet between the extreme centres. The leading wheels were coupled to the drivers, by return rods inside the connecting rods, and the distance between the centres of the cylinders was 6 feet $4\frac{1}{2}$ inches. The connecting rods were 8 times the length of the crank, and though there was not the usual rigid coupling between the engine and tender, yet (the wheels being carefully balanced) these engines were practically steady when lightly loaded, at any speed which the length of line at his command admitted. But when heavily loaded, or working up severe inclines, the way in which they, as it were, worked their shoulders was very conspicuous. It was clear that this action would be more considerable in proportion as the cylinders were farther from the centre of the engine. In modern inside-cylinder engines, with the ordinary loads, it was not generally visible, except in the case of goods engines, which were generally well loaded, and had the additional disadvantage of inclined cylinders, which concentrated the action referred to in one of the strokes, instead of dividing it equally between the two; and no balance weights could modify the effect of this tendency. Even in the duplex arrangement, where the balance, so far as the parts in motion were concerned, was practically perfect, he believed that, when heavily loaded, the action to which he had directed attention would be traceable, in spite of the excellent disposition of the weight upon the wheels. He thought the operation of balance weights was in some cases less satisfactory than it might be, because they were very commonly wrongly placed. He had seen some few inside-cylinder engines correctly balanced, having upon each wheel two balance weights;

the amount due to each set of parts (crank, piston, &c.) being divided between the two wheels in amounts varying inversely with the distance from the crank. Thus there would be a large and a small weight upon each wheel, and the same effect was sometimes obtained by the use of a single weight in each wheel of such amount, and in such intermediate position, as would be equivalent to the two weights described; but he never yet saw an outside-cylinder engine correctly balanced. It was clear that a single weight in each wheel, radially opposite the crank, could not be right, unless the planes of motion of the piston, &c., and of the wheel coincided, and it also seemed to him that for inside-cylinder engines, the position of single weights in each wheel must be at an angle with each other smaller than a right angle; and the amount of such weight would be somewhat less than would be required if the weight revolved and the piston moved in the same plane. For outside-cylinder engines the two weights must be at an angle greater than a right angle, and the amount must be greater than that due to the action of the piston and balance weight in the same plane.

Mr. W. Naylor could only agree with the Author as to the bare fact that there were sinuous and oscillatory motions in locomotive engines, but he differed entirely as to the cause. He was prepared to show, that sinuous and oscillatory motions took place in all railway vehicles, as well as in locomotive engines, and that it always must do so. He was well aware that this motion might be greater in one class of vehicle than in another, and that one class was more destructive to the permanent way than another which had more oscillation, and he could give many instances of parts of trains running off the line, when the engine and tender had not done so.

Mr. C. B. Lane observed, through the Secretary, that some of the results of the disturbing forces at which the Author had arrived, would be much greater in a broad gauge than in a narrow gauge engine; for the unbalanced reciprocating portions of the engine referred to in the Paper, which were estimated to weigh 495 lbs., and to the action of which was ascribed that sinuous motion of the locomotive on the rails that tended to burst the road, must, in the case of the broad gauge, as contrasted with the narrow gauge, act at the extremities of levers in the ratio to each other of 7 feet to 4·66 feet, and therefore produce a much greater amount of deleterious action in the former case than in the latter. As many of the points relating to the question of gauge had turned more upon commercial or financial considerations, it appeared important to notice this mechanical one.

Mr. Makinson, in reply upon the discussion, said Mr. Bramwell appeared to hold the opinion, that the steam pressures in the cylinders had no tendency to produce sinuous motion in the engine.

He thought the contrary was proved, simply by reference to the well-known proposition in dynamics, that in order that a body might move with a motion of translation only, the resultant of its impressed forces must act through its centre of gravity. With regard to the remark, that any oscillation in the engine, produced by the internal disturbing forces, would be the same at all velocities, two erroneous assumptions were made, when it was taken for granted that the time allowed for the oscillation varied inversely with the velocity of the engine, and that the disturbing force varied directly as the velocity. Now the Diagrams No. 8 (Plate 3), showed that at the velocity of 26 miles per hour, there was a tendency in the engine to make two oscillations in each revolution of the driving wheel, and that at the velocity of 52 miles per hour there was a tendency to one oscillation only in each revolution; so that the time allowed for an oscillation was the same whether the train was moving at 26 or at 52 miles per hour. The pressures shown in the diagrams, as tending to produce oscillation, were opposed by the friction of the wheel on the rail. The difference of these two pressures was the moving force producing transverse motion; and whether this friction were assumed at $\frac{1}{4}$th or only $\frac{1}{10}$th of the insistent load on the wheel, it would be seen from the diagrams, that the moving force increased somewhat more rapidly than the velocity. He must contend that the diagrams were quite sufficient to prove the incorrectness of Mr. Bramwell's conclusion; and although they had a special and not a general character, they were at all events suggestive of the conclusion, that engines travelling at high speed acquired a dangerous amplitude of oscillation.

Mr. Scott Russell, while accepting the accuracy of the numerical values assigned to the forces tending to give lateral motion or oscillation to the engine, regretted that attention had only been directed to those tendencies, and suggested that the Paper would have been more complete, if a measure had been given of the amount of the lateral motion. Mr. Russell had himself undertaken to supply this deficiency, and gave as the result of his calculation, that the lateral movement would be between $\frac{1}{10}$th and $\frac{1}{5}$th of an inch. Considering this as insignificant, and, in fact, hardly worth eliminating, the conclusion was arrived at by Mr. Russell, that the remedy, the single-cylinder engine, for so slight an evil, was worse than the disease. Had a three-cylinder engine, like that constructed by Mr. Stephenson some years ago, or a four-cylinder engine, like the 'duplex' in the late International Exhibition of 1862, been proposed, then Mr. Makinson could have recognized more force in the conclusion; but, so far from any complexity being introduced, the single-cylinder engine would be simpler in construction, and have fewer working parts than the present engine. He had wished to avoid the attempt to determine, theoretically, the lateral movement of the front end of an engine while running, but as it appeared to be desired that he should do so, he would apply to the case the same dynamical law as had already been employed in the course of the discussion, which was represented by $\frac{f}{2} T^2 = \int$, where f is the increment of velocity produced in 1 second by the moving force, T the time in seconds of the application of that force, and $\int$ the space in feet. Let W be the weight on the leading axle, n W the friction of the wheel on the rail, and let F represent the outward pressure on the rail generated by the engine; then $F - n W$ was the moving force, and $f = 32 \times \frac{(F - n W)}{W}$ and $\int = \frac{32 (F - n W)}{2 W} . T^2$. But when the front end of the engine had been thus moved through the space $\int$ by the moving force $(F - n W)$ it had acquired a certain velocity of lateral motion; so that when the moving force ceased, a further lateral motion $= \int$ would take place before a state of rest was attained. Therefore the whole lateral displacement would be $\int = \frac{32 (F - n W)}{W} T^2$. The utmost value of $\int$ which the play of the wheel would allow was $\frac{1}{4}$th of an inch, or $\frac{1}{48}$th of a foot. The friction n W would vary with the state of the rails from $\frac{4}{10}$ W to probably $\frac{1}{10}$ W. He was at liberty to assume the most greasy and slippery state of the rails, and therefore to take $n \text{ W} = \frac{1}{10}$ W. There was then obtained $\frac{1}{48} = 32\left(\frac{F}{W} - \frac{1}{10}\right)T^2$; and assigning the numerical values to F and W, the time T through which F must act to drive the flange of the wheel laterally $\frac{1}{4}$th of an inch, and into contact with the rail, would be arrived at. One of the diagrams (Fig. 8 B) showed that F attained a maximum value of nearly 2 tons; but supposing that $F = 1\frac{1}{2}$ ton only, and that $W = 5$ tons, then

$$\frac{1}{48} = 32\left(\frac{1 \cdot 5}{5} - \frac{1}{10}\right)T^2 = \frac{64}{10}T^2;$$

$$\text{therefore } T^2 = \frac{10}{48 \times 64} = \frac{\cdot 21}{64^2}, \text{ and } T = \sqrt{\frac{\cdot 21}{64}} = \cdot 056,$$

or about the $\frac{1}{20}$th of a second.

Now in this same diagram, Fig. 8 B, each inch on what represented the rail line corresponded to about the $\frac{1}{200}$th part of a second; and it would be seen that there were 10 consecutive inches where the outward pressure exceeded the assumed value of $F = 1\frac{1}{2}$ ton. So that it might be proved, that even so well proportioned an engine as the "Great Britain," at 52 miles per hour, with a greasy state of the rails, would twice, in every revolution of the driving-wheel, move laterally $\frac{1}{4}$ of an inch and strike the rail with the wheel flanges. A blow, once originated, between the flange and rail, would, from their elasticity, entail a series of blows at regular intervals of time; and should any of this series coincide in time and direction with the constantly recurring impulses to transverse movement originated by the engine, a heavier blow between flange and rail would follow, and so on again and again. And just as a small pressure suddenly applied to extend a metal rod of however large a section, and applied isochronously with the vibrations of that rod, would ultimately create a force of tension great enough to rupture it, so was it conceivable, that the pressures shown on these diagrams, occurring simultaneously with the rebound of the wheel from the rail, might ultimately produce a lateral pressure large enough even to drive the flange of the leading wheel up the rail, and so cause the engine to leave the rails. The foregoing was not proposed as indicating all the series of movements which together determined the amount of oscillation of the engine. But he believed sufficient had been said to refute the attempt which had been made to prove that the internal disturbing forces of the ordinary two-cylinder locomotive could not give rise to oscillations of that magnitude and danger which, as a matter of constant observation, did occur in such engines at high speed.

It seemed to be considered, by Mr. Vignoles, that the present engine, so far as steadiness of motion was concerned, was practically perfect, and that all oscillation might be referred to defects in the permanent way. Then he would ask, why travelling on a railway worked on the atmospheric principle was so decidedly superior, in ease and steadiness of motion, to travelling on a railway worked by the locomotive? The variations in the amount of tractive force of the locomotive engines of the present construction might be illustrated, and rendered more appreciable by the mind, by a consideration of the inequalities of permanent way, to which they were, in their effect on coupling-rods, and shaking and jarring of carriages and passengers, virtually, equal. Referring to diagram Fig. 5, it would be seen that at 26 miles per hour, while the crank pin moved through 30 inches, and the train therefore through 120 inches, the engine yielded, in excess of what was required, 71 units of work to each square inch of piston, or a total of $71 \times 260 = 18{,}460$ units. A train of 200 tons weight would have the same accumulated work put into it by a fall through $\frac{1}{2}$ an inch, so that the varying tractive power in the engine produced effects in the train such as would be produced by a depression of $\frac{1}{2}$ an inch in every 25 feet, the distance covered during one revolution of the driving wheel. Similarly, at 35 miles per hour, the varying amounts of tractive power represented changes such as would be produced by the train descending about one inch, and again rising one inch, in every 25 feet of its advance. And at 52 miles per hour, the same variations were equivalent to a descent, and then an ascent of the train, in every 25 feet of its advance, of nearly 2 inches.

Some admissions had been made by Mr. Bidder which were valuable to the cause advocated in the Paper, inasmuch as he had stated, as the result of his great experience of two-cylinder locomotives, that violent oscillations did sometimes occur when least expected, as when the engine was travelling on a straight road, and that road an excellent road; thus fully confirming the opinion, that great causes of oscillation existed essentially in the ordinary two-cylinder locomotive, so that at high speeds great oscillation would occur, however perfect the road on which it travelled.

He thought he had established, if not a certainty, at least the probability, that were the permanent way mathematically perfect, locomotive engines of the ordinary construction would create lateral and oscillatory movements of a grave character at high speeds, and also results similar to those produced by numerous small depressions in the level of the rails.

December 9, 1862.

CHARLES HUTTON GREGORY, Vice-President, in the Chair.

The discussion upon the Paper No. 1,076, "On some of the Internal Disturbing Forces of Locomotive Engines," by Mr. A. W. Makinson, was continued throughout the evening, to the exclusion of any other subject.
