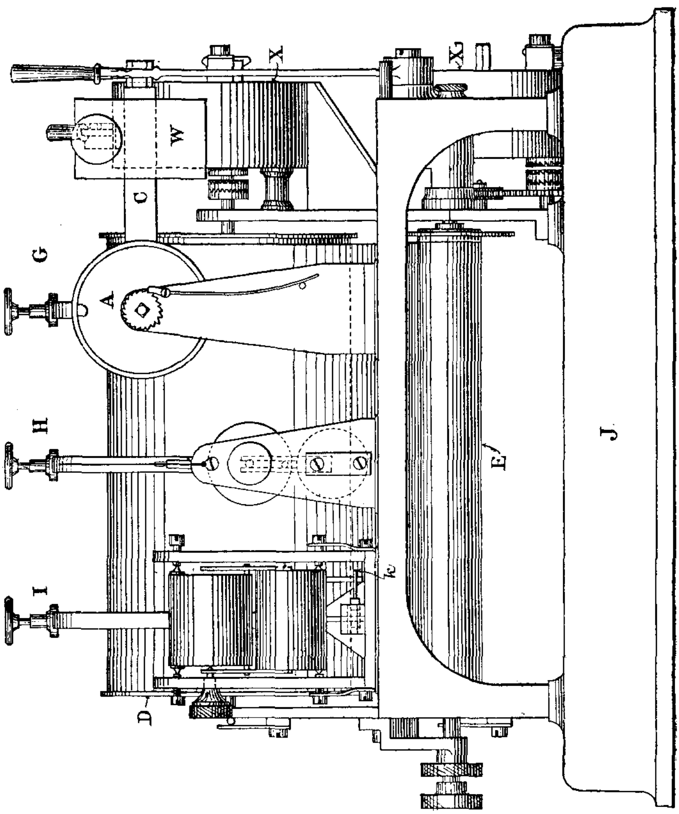
Discussion.

Sir John Coode. Sir JOHN COODE, President, said that the Paper just read was one of no ordinary value, for it afforded an insight into the action of locomotive engines upon the permanent way such as would be attended with beneficial results to the working of railways under all conditions. He was sure, therefore, that the members would desire to convey their very best thanks to the Authors.

Fig. 4.

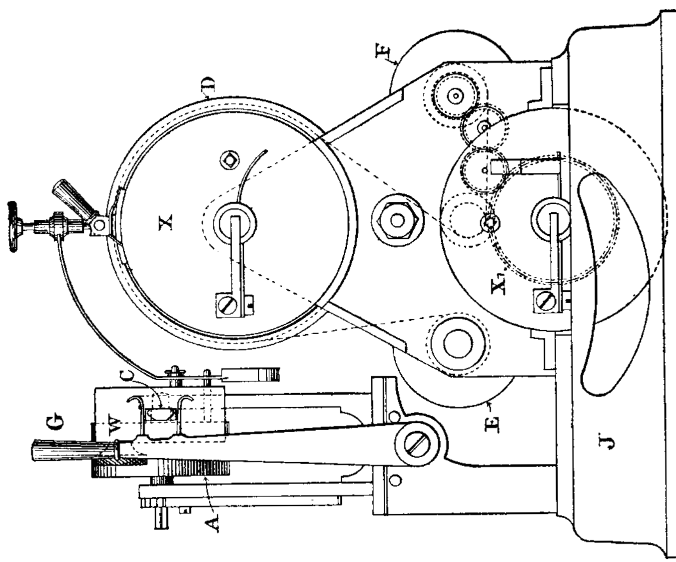


SIDE VIEW.
Scale 4 inches = 1 foot.

Mr. BRACEBRIDGE MILLS said that during October and November 1889, he had made a few journeys with one of Professor Milne's vibration recorders, one of the first constructed in this country. It was a triple instrument for registering vertical, transverse, and longitudinal motion, and was represented by *Figs. 4, 5, 6.*

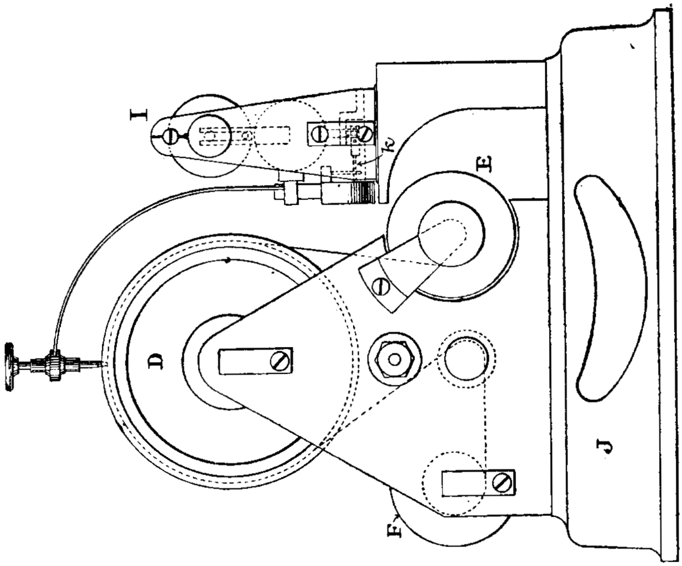
Mr. Mills.

Fig. 5.



End View, Right Hand.
Scale 4 inches = 1 foot.

Fig. 6.



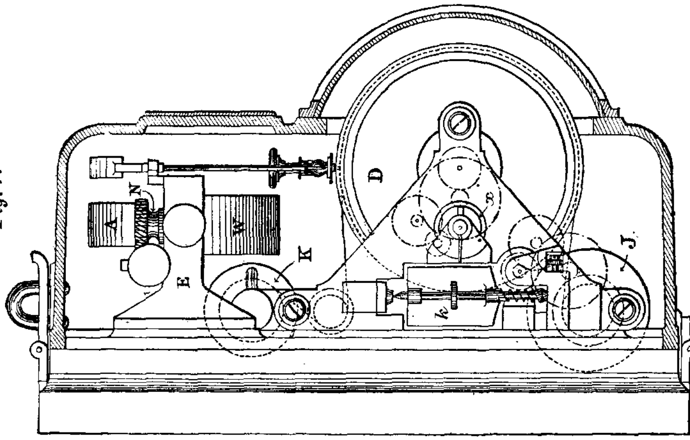
End View, Left Hand.
Scale 4 inches = 1 foot.

Mr. Mills. Although that form of apparatus was now obsolete, or only recommended for use under special conditions, he would briefly describe its construction in order that the nature of certain modifications in the present form of instrument, and the reasons which led to their introduction, might be more readily understood. G was the system for recording vertical motion, A being a box containing a coiled spring, attached to which was an arm C, carrying a weight W to balance the moment of the coiled spring. The system H, which was on the same principle as that shown in Fig. 8, Plate 2, was for recording transverse motion, and the system I, for recording fore-and-aft motion, was similar to H, but set at right angles to it, the pencil being made to write in the same plane as that of H by the intervention of a bell-crank lever *k*. D was a drum 12 inches in circumference rotated by the clock X at a uniform speed of 1 revolution per hour. E was the reserve coil of paper, the free end of which was taken over the large drum D, and secured to the winding roller F, upon which the diagram was coiled by means of the clock X_1. The instrument was mounted on a somewhat heavy cast-iron base J, so that it would stand firmly on the floor of a carriage without being secured by screws or other fixings. He had on two occasions travelled with this instrument to Hastings and back by the South Eastern Railway from Cannon Street station, and obtained some very interesting diagrams, which were being exhibited in the library. On the whole the instrument recorded very satisfactorily, but it appeared at times to be too sensitive, and probably, under abnormal agitation, its records might not be taken to measure or compare with the actual motions of the coach. For obtaining general information respecting the condition of the permanent way, however, he did not think that that was of much importance; the information was in his opinion sufficient if it showed that a defect in the permanent way existed at a certain point, without ascertaining the exact range of movement imparted to the coach or locomotive. He changed trains at Sevenoaks; and he wished to draw attention to the difference in the fore-and-aft motion recorded by the instrument, it being much greater between London and Sevenoaks than between Sevenoaks and Hastings. As the irregularities in the permanent way produced very little, if any, fore-and-aft motion, he suggested that the engine employed between London and Sevenoaks was not so perfect in its balancing as the one which took the second train from Sevenoaks to Hastings. He could not account for the difference in any other way. Curves and gradients were distinctly recorded on the diagrams exhibited. At increased speeds, and when the range of the transverse motion was great, that part of the instrument did not appear to record so accurately. He did not, however, think that this result pointed to faulty principles in the vibration recorder, but simply showed that the particular one employed was unsuited for such a great range of motion. There must obviously be a limit in each case to the amplitude of oscillation which could be recorded with accuracy, and on this occasion he was using it beyond that limit. Professor Milne, he believed, had found that the instruments which in Japan had been successful in producing accurate diagrams, were quite unsuited to American and European lines. In Japan the working speed was from 25 to 30 miles an hour, whereas in England, America, and on the Continent, the speeds which had to be dealt with were about twice as great, as were also the oscillations, and it was for this reason that it had been necessary to modify somewhat the Milne-McDonald apparatus. The speed, of course, affected the width of the diagram, and determined in a great measure the useful range of motion required in the instruments. Before introducing to the notice of the meeting the improved forms of vibration recorder which he had brought with him that evening he would briefly refer to the limit of accuracy in the original instruments. There was a limit to the accuracy of the recording mechanism—the pencil and its supports, and also to the indicating system—the cylindrical pendulums A and B, Fig. 8, Plate 2. The Authors had given a Table showing the relative proportions existing between the range of actual motion imparted to the instrument, and that registered on the diagram, and by referring to that Table it would be seen that, in the case of transverse motion, the pencil recorded only half the actual motion of the coach or locomotive when this exceeded about 5 millimetres. The main cause for the relative diminution in the records as the range of oscillation increased was, he thought, to be found in the method of writing the diagram; for by reference to *Figs. 4, 5, 6*, it would be seen that the pencil moved in a circular path whilst the diagram was written on a straight surface. Such a method was very suitable so long as the range of motion was small, as in Japan, where the speeds were slow and the lines particularly well constructed; but it had shown itself unsuitable for great speeds, inasmuch as abnormal motion was not so faithfully recorded as the normal swing of the coach or locomotive at that speed. For ascertaining the condition of a line it was the abnormal motion, or jerks, which it was desired more particularly to register, and it was by the position and magnitude of these excrescences on

Mr. Mills.

Mr. Mills.

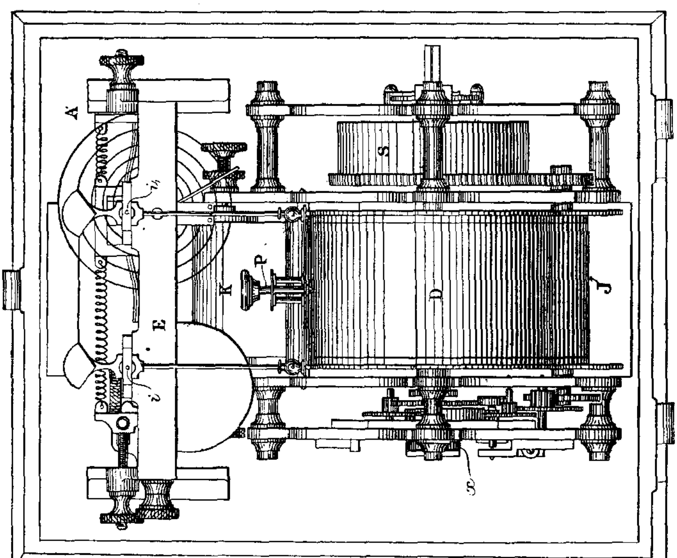
Fig. 7.



Side Elevation, Cover in Section.

Scale 4 inches = 1 foot.

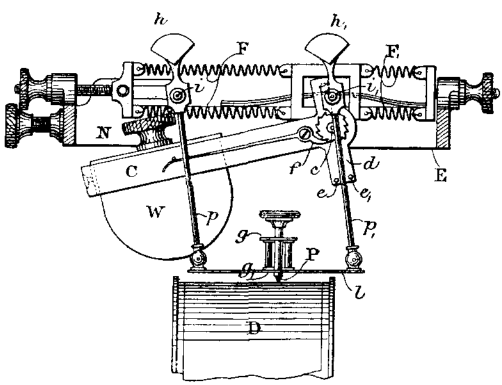
Fig. 8.



Front Elevation, Cover Removed.

the diagram that faults in the permanent way were located. In the latest forms of the instrument a modified arrangement of the rocking arm or beam (C, *Fig. 4*) and the pencil gear, suggested by himself, had been adopted. He was able to exhibit two of those instruments to the meeting, one of which was upon the table, the second being fixed to a vibrating board in the library. The one upon the table was represented in *Figs. 7, 8, 9,* and was for recording vertical motion only. It would be seen that the pencil remained in a vertical position under all circumstances, and as its centre of motion was some distance from the point at which the diagram was written, the curtailing of the diagram under abnormal motion was greatly reduced. Great vertical Mr. Mills.

Fig. 9.



Scale 4 inches = 1 foot.

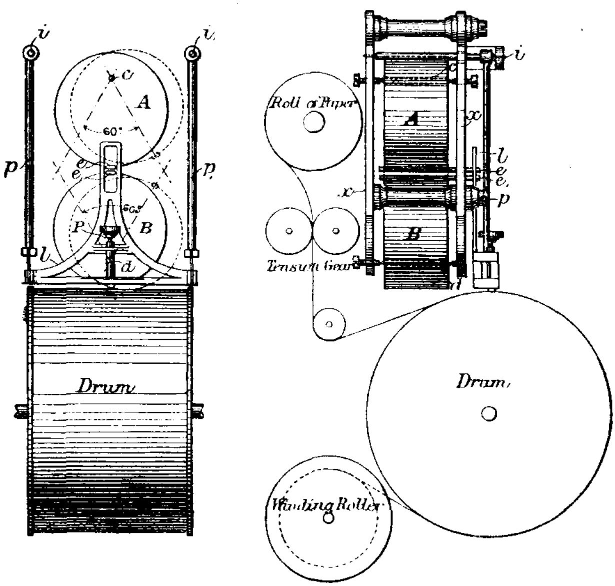
rigidity was given to the pencil gear by such an arrangement. C was the rocking arm or beam turning on hard steel centres at *c*. W was the weight capable of being clamped by the nut N at varying distances along C from the centre *c* in order to increase or decrease the magnification of the instrument. The spring A had its inner end attached to the arbor or spindle working on centres *c*, and its outer end secured to the fixed frame E; it was capable of being "set up" the requisite amount to balance the weight W by means of the rachet and pawl *f*. F F_1 were the compensating springs, the function of which was the same as that of the spring D, Fig. 1, Plate 2. The pencil suspension-rods $p\ p_1$ worked freely about the centres $i\ i_1$, and were connected at their lower extremities by knuckle-joints to the link *l*, carrying the pencil P, which, being made to slide freely in the guides $g\ g_1$, rose and fell

Mr. Mills. so as to maintain contact by its own weight with the travelling strip of paper. The fan-shaped weights $h h_1$ were extensions of the suspension-rods $p p_1$, and were employed to balance the moment of the pencil and its supports; in other words, to bring the centre of gravity of the system into a line with the centres of suspension $i i_1$, in order that the instrument might not be affected by the horizontal component of the motion. The oscillatory movements of the beam, relatively to the fixed parts of the instrument, were imparted to the recording gear by means of two steel pins $e e_1$ projecting at right angles from the arm d forming part of the beam C. J was the pulling or coiling roller driven by the spring barrel S, its speed being controlled by a fan k. D was the drum rotated by the friction of the moving paper, and timed by means of the escapement x to make one complete revolution per hour. K was the reserve coil of paper. The whole instrument was mounted on a cast-iron back-plate, and protected by a thin cast-iron cover with semicircular brass bezel and plate-glass, so as to effectually exclude dust and damp. The cover was swung on detachable pin hinges at the bottom, and secured by an ordinary padlock at the top. This instrument could be used to measure vibrations to a much greater range than that shown in *Figs. 4, 5, 6*; it was more sensitive to slow motions of short range and at the same time more nearly dead-beat. It was one of the commercial forms which it was proposed to manufacture in this country. Returning to the triple instrument, and referring particularly to the systems for registering horizontal motion, he had already stated that there were two things which determined the limit of accurate recording. He had dealt with one of them, namely, the means adopted for writing the diagrams. The second related to the size of the cylindrical pendulums and the way in which they were connected. He had found that with the instrument shown in *Figs. 4, 5, 6*, a horizontal motion of about 10 or 12 millimetres was the greatest which could be accurately recorded. From his experiments during the past twelve months over various lines, and under varying conditions, it seemed evident that the system for recording horizontal motion needed modification to enable it to respond with a similar degree of accuracy to oscillations of varying and extended range. This was especially required for car and locomotive testing, where it was necessary for an instrument not simply to announce that an abnormal movement in a certain direction had occurred, but to measure that motion with a fair degree of accuracy. *Fig. 10* illustrated a modification of the Milne-McDonald instrument which he had designed, and which was now adopted when an

Mr. Mills.

extended range of motion was required. A B were the cylindrical pendulums pivoted at $c\,d$ in the frame x. The pencil gear was somewhat similar to that in the vertical instrument, *Figs. 7, 8, 9.* $p\,p_1$ were the suspension-rods swinging on centres $i\,i_1$ and connected at the bottom by the link l, which was continued upwards at the back of the pencil P, and provided with a slot in which the pins $e\,e_1$, projecting from the circumference of the cylindrical pendulums A B, worked. In other respects this instrument resembled that shown in *Figs. 7, 8, 9,* the only difference in the

Figs. 10.

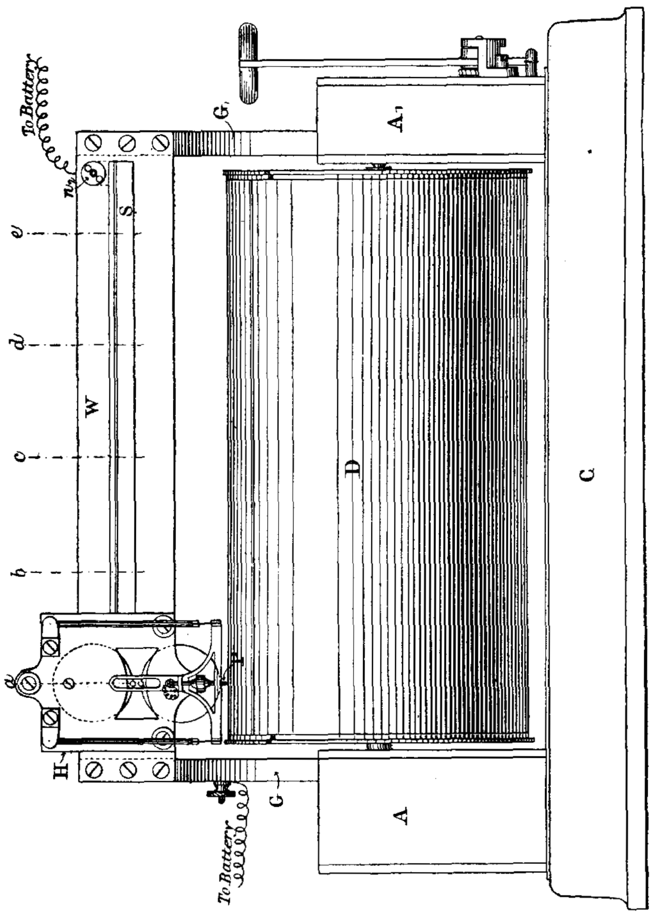


Scale 4 inches = 1 foot.

two being that the rocking beam and weight in *Figs. 7, 8, 9,* was replaced by a horizontal recording system in *Fig. 10.* This instrument had worked perfectly, and, by reason of the new method of communicating the motion of the pendulums to the pencil, the useful range had been trebled; in fact the diagrams could be used to measure oscillations up to 35 millimetres on a level road. Even at such a great range of motion he had failed to detect any inaccuracies in the records by the system tending to acquire a periodic swing of its own. During August and September 1890 he had taken the instru-

Mr. Mills. ments to the north of England and Scotland, obtaining some first-rate diagrams on the North British, Caledonian, London and North Western, Furness, and Midland railways. (Some of the diagrams referred to were exhibited.) One of these, to which he wished to call special attention, was that of a journey from Manchester to London by the Midland 1.20 P.M. express; and it was, he thought, a wonderfully characteristic diagram of the line. All the familiar shocks could be easily recognized, and the shaking commonly felt when emerging from Dove Holes tunnel near to Peak Forest Station where the down-grade commenced, was very distinctly marked. He had two vertical-motion recorders placed side by side in one box, which was placed on the floor of the Pullman car, and he was thus able to watch and compare the working of the two instruments. Throughout the journey they both acted extremely well, in fact it would be difficult to imagine how their working could have been improved. It was most interesting to watch the apparent oscillations of the two beams. They moved—relatively to the fixed parts of the instrument—perfectly in unison; and so synchronous was the motion that one might have imagined the two linked together by some invisible mechanism. That which the Authors had referred to as the "multiplication" of the instruments differed in the two machines he was using; as did also the speed at which the paper travelled in them. Notwithstanding those differences both in the longitudinal and transverse dimensions of the diagrams, corresponding disturbances might be easily traced. The shapes of the two agreed. When travelling on the Furness Railway from Whitehaven to Preston, which runs along the coast of Cumberland and Lancashire in a succession of sharp curves, and crosses the Duddon, Cartmel, and Lancaster sands, the instruments had a very severe test; indeed, he could not imagine any line where they would be more severely tested. They nevertheless worked very well under the circumstances. Curves when being traversed at a high rate of speed had a certain disturbing influence on the accuracy of the records, owing to the action of centrifugal force, the diagrams being generally larger in such cases. That might, however, be thought a valuable feature of the instruments, as the diagrams were in some degree a measure of that force. He was never able to detect any error due to the beams acquiring a periodic swing of their own—a fact which he considered of the greatest importance, but he had often noticed that an abnormal motion was recorded which he himself did not perceive until its presence was announced by the instrument. This showed that the vibration recorder was much more sensitive to changes in position and slow movements than the observer, a peculiarity of the apparatus which was often manifested when crossing bridges and swampy lands. He regretted being unable to exhibit to the meeting Mr. Mills.

Fig. 11.

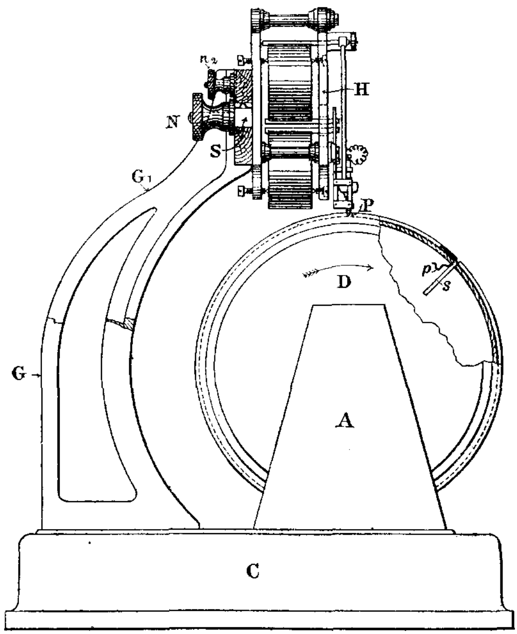


Scale 3 inches = 1 foot.

one of the instruments designed by Professor Milne for the purpose of determining the jerkiness of locomotives due to faulty balancing, and the motions of different types of carriages. He had, however, placed on the wall a diagram prepared from a working drawing of one of them. In *Figs. 11, 12,* D represented

Mr. Mills. the drum, 20 inches in circumference, rotated, at a uniform speed of one revolution in twenty seconds, by clockwork concealed within the pedestals, A A_1. G G_1 were light cast-iron supports carrying the horizontal recording-system H, which was made to slide along the surface of W, and was capable of being clamped in any position by the lock-nut N. It had been suggested to use electrical means for recording the relative movements of the pencil P and the drum D. For that purpose chemically prepared paper

Fig. 12.



Scale 3 inches = 1 foot.

was employed, and the current from a small battery was made to pass through the paper from the pencil—which in that case was merely a hard steel point—to the drum D, the path traversed by the point being indicated by the appearance of a coloured stain on the paper. Such a system of writing the diagrams had, he thought, many advantages; the writing-point required no attention, such as sharpening, or supplying with ink, as was the case when pencils or pens were used. The friction on the paper was very small, and thus the instrument was made more sensitive—a matter

Mr. Mills.

of great importance for locomotive testing. Lastly, the instrument did not write until the moment it was required to make a diagram, when the operator had only to depress the battery-key. The battery was fitted in the carrying-box of the instrument, and it was not necessary to remove either from it in order to make a test. When a diagram had been obtained with the recording system in position *a*, the lock-nut N was slackened, and H pushed along the slot S to position *b*; a second test could then be made, and the recording system again shifted, so as to occupy the positions *c*, *d*, *e*, successively producing in all five diagrams side by side on one sheet of paper. By using the electrical means of writing, it was rendered unnecessary to stop the drum during the five experiments. This was of some importance; for when pencils were used the drum had to be stopped and re-started at each test, and consequently never acquired a really constant velocity. Before concluding his remarks on Messrs. Milne and McDonald's apparatus, he would say that from the year's experience he had had, his opinion was that they were thoroughly reliable instruments for recording the oscillations of railway carriages and locomotives; but it had of course been found necessary to somewhat modify the original patterns made by Professor Milne in Japan, as the conditions of working there and here were quite different. As to the many uses to which the vibration records could be applied, some of which had been indicated by the Authors in their very interesting Paper, that was a branch of the subject which he must leave to be discussed by those who were more intimately acquainted with the requirements of the railway engineers than he was himself. The Western Railway Company of France appeared long since to have recognized the value of the information given by such diagrams. At the Paris Exhibition instruments were shown which had been in use on their line since 1879 for recording the motion of locomotives. The system differed greatly from that suggested by the Authors of the Paper. They measured the relative motion of the foot-plate of the locomotive and one of the leading axle-boxes; in other words, the vertical motion of the spring. The apparatus consisted of a lever, one end of which was attached to the top of the spring-box, and the other was connected with a stylus pressing against a plate or cylinder. The cylinders were of zinc, coated with a protective varnish, which was scratched away by the rubbing of the stylus. At the end of the journey the cylinders were removed from the instruments, and treated with acid, when the parts laid bare by the rubbing of the stylus were eaten in. From the time the instruments had been used, it would appear

Mr. Mills. that they had given satisfaction, but, in his opinion, they must have been somewhat inaccurate, because the motion recorded was one compounded of the motion of the spring-box, and the whole vibration of the engine; it was not a simple motion. The diagrams were not to be compared with those produced by the instruments of Messrs. Milne and McDonald, and they were very difficult to read. He understood that the London and North Western Railway Company had also taken the matter up. Last year he had had the pleasure of showing one of the Milne-McDonald instruments to Mr. Footner, who had told him that the company had carried out an elaborate series of experiments, as to which, perhaps, the Institution would receive some information. In 1883 a very complete series of experiments was conducted on various lines in Germany by, he believed, Mr. Klose for the Society of German Railway Engineers. The object of those experiments was to determine the difference in the oscillations of carriages when linked axles, coupled or independent, or rigid axles were used. The apparatus employed, which was for recording motion in a horizontal plane only, appeared to him to have been rather complicated; and as the instrument consisted of a specially constructed coach, it must have been very costly. The under frame of the coach to be tested was connected by a steel wire to the instrument-car in front of it; the horizontal motion of the wire being imparted to a system of levers under the instrument-car, which in turn gave motion to the pencil of the recording instrument. The diagrams were written on strips of paper similar to those employed in the Milne-McDonald instruments. There was a striking resemblance in the records of the two instruments. Although these experiments appeared to have been crowned with a marked degree of success, furnishing valuable information, and, he believed, creating great interest among railway engineers at the time, they nevertheless appeared to possess the same element of inaccuracy as had existed in the experiments of the French company. The motion recorded was not a simple one; but must necessarily have been the motion of coach compounded with that of the instrument-car itself. An American engineer, whose name he could not remember, had also constructed apparatus for a similar purpose. Professor Milne had informed him that when in the United States last year he had called at that gentleman's office hoping to obtain an interview, but was informed that the engineer referred to would not be visible for some time, being engaged *inside* his instrument. From that he (Mr. Mills) thought that it might be safely concluded that the instrument in question was not a portable one. His object in referring to these experiments was merely to draw attention to the difference there existed in the apparatus used on those occasions and the vibration recorders then before the meeting. In one case they had either specially-constructed coaches, costing probably some hundreds of pounds, or else instruments necessitating structural alterations to existing coaches and locomotives before they could be applied. In the second case, they had small portable self-contained instruments, which at a few moments' notice could be prepared for a test, and could be as readily used on one carriage or locomotive as upon another. These, he ventured to think, were advantages possessed by the Milne-McDonald instruments which could not fail to be appreciated by those whose duty it might be to use such apparatus. Mr. Mills.

Mr. R. M. Moir said he presumed that the invitation he had received to attend the meeting was due to the fact that he had been in close correspondence with Professor Milne with regard to the instrument described in the Paper. He perhaps looked at the matter from a different point of view from that of many present, who dealt with the subject technically. He thought that the instrument was practically of very little use, unless those who had to pay for railways, and to get dividends from them, could ascertain that by the employment of it the condition of the road could be carefully watched, working expenses reduced, and dividends increased. Professor Milne had been giving his attention to that subject. The work which Mr. Bracebridge Mills had so ably carried out in designing and superintending the construction of the models, had been very interesting, and he had watched it as closely as it was possible for a non-technical man to do. He had also had the benefit of the advice of a very practical friend, Mr. Connell. When the instrument on the table was constructed last year he had put it in charge of Mr. McQueen, an engineer now in Persia, in order that he might take it to Glasgow and exhibit it to Sir William Thomson, who had taken a very warm interest in the subject from the outset, and who had expressed his opinion that the vibration-recorder had a future before it. Mr. McQueen had to sit watching the machine all the way from London to Glasgow. As far as he (Mr. Moir) could judge the machines already constructed were only of value to an engineer who could sit in his testing-car and watch the diagram as it was rolled off on the cylinder and follow it very closely. The diagram which he held in his hand was, he believed, one of the most perfect that had been taken. At a very early period an engineer pointed out to him that the instrument had a serious defect from a practical point of view, Mr. Moir.

Mr. Moir. because it did not record the speed at which the train was travelling. Mr. Moir did not place any great reliance upon a diagram taken on such a short journey as that from Yokohama to Tokyo, where the train had scarcely started before it had to slow down again. Until the machine had been used on a long-running line it could hardly be of much value in testing the conditions of the road. But if it was used in its present form in a train running for any considerable distance of course it only told the time during which the train was in motion. If an engineer brought him a diagram and said that the train had been in motion so long, and that a weak point was shown upon the diagram where a girder or a bridge had deflected too much, he should ask to be shown the point where it was likely to have taken place, and the speed at which the train was travelling. Knowing the speed at the time at which the vibration took place it would be possible to ascertain within a $\frac{1}{4}$ or $\frac{1}{2}$ mile the place which required attention. That was a point to which Professor Milne had been giving very earnest attention, and he had sent to Mr. Moir the plan of an apparatus which he had designed for use in Japan whereby the speed of the train could be recorded on the diagram. The drawing was at present incomplete, and he could not, therefore, exhibit it. He had, however, placed it in the hands of a practical engineer, who had expressed his opinion that the instrument would act, but recommended a more simple plan by which the same thing could be accomplished, and any effect on the working of the instrument due to the putting on of the brakes would be counteracted. The drawings of the latter proposal would be sent out to Professor Milne, and until he and Mr. McDonald had expressed their opinion upon it it would be unwise to give it publicity. Some of the diagrams taken last year by Professor Milne had been kindly received by two or three engineers of English railways, and in one instance where a weak point in the line had been indicated by the diagram, the engineer had stated that that was the very point to which they were at that moment attending, in the belief that there was something wrong in the ballasting. One engineer had remarked that he did not know whether these particulars would be always welcome to him, because it would not suit him to have his directors coming and telling him that the instrument had shown a part of his road to be faulty. Another point to which Professor Milne had been directing his close attention was one which Mr. Moir could not quite appreciate except so far as it related to the fact that an engine might be made to burn less coal and do less damage to the permanent way: he referred to the testing of locomotives. Writing from Tokyo, on the 2nd of August, Professor Milne had stated that the quick running type was being used for the testing of balancing; and enclosed tracings of portions of diagrams from two sister engines and an American engine. He added: "In consequence of diagram from No. 138, this locomotive is now being altered; it tends to break the couplings, &c., and is certainly dangerous." He had also sent a set of diagrams, which he believed to be of commercial value. No. 138 engine, he said, as altered, could now be run safely at forty miles; before it was dangerous at thirty, and more than once broke the draw-bars. It would now run about one mile further without steam, and it burnt less coal than formerly. The instrument at present employed by Messrs. Milne and McDonald, in Japan, appeared to be somewhat simpler in detail than that which had been constructed by Mr. Bracebridge Mills. He thought it would be unwise to pass the instrument at once into commercial use, or ask any firm to undertake its manufacture until Professor Milne and Mr. McDonald knew the result of that evening's discussion, and had fully considered an elaborate report presented by Mr. Bracebridge Mills, together with other criticisms which had been offered by engineers, all of which would be sent to Messrs. Milne and McDonald, who would then decide as to the form of instrument they would ultimately adopt. For his own part he did not think the vibration-recorder would be of much value until it could be put into the guards' van by an assistant, before an express train started, and then locked up, to be taken out by another assistant at the end of the journey, when it would tell its own tale as to what had occurred during the transit. There was one remarkable circumstance to which Professor Milne did not refer in his Paper, and probably it had not come within the observation of Mr. Bracebridge Mills, namely, that when brakes were put on to a train rather suddenly, there was a corresponding mark on the diagram in the nature of a wavy line. If the speed at which the train was travelling could be recorded, they might by means of that indication be able to tell within a few yards where the brakes were applied. They could also tell by the diagram which he had placed on the table, the places where the train had been stopped by signal. He had shown it to the President of an American railway, who said: "We have something of the kind by which we can record the speed of our locomotives, but this gives time, and the stoppages; it would be extremely useful, especially in the case of our freight trains out West, because it would act as a kind of automatic inspector. Some of our men run the trains Mr. Moir.

Mr. Moir. into sidings, go and see their wives and children, and then start again." If they could produce such records by means of the Author's instrument, he thought that a great boon would be conferred upon the engineering fraternity.

Mr. Shelford. Mr. W. SHELFORD said that Mr. Bracebridge Mills had referred to a run from London to Hastings and back, where he had found that much greater longitudinal motion was shown by the diagram of the instrument between London and Sevenoaks than between Sevenoaks and Hastings. It would be interesting to know whether on the return journey he had found the longitudinal motion greater between Hastings and Sevenoaks than between Sevenoaks and London. Mr. Shelford's impression was, that a great deal more was attributed in the Paper to the balancing of engines than ought to be. The Author had said, that the only difference between two of the engines he had referred to was in their balancing, and that difference was shown in Fig. 12A, the saving of coal in the one having the larger balance over the one having the smaller being about 14 per cent., which was attributed entirely to this cause. Other similar references were also made in the Paper. With regard to the run between London and Hastings, Mr. Shelford believed that Sevenoaks was the summit of the line, there being a rising gradient between London and Sevenoaks, and a falling gradient between Sevenoaks and Hastings, and *vice versâ* on the return journey, and he should expect to find that there was greater longitudinal motion in the engine when it was doing the greatest work, viz., on the up grade, irrespective of balancing. In that case if an engine ran through from London to Hastings or *vice versâ* without change, the instrument would indicate the greatest longitudinal motion when approaching the summit of the railway from either side. It was important to ascertain whether Mr. Bracebridge Mills' diagrams showed this to be the fact.

Mr. Mills. Mr. BRACEBRIDGE MILLS said that on the return journey he had not found that the fore and aft motion was greatest between Hastings and Sevenoaks. It was the only instance in which he had had to change trains at Sevenoaks, and he had never noticed the difference in the diagrams before or since. If the sudden alteration in the fore and aft motion was to be attributed to the cause suggested by Mr. Shelford, he (Mr. Mills) should have expected to find a characteristic diagram of the line repeated at every experiment. So far as he could remember, not having his diagrams before him, there had not been any such similarity of the diagrams. In the case of the test illustrated by *Figs. 30 & 31*, he believed that the range of the fore and aft motion of the locomotive when the balancing was defective varied between 12 and 20 millimetres. The diagram on the wall was enlarged about twelve times. Mr. Mills.

Mr. WILFRID STOKES said it would be advisable to have some information as to the position of the instrument in the train. If it were on the carriage next the engine many more pulsations might be expected than if it were in the guard's rear van. Moreover, the position of the instrument on the engine or in the carriage would have a marked effect on the resulting diagram. A record taken in the guard's van immediately behind one of Mr. Webb's compound engines would probably give some instructive information as to the action of the low-pressure cylinder. He hoped that the promised diagrams taken on the London and North Western Railway would contain the results of such experiments. He also thought that the system of draw-bars and springs and the tightness of coupling-up would greatly influence the indications of the vibration-recorder. Probably the best position for the instrument would be suspended from an axle, in which case it would be very easy to adapt it for recording distance. Mr. Stokes.

Mr. ALFRED J. HILL asked whether the Authors had tried placing the instrument in different portions of the train or of the locomotive. He thought it would be important to try one in front of the buffer beam, so as to get the motion of the leading wheels; and it might be interesting to have an experiment on two locomotives similarly constructed, but one having radial axle-boxes, and the other a bogie. Useful results also might perhaps be obtained if locomotives were tested with different arrangements of hanging springs, as india-rubber cushions had recently been inserted, in some cases, in addition to the ordinary steel laminated springs. Also locomotives were being built with volute springs of the Timmins, or some similar form, and it would be interesting to know the various results which would be recorded in each case by machines such as those described. He should be glad to know if particulars could be given with reference to the cylinders of the locomotives. The one shown in Fig. 12 had outside cylinders; but the Paper gave no information as to whether on the engines mentioned they were inside or outside. A good deal had been said about balancing locomotives, but nothing had been mentioned as to any experiments on carriages. Carriage wheels were now accurately balanced, and it might be well to have some diagrams taken on similar vehicles with true wheels and with wheels slightly out of truth, to see if any appreciable difference Mr. Hill.

Mr. Hill. was shown. Perhaps some information on these points might be obtained from the London and North Western Railway Company.

Mr. Marriott. Mr. W. Marriott said he had taken great interest in the Paper, having had charge both of locomotives and of permanent way. About five years ago General Hutchinson remarked to him that there was no instrument which recorded the strain of a locomotive on the permanent way when the road was rough and the engine lurched. It now appeared that if the diagrams could be accurately measured they would show the strain coming on the rails in excess of the weight of the locomotive. Of course when an engine lurched, or as engine drivers said, "put its foot into a hole," there must be a considerable amount of strain on the permanent way which could not be recorded except by such an instrument as had been described. With regard to the question of balancing referred to in the Paper, he thought that a good deal too much was made of it. He could not follow the weights given, because of course balancing had to do with the distance of the centre of gravity of the crank-pin and reciprocating parts from the axle, as well as the distance of the centre of gravity of the balancing weight. He was inclined to think with Mr. Shelford that if a record had been taken from Hastings to London the diagram would have been much the same, showing that the difference was due rather to the engine being in collar, and pulling hard, than to the balance-weight. It was somewhat strange to put down the extra consumption of coal entirely to the defective balancing of the engine, for it was well known that engines of exactly the same type and coming from the same shops, sometimes showed a difference of 4 or 5 lbs. per mile in coal consumption, which was due perhaps to longer rods, or springs of a different height, or to a change of drivers. No doubt if a machine of the kind described could be locked up in an engine, or in a guard's van it would be a useful addition to the ordinary inspection of the permanent way.

Mr. Cowper. Mr. Charles Cowper asked whether it had been found possible to ascertain, from the character of the recorded vibrations alone, in any isolated case, where the fault was due to the engine, and where it was due to the permanent way. He imagined that it might be possible, but rather difficult. The value of the record appeared to him to depend upon comparison under equal conditions; thus, in order to test several lines or parts of a line, he thought that the instrument should, in each case, be placed upon the same part of the same engine (or carriage behind the same engine), with the same driver, the weight and length of the train and its speed being also the same. In like manner, when different engines were to be tested, they should run on the same line, and under similar conditions in all other respects. Under such circumstances he thought that the tests might be extremely useful, but he did not think that a series of journeys in a tour over different railways, such as had been mentioned by Mr. Mills, could give any definite results, as the record would be a compound one, in which the faults of the rolling-stock would be mixed up with the faults of the permanent way. Mr. Cowper.

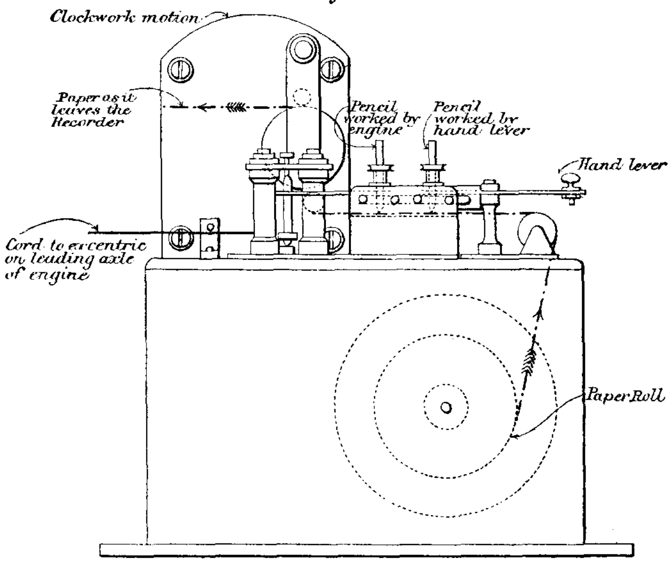
Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL said the principle of the machines before them appeared to be identical with that of the well-known pedometer invented by Mr. Payne. Having sought in vain to find the instrument that his father had, and which he had used many a time, he thought that the next best thing he could do was to obtain the specification of the patent, dated 1831, and he would refer especially to Fig. 8 in that specification, which showed a weight held in a horizontal position by means of a wound-up spring, the implement containing it being carried in the waistcoat pocket. The rise and fall in walking produced a relative motion between the quiescent weight and the moving case, and in that way the number of steps was recorded, and the instrument being set for the particular step of the particular wearer, it showed the number of miles walked. The instruments now exhibited seemed to depend upon this same principle of the inertia of a weight in an apparatus, which apparatus was itself moved by the thing of which they desired to record the movement, and in that way from the inertia of the weight and the movement of the case the motion was obtained which was recorded. Payne's pedometer recorded only, by the movement of a hand on a dial, the number of miles walked, while the record in the instrument before them was graphic, giving not merely the sum of all the motions, but the individual motions, and in that sense it was useful for its purpose. Although he hardly liked to exhibit so coarsely made a piece of work in the presence of instruments of the beauty that were before them, it might be interesting to the Institution to see a brake-recorder which he devised a good many years ago, when the question of the Le Chatelier counter-pressure system of stopping trains was put before the Institution of Mechanical Engineers; this apparatus was made at that time by him, but in a very incomplete sort of way. He tried it on one occasion with the son of the late Mr. Beattie, and only on one occasion. Some years afterwards, when an inquiry into continuous railway brakes came before Mr. Cowper and himself, he had that same machine made in

Sir Frederick Bramwell.

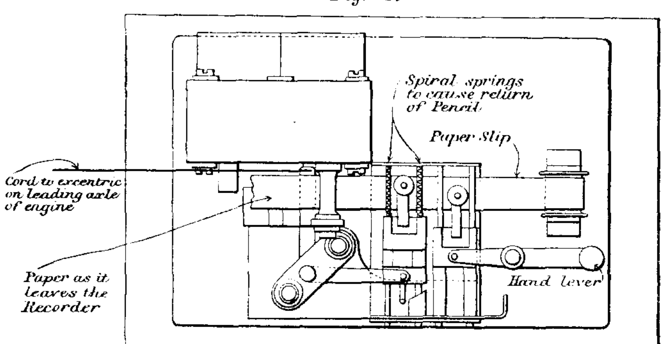
the form in which it now appeared, and he put it to work with very great success. He had brought it forward because it was said on the last occasion that it would be well if a machine were

Fig. 13.



SIDE ELEVATION

Fig. 14.

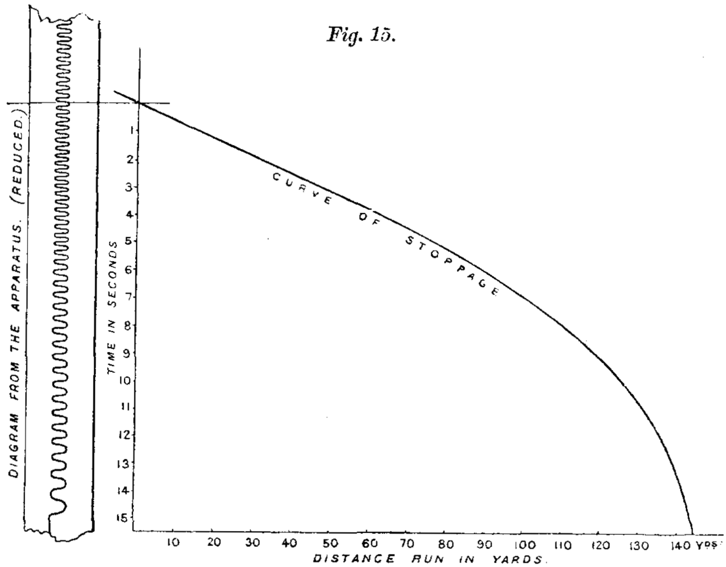


PLAN

made that could be actuated by the leading wheels of the locomotive to eliminate divers questions as to time, and so on. That construction was adopted in the machine he now brought before them (*Figs. 13, 14*). The knife-grinding arrangement on which

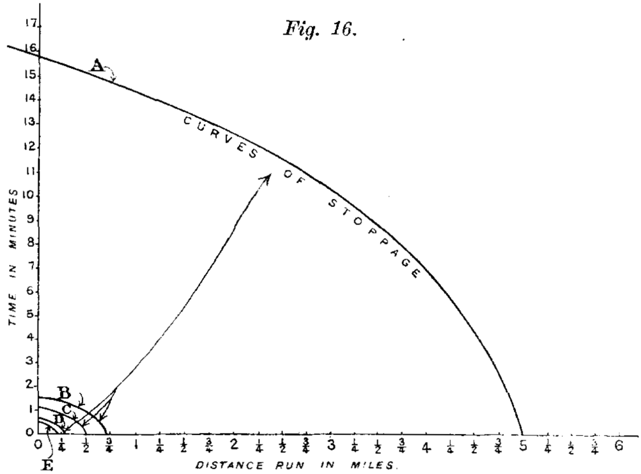
Sir Frederick Bramwell.

Fig. 15.



NOTE.—The distance was measured in yards from the moment at which the brakes were applied or directed to be applied.

Fig. 16.



REFERENCE TO CURVES.

(A) Steam shut off, brakes not applied.
(B) „ „ hand-brakes only applied.
(C) „ „ steam- „ „ „
(D) „ „ continuous brakes only applied.
(E) „ „ all brakes applied.

NOTE.—The speed of the train on all occasions was 50 miles per hour when the steam was shut off.

Sir Frederick Bramwell.

it was mounted was really not part of the apparatus, but was simply a means to illustrate its behaviour when in use. By working the treadle so as to put the fly-wheel of that apparatus into motion a certain number of revolutions per minute were obtained, and the letting those revolutions die out, caused the pencil to make the same kind of diagram as was made when a train was gradually brought to rest by the action of the brakes. He had placed on the wall an enlarged diagram taken from the instrument, *Figs. 15, 16.* The paper was travelling at a uniform rate in the direction of its length; the transverse movements were made one for every revolution of the leading wheel, and a black line near the top showed the point at which it was directed that the brake should be applied. Before that line was reached the reciprocations of the transverse working-pencil were at a certain rate, so that a certain number of them were made in a second depending on the speed of the train immediately before the brakes were applied. All they had to do, knowing the circumference of the leading wheel, was to count those reciprocations and compare them with the time, as given by the paper travelling at a uniform pace, thereby ascertaining with absolute certainty the speed at which the locomotive was running at the time the brakes were ordered to be applied. Looking at those reciprocations on the portion of the paper below the line it would be seen that they became gradually more and more coarse in pitch—that was to say, the engine was travelling more and more slowly, the train being under the action of the brake, until, as seen towards the bottom of the paper, the train having come to rest, the pencil no longer reciprocated, and a mere straight line was drawn. In that way there was obtained a record of the gradual dying out of the speed of the train under the influence of the brake, and also an accurate record of the distance passed over. With those materials it was competent to construct a curve, as would be seen on the diagram. This was made simply by taking the time and the rate of the traverses of the pencil oscillations. There were several curves on the paper (*Fig. 16*) showing the differing times and the varying distances travelled under conditions changing from stoppage without the use of brake-power, through instances of the use of a portion of the brake, to that where all the power was put on. The speed in all cases was 50 miles an hour before an observation was begun. Steam being shut off when no brakes were applied, the train (see curve A, *Fig. 16*) ran 5 miles and 5 yards on a dead level line by the energy stored up in it. Curve B, where the steam was shut off and the hand-brakes only applied, showed that it ran about $\frac{11}{16}$ths of a mile. In curve C the steam-brakes

Sir Frederick Bramwell.

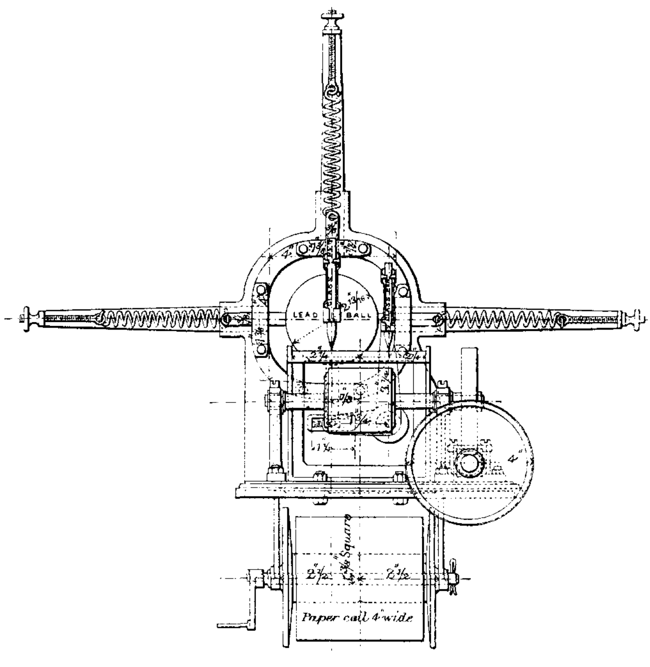
were applied, and it then ran about half a mile. In curve D the continuous vacuum brakes were applied, and it ran about $\frac{1}{4}$ of a mile; while in curve E the steam, vacuum, and the hand-brakes were all applied, and the comparative distance that the train ran was shown. He was aware that this statement was not strictly germane to the present inquiry, but the apparatus was an instance of a recorder worked off the leading wheel of the locomotive. He would ask Mr. Molecey to be kind enough to turn the "knife-grinder" for a short time, and to cause the fly-wheel to revolve. This would represent the leading wheel of a locomotive in motion, the clockwork at the same time drawing the paper through the rolls. One of the pencils would be seen working, making the transverse stroke, and having got the wheel into rotation if they let the speed die down the pencil would make a vandyke of the gradually coarsening pitch of which he had spoken. There was a second pencil, the object of which was to enable the operator to make a record by means of a cross stroke at any desired moment, say, when the brakes were ordered to be applied, or in passing a particular mile-post or anything of that kind. As he had said, he was not putting this apparatus forward as being at all an anticipation of the kind of automatic recorders which formed the subject of the paper, but as being an instance where there had been a connection with the leading wheel of the engine by means of which it was possible to ascertain the absolute speed of the train at any moment, and also to ascertain the other incidents he had mentioned. About the time when these brake experiments were being made there was an article in some of the scientific papers stating an eminent foreign engineer had proved that the leading wheel of an engine went more quickly than did the engine itself. There was another article by another eminent engineer to show that it went more slowly. He (Sir Frederick Bramwell) took an engine, moved it carefully along the line, measured the distance travelled due to the circumference of the wheel, and having ascertained accurately the circumference and the distance moved along by the engine for one revolution, he repeated the observation at the mile-posts on the line at a speed of 50 miles an hour with the result (by which he was not at all astonished) that the leading wheel was in truth an absolute measure of the speed of the engine at any velocity at which it might run.

Mr. Dawson.

Mr. William Dawson said that a machine such as the one he exhibited was made by the London & North Western Railway Co., five years ago, for the purpose of taking a record of the condition of the permanent way in much the same way as was done by the

Mr. Dawson. Author's instruments. It consisted of a lead sphere (*Figs*, *17*, *18*, *19*) 5 lbs. in weight held in position by three springs, one vertical and two horizontal. Guides were provided so that motion could only take place in a vertical plane. As the carriage jolted the inertia of the sphere prevented it from moving to the same extent, and the relative movement of the two was recorded by pencils moving with the sphere upon paper connected with the framework of the

Fig. 17.



PERMANENT-WAY TELL-TALE.

Scale 2 inches = 1 foot.

machine. The pencil which registered the horizontal movement worked easily in a vertical socket attached to the sphere itself so as to be independent of the vertical motions, which were recorded by the second pencil, the connection with the sphere being made by a bell-crank which transformed the movement to a horizontal one; consequently lines were drawn on the paper both by the vertical and the horizontal motion of the carriages (*Fig. 20*, p. 94). The paper was drawn forward at the rate of $8\frac{1}{2}$ inches for every mile the train ran by an arrangement of cords and pulleys connected with

Mr. Dawson.

the axle of the carriage, and consequently the diagram was the same if the train was going 10 miles an hour as if it was going 50 miles. On one side of the machine was an electro-magnet connected with a clock in such a manner that contact was made once a minute, causing an oblong notch to be made in the line marked by a third pencil. This electro-magnet could also be actuated by pressing a button, and in this way the quarter-miles and miles were registered by one, two, three or four marks made as the posts were passed, so that the speed of the train could be at once known. When passing a station, the "push" might be worked five or six times, and the name of the place written on the side of the paper. Several diagrams were made over the same piece of road on the same day and showed the accuracy of the machine, almost appearing as if they had been traced from one another.

Fig. 18.

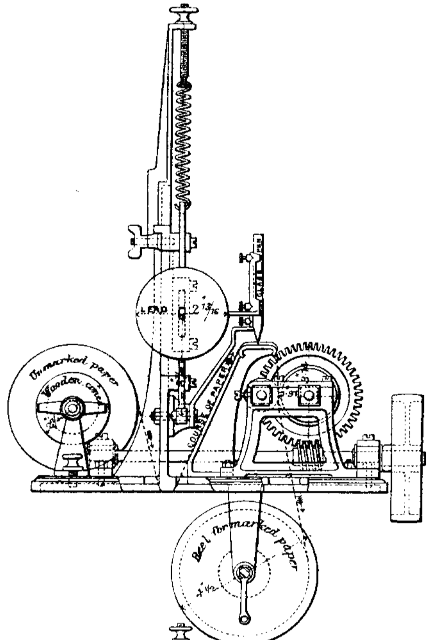
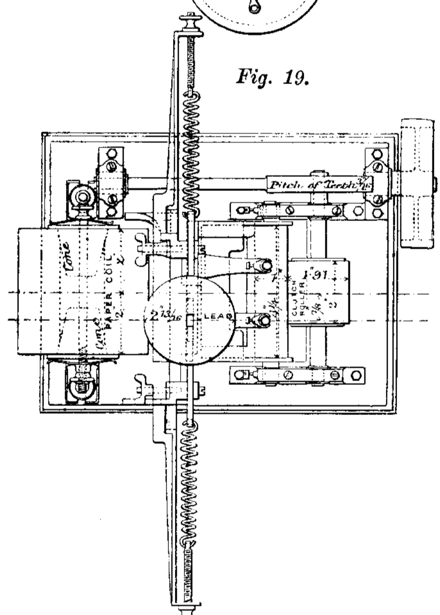


Fig. 19.

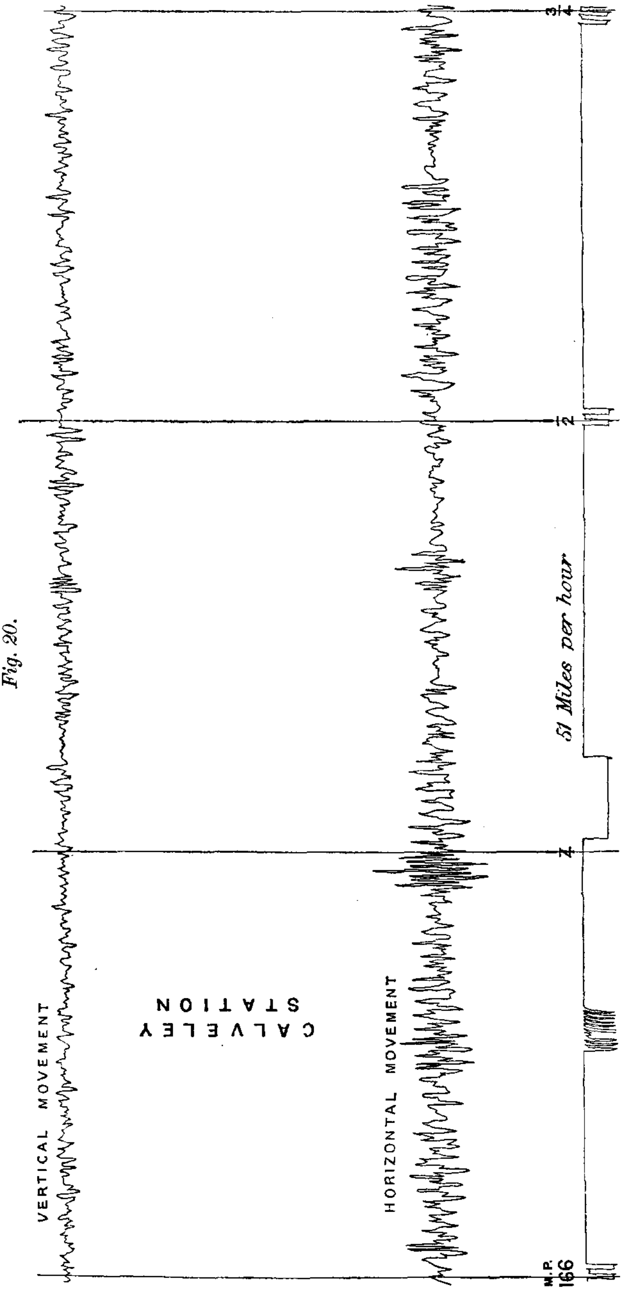


Sir John Coode.

Sir JOHN COODE, President, said a little further explanation was required. Mr. Dawson had said that the diagram was the same

Sir John Coode.

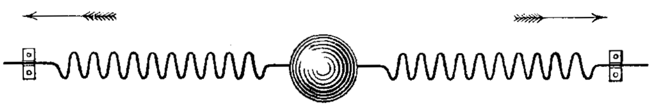
Fig. 20.



whether the train was travelling at 5 miles an hour or at 50. In what respect was it the same? Sir John Coode.

Mr. DAWSON: Only as regarded the distance between the horizontal vibrations. One defect of ordinary machines, where the paper was drawn out by clockwork at a uniform rate, was that the horizontal disturbance as indicated by the movement of the pencils varied with the speed of the train. The faster a train travelled the greater was the vibration of the sphere, and consequently if the speed of the train was not known it might be thought that one part of the road was in worse order than another, when in reality the difference was caused by one diagram having been taken at, say 50 miles, and the other at 5 miles an hour. He had suggested, and in fact shown in the drawing exhibited, that a Watt's governor driven by the wheels of the train might be connected with the springs in such a manner as to pull them farther apart as the speed of the train increased, thus giving increased resistance to the motion of the sphere (*Fig. 21*). In this Mr. Dawson.

Fig. 21.

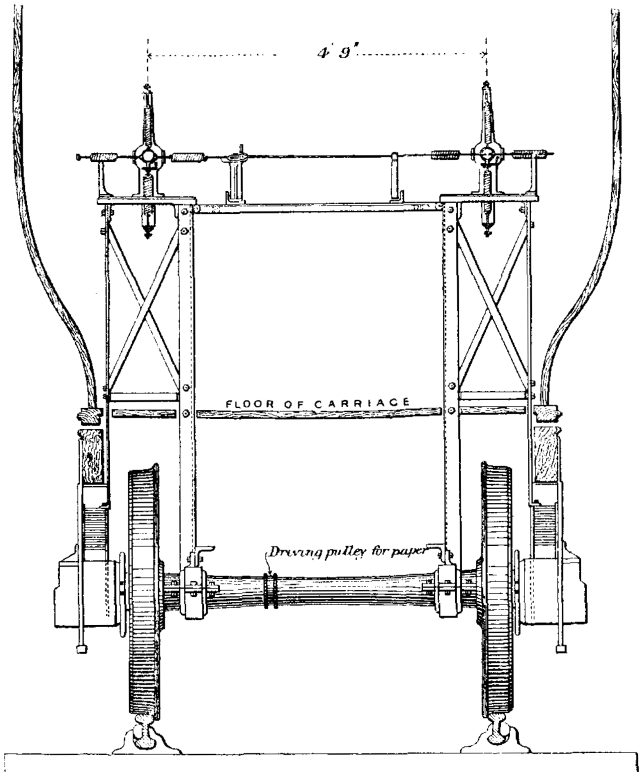


way the range of the lateral movement of the pencil could be made independent of the speed at which the train was travelling. One great defect in all the machines was that they were fixed to the floor of the carriage, and had to encounter the uncertain movements of the springs, as well as the varied motions of the framework in different classes of vehicles, such as four-wheel, six-wheel or eight-wheel coaches. It had been suggested by Mr. Footner, M. Inst. C.E., and partly carried out at Crewe, that they should fix the machine directly to the axle of the carriage in the manner shown in *Fig. 22*, p. 96, and so avoid the sources of irregularities complained of. Another important thing was that if the diagrams were to be of any use at all for indicating defects in the road, they must show each rail separately, because one joint might be down and the opposite one not, and this could be done by having a separate machine fixed over each wheel on one of the axles.

As a matter of practice, however, these diagrams were not found to be of much use. Each portion of the line was examined morning and night by the gangers, who were responsible for a length of

Mr. Dawson. $1\frac{1}{2}$ to $2\frac{1}{2}$ miles, and they and their staff immediately repaired any defect, and were continually advising the District Inspector of the general condition of the portion under their charge; the District Inspector on the other hand was kept constantly employed in supervising the work done by the platelayers, and in calling

Fig. 22.



Proposed Permanent-Way Recording Apparatus.

Scale $\frac{3}{8}$ inch = 1 foot.

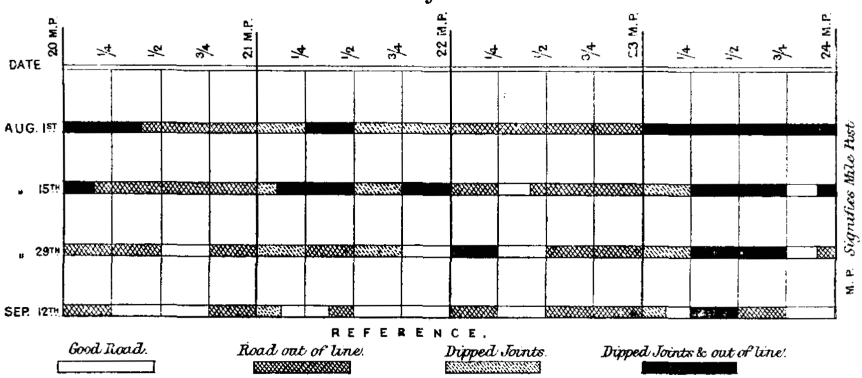
attention of the gangers to any defect that they might not have attended to, and the District Inspector was under a Chief Inspector, whose principal duty it was to make periodical inspections from the foot-plate of the whole of the lines under his charge. His report included the condition of each portion of the road in the following form— Mr. Dawson.

Line. Date of Inspection,

From		To		Condition of Road.
Miles.	Yards.	Miles.	Yards.	

which was afterwards plotted as shown (*Fig. 23*).

Fig. 23.



The London and North Western Railway Co. had taken a large number of records, but it took more time to attend to the instrument and inspect the diagrams than they were worth. They showed miles of road that the engineers did not want to see. Yards of paper were drawn out over lines that were most likely in very good order, but they wanted information about bad roads. Although they had used the machine for some months when it was first brought out, they had not used it during the last four years until last week.

Mr. Dawson. He did not think that any machine would ever supersede or even lessen the necessity of inspection by competent men. A personal inspection not only discovered the defects, but as a rule traced the causes of them, such as "dipped joint," "worn crossing," "rail out of line," "junction," "want of surface drainage," "decayed sleepers," "spent ballast," "loose fishplates," "sleepers insufficiently packed," and so on. The results did not seem very satisfactory so far as they had gone, and the instrument was looked upon as more of a toy than of practical use.

Mr. Kapteyn. Mr. A. KAPTEYN said the subject was one which had engaged the attention of engineers for many years. The Paper might be divided into three parts; the registering apparatus, the diagrams, and the interpretation of the diagrams. With regard to the first, the Authors would have greatly enhanced the value of their Paper if they had dealt more fully with the apparatus used by others during the many years in which this problem had been studied. He would refer to one or two instruments which closely resembled what the Authors had produced. In the "Annales des Mines" in 1885 there appeared in the November and December numbers a paper under the title of "Application de la Méthode rationelle aux études Dynamo-metriques," by Mr. Desdouits, one of the chief engineers of the French State Railway. Mr. Desdouits, who had been experimenting for some years on this subject, commenced by employing a single pendulum to which a pencil was attached, and he found, as the Authors had no doubt observed, that under certain conditions of the road oscillations were set up which falsified the diagrams to a great extent. Mr. Desdouits' first idea was to couple together two pendulums, one with the ball below, in the ordinary way, and the other inverted with the ball above, the two being coupled together by a link. The Authors had done a similar thing in putting two pendulums exactly above each other. Of course there was no difference in principle between the two. For practical purposes Mr. Desdouits preferred another form of apparatus—which he would call a differential pendulum—consisting of a heavy disk, or part of a disk rolling over a horizontal surface, and furnished with a movable weight, by which the centre of gravity of the whole could be made to coincide with the centre of the figure, when the whole would be in a state of indifferent equilibrium, so that it could be jerked or shaken into any position. The great defect in this apparatus, as also in that shown by the Authors, was that in experiments of this kind, it was absolutely necessary that when the normal movement or the normal condi-

Mr. Kapteyn.

tions were attained, the pencil should rapidly and automatically be brought back to zero. An engine when running jerked to the right and the left, and in order to investigate the extent of those jerks and their duration the diagram ought to show distinctly a zero line to separate the two kinds of movements. He noticed in the Authors' diagrams there was no zero line. He observed that in Fig. 1 the Authors had provided a spring D in order to bring the pencil back; but in the other recorders he found no similar provision. Two and a half years ago he witnessed some brake trials on the French State Railway with a train of fifty carriages, fitted with the Westinghouse brake. In the last vehicle of that train sundry diagram apparatus and recorders were put, and amongst others, Mr. Desdouits had his rolling disk, or *pendule dynamometrique*, as he called it. It was particularly noticed that in certain jerks of the vehicle the disk acquired a swinging motion like that of a pendulum, imparting to the pencil very undisciplined movements which falsified the diagrams to such an extent that Mr. Kapteyn considered them useless for measuring the retardation and acceleration of the train. He (Mr. Kapteyn) had also in the van some apparatus for investigating different problems connected with the question of continuous brakes; but as they did not bear immediately upon the point before the meeting, he would not at present refer to them further. The instrument was on the table. But to complete that apparatus he wished to add an instrument to measure the retardation of the train, and the shocks, if any were produced. He thought he could improve upon Mr. Desdouits' plan, and constructed the apparatus shown in *Figs. 24, 25, 26*, p. 100. He had called it the retardation-meter because he was principally interested in the methods of retarding a train. The body of the apparatus and its base E were fastened to a solid table fixed to the floor of the experimental van. The lever B was suspended from the point C, and at its bottom carried a weight A. Near C would be seen the point G, where the lever bore against the stem of the piston P, and this piston was attached to a diaphragm which closed a chamber N, filled with water or any other suitable fluid. The piston P also bore against the stem of a small inlet valve O, which cut off a supply of water under high pressure from the tube D, and prevented it from entering the chamber N. On the left-hand side of the lever a similar apparatus was placed for taking records when the train was moving in the other direction. Supposing the train to be moving to the right, and the brakes were applied or retardation caused in any other way, then the acquired momentum of the weight

Mr. Kapteyn. A would push the lever B against the piston P, which in its turn would open the small inlet-valve O. Water under high pressure would immediately be admitted to the chamber N, until the

Fig. 24.

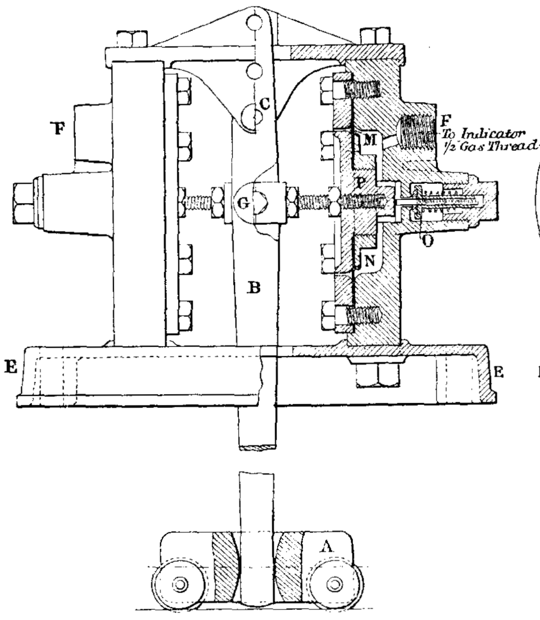


Fig. 25.

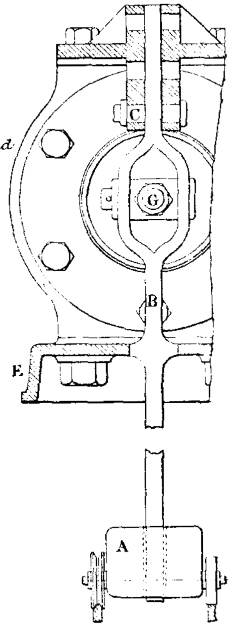
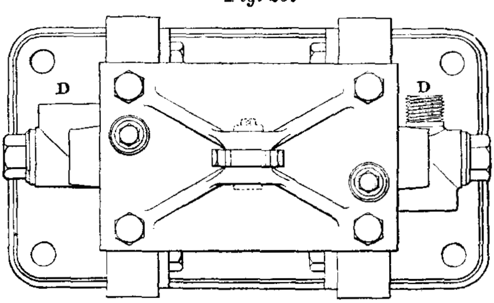


Fig. 26.



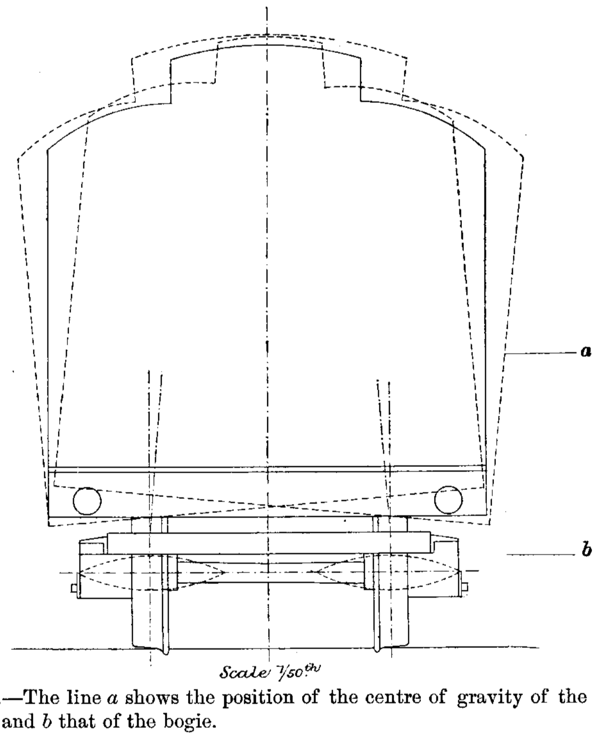
Retardation-Meter.

diaphragm M was in equilibrium between the pressure exerted by the lever B, and the water on the other side of it. The pressure in the chamber N was therefore a direct measure of the retardation of the vehicle. The apparatus was coupled at the point F to one of the indicators of the instrument exhibited. This indicator was fitted as usual with a piston and pencil, which traced a continuous diagram on a strip of paper moved either by clockwork, or by the train in motion, so as to produce a record of the retardation of the train. The apparatus was used for the first time in May last, in some further brake-trials on the French State Railway, and the undisciplined excursions of the pencil were certainly much less. His idea was that if he could constrain the weight A so that it would be unable to make large excursions, the pendulum action would also be greatly reduced. The results obtained, although they showed considerable improvement, were not to his satisfaction, and he attributed that to three causes. In the first place, difficulties experienced on account of the air contained in the water, but by several times opening a vent-plug and compressing the water, it was possible to get rid of this air, and, therefore, he would say nothing further about it. The second cause was, that the lever B was made very light and elastic, and bent back when a considerable retardation occurred, thus producing vibrations which otherwise they would not have had. In the third place the excursions of the weight A were sufficient to make the action of gravity palpable, and this also caused oscillations which they wanted to avoid. He had now decided to modify the apparatus by making the lever B much stronger and more rigid, and by extending its top, so as to bring the centre of gravity exactly at the point of suspension C; otherwise the lever itself would constitute a pendulum, and give rise to the difficulties which he desired to avoid. The weight A he detached entirely from the lever and made a carriage of it, with a hole through its centre in which the end of the lever worked. The carriage A was placed on horizontal rails fixed on the floor of the van, thus avoiding any tendency for the weight, when once disturbed from its normal position, to fall back, as in the case of a pendulum. It might appear that through these alterations the apparatus also had come into a state of indifferent equilibrium, which he criticised in Mr. Desdouits' apparatus, and in that of the Authors, but really that was not so. The weight A opened through its pressure the valve O, and a certain quantity of water entered the chamber N, and was registered on the indicator. Supposing the retardation to cease, the weight would stop pressing the lever, and the valve O would close. There might be some pressure left in the chamber, but the piston of the indicator was always more or less leaky, and its leakiness could be in- Mr. Kapteyn.

Mr. Kapteyn. creased to any extent. Immediately one or two drops of water had leaked through the piston, the pencil would fall to zero, because there would be no more pressure upon it. In regard to the use of the apparatus on gradients, he would point out that brake-experiments were generally made on known gradients, which therefore could easily be allowed for by raising or lowering the zero-line of the diagrams. He thought the Authors had not paid sufficient attention to this question of the return of the pencil to zero. In all the experiments he had made this seemed to be a most essential point. An examination of the diagrams exhibited by the Authors would show that in several instances the pencil appeared to have remained for some time at a considerable distance from the median line, and this seemed to bear out his contention. Furthermore, he thought the Authors should have given a greater speed to the paper, because all the lines of the diagram were blotted together, and it was really very difficult to see what actually happened. He would not go into the question of the interpretation of the diagrams which the Authors had given, because it required a close examination of all the elements involved and a great deal more study than he had an opportunity of giving to the Paper. The whole matter depended upon the object the Authors had in view. If their desire was to produce an instrument for daily use in which it was sufficient to have an approximate idea of what was going on, he thought they might be considered to have partly succeeded; but in that case, perhaps, a ride on an engine would be equally useful. If, however, the Authors had intended to devise a means of correctly investigating what really happened, then, in his opinion, their instruments were not sufficient.

Mr. Aldridge. Mr. J. G. W. ALDRIDGE said the Authors had stated that carriages might be tested in the same manner as locomotives. It was a matter of common knowledge that so long as the speed was low there was comparatively little oscillation, but by looking along a train of carriages, more particularly Pullman cars, travelling at the rate of 40 miles an hour or more, it might be seen that the oscillations were not only much larger, but that they took the form of a continuous rolling curve throughout the entire length of the train. Another strange fact was that if they could measure the relative position of the two ends of one of these cars, which were over 40 feet long, they would find that the curve was formed not only by the motion of the carriages with respect to each other, but also by oscillations between the two ends of each individual car. This was scarcely noticeable except by measurement, but he had found from observation that the two ends of the car were out of alignment, as it were, and therefore the whole train from one end to the other, excepting the engine, was forming one continuous and almost regular curve. When the train started these motions were not observed, but as the speed got up, on what old railway engineers knew as "crooks" and "slacks" on the line, in the first place the carriage, especially if it were a light bogie-car, would fall slightly, first on one side, and then on the Mr. Aldridge.

Fig. 27.



Note.—The line *a* shows the position of the centre of gravity of the body of the car, and *b* that of the bogie.

other; each drop giving a swing to the upper body, which in most cars weighed not less than 20 tons; so that gradually, owing to what Mr. Pullman had happily termed the accidents and the incidents of the line, the peculiar motion which he had described was set up, and as the speed becomes greater, it resulted in a complete rolling oscillation. By referring to the drawing of the cross section of a car (*Fig. 27*) it would be seen that the plane of least motion was on the axle. Of course even there

Mr. Aldridge there would be small vibrations—the minute rattlings as it were of the axle—but up to the present these had not accurately been measured: whether Professor Milne's instrument could do so remained to be seen. At the roof of the car, however, they got a considerable motion. He had the pleasure a short time ago of accompanying Mr. Bracebridge Mills when testing one of the Authors' recorders. At first it was placed on the floor of the car, and being capable of recording a movement of $1\frac{1}{2}$ inch it worked in a satisfactory manner. But when, immediately after, it was fixed as close as possible under the roof, the amplitude of the oscillations was altogether beyond the range of the instrument, the pointer going over with a jerk from one side to the other continually, showing that the leverage was considerable and the motion great. He placed these diagrams on the table (*Figs. 28* and *29*). Such oscillations, as long as they were regular, were not altogether hurtful to rolling stock, but he thought railway men would agree that the greatest damage was done when the car suddenly took a curve; when there was a distinct thud, which most travellers had felt. If each car were of the same dimensions and always travelled at the same speed over a given curve with an exactly suitable super-elevation of the outer rail, there would probably be very little oscillation, because all these forces would counterbalance each other; but as it was, cars of varying weight, size, and speed, went over different curves, and as the forces did not agree they got these disastrous motions. Not long ago he saw a new car, which had been used only a few days, with the body partly torn away from the lower frame, and some of the bolts stripped. The whole of the frame had to be renewed before that carriage could go out again. The superintendent ascribed the result almost entirely to the excessive curves over which it was continually passing. In order to obviate this as much as possible, the Pullman Company and the Sleeping Car Company on the Continent were endeavouring to dispense with the upper berths, because when these were occupied at night, they constituted a great weight high above the centre of gravity of the car. This weight increased the swaying, and he believed that these berths were fast being taken down, or used as little as possible, so as to avoid the excessive oscillation at the upper part of the car. The adoption of gas or electricity was also an advantage to the rolling-stock, as the car-builders had had to put their apparatus for generating and storing the power underneath the car. In the case of gas they got about 5000 lbs. dead weight under the floor, and a little less with a storage battery. This brought down the centre of gravity somewhat, and he had

Mr. Aldridge.

Fig. 28.

Diagram taken near the Roof of the Carriage.

Fig. 29.

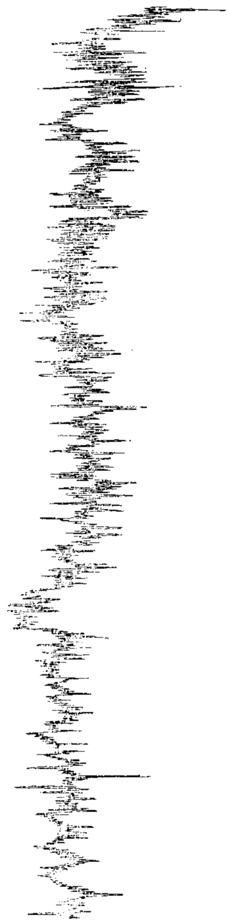


Diagram taken on the Floor of the Carriage.

Mr. Aldridge. been told that it conduced to better running. Referring again to *Fig. 27*, which he had prepared from actual observation, it would be seen that the centre of oscillation was continually changed from one side to the other; at one time it would be found on the plate springs at one side, but as the car went over it would change to the other side, so that it seldom coincided exactly with the centre of the car. That was one reason why there was greater leverage and more disastrous effects with these oscillations. As far as he could learn, one cause of the evil was the use on the side of the bogie of the plate-springs so well known to carriage builders, and he believed in one of Mr. Pullman's latest cars, he had entirely discarded the plate-springs and employed only cylindrical coil-springs. On the Midland he was told they had two cars which had been running some time with such springs; and, moreover, with six wheels to each bogie. These were said to have done the best running of any cars, and to have given no trouble, and it was evidently a step in the right direction. The friction, and consequently the wear and tear, might be greater, but he thought this arrangement overcame most of the jerks incidental to the line. With regard to the best means of measuring these oscillations an apparatus was required with double the range of the one he had seen used, and it was desirable not only to record the movements of the car but the time and the distance travelled as well. For this purpose, the time record and the speed record should be electrically connected, so that on depressing a contact key the two readings should commence and also finish simultaneously. This was necessary in order to get reliable information, because a car that ran capitally at 40 miles an hour might run very badly indeed at 60 miles an hour. In order to test cars properly there should be a sort of measured mile of level track in perfect order, because otherwise the car might be blamed when the fault was in the road-bed. At present the uncertainty in car-building seemed very great; he had seen two cars in which the only apparent difference was in the temper of the springs, and yet in their behaviour they were very unlike, showing that even minor details should be considered. Up to the present the builders had no real guide beyond their own experience, but he thought that this apparatus, for which he supposed they were primarily indebted to Professor Milne, would give the car-builder what the indicator had given to the marine engineer long ago, namely, a diagram by which he could see and ascertain all the varying forces which act during the running of a car, and thereby enable him to attain uniformity of results which now were to a large extent dependent on chance.

Mr. W. W. BEAUMONT inquired the length and weight of the upper car that was described as pushing itself off the under frame. Mr. Beaumont.

Mr. ALDRIDGE said the weight of the upper car would be about 16 tons, and the two bogies 5 tons each. The new bogies that were very likely to come into use were made with Fox's compressed steel frames, and were much lighter altogether; but the present bogies could not be put down at less than 5 tons each. The length of the upper car was 42 feet. So far as he knew there was nothing novel in the connection between the body and under frame of the car, excepting that the side frames were filled in with wood; they had considered the accident to be due simply to the bad road. Mr. Aldridge.

Mr. W. W. BEAUMONT said a great many serious accidents happened in the United States with the very long and heavy cars there used. Although he had no practical experience of them, he often examined the statistics of these accidents, and was led to suspect that some of them must be the result of the enormous weight and length of the cars. The inertia of this heavy load had a great deal to do with the derailments which formed so large a proportion of the whole of the accidents in the States. Of the total number of accidents that occurred, derailments constituted something like 33 per cent., and of these about 52 per cent. arose from unexplained causes. He thought that the fact that this carriage body had by its inertia pushed itself off the lower frame might throw some light upon this matter. Possibly it was because it was very heavy, as it did not appear to happen with shorter and lighter carriages running on the same lines. Mr. Beaumont.

Mr. F. BRACEBRIDGE MILLS in reply for the Authors said, that in his opening remarks he had referred to a diagram he had taken on the 30th of November last year, on a journey from Cannon Street to Sevenoaks in one train, and from Sevenoaks to Hastings in a second train, and he had called attention to the fact that the diagram showed the fore-and-aft motion to be much greater between London and Sevenoaks than between Sevenoaks and Hastings. This he had suggested was due to a difference in the balancing of the locomotives employed to draw the two trains. Mr. Shelford had suggested that the change in the diagram was not due to a difference in the balancing of the two locomotives, but to the fact of Sevenoaks being about the summit of the line, and had further said that if Mr. Mills had taken a diagram on the return journey, he should expect to find the nature of the diagram reversed. Mr. Mills had referred to those diagrams he possessed of the Hastings line, but did not find that Mr. Shelford's expectations were realized. With Mr. Mills.

Mr. Mills. a view of arriving at some satisfactory conclusion on that point, he had made it his business to take another journey to Hastings and back, and he had brought with him the diagrams obtained. He had had the benefit of the valuable assistance of Professor Perry in making the tests. It would be seen from the diagram that on the down journey the average range of motion between Cannon Street and New Cross was 1 millimetre, from New Cross to the mouth of the tunnel between Chislehurst and Dunton Green $1\frac{1}{2}$ millimetre, being greatest just after passing Chislehurst where the road appeared to be level, namely, 2 millimetres. It appeared that the down grade commenced just before Sevenoaks, as shown by the instrument and his observations; and from that point to Tunbridge Wells the diagram averaged $1\frac{1}{2}$ millimetre, not being so wide at any point as on the straight run through Chislehurst. From Tunbridge Wells to about half way to Wadhurst the diagram showed a slight rising gradient, where the motion recorded was less than 1 millimetre. From Wadhurst to a point a few minutes before passing Battle the diagram was larger than on any other part of the line, averaging 3 millimetres, and measuring in some places as much as 5 millimetres. From Battle and St. Leonard's to Hastings the range of motion fell to about 1 millimetre. On the return journey there was nothing to call for special notice until Battle was passed, where the fore-and-aft motion perceptibly increased, measuring in places between Battle and Etchingham as much as 5 millimetres. It would be observed that the diagram was the same on the down as the up journey, the greatest motion occurring at the same point on the line. From Etchingham to Tunbridge Junction the average motion was $1\frac{1}{2}$ millimetre, and from Tunbridge Junction to Sevenoaks it was the smallest recorded during the whole journey, the diagram between those stations being everywhere less than 1 millimetre in width. After passing Sevenoaks there was a slight increase in the range of oscillation, and a few minutes before reaching Chislehurst the diagram expanded to about 3 millimetres at a part where the road was apparently level. About the same range of motion was recorded at that point on the down journey. From Chislehurst the average was about 1 millimetre, which, however, increased to about $2\frac{1}{2}$ millimetres when running through New Cross. In measuring the diagrams he had, of course, been careful not to take in account the effect produced by the application of brakes, or by the train being suddenly started from rest, or the speed suddenly increased or retarded. He did not. however, think that the experiment could be taken as conclusive; he was not himself satisfied with it. The only way of arriving at a satisfactory conclusion would be to take diagrams on the two engines to which he had referred, when running under exactly similar conditions; that was, if they could possibly be identified. The diagrams were before the members, and would speak for themselves. He believed that the instruments recorded the fore-and-aft motion with perfect accuracy. With regard to Mr. Kapteyn's apparatus, he confessed that he was unable to follow closely the explanation given of it. It appeared to be rather complicated, aiming at a good deal more than had been laid down in the Paper. There was one point to which Mr. Kapteyn had drawn attention, namely, that in Fig. 1 there was a spring D shown which drew the weight W back to its position of rest, and he seemed to think that that was not shown in any other instruments. Mr. Mills would like to call attention to *Figs. 7, 8, 9.* It would be seen that there was a view of the beam and weight in a deflected position, and there were compensating springs shown. Mr. Kapteyn had laid great stress upon the point of returning the pencils to zero line, and seemed to suggest that in the Milne-McDonald diagrams, the serpentine appearance indicated faulty recording because the pencil remained in a deflected position when it should have returned to zero. That was not the case; the zero line was always the centre of the diagram of normal motion at any place, the serpentine appearance being caused by the inclination of the carriage when on curves or gradients. Mr. Mills thought that the action of gravity in tending to make the pencil set itself in a true vertical position appeared to indicate a probable defect in Mr. Kapteyn's apparatus. So far as he had been able to follow the description of Mr. Kapteyn's retardation-meter, he understood that by the inertia of a weight attached to the end of a long lever which operated certain valves so as to admit water under varying pressure to ordinary piston indicators, a record of the retardation or acceleration of a train was made by the pencils of these indicators on a continuous strip of paper. If the coach containing Mr. Kapteyn's instrument were running on a steep gradient, or round a curve, the diagram would, he thought, be a very unreliable record of the retarding effect of the brakes. Suppose the coach to be running down a decline, the weight at the end of the lever would tend to move forward, and acquire a vertical position by the force of gravity, causing the lever to bear against the valves and so increase the pressure of the water behind the pistons of the indicators; thus a diagram representing a retardation of the train would be produced, whereas in all probability

Mr. Mills.

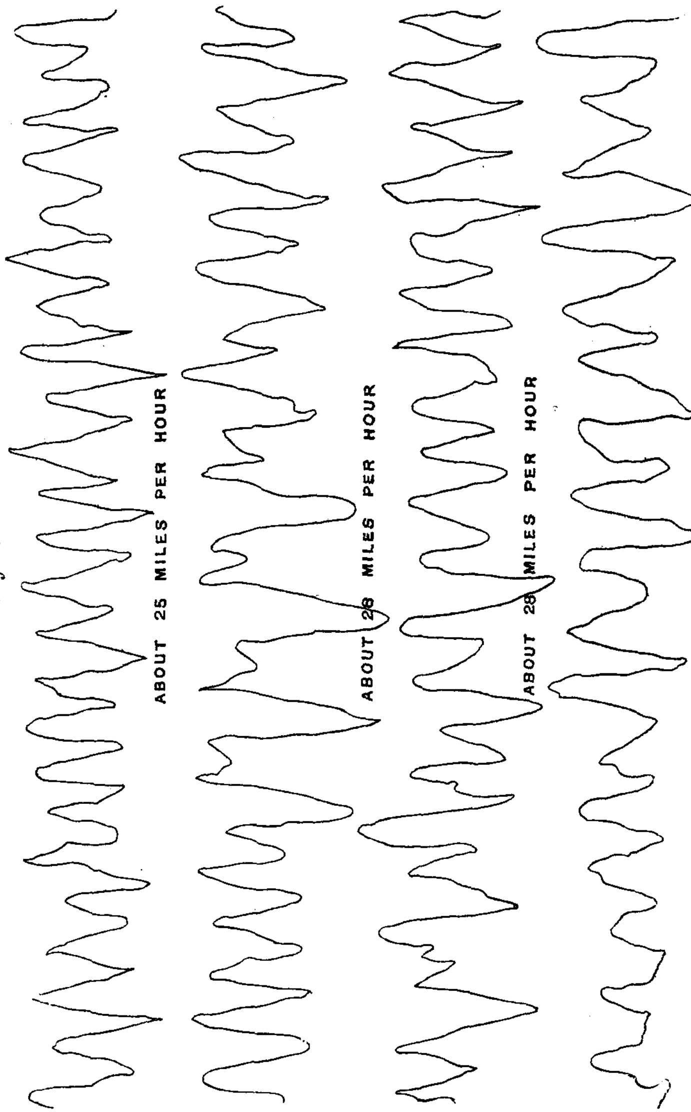
Mr. Mills. under such circumstances the speed of the train would have been accelerated. If Mr. Kapteyn would examine the original diagrams produced by the Milne-McDonald instruments, he would find that in many cases, according to the purpose for which they were taken, the speed of the paper had been such that every movement of the pencil could be traced. Mr. Kapteyn had evidently drawn his conclusions from the exaggerated diagram on the wall, which was really only an outline representation of the original. Mr. Moir had called attention to a peculiarity in the diagrams which he had himself inadvertently omitted to mention, but which had been referred to by the Authors, namely, that they gave a record of when the brakes were applied. That was, indeed, one of the most interesting features of the diagram of fore-and-aft motion when taken in an ordinary carriage, and by referring to the Hastings diagrams it would be seen quite distinctly where the brakes had been applied suddenly or gradually. The instrument would record the retardation of a train by the careful application of the brakes before it could be noticed by an ordinary passenger. Reference had been made as to the best position in which to place the apparatus in the train, and the President had expressed an opinion that the position in the coach itself might also be a matter of importance. Those were points which were decided by the nature of the particular experiment, and the purpose for which it was being conducted: whether for testing a coach, a locomotive, or the permanent way. For testing the running of a coach it was necessary, as had been pointed out by Mr. Aldridge, to take it over the same track many times at varying speeds, and with the instrument in different parts of the coach; or preferably with three or four instruments fixed in different positions and recording simultaneously. Similarly with locomotives being tested for balancing; an instrument for recording fore-and-aft motion, such as was represented by *Figs. 11* and *12*, was stood on the foot-plate, and the engine run backwards and forwards over a level track at different speeds, both with steam on and steam off. For testing tracks he had always endeavoured to place his instruments as near as possible directly over one of the axles, or in the case of bogie cars, directly over the foremost bogie. They could of course be fixed in front of the buffer-beam of the locomotive, as had been suggested by Mr. Hill, so as to get the motion of the leading wheels, but he was not aware that that had ever been done. When designing instruments especially for car-testing, he would not fail to consider the valuable suggestions made by Mr. Aldridge.

Mr. Mills.

It had been suggested by Mr. Dawson that one of the greatest defects in all such instruments for recording irregularities in the permanent way, was that, being fixed to the floor of the coach, the uncertain movements of the springs were experienced. Mr. Mills did not think that any serious error was introduced in this way when the instrument was placed directly over an axle. Mr. Dawson had claimed great accuracy for the instrument he exhibited, constructed by the London and North Western Railway Company, in which the free swing of a heavy ball was constrained by three springs pulling in different directions. Mr. Mills would have thought that the springs of the instrument were as liable to uncertain movements as those of the carriage. It was probably the combined uncertainties of the carriage and instrument springs that had apparently caused Mr. Dawson some trouble. Mr. Mills had noticed that when the ball of the London and North Western Company's instrument was displaced from zero and then released, it occupied an appreciable time in coming to rest. It did not appear so dead beat as the Milne-McDonald vibration-recorder. He admitted that he thought it preferable to eliminate the effect of the springs by taking the motion directly from the axles, but the plan suggested by the London and North Western Railway seemed rather costly to carry out, as it necessitated a specially constructed coach. It had been proposed to apply the Milne-McDonald vibration-recorders for track-testing in a like manner, but in the case of those instruments it was only necessary to fix two of them at the backs of two opposite axle-boxes as shown in the sketch. By such an arrangement all the advantages of the London and North Western Company's instrument would be obtained without the cost and inconvenience of a special instrument-coach. Unfortunately, however, by the nature of this instrument no such ready means of application could be adopted. He did not quite see the point of Mr. Dawson's reasoning when he said that it took more time to attend to such instruments and inspect the diagrams than the records were worth, as they showed miles of road the inspector did not want to see; yards of paper being drawn out over good roads, whereas they only required information respecting bad roads. Mr. Dawson had already told the meeting that gangs of men were despatched to inspect short lengths of the line every night and morning, but Mr. Mills would have thought that the inspection could have been more economically and expeditiously carried out by one man with a recording instrument travelling on a locomotive or carriage, who would thus obtain a record of the condition of say 50 miles of the line in less time

Mr. Mills.

Fig. 30.



Before the Alteration of the Balance-Weights on the Driving-Wheel.

Mr. Mills.

Fig. 31.

AFTER THE ALTERATION OF THE BALANCE-WEIGHTS OF THE DRIVING-WHEELS.

Mr. Mills. than would be occupied by several gangs of men walking along the track, not to mention the possibility of the men overlooking faults. He could quite understand that such an instrument as Mr. Dawson had described was inconvenient to use, and that a great deal of time might be occupied in coupling the instrument-car up to the train and preparing for a test, but surely if a defect existed anywhere it was in the particular apparatus or its method of application rather than in the general system of inspection of the permanent way by vibration-recorders such as those described by the Authors and himself. It had been asked if it were possible from the character of the diagrams to ascertain where the fault was due to the engine and where to the permanent way. Faulty balancing of the engine affected mainly the fore-and-aft motion, but irregularities in the permanent way had practically no effect on that portion of the diagram; indeed, the fore-and-aft motion might be taken as produced wholly by the locomotive itself. Faults in the permanent way showed themselves most in the diagram of vertical motion, and when such diagrams were taken in carriages or on locomotives, it was the excrescences upon them which indicated the jerks caused by defects in the line. It was true that each locomotive or carriage had its characteristic oscillation, but that would be repeated throughout the run, only becoming larger as the speed increased. It had also been found that the same line reproduced its characteristic diagram on whatever locomotive or carriage the tests were made; the diagrams only differing in dimensions. The opinion had been expressed that too much was made of the balancing of the engines in this Paper, and it seemed to be suggested that there was no absolute proof that the abnormal fore-and-aft motion that had been experienced, was so much due to defective balancing. He would call attention to *Figs. 30, 31*, representing diagrams taken by the Authors on engine No. 138 of the Imperial Japanese Railway. In the first instance, *Fig. 30* showed the absolute motion of the locomotive when the balancing was known to be faulty; it was abnormally great. After making this experiment Professor Milne and Mr. McDonald had suggested that certain alterations should be made in the balancing; those alterations were carried out, and the engine was again tested, the result being shown in the diagrams in *Fig. 31*, which were obtained under exactly similar conditions. Those tests seemed to prove that the fore-and-aft motion was due in a great measure to differences in balancing, because the same engine working under similar conditions, except as to the adjustment of its balance-weights, produced in the one case a curve, and in the other almost a straight line. With regard to the difference in the consumption of coal, he thought the Appendix to the Paper clearly showed the opinions expressed by the Authors on that point to be well founded. It had been suggested that it would be better to drive the drums of the instruments from the axle of the carriage or locomotive, in order to more readily locate the faults. Undoubtedly they would get a better idea of the position of any particular disturbance if such a course were adopted; but he had always understood that Professor Milne regarded it as a great feature in his apparatus that it was portable, and capable of being used in any carriage or on any locomotive without special fixing. When once they came to drive the drums from the axle, it was necessary to have a specially constructed carriage, and the nature of the instrument was entirely altered. Mr. Mills.

Correspondence.

Mr. JOHN A. F. ASPINALL took exception to the statement of the Authors that the economy of fuel was due to balancing; more probably it was to be ascribed to the mere presence of an inspector or investigator watching the experiments. Perhaps 10 per cent. of the fuel could be saved on any of the best locomotive engines in England, without spending a farthing on additional mechanism, by sending an inspector to travel on the engine for a month. Knowing they were watched, the men would take more care. The wear of tires was affected by so many different things that it was most unwise to deduce any argument from so small a number as the Authors had tried. Mr. Aspinall.

Mr. D. K. CLARK could confirm the opinion of the writers of the Paper, that a proper equilibration of the engine was not only conducive to economy of maintenance, but was beneficial also in reducing the resistance on the rails at high speeds, and economising fuel. Mr. Le Chatelier, about forty-one years ago, experimented with an outside-cylinder goods locomotive on the Orleans railway. The engine had originally been unbalanced, and vibrated when running with considerable oscillatory movement. The wheels were afterwards fitted with counterweights so distributed as to effectually extinguish the oscillation. The following average quantities of fuel were consumed during three months, the engine Mr. Clark.