ing 160 tons, and cars for length of 550 feet, weighing 2000lbs. per foot. The suburban railway tracks were to carry a train 500 feet long, weighing 2240 lbs. per foot.

Concentrated loading for main roadway and motorway could be derived from conventional traffic loading. The footway loading adopted was 100lbs. per square foot for deck system, reduced to 80lb. per square foot for cantilevers and suspended span.

For length of span the proposed Sydney Harbour Bridge ranked third in the world, viz., (1) Quebec Bridge 1800 feet, Forth Bridge 1700 feet, Sydney Harbour Bridge 1600 feet, Williamsburg Bridge 1600 feet, whilst for amount of headway it would rank first, viz., (1) Sydney Harbour Bridge 170 feet, as against 150 feet headway for Quebec and Forth Bridges, whilst the traffic it was designed to carry aggregated 14,600lbs. per lineal foot, as against 10,000lbs. per lineal foot for Quebec Bridge, and 4,480lbs. for Forth Bridge.

In conclusion the author wished to state that all information and illustrations of the Sydney Harbour Bridge, and other examples of long span bridges, were extracted from the paper prepared by J. J. C. Bradfield, M.E., M.Inst. C.E., on "Linking Sydney with North Sydney," and read before the Sydney University, Engineering Society, in November, 1913.

Discussion.

Mr. Tournay-Hinde said he desired to propose a very hearty vote of thanks to Mr. Fry for his interesting and descriptive paper, to which he was sure everyone present at the reading thereof had listened with the keenest pleasure.

It was not his intention to attempt anything in the way of criticism upon the various matters embraced by the paper, but only to secure, if possible, a little more information. Mr. Fry referred to the weighting of floating caissons with concrete at the top of the caisson. He regretted he was unable to understand why the concrete was placed at the top of the caisson; possibly he may have misunderstood the author, as the concrete in this position would appear to make the caisson top-heavy.

Another matter which forced itself upon his attention was the difference between the diameters of the caisson air-lock shaft and the recovery shaft. In the section shewn upon the screen the caisson air-lock shaft was shewn of a diameter of 3ft. 6in., while the diameter of the outer casing in which the men entered in order to recover was only 7ft. It seemed, therefore, that the annular space wherein the men remained during the period necessary for recovery would only be 1ft. 9in. wide. He would like to ask the author if it was usual to provide so limited a space. In reference to the air jet for ejecting excavated material from the inside of the caisson, it seemed to the speaker that the reason of its successful operation might possibly be accounted for in much the same way as an air-lift pump, that was to say, the column of water which discharged the material would be partly water and partly air, on account of the intermittent manner in which the material was introduced to the ejector pipe, and therefore it was possible that the static head in the pipe was less than the static head in the water outside, owing to the expansion of the air bubbles as they traversed the pipe. He put this forward as an alternative suggestion to account for the apparent anomaly; for the explanation given by the author of the paper, viz., the difference in the velocity of flow of water and air with the same static pressure, appeared to be quite sufficient to explain the phenomena.

He understood, however, that there were other gentlemen present who wished to speak on the paper, and who were better fitted to enter upon a criticism of same than he was, and so he would not encroach on the limited time available for discussion.

Mr. HART, who seconded the motion, said he did not know whether he was one of the persons referred to as being more competent than the mover of the vote of thanks to discuss the paper which had been read to them that evening. There were, however, one or two matters upon which he would like to dwell for a little while. It seemed to him that the author evinced a tendency to accept as facts a great many things which he was sure most of them still regarded as only theories yet to be proved. Mr. Fry referred to the various types of bridges, and among these the beam or girder type. He thought they would agree with him that there were still a great many things about that kind of bridge of which they knew very little indeed. The first thing which usually occurred was the question of stiffeners in the webs of girders. As far as he was aware, no one had yet given proof of his ability to design these stiffeners; they were put in as one thought fit, and, occasionally, the girder fell down. Secondly, the author stated that the next favoured type of bridge was the arch type. He had no hesitation in saying that, of all the things which it had been impossible to calculate, the arch was, perhaps, the most notorious example. Take, for example, the masonry arch, not a steel arch. A kind of trial arch was drawn and a diagram constructed from which was obtained some sort of a commencing line. If things came out all right well and good, if not, ill and bad.

In reference to the cantilever bridge girders, the joints in the big trusses were very often taken simply as pin joints. But when in actual practice if, instead of a pin joint, a great gusset-plate of inch steel with 40-odd rivets was used, then it was a very different matter.

Some mention had been made by the author of the causes of the failure of the Quebec bridge, and the tests which were afterwards conducted in reference to the same. Mr. Fry also mentioned another bridge designed to carry a load of 8 tons per lineal foot, and stated that a certain professor said it would only carry 4 tons. He thought that statement in itself was a very strong example of the differences of opinion which existed among theorists. It was mentioned that the Quebec bridge failed by reason of the buckling of the struts. The columns which buckled were made up of built sections which were latticed together. In the Quebec Bridge Inquiry which was afterwards held it was shown that the calculations of six or seven of the highest bridge authorities known in connection with the compression-members which failed, varied considerably. These different authorities showed that the amount of latticing required was anything from ¾ of 1 per cent. to 6 or 7 per cent. of the total cross-sectional area of the pillar. The actual amount provided in the bridge which failed was a little over 1 per cent. These remarks go to show yet another striking instance wherein our theories and our knowledge have been proved wholly deficient.

In a very casual manner the author referred to wind pressure, and said it was very strong. In the case of the Forth Bridge the stresses of the principal members were about 1000 tons to the live load; for the dead load 2300 tons; and for the wind load 3000 tons, which meant that the stresses produced by the wind in the case of some of these principal members were three times those produced by the train load passing over the bridge. It seemed to him that wind pressures of that nature deserve more discussion than they received this evening. He would like to point out that it was a very difficult matter to calculate the wind pressure on a bridge. First of all, it was necessary to take the wind pressure per cubic foot, which was an extremely difficult thing. It was measured by means of a little gauge, which usually proved unreliable.

In reference to the question of the area of the bridge, it should be noted that while some people took the area given on a drawing of the elevation, others, and more occasionally, he thought, after allowing for the wind blowing completely on the bridge, would take several times that area.

In connection with the question, the shape of the different members, whether circular or channelled, made a very big difference, as can well be imagined from the pressure blowing on an umbrella on the outside, as compared with on the inside when walking round a street corner. It would also afford him very much pleasure to hear the author once more upon the subject of rocker bearings. Diagrams had been displayed upon the screen of suspension cables which were carried over the top of the suspension towers, and these cables were slung over rollers which would enable them to adjust themselves to various loads. A design of a certain type of bearings for the abutment of a big bridge was also shown. He must say that he did not know, of his own knowledge, whether these things were really satisfactory. He did know that in the construction of the New Street Station roof in Birmingham—a very big roof—it was originally provided with roller bearings. When additional strengthening members were put in the roof it was found that all of these old bearings had rusted, and that the roof had acquired a kind of normal position, and remained therein. He thought it extremely probable that a bridge would act in much the same manner.

In conclusion, he would say that he had no desire to criticise the paper in an adverse manner. He considered it had been an extremely interesting and instructive one, and that its author was deserving of their highest gratitude for the obvious trouble taken in its preparation.

Mr. Hasemer said he had very much pleasure in supporting the vote of thanks to Mr. Fry.

In reference to the paper, he thought they must admit that the author had made some very able statements. In one of these he made an attempt at equalising the costs of the piers to the superstructure of the bridge. It seemed to him that the respective cost of these would depend entirely on circumstances. It was not difficult to imagine circumstances in which the superstructure would cost a far greater amount than the pier supports. The author had also gone to a great amount of trouble to explain that suspension bridges were very hard to calculate. He thought it was generally agreed that the calculation of almost any bridge exceeding a certain size was a difficult matter, and indeed, they had been shown later on in the paper that a particular bridge was not carrying 50 per cent. of the load that the theoretical design assumed it would carry.

Personally, he would like to see more research work carried out in connection with the construction of bridges, as it might have the effect of lightening these and reducing the enormous ratios at present existing. It would, of course, appear strange that he should suggest such a thing in view of the disastrous collapse of the Quebec Bridge, but perhaps the fact that we now had the knowledge which convinced us that such a collapse could not again happen, rendered the reduction of these ratios perfectly feasible.

In reference to the Sydney Harbour Bridge, he thought it must appeal to some of us, at any rate, that the loading factor was responsible for much unnecessary expense. The loading was so excessive that it seemed impossible to conceive it would reach the amount provided for in the design.

Mr. L. M. Roberts said he did not wish to criticise the detail matter of the paper in view of the fact that the ground covered was so immense. It was not generally known what a vast and troublesome thing it was to design a long-span bridge, and it would cause surprise to state that about £120,000 would be required to cover the cost of the design and preparation of working drawings of a bridge equivalent to the long-span bridge proposed for Sydney Harbour. There were many, many things of which we knew practically nothing at all that would have to be determined. He considered that about £30,000 would be required for research work before even the various columns could be satisfactorily designed. There was no truer statement than that if there was anything which the average architect or engineer considered he knew a lot about, it was the plate girder. Yet he could assure the meeting that the plate girder was one of the things about which we knew very little indeed. There would have to be a series of tests on plate girders alone before it would be possible to cut down the weight to the desirable bedrock minimum.

In the model upon the table could be seen the commencement of some research work in which he was at present engaged, and which would, in all probability, require three years to complete. One of his objects was to discover, if possible, whether it was desirable or not to wind-brace the upper chords in the cantilevers of such a long-span bridge. He thought it could be definitely ascertained from qualitative tests from models, if they were used in the same spirit as the naval architect used the testing tank. The model had a second object, which was to demonstrate the suitability of the "K" type of bracing. In the Works Committee's recommendations for the Sydney Harbour Bridge the "K" bracing proposal was ruthlessly thrown out. He thought it would involve the greatest difficulties to erect a bridge of that size with any other than "K" bracing.

In conclusion, he desired to say that it gave him the greatest pleasure to support the vote of thanks to Mr. Fry, because the amount of labour entailed in collating the mass of information embodied in his paper must have been very great, and the highest credit was always due to the man who would engage upon such a task.

The President, Mr. Reeks, said it afforded him very great pleasure indeed to convey to the author the thanks of the Engineering Association for his most interesting and instructive paper. He would have liked to take this opportunity of making further observations upon it, but as the time was limited, he would therefore refrain from encroaching upon the little they had at their disposal.

Mr. Fry, the Author, said he wished to thank the Council of the Engineering Association for the honor conferred upon him in asking him to read the paper, and also the meeting for the kindly remarks made by various members.

Mr. Tournay-Hinde made a reference to the weighting of floating caissons at the top. He should have said the caissons were weighted above the working chamber. The concrete was poured in above the ceiling of the working chamber to enable the caisson to float out, as otherwise it would have been top-heavy. When in position and properly anchored, the concrete would be placed in, in order to sink the caisson.

In regard to the diameter of the annular space in the air lock being so small, he would say that the data was extracted from diagrams issued in 1891 in connection with the Forth Bridge. He expected they found it satisfactory, as otherwise they would not have used it.

As regards the work of the air ejector ejecting the material, it was really hard to say what was the true theory, but personally he held the opinion that owing to the velocity of the material in the pipe, it had a dynamic effect which overcame the static pressure of the water precisely as in the case of the injector on a steam boiler.

Mr. Hart referred to the fact, stated in the paper, that the girder was the easier to construct. Upon the very little they did know did they base their designs. It was really one man's opinion against another's.

As regards the arch bridge, it was certainly difficult to construct, and he thought the paper contained a statement to that effect. For these reasons, and also for the reason that no arch bridge or girder then existent exceeded the length of 1000 feet, they were not discussed.

With regard to the failure of the struts and compression members in the Quebec Bridge, he would say that they knew very little about the compression members, because it was very hard to ascertain exactly what was taking place. They knew the stresses, but not how the different combinations of webs and lattices really worked. These facts were in the mind of the Board of Engineers which ordered the tests to be made on the compression members, and in so doing they were successful in acquiring much useful information. They took the tests under different loadings, and different conditions of loading, and the bearing areas of the pins. It was the lower chord on the shore side, fairly close to the pier, which buckled, and subsequently collapsed, thus bringing down the whole structure.

As regards the wind pressure and loads on the Forth Bridge, the ratio of wind load to traffic was certainly very great, but it must be borne in mind that the Board of Trade imposed very stringent regulations on the engineers in compelling them to provide for 56 lbs. wind pressure to the square foot, whereas in modern practice 30 lbs. was considered sufficient. The traffic load was very small for that size of bridge, and it was therefore unfair to make a comparison between the two.

In reference to the rocker bearings and whether they really worked or not, he confessed it was difficult to say, but in the case of the Beaver Bridge—which was under the 1000 ft. span, but which deserves mention on account of the very heavy traffic load carried—the railway companies which had the bridge constructed spared neither money nor pains, and employed the best brains—the engineers considered it the best system. It was a fact, however, that it had lessened the theoretical stresses very considerably. He had no record of whether measurements were taken to ascertain if the rollers were working or not.

Mr. Hasemer referred to the question of equalising the cost of piers for superstructure. Certainly the locality in which a bridge was constructed largely determined the question of the relative costs. It would be easily seen that, in the case of a big bridge, the cost of the superstructure would greatly exceed the cost of the piers.

He thought the research work should be carried on very considerably in connection with the designing of big bridges. It was only when a big bridge was to be designed that such work can be conducted financially, that was, when the ratio of cost of research work to the final cost was small. Such a lot ought to be ascertained with regard to the various sections of the design, and if we possessed the information, it might be that the same strength could be achieved in a very much lighter design than those discussed.

He thanked them again for the kindly manner in which they had listened to his remarks.