Discussion.

Professor Inglis.

Professor C. E. INGLIS said that he welcomed the opportunity of offering his congratulations to the Authors on the excellence of their Papers, and also on the parts they had played in the successful completion of an engineering undertaking of the first order of magnitude.

The Papers were complementary one to the other; one presented the somewhat detached point of view of the resident engineer directing the work, and the other presented the more mundane point of view of the contractor who, in the course of carrying out the work, inevitably found himself at variance with some of the conditions and regulations contained in the contract. The description of the work from two different aspects gave a picture which stood out in strong relief. Both Papers gave considerable prominence to well-sinking, where that dual representation was of great value. Mr. Handman was perhaps a little over-optimistic in assuming that all his readers would know the exact significance of the term "sinking-effort." Professor Inglis himself had to confess that it had not been clear to him until he reached p. 399, where Mr. Howorth had the kindness to define it. Only then did he realize the true meaning of the graphs in *Fig. 16* (p. 342) of Mr. Handman's Paper. When he had first seen those graphs he had hoped that they would give some information about sinking-resistances, and about their connection with the nature of the material through which the wells were sunk. By close examination, however, he found that, provided the section of the wells and the weight of the concrete were known, the graphs only gave the height of the well which was protruding above ground-level at the various stages of sinking. Mr. Handman's remarks on p. 342 regarding the average sinking-effort seemed of doubtful validity, as the graph only showed that the final sinking effort was 4·7 cwts., and did not give the average sinking-effort. The determination of skin-friction was a question of outstanding importance, but, as Mr. Howorth pointed out on p. 400, there was unfortunately only one case in which a definite measurement was obtainable. That was in the case of well No. 27, which appeared from the drawings to have been sunk to a depth of about 100 feet; at one stage of sinking the cutting-edge of that well was entirely free, so that the well was merely supported by skin-friction and air-pressure. By reducing the air-pressure until the well began to sink an accurate estimate could be obtained of skin-friction, the value found being $4\frac{1}{4}$ cwts. per square foot. It would have been valuable if many more observations of that nature could have been obtained. The conditions for making them were ready to hand, but a contractor could hardly be expected to spend time and money on such researches without adequate encouragement. Professor Inglis.

Mr. Handman had stated that no major difficulties in sinking wells had been encountered, but it was evident from the Paper that the technique of well-sinking had not yet reached finality. The great resistance offered to sinking in clay, even when the clay was dredged to a depth of 10 to 17 feet below the cutting-edge, was very remarkable, and Mr. Howorth's explanation that the failure of the wall of clay to break down under those circumstances could be attributed to the arch-action arising from the comparatively small bore of the cylindrical excavation seemed to be a reasonable one. However, human ingenuity should be able to devise some grabbing mechanism whereby the excavation could be carried almost to the limit of the cutting-edge; also, in sinking through granular materials the amount of kentledge required might perhaps be greatly reduced or even eliminated if a rapidly-vibrating load were applied so as to substitute dynamic friction for static friction, and to prevent the well from "going to sleep."

The question of impact-allowances was always of interest. For the Zambezi bridge an impact-load of 71 tons per bearing was mentioned; did that mean that the total impact-allowance on girders was going to be the very high figure of 284 tons? What allowance had been made for wind-pressure, and had high wind-velocities been taken into account? It was to be hoped that the allowance was of a more enlightened character than the easy method of allowing 50 lbs. per square foot when the bridge was unloaded, and reducing it 30 lbs. per square foot when a train was on the bridge. In that method it appeared to be assumed that when a high wind was blowing the trains ceased to run; but that was not perhaps quite so ridiculous as it sounded, for if the wind-pressure were 50 lbs. per square foot, a train would simply lie down on its side!

The choice of the span had presumably received careful consideration, and he would like to know how the figure of $262\frac{1}{2}$ feet had been determined. There was a well-known and logical foundation for the belief that in a multiple-span bridge the maximum economy was achieved when the cost of the superstructure approximated to the cost of the foundations and piers. In designing the Zambezi bridge, where the sinking of the piers was rather an unknown quantity, the engineers might have been expected to play for safety by putting

Professor Inglis.

rather more money into the superstructure and rather less into the piers. Actually, the excess seemed to be in the other direction, as it was stated that the ratio of the cost of the superstructure to the cost of the piers was 0·92 for the main spans and 0·84 for the secondary spans. The obvious inference from those figures—though that inference might well be entirely wrong—was that the spans were rather too short for the maximum economy.

Mr. Codrington.

Mr. W. M. CODRINGTON remarked that as Chairman of the Central Africa Railway, which was responsible for the bridge, he had been in close touch with the work throughout. He would not attempt to discuss any of the technical points which had been so admirably set forth in the Papers, but he wished to draw attention to the question of health, to which Mr. Howorth alluded. The site chosen for the Zambezi bridge was an ideal place for the breeding of mosquitoes, and he trembled to think what would have happened had not very careful precautions been taken to guard against that danger. The advice given by the Ross Institute had been essentially of a simple nature, but it had entailed a considerable degree of discipline and care on the part of all those in the bridge zone, where there had been a very heterogeneous temporary population of some thousands of Europeans and natives drawn from many different territories. The discipline and organization needed to guard against the danger of disease would never have been achieved if the two men particularly concerned—the Authors of the Papers—had not had very high personal qualities, and he desired to take the present opportunity of paying his tribute to their success.

An unusual feature of the work was that the money for building the bridge had been advanced by the British Government to a private commercial company. The company had naturally had to rely on its technical advisers, but had felt very safe in that respect, having the support of two famous firms of engineers—Messrs. Rendel, Palmer and Tritton and Messrs. Livesey and Henderson. Sir Robert Gales had visited the site in the early days, and Mr. Codrington had accompanied Sir Brodie Henderson when he had visited the bridge just after construction had started. He wished to take the opportunity of paying a tribute to Sir Brodie Henderson, The Institution's very eminent Past-President. The Zambezi bridge was the child of the later years of his professional life, and on it he had lavished the fruits of a very varied experience gained in many parts of the world. Sir Brodie was the ideal consulting engineer, and to those on the Board of the Company who were his clients he had been much more than merely a technical expert. In him they had had a trusted counsellor and a wise friend to whom they had often appealed, and never in vain, for help and advice. The Zambezi bridge would perhaps be regarded as Sir Brodie's greatest achievement and, in that sense, his memorial. Mr. Codrington.

Mr. H. J. Nichols observed that the very great length of the bridge was notable, in view of the comparatively small discharge and maximum velocity of the river during high flood; that velocity was in fact so low that it had apparently been considered unnecessary to protect either abutment. Had the question of building training-works been considered? The effective length of the bridge might thus have been reduced to approximately the width of the wetted channel, or, at least, the viaduct and the secondary spans might have been eliminated, and the navigable channels might have been improved and perhaps stabilized. That method of approaching a bridge problem in such a river was almost standard practice in India. Mr. Nichols.

The span adopted seemed to be short in comparison with the size and depth of the piers; further, the spans were only designed for a single 3-foot 6-inch gauge track, so that the weight of steel per foot of girder would be quite small. It was stated on p. 367 that the ratios of the cost of the superstructure to that of the piers were 0·92 and 0·84 for the main and secondary spans respectively. With the help of the figures given in the Papers he had deduced a price of £42 per ton in place for the steelwork of the main spans, and £42 10*s*. per ton for that of the secondary spans. Those figures might not be exact, but they did appear to be unusually high, and were approximately double the usual price of steelwork erected in India. Difficulties of transport and other contingencies might have accounted for the relatively high price of the steel, but, in view of that price, it seemed strange that the viaduct at the end of the bridge, if it had to be built, had also been built of steel, instead of employing reinforced concrete. The cost of one of the major piers seemed to be about £15,000. That compared with a figure of about £14,000 for a somewhat similar pier sunk to a similar depth at the Nerbudda bridge in India, which carried two heavy tracks. As the pier costs were not dissimilar from those ruling in India, whereas the steel costs were about twice those in India, it was to be expected that the general profile of the Zambezi bridge would differ greatly from those of corresponding bridges recently erected in India.

Why had small horizontal sub-struts been adopted to support the main verticals of the main trusses? Those struts produced very unpleasant secondary stresses; the main verticals were not particularly long—about 37 feet between gussets—and it was possible that they could have done their job without support. The impact-loading allowed on the bridge seemed to be unusually high.

With regard to the use of 1 : 3 : 6 concrete for the bottom plugs, Mr. Nichols. which had been placed under water, it seemed that some of that cement would have been washed out during placing, and that the concrete might therefore be considerably weaker than 1 : 3 : 6 when placed. He desired to add to the Authors' praises with regard to the excellent dome attachment for well-sinking, designed by Mr. Fereday. He himself had also had occasion to use it, and had found it to be of the greatest value. The foundation-loads applied were of interest. The live load on the main piers was only 0·83 ton per square foot, or 11·4 per cent. of the continuous dead load, and he would like to ask whether, in such deeply-founded piers, it was necessary to take into account the live loading, provided it were within some agreed limit—perhaps 25 per cent. In testing so large a pier no one would accept a test if the test-load were left on for only a few minutes ; and, after all, a live load was usually of equally short application. The figure of 11·4 per cent. might be regarded as a measure of efficiency of the piers in doing their job—which was merely to carry a pay load—and it seemed to be rather a low one. Incidentally, if the live load were to be disregarded a pier would be designed solely from the point of view of convenience in sinking, and it did appear, as Professor Inglis had suggested, that pier design had not yet reached finality. While the pier was being sunk its weight was the engineer's best friend, but as soon as the pier was founded and plugged at the bottom it became his worst enemy. A circular cross-section was certainly the easiest to deal with in sinking, and he desired to ask whether the question of using a single 20-foot diameter well for each of the Zambezi bridge piers had been considered. If that had been done, the same diameter of dredging-well being adhered to, the sinking-effort would have been increased by 34 per cent. ; or if the sinking-effort had been reduced to its present value, then the final total live and dead load on the base of the pier would have been increased only from 8·17 to 8·2 tons per square foot.

Dealing with the track, he questioned whether the sleeper-spacing of 2 feet with 10-inch-wide sleepers was in fact sufficiently close to prevent a derailed wheel from going through. In India on metre-gauge track the corresponding dimension was 16 inches with an 8-inch sleeper, leaving an 8-inch clear space. The anti-creep plates shown in *Fig. 25* were practically identical with the four-key steel sleeper used in India ; from experience there it had been found that those four keys did not provide an absolute anchor, so that the rails did require some attention at fairly frequent intervals.

The practice of removing concrete test-specimens for special curing was open to criticism. It would appear that in order to be representative of the concrete in a pier they should remain on the pier and thus be cured under the same conditions as the pier itself.

For how long had the concrete in the piers been kept wet before being allowed to dry? Mr. Nichols.

He agreed with the opinion expressed on p. 363 regarding the removal of mill-scale, but it was an operation of considerable difficulty. Something might be done to loosen the scale during the last few passes through the rolling-mills. It had been his experience that the scale on steel rolled in England was slightly easier to remove than the scale on steel rolled in India, and there might be something in the manipulation of those last few passes.

Mr. Howorth was to be congratulated on having carried through such a great work in 3½ years. Those concerned had been fortunate in meeting with so much clean sand through which to sink, so that steady progress could be made throughout the major part of the work. From the progress-chart it would appear that seven piers had been sunk from floating sets, and that compressed-air sinking had been used in the case of five; when working from Pier No. 1, however, there appeared to be a considerable interval between the completion of the piers and the erection of the steelwork. Had that been due to the difficulty of arranging the arrival of the steelwork at the site just when wanted, whilst avoiding too great an accumulation of steelwork on ground which was subject to inundation?

It would appear that the difficulty in pitching pier No. 32 might have been overcome had the pier been water-borne and fitted with sea-cocks to enable the last few feet of sinking to have been done rapidly before any appreciable scour took place. That idea had been used in India, and had in all cases proved to be extremely effective.

It was interesting to read on p. 401 that some of the wells were inclined to "hang" when they were in sand a few inches above clay. Similar trouble had been met with in India, and on such occasions large lumps of sand conglomerate had been brought up. It appeared that there was a tendency for sand overlying clay to become consolidated in that manner, and it was possible that a similar cause of trouble had been present in the Zambezi well-sinking. Finally, he would like to question the practice of sinking piers to a depth at which they were considered to be immune from scour, and at the same time entering on the never-ending process of dumping rubble around them in order to prevent that scour. It would appear to be quite unnecessary to adopt both precautions.

Mr. ERNEST BATESON remarked that the Papers gave clear evidence of the co-operation which had existed between the Authors during the construction period, and which had contributed largely to the successful completion of the work. Mr. Howorth referred to difficulties which had occasionally been encountered, but when account was taken of the magnitude of the work, the difficult climatic Mr. Bateson.

Mr. Bateson. conditions, the scarcity of skilled labour, the fact that the two ends of the bridge were in different territory, and the numerous authorities, companies and persons concerned, it would be agreed that those difficulties had been surprisingly few, and that the satisfactory progress of the work was evidence of the cordial co-operation of all concerned.

Mr. Howorth, on p. 398, referring to the sinking of wells Nos. 21 to 28 inclusive, stated that all the wells had given trouble, but examination of the well-sinking chart did not indicate that there had been any substantial reduction in the average rate of sinking. Well No. 24 had refused to sink by open dredging beyond 106 feet below low-water level, in spite of the addition of 1,000 tons of kentledge combined with 35 feet of pumping. That refusal had been due to the inability of the grabs to remove hard material more than 2 feet below the cutting edge. In that case the method of sinking by open dredging had ceased to be practicable, and further sinking would only have been possible by removal of the material, either under compressed air or by other methods. The delay with wells Nos. 25 and 26 to which Mr. Howorth referred had been due to the rise of the river, and not to difficulties in sinking. On pp. 398 and 399 Mr. Howorth made several references to contract-depth. By that he understood him to mean the depths shown on the drawings issued to the contractor before the work had been commenced. The specified depths for wells founded in rock were as given by Mr. Handman on p. 340, whilst the depth specified for wells founded in sand was 110 feet below low-water level. It was not specified that 110 feet should be the maximum depth for any well, and it had not been intended that any well would be accepted for founding at that depth unless it was in acceptable material. The contract schedule had included an item for sinking by open dredging below 100 feet below low-water level, and did not limit sinking by that method to a depth of 110 feet. Wells Nos. 23, 25, and 26, when they had reached a depth of 110 feet, had been in very poor material, and the extra sinkages of 11 feet, 14 feet, and 4 feet respectively had been fully justified and had carried them into much better material—soft sandstone, in two cases. The maximum depth specified for the use of compressed air had been 100 feet below low-water level, and the contractor was to be commended for employing compressed air below the specified limit in the case of well No. 27, thereby enabling it to be firmly founded in soft sandstone rock.

The Papers dealt with the work as constructed, and a little supplementary information regarding the design of the bridge might therefore be of interest. On the contract drawings the bridge was shown as built, except that the portion now occupied by the seven secondary

Mr. Bateson. spans had been originally shown as viaduct. The information available regarding the behaviour of the river-bed during the flood-season had been incomplete, and the resident engineer had been instructed to carry out further investigations during his first season at the site. From the first it had been recognized that the viaduct could only be regarded as a permanent bridge if the main channel of the river remained within that portion bridged by the thirty-three main spans. The investigations carried out by the resident engineer disclosed the presence of more or less active channels in the neighbourhood of the junction of the main spans and the viaduct, and it was considered advisable to add further spans carried on piers founded on wells, and to shorten the viaduct portion accordingly. In the meantime the driving of the piles for the viaduct had disclosed the presence of a hard stratum, which had been diagnosed from borings as sandy clay. Consideration of the question of adding further spans showed that, in consequence of the occurrence of that hard stratum at a relatively high level, it would be more economical to utilize spans of a shorter length than the main spans. As a result seven secondary spans of 165 feet each were substituted for an equivalent length of viaduct. Consideration of that important modification had been responsible for suspension of the pile-driving operations for a period of 8 weeks, and its adoption was responsible for the discontinuity referred to by Mr. Howorth on p. 405, as it prevented Mr. Howorth from connecting up the viaduct with the main piers already completed.

Although the hard stratum, which Mr. Howorth, for want of a better name, had described as fairly hard decomposed sandstone, was hard enough in situ to resist displacement by the pile-tube with its blunt-nosed shoe, pieces of it were easily crumbled between the fingers, and when placed in water had been found to disintegrate with surprising rapidity. The material was in fact nothing more than sand inefficiently held together by a natural binder, and it had been realized that if the channels previously referred to became active the overlying sand would be scoured away and the hard stratum would also be affected. It was obvious that the clauses of the specification regarding wells founded in rock were not applicable to that material, and the secondary wells had therefore been taken sufficiently deep to ensure security in the event of scour of the river-bed. The deepest penetration into that material had been about 26 feet, giving a total depth of 73 feet below low-water level, as against 110 feet in the case of main wells founded in sand.

The bridge was required to comply with the Portuguese Government's regulations, which included the making of tests under a train consisting of two of the heaviest locomotives in use on the line, Mr. Bateson. together with a sufficient number of loaded wagons to cover the longest span. Stress-readings were to be taken in members selected by the Government Engineer, and the deflexions of the various spans were to be measured. Calculated stresses and deflexions had had to be supplied to the Government Engineer, and the regulations specified that the actual stresses and deflexions should not exceed them by more than a specified percentage. As it had not been known which members would be selected for the tests, the calculated stresses in every member in each different type of span had had to be supplied. The Fereday-Palmer stress-recorder and the Fereday deflectometer had been used for carrying out the tests. As was usual in such cases, the recorded stresses and the deflexions had all been slightly less than the theoretical figures. Mr. Handman and Mr. Howorth having left the site, the tests had been carried out by Mr. Whitehouse, who had then joined the staff of the Railway Company, Mr. Learmouth, who had been acting Resident Engineer, and Mr. Anderson, who represented the contractor. Their cordial co-operation with the engineer representing the Portuguese Government and with the Railway Company's staff had enabled the tests to be carried out most expeditiously and without hitch of any kind.

Hon. Philip Henderson. The Hon. PHILIP HENDERSON remarked that the question of shortening the bridge and of regulating the river-channel by means of a bund had been fully studied, but it had been agreed that the use of a bund would have been extremely dangerous, as the river had most peculiar habits ; it might even have caused the river to change its course. When the river was in normal flood it was probably not more than 3 miles wide, but during a high flood the country was inundated for perhaps 40 miles. In 1922 Mr. C. Seager Berry, M. Inst. C.E., had been sent out to study the conditions governing the choice of the bridge-site. He had been followed by Mr. R. J. Hallidy, who had had a very wide experience of Indian rivers, and it had been originally his idea that a bund should be employed, but he had subsequently come to the conclusion that a bund was absolutely impossible.

*** The Correspondence on the foregoing Papers, together with the Authors' replies, will be published in the Institution Journal for October, 1937.—SEC. INST. C.E.