Discussion.

The AUTHOR remarked that he wished to take the opportunity of expressing his gratitude to the Bridge House Estates Committee of the City of London, who had sanctioned the use of the Tower Bridge for the experiments described, and also to thank Mr. John Gass, M.Inst.C.E., the engineer, who had given all possible facilities for carrying out the work. The Author.

The PRESIDENT said he was sure the Bridge House Estates Committee and Mr. Gass would appreciate the Author's remarks. It might be of interest to recall that in the discussion on Dr. Stanton's first communication on Wind-Pressure,[1] Sir Benjamin Baker asked how it was that windows did not get blown in by high winds. Sir Benjamin determined the breaking point of ordinary glass windows by inserting a window in the bottom of a water-tank, which was grádually filled with water until the window failed; and he estimated that ordinary windows would not bear a heavier pressure, on the average, than 30 lbs. per square foot. The President.

Mr. J. S. DINES remarked that on p. 137 it was stated that there seemed to be no doubt of the persistence, without appreciable decay, of a definite gust of high intensity between the extreme stations, which were about 14 miles apart. That was somewhat contrary to meteorological views on the structure of gusts. In the first place, he would like to explain the difference between what was called a gust and what was called a squall. Referring to the Kew anemometer record shown in Figs. 13, Plate 1, during the first hour of that record the wind was gusty; the mean velocity was about 25 miles per hour, but the record fluctuated within wide limits above and below that line. Each excursion was of the order of about 1 minute in duration. Those excursions were termed gusts. At 2.45 p.m. the mean wind-velocity rose from 25 to 35 miles per hour. That rise, which lasted for 5 or 10 minutes, was called a squall and its nature was fundamentally different from that of a gust. A gust was almost certainly due to the obstruction of that part of the air which passed over objects on the surface of the earth, such as trees and buildings. The certainty of that statement arose from the fact that the wind was much less gusty over the sea, where there were no obstructions, than over the land, and was also less gusty over flat Mr. Dines.

[1] Minutes of Proceedings Inst. C.E., vol. clvi, p. 119.

Mr. Dines. land, or unbroken ground, than over broken ground where there were trees and buildings. A squall was due to what he might call meteorological causes—to the contiguity of two masses of air of different temperature, which gave rise to a difference of pressure at the surface and to corresponding effects on the wind. Such masses of air might travel across the country, retaining their temperature-difference for a considerable distance; and cases were on record where a well-marked squall, known as a line squall, had been traced for many hundred miles. A gust, on the other hand, which was due to the effect of a building or a tree, would not be expected to travel for longer than a fraction of a minute, or to cover more than a small fraction of a mile. The conclusions as to the distances traversed by individual gusts which the Author had drawn from a study of the diagrams might be only the result of a coincidence. As was evident from the anemometer records, there were many gusts in a period of $\frac{1}{4}$ hour. It was easy to imagine that, in taking a series of observations such as were given in the Paper, cases would occasionally be found, though due purely to chance, where one gust struck Kew Observatory, another the Tower Bridge, and a third Greenwich Observatory, each at the right instant for all three to appear to be one gust. On p. 136 the Author stated, in connection with the fact that the duration of a gust was found to be no greater in the average-pressure record than in the record for a single reference-point, "that this is not the case proves that in these cases at all events the lateral dimensions of the wind-filament must have been of the order of the span of the girders." According to that statement, the width of a gust of wind might be of the order of 250 feet. A gust might be expected to be of the same order of magnitude as the obstructing building or tree which formed it. The Tower Bridge was situated in the neighbourhood of very large buildings, such as St. Paul's Cathedral and Cannon Street railway-station, and it was not unreasonable to suppose that such buildings might give rise to gusts of much bigger lateral dimensions than would a small obstruction like a house or a tree. The most marked cases where the mean pressure over the bridge girder was equal to the pressure at the cluster were illustrated in Figs. 6, 8, and 10, Plate 1. In each of those cases the wind was approximately north-west. The wind did actually fluctuate considerably in direction, as in velocity, though these fluctuations were not shown in the Figures, and it was quite possible that at the moment when each of these gusts occurred the wind was blowing directly from Cannon Street station. A large horizontal obstruction like that station might give rise to a big eddy with a horizontal axis as the air turned over the top of the station and rolled along with a fairly free passage down the Thames to the Tower Bridge. If that could happen, it was not unreasonable that the eddy should strike the footway of the Tower Bridge practically solidly through its whole length, in the same way as a sea-wave might strike a sea-wall if it was coming in perpendicularly to the wall. If that were the case—and it was only a hypothesis—it was of considerable importance. Whereas a uniform pressure equal to the pressure of the highest gust could occur at the Tower Bridge, at another structure like the Forth Bridge, where there were no large gust-raising agencies in the neighbourhood, the same result would not be obtained. If his suggestion should prove to be unsound, then he thought it would be necessary to fall back on the hypothesis that the individual gusts which struck the whole bridge at once were not what he had defined as gusts, but were squalls due to meteorological causes, only of short duration. In gust-records such as those shown in the Figures there might be an occasional line which was not what he would define as a gust due to a building or similar obstruction, but was a squall due to a real meteorological cause, though lasting, instead of 5 or 10 minutes, only for a very short time. Mr. Dines.

The AUTHOR asked if Mr. Dines had any information as to the speed of squalls travelling across country. The Author.

Mr. DINES said that, judging from records in the Meteorological Office, he thought 40 miles per hour was a common speed. Mr. Dines.

Mr. G. E. W. CRUTTWELL remarked that he was particularly interested in the Paper, as he had been resident engineer on the construction of the Tower Bridge. Since its opening in 1894 he had made quarterly visits, so that he was pretty well acquainted with the bridge itself, and he had also seen the Author's apparatus in use there. He had not had the advantage of seeing the apparatus erected at Teddington, but the Author stated on p. 131 that it was evident that the observations on that site could not be regarded as of general application, and that it would be necessary to carry on the work outside the laboratory. No particular reasons were given why the site was not to be regarded as of general application, except the mention of trees being in the way; but, to his mind, neither was the Tower Bridge a site to be regarded as of general application. In the first place, the high-level footways had a span of 225 feet, and on each side of that span there was a tower 45 feet in width with a height of 65 feet above the top, and 141 feet beneath the bottom, of the footways. It seemed to him that the wind occupying the width between the centres of the towers, which would be 270 feet, had to pass through an opening of 225 feet between the towers, with the result that the velocity between the towers Mr. Cruttwell.

Mr. Cruttwell. must be increased by 20 per cent. on the average. That increase of velocity was not uniform throughout the width of 225 feet. He imagined that the tubes at the points B and G (*Figs. 4*, p. 132), which were situated at a distance of 27 feet 6 inches from the towers, would give a considerably higher reading than the others. That, he thought, would be the cause of the average of the six tubes being too high, as compared with the record given by the cluster, which was 87 feet 6 inches from the nearer tower. Since that cluster was such a long way from the towers, the velocity of the wind there must be less than the average for the six separate tubes. That was one of the reasons why the Author found that the ratio varied so little between the cluster and the average of the six pressure-tubes. On the other hand, taking an average of the Teddington records given in *Fig. 3*, he found the average of the six tubes was only about 74 per cent. of the records given by the cluster, and that was more in accordance with the general idea that pressures on small plates were very much higher than those on large plates. The Author stated (p. 135) that in the case of the records in Figs. 6–11, Plate 1, the agreement was fairly satisfactory, the measured ratios ranging from 0·98 to 1·04, and he had obtained those figures by finding the areas enclosed by the line representing the pressure at the cluster and the line representing the average pressure on the six tubes. That might be so, so far as the actual area was concerned; but the peaks of the various gusts were in every case considerably higher with the clusters than with the average pressures. Taking the actual pressures on the clusters, he had made a few comparisons. In Fig. 6 the first peak of the cluster gave a pressure of 5 lbs. per square foot, as against 4 lbs., the average. The next gave $5\frac{1}{3}$ as against 5, the next $6\frac{1}{3}$ as against $6\frac{1}{3}$, the next 6 against $5\frac{1}{3}$, then $5\frac{3}{4}$ against $5\frac{1}{2}$. The average of all the peaks representing the gusts in Figs. 6–11 showed that, if 100 per cent. was taken as the peaks of the clusters, the average of the peaks of the six tubes only came to 86 per cent. In the case of Figs. 12 and 13 the difference was very much more pronounced. There the ratio between the peaks of the clusters and the average of the six tubes was 100 to 71. Those figures accorded closely with what the Author had found at Teddington, and it seemed to Mr. Cruttwell that the earlier records were not to be despised; he would be inclined to place more reliance on them than on those obtained at the Tower Bridge, which he thought, for the reason he had given, in regard to the wind having a much higher velocity near the edge of the towers, were not at all reliable. Results of experiments at the Forth Bridge had been given in several Institution Papers. A small plate $1\frac{1}{2}$ square foot and a large plate 300 square feet in area gave the ratio of 100 to 67. According to Mr. Dines's records, $2\frac{1}{2}$ square feet and 9 square feet gave a ratio of 100 to 89; and plates $2\frac{1}{2}$ square feet and 42 square feet gave 100 to 70. By measuring the peaks of the diagrams in the Paper, he had found that Figs. 6–11, Plate 1, gave 100 to 86; Figs. 12 and 13, 100 to 71; and the Author's experiments in the grounds of the National Physical Laboratory gave 100 to 74. Those results appeared to be consistent one with another, and he was inclined to disagree with the Author's conclusion that the pressures on the small plates might be taken as similar to those on the large plates. Mr. Cruttwell.

Mr. R. Corless observed that at the Meteorological Office lately the highest gusts ever recorded on pressure-tube anemometers in the British Isles during the last 30 years had been plotted on a map, and the results of that work might be of interest to engineers. The general conclusion was that the highest gust registered at an inland station by well-exposed anemometers had a velocity generally not greater than 80 miles per hour, except at a few places which were comparatively near the coast, where it might be greater. The highest gust ever registered was at Quilty, in the west of Ireland. Trains on the railway in that district were so light that they had been blown right off the line, and they had on occasions to be loaded with ballast to maintain their stability. An anemometer put up in that connection had recorded more than 116 miles per hour—the maximum limit of the chart of the instrument. Velocities exceeding 100 miles per hour had been measured in the neighbourhood of the Scilly Islands and in Cornwall. These figures referred to the maximum gust ever recorded. The instrument used was the Dines pressure-tube anemometer; and he questioned whether that instrument really did measure the highest gust. The apparatus consisted of a head, a vane, and two tubes, one of which was kept facing the wind, and the other ended in a row of holes at the top of the mast, just underneath the pressure-pipe. Those two orifices were connected by $\frac{3}{8}$-inch composition pipes to a recorder, situated at a considerable distance from the head, consisting of a tank half-filled with water and containing an inverted bell-jar. The pressure pipe opened inside the bell-jar, which, normally, was about half-full of water. The suction pipe opened in the space above the bell-jar. In order to produce a record of a sudden gust it was necessary to displace through the pipes a considerable volume of air, an operation which occupied time; and the highest gust had come and gone before the pen of the instrument reached its highest point. That had been proved at the Meteorological Office at South Kensington Mr. Corless.

Mr. Corless. by disconnecting the recorder of the pressure-tube instrument and connecting in its place a sensitive indicating instrument which consisted of a flexible membrane, stretched between the pressure and the suction chambers, and suitably connected to the dial. With an instrument of that kind the maximum pressure could quite well be recorded with considerable accuracy, once the instrument had been properly calibrated, for a negligible volume of air had to pass down the pipes in this case. The fluctuations of the indicating instrument were found to be very much greater than those of the recording instrument. The instrument at South Kensington was very badly exposed, so that the effect referred to would be exaggerated; but, even so, he doubted whether a well-exposed pressure-tube anemometer really did record the maximum gust; and he thought that a "factor of safety"—not yet determined—must be allowed on that account.

Mr. Tudsbery. Mr. H. T. Tudsbery remarked that the subject of the Paper was of ever-increasing importance, in view of the growing necessity for economy in the use of all materials of construction. The Author appeared to arrive at two conflicting conclusions: first, that, for moderate winds up to 50 miles per hour, the pressure (per unit area) on a large area during the passage of a gust was in most cases appreciably less than on a small area, and also that very definite evidence was obtained of gusts up to 50 miles per hour in which, on a front of about 250 feet, the average pressure over the whole area was sensibly equal to that at a point in it. The Author appeared to conclude, however, that no reduction of maximum point pressure could be safely allowed over any part of a structure that presented a large surface area to the wind. That conclusion was of great importance, and, if he had read the Paper correctly, he would like the Author to support quite explicitly that particular issue. In the design of structures of unusual types, it was advocated that a coefficient relating pressure and velocity should be obtained by placing a model of the proposed structure in a wind-channel, and that, in the case of an important structure, records should also be taken with a pressure-tube anemometer at the site. That procedure would undoubtedly give first-rate data on which a design might be prepared, but in the majority of cases it would not pay. There was, however, another means, namely, measurement of the actual stresses produced in an existing structure by wind-pressure. Theoretical considerations indicated that a known wind should cause certain loads and stresses in a given structure. It was not difficult, with the instruments available, to determine exactly the stresses caused by any wind and to measure accurately the wind-velocity, and thus to find out how the actual stresses in various types of structures differed from calculated stresses. Very often the actual stresses had been found to be less than they should be by any calculation. By the accumulation of sufficient records it should be possible to arrive at coefficients applicable to most of the ordinary types of structure. There were some structures in which the difficulty estimating correctly the stresses due to wind-pressure was almost insuperable. The Menai Bridge presented great difficulty in that respect, and practical results obtained there were therefore of much interest. In the preparation of plans, etc., recently, in connection with the renewal of 600 or 700 defective links in the main chains of that bridge, it became evident that some definite knowledge of the actual stresses in the main chains due to wind-pressure was essential. In a previous examination of the bridge[1] it had been ascertained that the stresses in the wrought-iron links of the main chains amounted to as much as 9½ tons per square inch in some cases, so that the margin of safety was known to be extremely low. With a view to obtain data from which the effects of winds on the main-span chains of the bridge might be deduced, an extensometer was placed on each of the five bars of each of the four chains forming the two main groups of one outer and one inner chain. The span itself was supported by four groups of chains, but with the limited number of extensometers available it was not possible to cover all the chains. The two chains selected were, however, representative of an inner and an outer group, and, the prevailing winds being from the south-south-west to west, they were normally in the most exposed position. The extensometers, which were placed on the span chains as near to the top of one of the supporting towers as practicable, were completely protected from the weather by box coverings. They were placed in position in November, 1923, and were not removed until March, 1924. The instruments had dials registering the elongation in thousandths of an inch, and these dials were set to zero at known temperatures of the iron in the chains, usually when the wind was calm or as nearly so as opportunity offered. The temperature was recorded by a thermograph, and the direction and speed of the wind were determined by a Dines pressure-tube anemometer installed on the top of a tower. Readings of the dials were taken practically daily during the period mentioned. By means of a friction gland on the instruments, the dial needle was retained at the limit of the maximum movement; and reference to the anemometer chart to ascertain the maximum wind during

Mr. Tudsbery.

[1] Minutes of Proceedings Inst. C.E., vol. ccxvii, p. 208.

Mr. Tudsbery. the period under review, with the necessary temperature correction deduced from the thermograph record, enabled an accurate record to be kept of alterations in the stress due to wind. The following were the general conclusions arrived at from the records :—

South-South-West Winds.—The two chains under observation were affected about equally by winds up to about 25 miles per hour. Winds exceeding 25 miles per hour affected the outer chain much more than the inner chain.

South-West Winds.—The outer chain was affected rather more than the inner chain by winds of 10 to 35 miles per hour and in fairly uniform amounts. For winds of 35 to 75 miles per hour the outer chain was affected proportionately more than the inner chain, the amounts again being moderately uniform throughout.

West Winds.—The outer chain was affected more than the inner chain. That was to be expected when it was considered that a west wind played almost directly through the Straits and struck the outer chain first.

East Winds.—The chain farthest away from the first action of an easterly wind was more affected than the chain immediately to windward of it.

For speeds up to about 40 miles per hour an east wind had the least effect of any wind on the two chains examined; for speeds of 40 to 55 miles per hour an east wind had approximately equal effect with winds from other directions; and an east wind of 55 to 75 miles per hour produced a greater effect on the chains worked on than did winds from other directions. That was not what was expected, though, notwithstanding the fact that the average dial movement for an east wind of gale strength was greater than the average dial movement produced by other winds, the maximum individual dial movement for east winds was less than for any other wind on those chains. Results of the investigation that appeared doubtful for any reason had been eliminated; thus definite information had been obtained, and it was established that the maximum stresses which might be expected in the chains due to wind alone at 75 miles per hour were :—

South-south-west wind	0·81 ton per square inch
West wind	0·71 ,, ,, ,, ,,
South-west wind	0·68 ,, ,, ,, ,,
East wind	0·61 ,, ,, ,, ,,

It was found possible also to plot a curve, which was sufficiently accurate for all practical purposes, showing the maximum change of stress due to any increase of wind-pressure between zero and 75 miles per hour. He ventured to hope that the Author would go still farther with his inquiries. He suggested that provision should be made for actual tests on, say, fifty typical bridges in this country, somewhat on the lines which he had just described. He felt that such a practical extension of that investigation into wind-stresses on structures would prove to be extremely valuable when taken in conjunction with the Author's more academic inquiries. It was a matter of regret that the Roads Department of the Ministry of Transport had not at present the necessary powers to assist in experimental work; but he hoped that in the next Ministry of Transport Act they would be given such powers. When that time came, and he hoped it might not be long delayed, he felt sure that the Department would give a sympathetic hearing to any suggestion of that kind, if it had the backing of an influential body of engineering opinion. Having regard to the interest of the Department in highway bridges, tests of their stability under wind-pressure would be of vital concern to it. Mr. Tudsbery.

Mr. E. H. MORRIS remarked that in a communication on the subject of Mr. Remfry's Paper on Wind-Pressures on Bridges, he had referred[1] to some interesting cases of failure of the lateral bearings of the Menai suspension bridge in two noted storms, one in February, 1903, and the other in December, 1914. Since he made that communication, he had recalculated the equivalent wind-areas, which he now estimated to be: in the first case (1903) 4,368 square feet; and after the alterations had been carried out in 1910, 3,852 square feet. He had taken the maximum velocities recorded at Holyhead on those two occasions as 87 miles per hour in 1903, and 79 miles per hour in 1914, and had used for the coefficient K in the expression $P = KV^2$ the value determined by Dr. Stanton in his earlier experiments for lattice structures, namely, 0·004. He calculated that in the first case 30 tons shear load occurred on one bearing, assuming uniform distribution of pressure over the whole structure. The two bearings, which were ascertained to be capable of sustaining about 25 tons each, failed simultaneously in the gust. The design of the bearing had been modified before the storm of December, 1914, and two bolts had been put into each bearing, designed to shear under a total load of 25 tons. The wind-pressure, worked out by the same method as previously employed, amounted to 23 tons on the single bearing, and only one bearing failed on that occasion. Another example (not previously cited) of a failure of a bearing on that bridge occurred in February, 1908, before the alterations had been Mr. Morris.

[1] Minutes of Proceedings Inst. C.E., vol. ccxvi, p. 60.

Mr. Morris. carried out, during a line squall with a recorded velocity of 81 miles per hour at Holyhead. In that case only one bearing failed, and the calculated pressure on it was about 26 tons. He did not wish to press those figures too forcibly; but their remarkable coincidence gave support, in his opinion, to the Author's conclusions that high gusts were distributed over a large area. In that bridge the net area of one elevation was 3,000 square feet, and the gross area was 15,000 square feet, so that the proportion of open space was as high as 80 per cent. It seemed to him that, leaving out tornadoes, which, although very rare in this country, were not unknown—more than forty examples had been recorded in 30 years—the line squall constituted the element of danger to be guarded against in the design of bridge structures in this country. A line squall travelled over a large tract of country and on a very broad front, and very often it moved in direction at a very wide angle with the direction of the wind, which seemed to explain the theory criticized by Mr. Dines. The length of a single bridge, however, bore a negligible ratio to the wide front on which a squall moved. The squall in February, 1908, had been clearly traced by him from the record at Scilly as well as at Holyhead; the time at the latter place was 1.20 p.m., and he had experienced the same squall at Doncaster, 130 miles eastward, $1\frac{1}{2}$ hours later. Unfortunately, there were very few anemographs in the country at that time, so that the full area affected by the squall had not been recorded.

Professor Bairstow. Professor L. Bairstow remarked that he was not quite an independent observer, because he had been associated with the Author for the last 20 years or more. It seemed to him that the conclusions in the Paper, in an engineering sense, could be hardly other than they were, on the records taken. Although it was perfectly clear that the average pressure over a considerable area was less than the pressure at any one point, yet there were occasions when the wind was distributed with full velocity over very large areas. He did not think there was any inconsistency between saying that the average pressure was only 70 to 80 per cent. of the maximum and the statement, given at the end of the Paper, that in designing any such structures, account should be taken of the wind as though it were uniformly distributed over the whole; because there were occasions on which that did happen. One of the matters to which the Author referred, and which had been mentioned in the discussion, was what might be called scale effect, namely, the difference between the pressures on a small plate and a big one, apart from the distribution of the gusts over a large area. That was a problem in which he was particularly interested in connection with aeronautics. Aeronautics had been built up to a large extent from experiments on models. A small wing, 3 feet long and 6 inches wide, was tested, and from the tests results were deduced for a wing which might be 130 feet long and 12 or 15 feet in width. There was little doubt in the minds of those interested in aeronautics that the scale effect was minute—so minute that they had not been able to trace it. That, of course, was not true for all bodies, but there was every reason to believe from study that it was true for the pressure on bodies when the wind was meeting them almost broadside. He had also been associated with the Author in connection with the experiments on plates which were exposed in the natural wind. Those experiments, embracing areas up to 300 square feet, again supported the conclusion that, apart from the distribution of the wind, there was little to choose between the intensity of pressure on small and on large plates. Whether the pressure was due to gusts or squalls did not seem to matter very much from an engineering point of view; if the wind might reach high uniform velocities, the engineer must provide a factor of safety to meet it. He had a very vivid recollection of one squall, in 1908 or 1909, which crossed Bushy Park and blew down 60 to 100 trees near the Teddington end. The extreme gust recorded at Kew, a few miles away, was 57 miles per hour. The peculiar feature of the record was that there was no other gust of anything like that intensity for hours before or after. The storm arose and was over in about 10 minutes. He thought there was little doubt that that was an illustration against one of Mr. Dines's contentions, that it might be a coincidence that gusts had gone past Kew and the Tower Bridge and reached Greenwich. It seemed to him that squalls were, as the Author pointed out, of appreciable width. In the case he had mentioned it was possible to see the track across Bushy Park, the squall being of sufficient intensity to uproot hawthorn trees standing by themselves. With regard to the effect on the Tower Bridge, the Author had had a model of the bridge made, and the pressures which he had measured on the footway included any effect on the towers. It was well-known from experience that if the wind was blowing broadside on to the footway the effect on the towers would be limited, not to 20 feet on either side of the towers themselves, but to an equally small number of inches. Some considerable way behind the towers the eddies would be felt, and they would cover a much wider area. There was little reason to suppose that the towers would have any appreciable effect on measurements made whilst the wind was coming up or down the river approximately at right angles to the bridge. If there was any appreciable lateral motion, then, as indicated by the Author, perhaps the effect of the towers might be serious. Professor Bairstow.

The Author. The AUTHOR, in reply, confessed that he had rather expected criticism from meteorologists when he presented the Paper, but Mr. Dines had treated him gently and had not taken any violent exception to his remarks. He had been very gratified to listen to Mr. Dines's observations, because, before he took up the subject of wind-pressure, he had a vivid recollection of studying the writings of Mr. Dines's father and learning all that was then known of the subject. Mr. Dines had made two main criticisms. In the first place, he doubted whether any gust was as wide as the Tower Bridge, namely, of the order of 225 feet. On that matter the Author could but point to the evidence of the records; he had no theoretical considerations to advance. He could only repeat, what he had said in the Paper, that many of the records of the variation of pressure over the whole bridge so closely resembled the corresponding records of the variation at a point of it that no other conclusion was possible than that the width of the gusts was of the order of the span of the bridge. In the second place, Mr. Dines disagreed with the suggestion that a gust could travel from one place to another, as this was opposed to the theory that a gust was purely a local phenomenon. Here again the Author had nothing but the evidence of the records to bring forward in support of his view, but it would be admitted, he thought, that the number of cases in which an apparent identity had been established between isolated peaks on the anemograms of stations say 14 miles apart, made this evidence fairly strong. He had recorded in the Paper six cases where an apparent identity had been established. Quite recently he had learned of another case from Mr. S. B. Donkin, M.Inst.C.E., who wrote to him about an accident at Greenwich on the 30th October, 1924, when a crane was blown into the river. No observation had been recorded at the Tower Bridge, but he had identified on the Kew anemogram a gust which, travelling at the known speed of the wind, would have arrived at Greenwich at the time of the accident. With regard to Mr. Cruttwell's criticisms of the Bushy Park experiments, he did not want unduly to stress the importance of those experiments, but he thought they were particularly applicable to cases of buildings where the ground effect mentioned was obtained. It would be quite safe, he thought, to take a pressure over a large area near the ground as being 75 per cent. of the pressure at a point. Those experiments had been made in the vicinity of numerous obstructions, and the observation-points were about 60 feet above the ground. These results, however, were not of general application, because where the exposure was extremely good, as in the case of the Tower Bridge, different conditions obtained. He gathered that Mr. Cruttwell was

The Author.

not satisfied with the exposure of the cluster on the Tower Bridge, his point being that the cluster was not equally exposed with the rest of the points. He joined issue with that statement very strongly, because it seemed to him that the time integral of the areas under the two curves could not agree to within a few per cent. if the exposures of all the points were not equally good. He had referred to that in the Paper. In Figs. 12 and 13, Plate 1, those time integrals did not agree. In those cases it would be noticed that there was a large south-west component in the wind, and he thought that eddies from one of the towers affected one of the pressure-tubes and prevented an agreement from being obtained. Mr. Cruttwell appeared to have come later to the conclusion that the whole of the experiments were after all in good agreement, and that they showed it was possible to take a 0·75-per-cent. factor. That was not quite consistent with his earlier criticism of the cluster. In the first place he had said that the cluster was not properly exposed, and that the Tower Bridge records were worthless; but on second thoughts he appeared to consider that the records of the Tower Bridge were in agreement with those at Kew. He did not know whether Mr. Cruttwell, if he were going to design a high-level bridge, would, on the evidence of the records, like to take a factor of 0·75; the Author confessed that he would not. The more he studied the records the less inclined he felt to take any factor other than unity into consideration for such a case. The records did show, on the average, that the mean pressure was less than the pressure on the cluster, but there were cases in which those pressures were so nearly alike that no factor other than unity could be assumed. Mr. Corless had dwelt on the lack of accuracy of the Dines anemometer. The Author did not feel any qualms on that matter, as far as his actual experience was concerned. Professor Bairstow and he had spent many hours together in reading wind-pressure gauges, and their general experience was that the high gusts maintained their intensity for 1 or 2 seconds. If that were accepted, the criticism was not important. A similar criticism had been applied to the recording pressure-plate, on account of the inertia of its moving parts, but the Author was inclined to think that too much stress had been laid on the effect. He thought that Mr. Tudsbery's remarks concerning the two conflicting conclusions had already been answered. Mr. Tudsbery seemed to think that the solution of wind-pressure problems by means of a test of a scale model in a wind-channel was a costly process; but big bridges were enormously costly, and a small sum spent on a model experiment was a very small fraction of the total cost of such bridges—it was only in the case of large structures that

The Author he made the recommendation. He was rather alarmed at Mr. Tudsbery's suggestion that he himself should carry on the work. His first Paper on wind-pressure had been read before The Institution on the 22nd December, 1903; and he thought the work might now fairly well pass into the hands of some of the younger generation. He still thought that the way of treating the problem which he had suggested was as good a one to adopt as any. It seemed to him that the wind-pressure problem, from an engineering point of view, was practically settled. The variation of velocity with height was well-known. The experiments described in the Paper showed that it was not safe to take account of any considerable lateral variation, so far as spans up to 250 feet were concerned. He thought the remaining problem was for meteorologists, who, when informed of the locality in which a structure was to be put, would have to predict the biggest gust that was ever likely to happen in that locality.

Correspondence.

Mr. Donkin. Mr. S. B. DONKIN remarked that the Author's report on the experiments at the Tower Bridge gave very valuable information as far as it went, but that information was limited by the relatively small span of the bridge, and by the fact that the velocity of the wind at no time during the investigations exceeded 60 miles per hour. It appeared to be necessary that the research work should be continued, in order that engineers might be able to conclude whether, for bridge spans or structures exceeding 250 feet in length, the average pressure on the structure, or wind-pressure factor as defined by the Author, was reduced on account of lateral variation in wind-velocity. It also appeared to be very necessary to know whether that same factor was reduced with velocities exceeding 60 miles per hour. He hoped, therefore, that The Institution would influence the National Physical Laboratory to carry out further research work on this subject. He had in mind the use of apparatus on a bridge, such as the Forth Bridge, where winds of much higher velocity would be encountered. He believed that the Author had already designed an apparatus suitable for bridges of longer span, in which he had arranged for electrical transmission of pressure from each point, so as to obviate the great length of lead piping, which might lead to errors. Any continuation of the research work should also