APPENDIX II.

DETAILS OF RAIL-GAP MEASUREMENTS ON THOSE DAYS WHEN MAXIMUM RAIL-TEMPERATURE EXCEEDED 100° F.

Date.	Morning.				Afternoon.			
	Shade temp.: °F.	Rail temp.: °F.	Mean gap: inches.	No. of tight joints.	Shade temp.: °F.	Rail temp.: °F.	Mean gap: inches.	No. of tight joints.
23.5.1938 . .	50	55	0·53	Nil	69	105	0·19	1
4.6.1938 . .	56	67	0·45	Nil	67	101	0·19	1
9.6.1938 . .	59	71	0·38	Nil	67	104	0·22	1
14.6.1938 . .	59	59	0·39	Nil	75	103	0·22	Nil
16.6.1938 . .	54	58	0·42	Nil	74	101	0·19	2
17.6.1938 . .	59	64	0·39	Nil	73	106	0·16	3
20.6.1938 . .	55	60	0·42	Nil	71	105	0·14	4
21.6.1938 . .	58	67	0·39	Nil	75	115	0·13	7
22.6.1938 . .	60	61	0·37	Nil	76	102	0·14	6
24.6.1938 . .	63	64	0·36	Nil	77	109	0·14	7
25.6.1938 . .	62	63	0·34	Nil	73	107	0·13	7
19.7.1938 . .	59	61	0·37	Nil	75	110	0·11	7
30.7.1938 . .	62	68	0·38	Nil	74	108	0·13	4
3.8.1938 . .	63	63	0·28	Nil	79	107	0·09	9
4.8.1938 . .	61	61	0·28	Nil	78	102	0·09	10
5.8.1938 . .	67	70	0·25	Nil	82	106	0·11	9
6.8.1938 . .	62	63	0·31	Nil	86	110	0·13	11
8.8.1938 . .	64	64	0·30	Nil	78	103	0·13	7
9.8.1938 . .	63	63	0·30	Nil	79	103	0·09	11
11.8.1938 . .	60	60	0·31	Nil	77	107	0·09	10
12.8.1938 . .	58	59	0·34	Nil	77	103	0·19	1

NOTE.—Number of joints concerned, nineteen.
Morning readings taken between 7.0 and 7.15 a.m. Greenwich mean time.
Afternoon readings taken between 12.30 and 1.40 p.m. Greenwich mean time.

Discussion.

Mr. Loach, in introducing his Paper, said that the trials had been made under the most severe conditions which could be imposed at the time. Although relaying of track had to comply with a long-planned time-table, it should be possible to make arrangements so that a long length was not keyed up under extreme conditions of temperature; by keying up at a common moderate temperature stresses might be sometimes tensile and at other times compressive. Excepting in extreme temperatures the stresses would be considerably lower than those found in the experiment.

He wished to express his thanks to Mr. R. F. Samuels, who had faithfully assisted him in making measurements in all weathers and at all times of the day throughout the trials.

Mr. Campion introduced his Paper with the aid of a film and three lantern slides, the first of which illustrated the general set-up for taking the readings on the test length. The second slide showed a cross-section of the blanketing which was carried out at Hildenborough prior to laying in the long rails, and the third slide illustrated the Robel cranes which were used for handling the long rails. A gang of twelve men could handle a 180-foot rail with those cranes, and their initial cost had been fully justified by the saving in labour. They clamped on to the rail bogie and could be easily dismounted, whilst their use had avoided the crippling of rails.

Mr. G. B. Barton said that he was a little disappointed that the conclusions reached by the Authors were not more conclusive. Mr. Loach had stated in his Paper : " These experiments suggest that, from the point of view of stability, there is no technical objection to the welding of long lengths of bull-head track," whilst Mr. Campion had stated : " It is considered that the tests so far made at Hildenborough are insufficient for an opinion to be expressed as to whether bull-head rails of unlimited length can be installed with safety." What engineers wanted was some practical proof. and, whilst Mr. Barton appreciated that the tests in question were only in the experimental stage, he regretted that more definite conclusions had not been reached.

In February 1937, on the southern area of the London and North Eastern Railway, 120-foot rails were put in, with $\frac{3}{8}$-inch expansion. They had sixteen rail anchors to each rail, and wood keys were used. The rails were laid on plenty of ballast, but unfortunately during the summer trouble was experienced with expansion ; adjustments had to be made ; and it was found advisable to put in steel keys and also additional rail anchors. Not far away, at Finsbury Park, some 60-foot rails were laid in 1936 ; they were welded in situ to be 180 feet, and no trouble had been experienced with them. The speed of the traffic on that line, which was a goods line, was only 30 miles per hour, whereas the 120-foot rails to which he had referred were on the main line to the north, which carried fast-running traffic. In Scotland, in 1939, a section of 120-foot rails was put in, on a line which had already been laid, so that there was no danger of the ballast not being consolidated. Steel keys were used, but trouble was experienced with expansion and adjustments had to be made both in the summer and in the winter. In Scotland also 180-foot rails which had been welded were put in, and no trouble was experienced with those. It seemed a curious fact that the 120-foot rails caused trouble whereas the welded rails did not, and it would be interesting to discover whether there was some peculiarity about 120-foot rails which caused them to expand.

There was a wide difference between experiment and practice, and the requirements for the longer rails seemed to be that rail anchors should be used, at all events to start with, steel keys should be used, and a very good bed of ballast should be provided. That, unfortunately, entailed extra cost, and, whilst he was a strong advocate of the longer rails, it was desirable to ascertain whether they would pay in every way. At present it appeared that, from the maintenance aspect, no saving was effected, and he did not think that a really comparative test could be made until the whole of a ganger's length was laid in with 60-foot rails and compared with one laid in with long rails or with welded lengths.

Mr. Campion had expressed the view that in a well-maintained track buckling of rails should not be experienced, but unfortunately it did occur even on tracks that were thought to be very well maintained, and it was one of the dangers that had to be faced. Mr. Barton did not think that railway engineers were yet in a position to combat it.

His chief, Mr. J. C. L. Train, M. Inst. C.E., who was unable to be present that evening, was very interested in the problem and had decided that, in laying in some flat-bottomed rails during the present year, he would have some welded, for comparison with unwelded flat-bottomed rails and also with bull-head rails. It was intended to lay them in a 2-mile stretch on the main line ; that was an ideal place for an experiment, but unfortunately it was found impossible to get the rails welded, as the only welding-machine in the country was being employed elsewhere. It had been intended also to have welded bull-head rails in the slower line. It would be very advantageous if a Research Committee could be established which could give guidance in such matters, and through which railway engineers could carry out tests and experiments of the type that all wished to undertake. It was difficult to carry out tests and experiments under war conditions ; but it was very desirable that such experimental work should be done, and railway engineers should be prepared to adopt long rails and long lengths of welded rails after the war.

He wished to ask Mr. Campion about the Boutet type of welding. He remembered seeing some rails near Knockholt tunnel welded in that way, and he would like to know how those rails had lasted. His own Company would have introduced the method but for the war ; it was very cheap and did not require much capital expenditure.

Mr. R. C. Kirkpatrick observed that hitherto British railway engineers had had to rely mainly upon American results, which had been obtained with flat-bottomed rails.

Whilst Mr. Loach considered that technically it was safe to weld unlimited lengths, Mr. Campion had contented himself with welding three rails and was not prepared to sponsor unlimited welding. Mr. Kirkpatrick's own experience had been confined to three lengths of track in the down main line on the Great Western Railway within 5 miles of Paddington. The first was put in in 1935, and the other two in 1938. In each case alternate joints of 60-foot, 95-lb. British standard bull-head rails were thermit-welded, and intermediate joints were fixed with two-hole short fishplates ; the track was laid with normal sleepering and normal ballast, and teak keys were used. The expansion-gaps in the alternate rails were $\frac{5}{8}$ inch in one case and $\frac{7}{16}$ inch in the other two cases. In the first case the total length was about 800 feet; in the second case it was 1,600 feet, and in the third case 1,100 feet. No trouble had occurred in the first two cases, but the $\frac{5}{8}$-inch gap now stood at $\frac{3}{16}$ inch (it had not been touched by men), and the $\frac{7}{16}$-inch gap was now $\frac{1}{4}$ inch. The third length, which was $2\frac{1}{2}$ miles out of London, was similar in that two rails were welded together, making 120-foot lengths. The line was level and mostly straight, the speed of the traffic being about 50 miles per hour. It was laid in January 1938, with a gap of $\frac{7}{16}$ inch in the alternate joints, and the total length was 1,100 feet. The alternate joints were the same in that case, except in one place, where an insulated block joint was inserted on account of track circuit. The result was a maximum length of 240 feet welded together. During a very hot week in August, 1938, all the gaps were tightly closed and there appeared to be a risk of buckling. If that had happened after he had read Mr. Loach's Paper he would have had more confidence, but at that time he had been apprehensive of buckling. He did not say that it had occurred, but the joints were butt-tight and he had therefore asked the chief engineer to agree to cut off $2\frac{3}{8}$ inches at one end and re-space the sleepers. No subsequent trouble had occurred either in the greatest heat or in the severest cold that had been experienced since then. No thermit-welded joint had given any signs of failure whatever: the joints were perfect, and still remained so.

Mr. Campion had said that he had experienced no trouble with his 180-foot lengths, but Mr. Kirkpatrick would like to ask what he recommended, as the result of his practical knowledge, for the future. Would he favour the 180-foot lengths? Mr. Kirkpatrick rather suspected that that would not be the case, because Mr. Campion had pointed out that they had to have a very well-drained track, which could not always be obtained, and also that care had to be exercised with regard to the keying-up temperature, which was not always easy to arrange in routine relaying work. Possibly Mr. Campion would prefer 120-foot rails or 60-foot rails, and it would be very helpful if he would give the members the benefit of his experience. Even in the case of 120-foot rails practical difficulties arose in connexion with transportation. Mr. Kirkpatrick did not know whether Mr. Campion had had a mishap with any of his welded rails, but one could visualize considerable difficulty in dealing with a mishap if the joints were welded. On the credit side there could be put a definite saving in maintenance; but Mr. Kirkpatrick thought that for economic and practical reasons many years would elapse before 60-foot rails were superseded for general use in fast-running tracks.

Mr. J. Taylor Thompson observed that Mr. Loach had measured variations of gauge, and that appeared to be more a test of the fastenings than anything else. He would like to know what Mr. Loach had had specially in mind in making those measurements.

The Table of "Restrained Contractions of 507-foot Lengths of Rails" appeared to imply that wooden keys gave better results than steel keys. He did not know whether he had misinterpreted that Table, but it appeared as though less movement was experienced with wooden keys than with steel keys. He believed that so far all railway engineers had acted on the assumption that steel keys gave much better results than wooden keys.

He had had experience with welded track on the North-Eastern area of the London and North Eastern Railway. In 1935 a length of 420 yards of independent track at York, carrying chiefly goods traffic but also some odd passenger trains, was thermit-welded. The track was situated between two others, so that it had no free side, and it was well ballasted with stone ballast. Whilst no very accurate measurements such as those recorded in the Authors' Papers had been made, broadly speaking no change had occurred in the track, except that it had moved as a whole $1\frac{3}{4}$ inch in the direction of the traffic, and no difficulties had been experienced with maintenance or otherwise. He had looked at the track on the previous day, in order to refresh his memory, and had found that it had a perfectly good top and a perfectly good line. That had been the report on it throughout the 7 years. He thought that went a long way to confirm optimistic reports about long welded track, but the length of track to which he had referred was particularly well situated so far as ballasting was concerned, and that was an important element. In the open country, with an open side—an open cess—the conditions would be much less favourable. As there was on his Company's track, and probably on most tracks, a ballast shoulder of about 6 inches, or a little more, it was very doubtful whether there would be much end resistance for the sleepers, but there would be considerable frictional resistance on the base of the sleeper. His Company had also had a number of 90-foot rails and no difficulties had been experienced.

As a result of the experience with the 420-yard length, it was decided to butt up tight the ordinary joint for $\frac{1}{4}$ mile in four or six localities, on independent tracks or sidings; but the result was not very satisfactory. A rather bad line was obtained at the joint, which seemed to indicate that an ordinary joint was not quite good enough to transmit the stresses.

The advantages that would be expected to accrue from the welded track would perhaps be, firstly, greater comfort for the passengers. That, he supposed, had great publicity value; he did not know how much financial value it had—for instance, how much higher the fare from London to Glasgow could be if welded joints were adopted, in comparison with the fare with the joints unwelded—but doubtless public comfort was an important consideration. Secondly, there would be a saving in maintenance of the joints. He did not know whether anyone was yet in a position to put a financial value on that, but he thought it must have one.

It had occurred to him that at least part of those advantages might be obtained if a joint was designed to serve exactly the opposite purpose from that of the present joint. The British standard joint was specially designed to allow expansion and contraction. He thought it was well known that when the track was tightly bolted up one could lock up that expansion, and when the bolts were slackened the ends came together with a bang, showing that the stress had been locked up in the joint. But supposing that railway engineers set themselves to design a joint for strength and to prevent expansion—instead of designing to allow for expansion and then oiling the joint to make certain that expansion took place—and put the rails close to one another, or practically touching, they might increase the comfort of the passenger, through having no appreciable joint or no joint at all, and also achieve a saving in maintenance and without welding. It seemed to him that it would be at least worth while to discuss such a modification of railway engineers' views on the basic idea of a joint specially designed to eliminate expansion, presuming, of course, that the ballast and the surroundings would be fit for welded joints. If the rail was safe from a buckling point of view with welding, then presumably it would be equally safe with a joint designed for strength and to prevent expansion, and perhaps some railway engineers might turn their thoughts towards experiments along those lines.

Mr. F. C. Johansen said that earlier speakers had referred to more than one respect in which the conclusions drawn from the two experiments described by the Authors had differed ; but he thought it was true to say that in many respects a valuable degree of agreement existed between them. On the other hand, some points of seeming agreement were more apparent than real. One of those concerned the temperature-rise which Mr. Campion and Mr. Loach had recorded as causing movement of all the sleepers in a length. Mr. Campion's value for that was $23\frac{1}{2}°$ F., whilst Mr. Loach, for the different conditions at Medbourne, had concluded that the same effect would take place if the temperature-rise was $24{\cdot}1°$ F. Those two values were close enough to attract attention, but no more than a little analysis was needed to see that the factors underlying the agreement were different in many respects ; in fact, the agreement was the result of two conflicting sets of factors which tended to compensate one another.

The value obtained by Mr. Campion related to 180 feet of rail with seventy-eight sleepers, whereas Mr. Loach's value related to 203 sleepers spaced over 507 feet of rail. That was a tremendous difference which of itself would show that the results of temperature-rise were purely coincidental, were it not for the fact that each Author had derived a value for the average resistance to movement of the sleepers. Mr. Campion's value was very close to 1 ton per sleeper, whereas Mr. Loach's was only 9 cwt. It was in that respect that an appreciable difference appeared between the conditions of the two experiments, which might go far to explain a number of points of difference apparent from time to time throughout both the results and the conclusions drawn from them.

For instance, what would have been the case in the L.M.S. Medbourne experiment had the resistance to movement of the sleepers been as high as the 1 ton which Mr. Campion had found at Hildenborough ? Probably the answer was that, in the case of the very long welded length of 2,213 feet, the central portion, which was completely restrained by the sleepers, would have been longer than it actually was ; that the lengths at the ends subject to movement would have been correspondingly shorter ; and that the movement of rail at the free ends would have been smaller.

That seemed to point to the conclusion that it was advantageous, from the point of view of dealing with the troublesome expansions that occurred, for the rail to be as fast as possible in the chair which fixed it to the sleeper, and for the resistance of the sleeper to be as high as possible.

What would happen, in the practical case of a very long length, if a fracture occurred near the centre—as must happen occasionally ? As soon as such a fracture occurred the broken ends became free in exactly the same way as the free ends of the long lengths at Medbourne, and presumably they would tend to separate by an amount dependent upon the difference of temperature between that which obtained when the rails were laid and that which obtained when the fracture occurred. That might lead to a separation between those ends of anything up to 2 inches, and that separation might take place, in very favourable weather conditions, in so short a time that repair before trains could be allowed to pass over it would mean serious interruption of traffic. From that point of view, therefore, a considerable advantage would be realized if the resistance to movement of the rails could be increased as much as possible by improving the resistance of the sleepers, by maintaining the ballast, and by ensuring good fixation of the rails in the chairs.

How could a gap of the kind in question be repaired ? That presented many aspects, each of which would provide a problem for the permanent way engineer. One of them concerned merely the effect of temperature-change. He thought it would be fairly generally admitted that a tendency existed for fractures in rails to be less infrequent in cold weather than in warm weather. Probably, therefore, the fracture would occur in cold weather and the rails would tend to separate as a result of their previously having been in tension. If the railway engineer filled the gap with a short length, either without any precautions or, as would probably more often be the case, after removing a short length on each side of the gap, to ensure that no metallurgical defect remained, then if the closure length was welded a greater buckling stress would develop in that length of rail when the hot weather returned than there had been prior to the breakage. On the other hand, in the event of the rail having broken in the summer some difficulty of extra tensile stress in the winter would arise. It was important, therefore, to arrange the repair so that the conditions which had obtained when the rails were laid were reproduced as closely as possible, and it might be that the simplest and most straightforward method to adopt would be to effect a temporary repair by one or more closures with fishplated joints and then to leave the rail until the atmospheric temperature later in the year approached the temperature that obtained when the rails were laid, so that they would butt themselves together naturally and could be welded without any undue change in the stresses to which they might be subjected.

As bearing on all those points, the question of the thermal expansion of rail material was of some importance. A disparity of about 6 per cent. was apparent between the values obtained by the two Authors. That difference was too large to be attributed to experimental inaccuracy in tests carried out with as much care as had obviously been exercised by the Authors, and he wondered whether any of the difference in expansion values could be accounted for by other than obvious differences in the materials, such as might have arisen from carbon-content. On referring to some published data on the thermal coefficient of expansion of steel, he had found that the most reliable recent results pointed to the conclusion that for quite a large range of carbon-content, namely from 0·4 to 0·8 per cent., combined with a variation in manganese-content from 0·1 to 0·9 per cent., published values for the expansion of steel ranged only from about 11·0 to 11·2 parts per million per 1° C. difference of temperature. That was closer to the L.M.S. result than to the Southern Railway result, but he hesitated to ascribe the disparity to experimental error. He would like to suggest, on the other hand, that it might be due to appreciable differences between the heat treatment of the two steel rails in question, remembering in that connexion that the values cited referred to steel, and not to manufactured rails. In view of the great advantage in installing long welded lengths which would accrue from a much smaller coefficient of thermal expansion than was at present possessed by rail steel, he would suggest that research might usefully be directed to possible methods of producing a steel rail in which that desirable result would be achieved either by modification of works treatment or by slight changes in composition, always with a minimum of expense and certainly with no detriment to strength, wearing quality, or weldability.

Mr. C. L. Heeler said that a very interesting comparison might be made between the two Papers on the subject of movement of sleepers at the ends of the test-lengths. Mr. Loach had found that the degree of movement during expansion and contraction increased progressively, owing to heaping up of the ballast between the sleepers. Mr. Campion's experiment showed that the effect of frequent heavy traffic was sufficient to shake down and consolidate the ballast to such an extent that not only was the movement of the sleepers limited but also it decreased progressively. It would be interesting to know the length of time required for the final consolidation of the ballast, after which time fairly uniform expansion and contraction might presumably be expected, subject, of course, to any very unusual temperature ranges.

Mr. Heeler wished to raise certain points of interest with regard to the results of Mr. Campion's experiments.

With regard to expansion from the unstressed state, Mr. Campion had said that once the whole rail was able to expand the load on it remained constant until the gaps closed. It would be expected that with increase in the movement of sleepers, with consequent compression of ballast, the resistance to movement would increase gradually. To carry that a step farther, at any one moment the expansion of the rail was resisted by the equal and opposite reaction of the sleepers, and it seemed feasible that the load, vibration, and impact due to the passage of traffic, with its usual effect on rails, chairs, and fastenings, might cause the rail to slip in the chairs, relieving to some extent the compression. That slipping would be expected to occur at the limits of expansion and contraction and might partly account for the complication of stress-distribution during the transition periods. Mr. Heeler would like to know whether Mr. Campion had used any direct method of measuring the movement of sleepers, or whether it was merely deduced from the movement of reference-points on the rails. Were any reference-marks used on rails and chairs in order to ascertain whether slip had occurred ?

He had observed that $11\frac{1}{2}$-inch fishplates were used on the test-length, and he would like to ask whether that was the standard size of short two-bolt fishplate used on the Southern Railway. On the London, Midland and Scottish Railway a short fishplate was 9 inches in length.

Rails up to 120 feet in length had been laid in several localities on the London, Midland and Scottish system. For instance, near Boxmoor a mile of 120-foot rails had been laid with $\frac{3}{8}$-inch expansion gaps at normal temperature and with fifty-two sleepers per length. That was on the basis of twenty-six sleepers per 60-foot length. Those rails were rolled at 120 feet, and were not welded. Welded rails of 120 feet and 117 feet, made up of 60-foot and 39-foot rails respectively, had been laid, particularly at water-troughs, in widely separated localities in Westmorland, on the North Wales coast, near Manchester, and in Leicestershire. No detailed investigations had been carried out with regard to them, but he did not think that any trouble had yet been experienced.

Mr. E. T. Hutt said it seemed to him that both Authors, although nominally dealing with the question of long rails and welding and so on, were really at grips, as Mr. Carpmael had pointed out in his letter to the Chairman, with the oldest problem of the railway engineer, namely, the problem of the rail-joint and its expansion-gap. After all, a weld was only a special kind of joint and a long rail was just a way of avoiding joints.

The railways of Great Britain included more than 10 million rail-joints, and the expansion allowances were equivalent in the aggregate at normal temperatures to about 20 miles of track without rails. He thought that those were rather surprising facts, and they showed the great importance of the subject which the Authors had been investigating. It was deplorable that the war had put a stop to both of the inquiries reported on by the Authors. People would be very surprised if they heard that the war had put a stop to research in aviation. Were the railways really so much less important to the war effort? He thought it would be well if some way could be sought and found to carry on during the war such investigations as those described by the Authors.

He considered that railway engineers ought not to rely on friction between the underside of the sleeper and the ballast, a factor which was mentioned as contributing to prevention of lateral movement. It was a recognized fact that immediately in front of an advancing load the track usually heaved up in the form of a wave, and that at that moment it was entirely deprived of any restraining forces on its under-side and was dependent solely on the resistance of the ballast at the sleeper-ends. Curves could sometimes be seen where a ganger, in an effort to maintain his cant—and perhaps get a little surreptitious lift, as gangers liked to do—had shovelled most of the ballast, which ought to have formed the shoulder, under the sleepers as tamping material, thus depriving them of all that support. Therefore, if compressive stresses developed in the rails in the curve, the rails were at liberty to buckle sideways at the instant when they were immediately in advance of the approaching traffic. There was no need for him to enlarge upon the evil consequences.

In Mr. Campion's Paper reference was made to tests on the strength of the fishplated joints. Mr. Hutt presumed that the joint failed from shearing the bolts, because he had noticed that the load withstood by the four-bolt fish-joint was about twice that withstood by the two-bolt fish-joint. That seemed to indicate not that an effort should be made to reduce the tension put on the joint but that a stronger joint should be made. After all, fish-bolts had remained at 15/16-inch diameter for at least 60 years, and probably for a good deal longer. Was not it time that something a little larger was tried, such as a fish-bolt of $1\frac{1}{4}$-inch diameter? Such bolts were used in rails on the Great Western Railway as crossing bolts. They had 78 per cent. more cross-sectional area than had bolts of 15/16-inch diameter, and that would go a long way towards giving the extra strength which was required. It was true that in certain severe conditions a tendency existed towards rail-end fracture of the running-on rail through the 15/16-inch diameter fish-bolt hole, and perhaps that should not be made $1\frac{1}{4}$ inch. Therefore he would suggest a three-bolt fishplate with one $1\frac{1}{4}$-inch diameter bolt on the running-off rail-end, providing a very short cantilever, and two 15/16-inch diameter bolts on the running-on rail-end, as at present, providing a springy cushion to receive the advancing wheels. That joint ought to retain all the advantages claimed for the two-bolt joint, and at the same time provide the extra tensile strength which was apparently required to cope with the vagaries of long rails.

It was often said that the right place for long rails was in tunnels, where the temperature was very steady. He did not know what evidence existed in support of that, but Mr. A. S. Quartermaine, M. Inst. C.E., had started a very instructive and inexpensive research on the question of tunnel temperatures, and in two of the longer tunnels on the Great Western Railway the air temperature had been observed daily for the past four or five months. With Mr. Quartermaine's permission, he would give the overall figures.

In Box tunnel, which was 3,212 yards in length, the lowest temperature (45° F.) was registered in May and the highest (72° F.) in June. In Sapperton tunnel, which was 1,860 yards long, the lowest temperature (41° F.) occurred in September and the highest (77° F.) in June. As might have been expected, the longer tunnel showed the smaller range of temperature. The overall range, in the case of Box tunnel, was 27° F. (15° C.), whilst in the Sapperton tunnel it was 36° F. (20° C.). Those results were not of very great value yet, since they covered only the summer months, but when the investigation had been completed the full results should be of considerable interest.

With regard to Mr. Taylor Thompson's suggestion that a joint should be deliberately designed not to provide for expansion but to eliminate it, such a joint—the butt-jointed fishplate—was already in use on the Great Western Railway and, he believed, on some other railways. That was an ordinary fishplate, either two-bolt or four-bolt, with the centre holes deliberately closed up to such an extent that when rails were butted tight it was just possible to drive in the fish-bolts and the rails were held tightly. Those fishplates had not been used for the purpose of producing a long length of expansionless rails, but merely to provide improved joints at places where it was desired to reduce hammering, such as on bridges and viaducts. They offered possibilities, however, for carrying out research without the expense of welding, and perhaps something would be done on those lines some day.

Mr. C. E. Dunton observed that he was sorry he could not furnish any technical data. By malice of the King's enemies all the statistics which the London Passenger Transport Board had acquired had been destroyed. He thought, however, that experiments carried out not under service conditions were apt to be misleading, and he would like to make a suggestion to Mr. Loach which possibly could be carried out better on the London, Midland and Scottish railway than on the railways of the London Passenger Transport Board, where trains ran so close together. A differential expansion rail might be mounted with a micrometer at each end, and the stresses on the test-length of rail surveyed through that measuring differential rail, in between trains on a length which was actually in service. It would be possible to place the differential expansion rail on the top of a running rail between trains, rollers being interposed between the two, and any points in the test-length could be surveyed and the stresses determined. That could be done not only with long welded lengths, but also with lengths of track where the rails were 60 feet long.

He thought that the results would be very instructive, because he believed that most railway engineers would agree that the expansion allowances which should exist in well-maintained track did not, in fact, always obtain. He had no doubt that quite large compressive stresses would be determined in country track composed of 60-foot rails. Indeed, the experience in connexion with buckling usually was that it was most troublesome where the rails were short. That appeared to be due to the intrinsic weakness of the fishplated joint, and therefore he thought it was correct to say that as the length of the rails was increased so the number of cases of buckling was reduced.

The London Passenger Transport Board had undertaken some capital expenditure to weld rails, and before the war had made it standard practice to weld rails which were laid in re-laying or re-railing works. The engineer-in-chief, Mr. V. A. M. Robertson, M. Inst. C.E., and the assistant chief engineer, Mr. J. H. Condy, M. Inst. C.E., had been largely instrumental in inducing the Board to undertake that scheme. The total length of welded track of the London Passenger Transport Board, installed between 1936 and the outbreak of war, was $20\frac{1}{4}$ miles, $14\frac{1}{4}$ miles of which was in tubes, partly because it was desired to reduce the noise in the tubes and partly because rail-life was shorter in the tubes and therefore the track fell due for re-railing more often than did the ballasted track. He had no doubt that those lengths would have been very considerably increased by now had it not been for the war. On the outbreak of war it was decided to discontinue the installation of welded rails, and the re-laying and re-railing programmes had been reduced so much that no great advantage would have been gained by continuing the installation.

Of the lengths of rails installed, approximately 15 miles—about three-quarters of the total mileage—consisted of 300-foot welded rails; the remainder comprised 240-foot, 180-foot, and 120-foot rails. No trouble had been experienced with those welded lengths. The conditions where they were installed varied considerably; many of the welded lengths were fully exposed to the sun, on straights and on curves, whilst some were on tracks where the running speed might be between 60 and 70 miles per hour with electric stock.

He thought it was important to mention that in actual running the trains, particularly with electric traction, had a nosing tendency which would accentuate any inherent tendency to buckling. Therefore the straight lengths where it was known that the trains habitually rolled, however well the top was maintained, were watched with particular interest, although he could not say that any disquiet had been experienced on those lengths. It was true that under the very hottest conditions the joints were butted. The joints used on those long lengths were Brogden joints, and, whatever might be their advantages or disadvantages from other points of view, which was a question he did not propose to discuss, they were undoubtedly strong laterally. It appeared that if long rails were to be adopted it was necessary to look to the joints, and the joints at the ends of those long rails must be good joints.

The question of repair had been raised and terrible potentialities had been suggested by Mr. Johansen. As an analogy, he had once seen a brick wall which had expanded and cracked when it cooled, and a bricklayer had pointed it each winter and in the following year it expanded and cracked again. Gradually it would have walked quite a long way. He did not think that railway engineers would be so unintelligent in repairing a broken welded rail. The procedure adopted by the London Passenger Transport Board was to keep short rails handy and to burn out a length and introduce a temporary closure, which was followed by the reinstatement of a properly welded rail. Site welding of the rails had not been found at all satisfactory. Thermit welding had been tried, but it had been very unsatisfactory, and from his experience he could not recommend it for railway practice. On the other hand, the flash butt system of welding had been very satisfactory, and therefore he thought that when the welded rails in the permanent way had to be repaired the welding should be done by the flash butt method, which meant that a new welded rail had to be sent out.

The London Passenger Transport Board had no cranes on the trains for unloading the rails. In the open a very crude method was used. The end of the rail was chained and then the train was taken away; that method was very quick, and it did not cripple the rail. He would not be prepared to adopt it with short rails without special equipment on the truck, but long rails, which had all the properties of a ribbon, ran off very well. The rail was installed by mechanical means, by what were known as "iron men," and the rail would be picked up by very few men and run into the chairs mechanically, so that it was not necessary to have large numbers of gangs all the way along the 300-foot rail to handle it. In the tunnels the rail could be similarly pulled off over the back of the train and placed in the chairs with the "iron men", or, alternatively, it could be lifted by means of tackle fixed in the top of the tunnel and dropped straight down into its position. The Board had rail-pulling machines for regulating the expansion of rails which could deal with 300-foot rails without difficulty.

Some of the methods used by the Board were then illustrated by lantern slides.

Mr. Arthur Dean said that a point which had impressed him was that it was possible to obtain rails which would be fully restrained. The stress in them would follow the temperature, and, as Mr. Campion had pointed out, the range might be about 10 tons per square inch. Wherever rails were anchored one would get plus 6 or minus 4 or something of that order. Would railway engineers put load on their bridges, to give an extra 4 tons stress, and then add live load? They did not know what live load stresses they had in the rails, but they knew they had a margin. The question then arose whether that margin should be used to take up the stresses which would arise from welded long rails, or whether it should be kept for future developments after the war, including faster and heavier trains, with the present type of track retained. He thought that was a pertinent question, irrespective of the capacity of the track to withstand buckling.

It was unfortunate that during the stages of high stress on the Medbourne experimental track no attempt had been made to fettle it in order to ascertain what actually happened when a jack was used during shovel packing or lifting.

Some experiments had been made on the Southern Railway, and it had been found that the resistance of a sleeper to sliding was not so much from the ballast shoulder as from the friction on the bottom ; it was about 130 lb. per sleeper for the ballast shoulder and three, four, or five times that from friction on the bottom.

He wondered what would happen when the track was fettled. When 1 mile of rail was fully restrained it had to be fettled, and the temperature stress did not simply occur around the peak of high temperature, as in short rail track ; it occurred throughout the whole life of the rail. He felt that the whole problem of the use of long rails was dependent upon much greater knowledge of the actual stresses in rails than was available at present.

He thought that the experiments described by Mr. Campion showed that the 180-foot rails, under the conditions that prevailed at Hildenborough, fettled and carrying fast traffic, were stable. The gaps expanded to $\frac{7}{8}$-inch in winter and closed up tight in summer. Whenever very long rails were put in they had to stop somewhere—at a curve, or at points and crossings—and expansion had to be provided for. That was a very big problem, and it appeared that something in the way of a different joint should be considered.

Mr. G. F. Kent said that both Papers were very interesting, but he thought that engineers who were engaged on ordinary maintenance work should remember that neither length was typical of common everyday traffic. Mr. Campion's length, which had clay underneath it, was typical of very modern well-ballasted track, whilst Mr. Loach's length was disused when about 35 years old. He would like to ask Mr. Campion whether the very big shoulder of ballast and the fact that there was ballast above sleeper-level were due to the test or whether they had anything to do with the clay underneath.

The point that had impressed him most was the difference in the resistance of the sleepers. In tests of the kind in question a difference would be expected ; in fact, if it were not found one would be very suspicious of the way in which the tests had been carried out. He would like to ask both Authors to explain why the results were so different. One obvious reason, he thought, was to be found in the weight of the track. In Mr. Campion's case the track weighed about 190 lb. per foot and had very heavy traffic upon it, whereas Mr. Loach's track probably did not weigh more than 150 lb. per foot and, being disused, was probably on a rather smooth bed. Mr. Kent thought that nothing had been done except topping up the ballast before the test started. Another reason for the wide difference in the resistance of the sleepers might be that in old track the bearing area between the rail and the chair was very small and the key area also was probably small in comparison with that on modern track, and he wondered whether there was any movement between rail and chair.

Both Authors had used the expression " there was no apparent tendency to buckle." He would like to ask how they judged that. His experience was that the track was there until it had buckled, and he wondered what would happen if the railways started to introduce long welded lengths. In an old volume of the " Minutes of Proceedings " of The Institution an Author had concluded his Paper on a similar test [1] with a warning to be very careful in repairing the track, because it was like a coiled spring.

Mr. Loach had referred briefly to a live load test which he made, and Mr. Kent wondered why Mr. Loach had not obtained a higher speed than 42 miles per hour, and also why the engine had nosed so badly at the lower speed.

Mr. W. A. Willox said he had been reminded of the sudden opening of a sluice gate when, at a recent meeting of The Institution [2] Mr. W. A. Stanier, the chief mechanical engineer of the London, Midland and Scottish Railway, had poured forth his heart on the subject of rail joints, asking how much longer mechanical engineers would have to wait before civil engineers discovered how to avoid that evil. Obviously the rail joint was the main thing which prevented perfection in the permanent way. Almost perfect alignment could be obtained, but a perfect level could not be obtained with the standard joint, owing to its up-and-down movement. He had once heard a statistician calculate that every train which crossed the continent of America climbed 4,000 feet in the journey. The problem to be solved was how to obviate the up-and-down movement of the standard rail joint.

The two Papers would surely have whetted the appetite of members for further research, and at least they had given some more or less exact information on a subject on which engineers had hitherto been groping in the dark. But the remark of Mr. Stanier to which he had referred had prompted the suggestion that further research, necessary as it was, should be done jointly by mechanical and so-called civil engineers, because it was desirable to know not only the stresses in the rails and the extent to which the sleepers might be relied upon to absorb the tendency to buckle

[1] A. J. Moxham, " Experiments on the Expansion of Continuous Rails ", *American Street Railway Journal*, 1892, p. 650. (Abstract in Min. Proc. Inst. C.E., vol. cxi (1892–93), part I, p. 446.)

[2] Discussion on " Permanent Way Tests and Practice on the L.M.S. Railway ", Journal Inst. C.E., vol. 18 (1941–42), p. 146 (April 1942).

when the rails were restrained altogether, but also, from the chief mechanical engineers, what sort of side thrusts the engines and rolling stock were likely to impose upon the track. Had the track to withstand thrusts of 13 tons, as experience in France not very many years ago had indicated, with certain badly balanced locomotives, or would mechanical engineers balance their locomotives so well that no side thrusts would be developed ?

Until more exact information was available in regard to the conditions in the track, it was interesting to observe the results that had been achieved by the Delaware and Hudson Railroad, to which Mr. Loach had referred in mentioning a length of about 7,000 feet welded continuously. On that line a deliberate policy was adopted of eliminating rail joints except where necessary for track circuit insulation and junctions. No trouble had been experienced, but Mr. Willox doubted whether the engineers knew to what factor of safety they were working. They had, however, taken the precautions of having a very well ballasted and drained track, with as nearly perfect level and alignment as possible, and eliminating movement between the rail and the sleeper by a special type of fixing. That was important because, as Mr. Johansen had said, if a fracture occurred it was desirable that the rails should not jump apart, and they could not do so if they could not move relatively to the sleepers. He believed that when a fracture occurred on the Delaware and Hudson Railroad no ill effects were experienced, because the rails could not move apart. If railway engineers were going to have completely restrained rails, with compressive and tensile stresses, whether they were known or unknown, they should aim at having the track built up in the form of a girder so as to give maximum lateral strength. It should be possible thus to give bull-headed track almost as good lateral strength as flat-bottomed track.

With regard to the cost of welding and making the level as perfect as the alignment could be made, the conditions which would exist after the war were largely a matter of speculation. In fact, he thought that experts who were engaged on post-war planning had less justification for planning the future than persons permanently in residence on the slope of a volcano, because nobody could tell what the conditions were going to be. It was not inconceivable, for example, that after the war labour costs might be so high, depending on the degree of slaughter and destruction that took place between the present time and the end of the war, that very heavy expenditure in eliminating maintenance charges would be amply justified. Therefore he suggested that engineers should use every possible opportunity of undertaking research and that it should be comprehensive research shared by all departments concerned with both the provision and the use of the track.

*** **Mr. R. H. Cunningham** observed that although buckling did not

*** This and the following contributions were submitted in writing, occur in the experimental track at Hildenborough, the experiment described by Mr. Campion did not prove that it would not occur with 180-foot rails in otherwise normal track under normal conditions, and, therefore, it was to be regretted that those conditions did not apply.

Mr. Campion had stated that in cold weather the expansion-gap was undesirably large, and it appeared that a special fishplate was introduced 12 months after the commencement of the experiment. What was the largest gap measured ?

Mr. T. M. Herbert considered that both Authors were fortunate in having had the opportunity of experimenting on the full scale, and that they should be congratulated on having obtained results of considerable practical value. In their measurements of the coefficient of expansion a small discrepancy was evident, which might be accounted for by some difference in the chemical composition or heat-treatment of the rails used in the experiments. It would be useful if such data could be given, as it was not generally known whether such variations in composition, etc., were significant.

It was rather unfortunate that in both experiments the conditions had been a little abnormal. In the case of Mr. Loach's work, the traffic carried by the test-length was insufficient to elucidate any possible effects on stability that might be caused by more normal usage, whilst the blanketing of the formation at Hildenborough introduced a feature into Mr. Campion's tests which might or might not have been significant. Nevertheless, the generally favourable results of two independent studies of the problem had a cumulative value in creating confidence in the general principle of the welding of long lengths of track if a good case could be made for the general advantages, to both the Civil and the Mechanical Engineer, of eliminating a proportion of the rail joints. Unless, however, the practice became very general, and the lengths welded were really long, it seemed doubtful whether a material contribution would be made to the comfort of travel by rail.

Mr. J. W. Melville observed that Mr. Campion had very clearly tabulated the factors favourable to distortion of track ; but had he any evidence to prove that the absence of lubricant on the fishing surface was one of them, or even that an oiled joint when tightly bolted moved more freely than a dry one ? It was interesting that he was not aware of distortion due solely to absence of gaps, and that fact might be considered in relation to the query made.

In considering the relative lateral strength of 131-lb. flat-bottom track and bull-head track, should not some allowance be made for the British fastenings and heavy chairs ? In the Hildenborough experiment the levelling-up of cess and support given by brick rubble drains formed an excellent safeguard against buckling and Mr. Melville would be surprised if any distortion ever took place on the length.

The money spent in making good the formation and drains would probably be a very good investment, and one which would sooner or later have to be widely extended by British railways as a very important factor in improvement of track and reduction of maintenance costs.

The outstanding result of the investigation made by Mr. Campion was the clear indication that the rail might have been continuously welded over the whole length of 1,100 yards; but in a question of that kind one had to creep before one walked.

The track which had been referred to by Mr. W. K. Wallace, M. Inst. C.E., in his Paper[1], as being free from distortion under a compressive stress of 6·6 tons per square inch was old, worn track of light weight, and not at all in the same category as the Hildenborough track.

The results obtained should encourage Mr. Campion to continue his experiments and ultimately produce the almost jointless track.

Mr. Melville had been associated in a kind of "Contractor" capacity with the experiment on the Medbourne loop, and he welcomed Mr. Loach's very able and clear description. His explanation of the principle involved was necessary, as although it seemed so obvious when read, Mr. Melville found, when talking about long welded lengths of track, that the question "What will you do about expansion?" was almost invariably propounded.

Mr. Loach's experiment, and those made under more severe weather and traffic conditions in the United States, indicated that the expansion and contraction could be taken care of. The track in the Medbourne experiment was old and had been out of maintenance for many years, and the only work done, apart from welding joints and providing new keys, was to renew a few chairs and top up ballast. A favourable feature was a shoulder of ballast on the cess side slightly wider than usual—in fact, almost as wide as it should be, but beyond that the track was normal. On what was formerly the 6-foot side, there was part of the old ballast bed, so that conditions in that respect were about normal; that was, the cess side had less lateral support than the 6-foot side.

There was, he believed, no discernible movement of rails in chairs, indicating the efficiency of the steel keys. In the winter test two slight cracks appeared across the gas-welded rail heads, one on either rail, but, as Mr. Loach had observed, they were not expected to withstand any great tensile stresses, provision for which was made by the tapered fillet on baseplates. It would be interesting to know the extent of the change in length between the time the rails were unkeyed on 9 April and re-keyed on 13 April 1938, and how it compared with the calculated change.

Rail buckling was generally associated with insufficient ballast on one side or the other of the affected track, and with the presence of creep or creep stresses. Frequently, also, it took place suddenly when there was a *rapid* rise of temperature. No creep stresses were present, and it was

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[1] "Permanent Way Tests and Practice on the London, Midland and Scottish Railway", Journal Inst. C.E., vol. 17 (1941–42), p. 115 (April 1942).

quite probable, bearing in mind the age and weakness of the track, that had the line been in regular use some slight buckling would have been found.

However, the very valuable work done by Mr. Loach pointed to the possibility of long lengths of continuously welded track being laid in the near future and to the creation of some new problems for the maintenance engineer.

Mr. F. W. Slade considered that the rising cost of labour would tend to force engineers to eliminate as many rail joints as possible. A long length of jointless track should be conducive to sweeter running, which should be a contributory factor to decreased maintenance costs, although that condition would probably apply only to well-drained and ballasted track.

The stresses set up in those lengths were mostly of unknown magnitude, depending on various conditions and factors. Those stresses should be investigated and made known. Various methods had been used, and further methods were now suggested in the use of two suitably shaped metal strips welded to the selected parts of the test-rail and holding a quartz crystal, so that the passage of the wheel would cause the electrical voltage-changes to be very accurately inscribed by a cathode-ray oscillograph. Another accurate method was the use of the X-ray " gun." Within the limits of elasticity, the stresses could be deduced within fine limits.

From observations it was known that, for a long length of continuous track, thermal expansion and contraction of the rail was free only at the ends, whilst the middle of the length was constrained. Flat-bottom rail had certain advantages over bull-head rail, in that it had greater natural stiffness and lent itself to the use of heavier and stronger sections (up to 156 lb. per yard in America), with a consequent stiffer fishplate, and of the high-tensile elastic spike with a cast-iron baseplate (where cast iron had a high recoverable cash value). As there was a danger of early buckling in bull-head track, it might be desirable to concentrate on the further use of flat-bottom rails in British tracks.

Mr. Campion, in reply, said that the Boutet welds had given quite satisfactory service. One or two failures had occurred at the beginning, when the heat treatment was applied, but apart from that no trouble had been experienced, and the Boutet weld had been found to be one of the best of the thermit welds. He might say, however, that it could not be compared with the flash butt weld as a first-class job.

In reply to Mr. Kirkpatrick, the more 180-foot rails that came on to a line under his control the better he would be pleased. It was true that a special new bottom had been put in the test length, but the place was a particularly bad one, where something would have had to be done sooner or later. No difficulty had been experienced in handling the 180-foot rail, and so far no mishap had occurred. The 180-foot rail seemed to be quite as easy to handle as the 60-foot rail.

With regard to Mr. Johansen's question as to the difference in the coefficient of expansion as determined by Mr. Loach and himself, he had pointed out that the tests at Hildenborough were extremely crude and had been carried out by people who were not research experts but merely divisional staff, doing the best they could to solve the problem.

Mr. Johansen's suggestion that the similarity of temperature-rise needed to cause movement of all sleepers at Medbourne and Hildenborough respectively was purely coincidental, was correct. The rise in temperature necessary to move a given number of sleepers depended upon the resistance of the sleepers to movement and the cross-sectional area of the rails.

If the fracture of a very long rail took place, permanent repairs would need to be effected under technical guidance; otherwise the undesirable variation in rail stress visualized by Mr. Johansen might arise. It was possible that the disparity in the coefficient of expansion of rails as determined at Medbourne and Hildenborough was due, more to the less satisfactory means which were available at Hildenborough for determining the temperature of the free rail than to differences in the composition of the rails.

The $11\frac{1}{2}$-inch fishplate was the standard on the Southern Railway. The increased length provided protection for the traction bonds, which otherwise would be exposed.

Mr. Hutt had pointed out that the railways were carrying on with 20 miles less track than they should have, and Mr. Campion did not know what the chief engineers of the railway companies would say if some means were discovered by which they could put the 20 miles of missing rails in position during the war.

Mr. Hutt was correct in presuming that the failure of the joints in tension was brought about by shearing the fishbolts.

In reply to Mr. Heeler's inquiry as to the movement of sleepers, it was found that the sleepers at Hildenborough moved with the rails except for one or two cases in which there was slight movement of the rail longitudinally in the chair; but the amount of this slip was small compared with the movement of the rail at that point.

He thought that Mr. Kent had himself answered his question as to the difference in the sleepers. Presumably the sleepers which Mr. Loach had used were second-hand and probably the surface had been worn off them, whereas those put in at Hildenborough were new sleepers and under traffic all the time.

The big shoulder of ballast to which Mr. Kent had referred, had been provided in all cases where long rails had been laid, and was not confined to those instances where there was a clay formation.

The largest gap measured by Mr. Campion at Hildenborough was about $\frac{3}{4}$ inch. Mr. Cunningham had referred to the special fishplate introduced at Hildenborough 12 months after the commencement of the experiment. Those special joints had not been installed on the test-length, since their fitting necessitated machining the head and base of the rails on the outer side of the rail end, but they had been employed where long rails had been laid subsequently elsewhere.

It was not suggested that the blanketed formation at Hildenborough provided abnormally favourable conditions; its effect was rather to increase the stability of a soft clay road-bed to a degree comparable with that obtainable with a formation consisting of hard, well-drained gravel, chalk, or rock.

Mr. Campion had no data as to the relative friction between dry and lubricated fishing surfaces, nor had he definite proof that the absence of lubricant on the fishing surfaces caused heat distortion. Mr. Melville would perhaps agree that it was usually difficult definitely to assign the cause. Frequently the investigator could not arrive at the site before the track had been restored to correct alignment and he could then do little more than note the presence or absence of various predisposing factors and draw his conclusions from them. Mr. Campion agreed that rail buckling was frequently associated with insufficient ballast, or ballast of poor quality, and that it often took place with a rapid rise of temperature, particularly if that occurred in spring or early summer. He had no information as to the magnitude or, indeed, the existence, of appreciable stress set up in rails by creep.

Mr. Loach, in reply, said that several speakers had referred to the problem of joints. He thought that if welded rails were to be adopted in long lengths the question of joints would have to be reconsidered: the present type of joint was not suitable for long welded lengths. The fishplate should serve only as a link to maintain the two rail-ends in proper alignment, and if fishplates were required to do more than that trouble was being invited. Other means should be used for tying the rail-ends together. Butting together the rail-ends as some speakers had suggested was a poor substitute for welded track. Because of the discontinuity of the rail section, the joint would not be noiseless and it would have to be maintained. Even used as an inexpensive method of making a trial it was probable that the fishbolts would become distorted and so not retain the rail-ends butted together; in addition to which the joints introduced a number of places in the length which were weak laterally.

Mr. Barton had asked for practical proof of the suitability of long rails. He would refer Mr. Barton to an article about the Delaware and Hudson Railroad published in the *Railway Gazette* of 23 January 1942.[1] Even in Great Britain there was the length of welded rail at York, and he had been very pleased to learn from Mr. Taylor Thompson that it had given satisfactory service.

Some speakers had inferred that work done on the 2,213-ft. length was an experiment without practical application, but all things began by being

[1] "Seven Years of Continuous Welded Rail," *Railway Gazette*, vol. 76, p. 120; 23 Jan. 1942.

experimental, and if experiments were not made nothing would ever be found out. Experiments had to be made; they formed the first stage towards anything becoming usual instead of exceptional. He did not claim that his Paper was more than a description of an experiment, to show that at least a start had been made. He suggested that the next stage should be to put into service a length of welded track, preferably between other roads, so that it was well boxed in, such as occurred where there were four roads, and preferably the experiment should be carried out on some of the slow lines, so that if anything went wrong it would not be so detrimental to traffic. That would be a start under traffic conditions, and if nothing went awry the experiment could be continued elsewhere.

Surely scope existed for long welded lengths in tunnels, where the changes of temperature were smaller than in the open and it was gratifying to hear Mr. Hutt give some temperature figures showing that an interest in that aspect was being taken. It was well known that joints in tunnels were a bugbear. They were not so easy to maintain as in the open, and often the expense of maintenance was higher. It would be very advantageous if there were no joints at all inside tunnels, as, apart from the quieter running, maintenance of them would disappear.

Mr. Kirkpatrick had mentioned trouble with lengths of rail as short as 120 feet. If he might make the suggestion to Mr. Kirkpatrick, he thought that the trouble experienced was due to his use of wooden keys. Analyses of distortions of track due to heat had been made on the London, Midland and Scottish Railway for the past two or three years, and it was interesting to find that the ideas held by some people were not borne out by a scientific analysis. It had been said that if the rail lay in a north–south direction it was much more prone to buckling, and other ideas were contended. When the twenty-two cases of distortion of track which had occurred during the past few years were analysed the one outstanding feature was that in eighteen cases wooden keys had been used. In the other four cases the reports did not state whether the keys had been of wood or of steel. He thought that if a satisfactory steel key were used, so that the rail did not slip through the chair, many of the troubles of expansion would disappear.

In reply to Mr. Taylor Thompson's question as to why variations of gauge were measured, the primary motive was that the measurements served as a very satisfactory check-up on lateral movement. Minor variations of gauge occurred; in wet weather the gauge was found to be slightly different owing to the condition of the wood. The sleepers had been laid in the branch line in question in 1903, and were fairly well rotted away.

One or two speakers had referred to the question of ballast and had asked why Mr. Campion had obtained results so different from those which he had himself obtained on the long welded length. In the case of his own experiment the ballast was poor. It had been in the track for many years and some of it was gravel. Pieces of gravel had round corners so that they did not key into one another as well as did pieces of stone ballast. Another point was that in his experiment the sleepers were surrounded by ballast on the long welded length to a depth of only about $4\frac{1}{2}$ inches, whereas in the experiment at Hildenborough the corresponding depth was 7 inches or 8 inches, and that obviously made a tremendous difference. The question of the behaviour and resistance of ballast was a very important one, about which he thought very little was known. It was a subject under investigation in the London, Midland and Scottish Railway's research department, and some interesting facts would probably come to light eventually. Already there were indications that the ballast contributed more to the resistance to lateral movements than other factors; so that, although not realized until recently, it would appear that the lateral stiffness of the rails themselves was not the only important feature controlling distortion in the normal British track. It was suggested by Mr. Willox that engineers did not know with what factor of safety they were working. On the 2,213-foot length at Medbourne, Mr. Loach was unable to say what factor of safety existed, but he was able to state that it could have been four times greater than it was during the experiment. Had he used ballast in the same way as Mr. Campion and obtained a resistance of 1 ton per sleeper against longitudinal movement with a presumed corresponding increase in lateral resistance, and had he keyed up at a medium instead of an extreme temperature, such conditions would have prevailed.

In reply to Mr. Kent, the conclusion that there was no tendency to buckle was deduced by the eye because the rails appeared to be normal.

Mr. Kent had also asked why no higher speed than 42 miles an hour was obtained in the trial of the welded length under traffic. The reason was that the run which could be taken with the locomotive was limited. The engine was an L.M.S. two-cylinder 2–6–0 engine, in which the cylinders were well inclined, and at one particular speed a "nosing" action was obtained—nice for the purpose of the test. It certainly seemed to have had no detrimental effect upon the track.

Undoubtedly, there was a tremendous field for research in finding the features of a rail steel which had a bearing on its linear coefficient of expansion and, in response to Mr. Herbert's suggestion, the percentage composition of the rails used at Medbourne was C 0·38, Si 0·018, Mn 0·80, S 0·097, P 0·058. The rails were rolled by the Bolton Iron and Steel Company in 1903, and it was probable that the steel was manufactured by the acid Bessemer process.

Mr. Melville had asked about the changes of length of 2,213-foot rails when unkeyed during April 1938. They had not been recorded, but the differences in length of the rails when being keyed up on 3 September, 1937, and when being keyed up on 13 April 1938, were 8·28 inches (left-hand rail) and 8·26 inches (right-hand rail), the temperature-difference being 34·7° C. (62·5° F.). If completely free, the change in length would have been 10·62 inches; those figures suggested that the rails were still lightly restrained as they rested in the chairs.

With regard to Mr. Slade's suggestion that stresses should be measured in rails during the passage of wheels, some work of that sort had been done in America and published. Similar work had been done in Great Britain, but Mr. Loach was not aware that it had yet been published. The stresses due to temperature-changes in a long welded length were, of course, additional to those imposed by passing loads.

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