Discussion.

Mr. Symons. Mr. G. J. SYMONS said that Mr. Bender had spoken of some damage by whirlwind storms being due to a reduction of atmospheric pressure up to 180 lbs. per square foot. That practically meant $2\frac{1}{2}$ inches diminution of barometric pressure, but he did not think there was any evidence to favour that view. If Mr. Bender quoted in support of it the uplifting and disruption of structures, the raising of horses, carts, cattle, &c., or the floors of large buildings, or the outbursting of walls of houses, there might be evidence in support of his statement, but certainly there was no such evidence as far as barometric records were concerned. Mr. Bender had stated as a remarkable fact that in Saxony a man and two horses had been lifted up, but that was a mere nothing to what had occurred in this country. On one occasion, about twenty-two years ago, a four-wheel wagon, standing on one side of a hedge near Calne in Wiltshire, was caught up by a tornado or whirlwind, lifted in the air, without being turned over, was taken horizontally a short distance, and dropped down on its wheels on the other side of the hedge. He could not help thinking it desirable that these facts should be reduced to actual measurement. The weight of the wagon could be approximately determined, and also the area; and thence the lifting power might be ascertained. Reference had been made in a casual way to the fact that the velocity of the air was less near the surface of the earth than above, as proved by travels in the air, such as that of Lunardi's balloon, which travelled 70 miles an hour while perfect calm reigned below. That fact was also known from the experiments of Mr. Glaisher and of Mr. Thomas Stevenson of Edinburgh. With regard to the cup anemometer (of which there were many specimens in the adjoining room), it was suggested that it might be tried by placing it in front of the locomotive of a fast train. The cups of the anemometer rotated in consequence of the greater pressure of the wind on the hollow than on the convex surface; and it had been assumed by Dr. Robinson that they rotated at $\frac{1}{3}$ of the velocity of the wind; that if the cups travelled through a circumferential distance equal to 1 mile, 3 miles of air had passed. In order to determine whether that value was correct, Mr. Bender suggested that "by placing the anemometer in front of the locomotive of a fast train, and by connecting a speed-recorder with the engine, the true relation between the velocities of the air and the

instrument can be satisfactorily ascertained." He believed that Mr. Symons. that had already been done by Dr. Robinson; but it had certainly been subsequently tried by driving on a level turnpike road in Scotland, in a gig with an anemometer on a pole 15 feet above it. It had also been tried at St. Petersburg, and by lighting a fire which would make a large amount of smoke, and noticing the time the smoke took to travel a given distance. With regard to the running in front of an engine there was one considerable difficulty arising from the fact that the engine carried a skin of air along with it, which would disturb the indications. In reference to other points in Mr. Bender's Paper, the Author had alluded to the checking of the anemometers by means of whirling machines, and quoted a case in which experiments were made in Paris with a whirling machine of 10-feet radius, but he had said nothing about what had been done in this country. He was glad to say that as much had been done in England as in France in that respect. In 1860 experiments of that kind had been tried in Greenwich Park; and others, probably analogous to those in Paris, were tried at the Crystal Palace, under the direction of the Kew Committee. A steam merry-go-round was engaged, and upon its arms anemometers were placed. The machine was driven at various rates, and the indications of the anemometers were checked. All those observations had been reduced by Professor Stokes.¹ The Author had stated "that the wind-pressure per square unit increases with the size of the surface was early noted by De Borda, Hutton, and Edgeworth; and this fact has to be borne in mind when calculating the maximum wind-pressure on a structure." Professor Stokes, when under examination at the Tay Bridge inquiry, said that pressure was nearly proportional to area of surface, not that it increased as the surface increased. Mr. Symons could not help thinking that it would be strange if the assertion were right; because with a pressure of 20 lbs. on a 2-feet-square surface it would be something enormous on a very large building. The rule seemed to be absurd. A remark was made in the first Paper as to the storm of 1783, which, "according to Toaldo's observation, passed from Venice to Naples in three hours, equivalent to 96 miles per hour." Mr. Symons did not think that in 1783 means were available for identifying a storm; he thought it much more likely that there were two storms than one. It was further stated that "On the whole it seems probable that the greatest velocity of

¹ "Discussion of the results of some experiments with whirled anemometers." Proceedings of the Royal Society of London. Vol. xxxii., p. 170.

Mr. Symons. the air in a cyclone is not much more than double the greatest progression of the storm centre." After mentioning that men had been blown away, and other things of that kind, as to which he would say nothing, the Author added: "It is also on record that horses have been lifted up, but it is more probable that they were blown along the ground, since it would require a lifting power of about 90 lbs. per square foot to raise them." In a previous part of the Paper allusion had been made to a lifting power of 180 lbs. per square foot, and he was therefore unable to understand why the Author should object to one of 90 lbs. Another statement he thought required a little explanation. "The maximum lifting power of a whirlwind, so far as experience goes, probably was never more than 40 lbs. on the same test-plate." The Author had previously been referring to horizontal pressure, and this was a sudden change to vertical pressure, and he could not understand why 180 lbs. was first diminished to 90 lbs. and then to 40 lbs. Allusion was made in the Paper to the damage done to the Brighton chain-pier, but he was under the impression that the damage had been caused not so much by the wind as by the waves. The Author had further stated that "The roofs of railway stations, if not protected by locality, require precaution against wind, especially where it acts from below, and has no other means of escape than by lifting up the covering. The columns, upon which roofs are frequently placed, are seldom properly secured to a block of masonry of such dimensions as to be safe against extraordinary wind." If the members would study the photographs in the library, especially those of the Walmer storm, they would agree that there was considerable risk of station-roofs like those of Charing Cross and Cannon Street being lifted bodily from the walls. Looking at the way in which an ordinary private house was sometimes treated by a whirlwind he was rather afraid for those large station-roofs. In the Walmer storm the whirlwind passed over an ordinary two-storied house with a roof overhanging about 18 inches. The two windows of the upper storey in front were provided with common linen blinds. When the whirlwind passed it lifted the roof bodily and sucked one blind along with it. It fell outwards, in the direction in which the storm was going, and when the roof dropped down into its place again it shut the blind into the space between the top bricks of the wall and the roof. If an ordinary roof like that could be so lifted by the expansion of the confined air when there was only a comparatively small area inside, what would happen in the case of a similar storm attacking the Charing Cross Station? In illustration of such expansion he would

refer to a malting at Baldock wrecked by a whirlwind. It was a Mr. Symons. structure not more than four years old, about 130 feet long by 30 feet wide, each of the three ranges of maltings of which it consisted running down in the form of the letter E; being a cross building at one end with three ranges at right angles. The path of the whirlwind was parallel to the single line, or perpendicular to the three ranges. These ranges were empty at the time, and the effect of the wind on each of the three roofs was nearly identical, lifting the entire slated roof up, moving it horizontally 4 feet, and dropping it down again, the result being a complete smash. The end building was in two portions, not thoroughly separated. One portion was stored with malt nearly up to the apex of the roof, while the other was empty. The empty portion had much of the roof taken off while the full portion remained intact, not a tile being removed. The interpretation he had put upon it was that in the one case there was air which could expand and lift up the roof, and that in the other there was no air to expand, and consequently no damage was done. A similar interpretation would explain another remarkable case—the case of a pair of semi-detached houses three stories high, good ordinary buildings, which stood in the path of the Walmer storm. The curious thing was that the two outer sides were blown out, one falling towards the direction from which the storm was coming, and the other falling into the path which the storm was taking. The staircases were at the sides, and therefore there was a large space occupied by air all the way up the house, and possibly some of the room doors were open. The idea had occurred to him that the air in the staircases had expanded laterally and blown out the sides, there being a partial vacuum outside. He had tracked a dozen so-called whirlwinds in different parts of the country. Wherever he heard of one he went to see what had happened, and his impression was that a whirlwind was practically the same thing as a cyclone only much smaller. The essential principle of a cyclone was a diminished barometric pressure in the centre and a general indraught of air towards it, and he thought that whirlwinds were something of the same sort but much smaller, with probably a diminished barometric pressure in the centre, producing a vacuum which caused the expansion of air underneath. With regard to the report to the Board of Trade, he felt some regret in connection with it. The inquiry might have been as exhaustive as possible; but instead of that it had only resulted in the reproduction of half a dozen anemograms, and certain opinions which he should have

Mr. Symons. received with more respect if they had been more thoroughly backed up by evidence. A formula had been given computing the maximum pressure of the wind in any hour from the mean velocity during that hour. If it had been tried to compute the mean pressure from the mean velocity, that would have been a very different thing; but to take the maximum pressure from the mean velocity struck him as being a very risky proceeding. There were ample materials available for computing both the mean pressure from the mean velocity, and the maximum pressure from the maximum velocity. It had been suggested that it was desirable to have upon the same record-sheet indications of velocity and of pressure. That was done as far back as 1862, when Messrs. Negretti and Zambra made a form of Osler's anemometer adapted to give indications of both; but he believed that up to the present time those indications had never been worked out. At Greenwich Observatory there had been a continuous record of pressure and velocity; but he believed nothing had been done in the way of calculating the results. Indeed, he thought there was always more readiness to pay for apparatus than for working up results. There were many instances throughout the country of gentlemen purchasing elaborate apparatus and doing nothing with it. The Board of Trade prescribed an amount of strength sufficient to sustain a pressure of 56 lbs. per square foot in all cases. It was well known, however, that the pressure was much greater on the sea coast than inland, and if provision against a stress of 56 lbs. per square foot was proper, say at Holyhead or on the bridge at Saltash, it would be very different across a street, say in Leicester. The next point to which he would refer was the elevation of a structure. If he understood the diagram of the pier of the Bouble viaduct rightly, he believed the computations were made on the idea of the same pressure striking the column all the way up. He had found in the Journal of the Scottish Meteorological Society,¹ a Paper by Mr. Thomas Stevenson, M. Inst. C.E., of Edinburgh, who described some experiments made as to the relative velocity of the air at different heights above the ground. They were shown in Fig. 11, in which the vertical lines were drawn at intervals of 5 miles of velocity, and the horizontal lines at intervals of 10 feet of altitude above the ground. He had suppressed the observations close to the ground, because they would be palpably no guide. They only went to a height of 50 feet, whereas the Bouble viaduct was

¹ Vol. v., p. 348; and *post*, p. 210.

200 feet high; but the upper portions of the curves might be imagined. It would be seen that some startling results had been obtained. With low velocities there was not much change for the first 50 feet of altitude. With a velocity at 5 feet above the ground of 10 miles an hour, there would be 13 miles, or 30 per cent. additional at a height of 50 feet. That showed pretty clearly that at a height of 50 feet, even with a moderate wind, at least about 70 per cent. of additional pressure might be counted upon, than existed at 5 feet above the ground. Taking the greater velocity of 20 miles at 5 feet above the ground, there would be a velocity of 37 miles at 50 feet. If, then, the velocity was nearly doubled in the first 45 feet, it was pretty obvious that the pressure must be enormously greater at a height of 200 feet. He thought

FIG. 11.

VELOCITY OF WIND AT VARIOUS HEIGHTS ABOVE GROUND.

it would be most desirable to get the pressure on the top of the Crumlin viaduct, which was admirably situated for such an experiment. Some anemometers had been placed on roofs and similar places, where the result was the record of the accumulated wind which could not go through the building and must therefore go over the top. The only way of getting comparable results was to put the instruments upon a pole, or a sharp cone, or a pyramid. In his own case he had a pole 30 feet high, which he used to climb in order to take the readings of his anemometer; but as he found that, although it was at a distance of 120 feet from the house, the house, being 5 or 10 feet higher, spoilt the readings, he discontinued the observations. If he rejected an anemometer so placed,

Mr. Symons, *à fortiori*, a great many others ought to be rejected. He should like to refer to a Paper by Mr. Ashbel Welch, a member of the American Society of Civil Engineers, read before that body in October 1880.¹ In the course of the discussion, one of the speakers said: "It is surprising, when we examine the question of the pressure of the wind, to find how little we know, and how unreliable is that little." Another instance worth quoting was given of the destructive force of whirlwinds. A house, with 13-inch brick walls, was levelled to the ground. Every brick was removed that was more than 2 feet above the soil. In the course of a discussion upon the subject at the meeting of the British Association at York, under the presidency of Sir Frederick Bramwell, Mr. Thomas Hawksley, Past-President Inst. C.E., read a Paper "On the Pressure of Wind upon a fixed plane surface," in which he handled anemometers very severely; and in the course of the discussion, the question arose as to what should be done if such enormous pressures as had been quoted had to be provided against. Sir Frederick Bramwell had put it very neatly when he said that bridges would become practically impossible. He thought engineers should provide for any gale that happened or was likely to happen, but not for whirlwinds, for if they were to fight against whirlwinds, their structures would somewhat resemble pyramids: short of them he knew few buildings, except, perhaps, Highgate archway, which would be likely to stand. He could not suggest what was the pressure of an ordinary gale which engineers ought to provide against. There was a considerable amount of evidence on the subject, and it should be thoroughly threshed out. That had not been done, and that was why he complained of the Board of Trade inquiry. Reference had been made to the vacuum at the back of the pressure-plate, and it had been stated that the indication of the pressure-plate could not be taken because the plate caused a vacuum behind; and that the difference between the front pressure and the back should be observed. He was delighted to know that Mr. Osler was at present trying some experiments on that point; and he believed Mr. Osler had found that the back pressure was probably a very insignificant element, much smaller than had been previously imagined. Probably, therefore, it was not far wrong to assume that the pressures shown by anemometers were near the true pressure upon surfaces of small size and in those positions. At the same time he did not know what was to be made of the observations at Bidston. The

¹ Transactions of the American Society of Civil Engineers, vol. ix., p. 391.

Commissioners had quoted the results of Bidston Observatory, Mr. Symons. where pressures of nearly 1 cwt. to the square foot were obtained; and they stated that the circumstances, or conditions, or physical configuration applicable to the observatory were such that they were not likely to apply to ordinary structures. As far as he remembered (but he had not been at Bidston for many years) it was little more than the erection of a two-story building on the top of Primrose Hill; and he did not think that was a very exceptional position, in fact Primrose Hill was 33 feet higher than Bidston Hill. He thought that the Bidston observations ought to be thoroughly overhauled, and the apparatus tested. He had never used a pressure-plate. The only one he had had was an anemometer, which had been left to him by the late Mr. C. O. F. Cator. The maximum pressure recorded at Beckenham with it was, he thought, about 35 lbs. per square foot, but that was on the roof of a house, and therefore open to question. Practically there were no proper data upon the subject, because in nineteen cases out of twenty the instruments were placed in unsuitable positions.

Mr. B. BAKER considered that the Papers reflected considerable credit upon the Authors, but discredit upon the profession at large. After looking carefully through the first Paper and the manuscript, he could not ascertain that any recent or original experiments had been made, so that Mr. Bender had been driven to trot out the old experiments with which engineers had been familiar from their earliest days—experiments made by their great-grandfathers long before railways were contemplated, and none of them really bearing directly upon the question. It was impossible to argue from small experiments, such as throwing down balls from the top of St. Paul's, or observations on pressure boards the size of a hat, what should be done in the case of a large structure. In the parallel case of water absurd results had been obtained, until Mr. Froude began experimenting with full-sized vessels. Even the statements in Mr. Scott Russell's work now appeared to be nonsense in many respects. He had said, for instance, that the chief work of a propelling vessel was to excavate a trench across the Atlantic for a ship to travel in. That statement was based upon experiments conducted with wedge-shaped vessels and little round vessels by the Academy of Sciences, and others. They were made at about the same time and were on about the same scale as the experiments on wind-pressure cited by the Author, and they had about the same value. He remembered speaking on the subject during a steamboat trial with Mr. Froude, who said, "If a few yards of cloth like that of

Mr. Baker. your coat were stuck on the vessel instead of copper the extra skin resistance would more than equal the digging of the trench, which Mr. Scott Russell says is the chief work which the propelling power has to do." The change that had taken place in the practice of naval architects during the past dozen years had been due to a series of experiments with full-sized vessels, and he was certain that a similar result would follow in the proportioning of structures to resist wind-stresses if engineers would only do their duty, and giving up toy experiments make observations on the structures themselves. Mr. Bender had been compelled to reason from these ancient experiments, and to apply them to bridges, and he had arrived at results which must be wrong, because if they were right there ought not to be a bridge standing in the country. He had taken a 400-foot span lattice bridge of the American type—of course a very open bridge. The depth, he had stated, ought not to exceed 45 feet; but in dealing with the wind-pressure he had taken it as a solid structure 49 feet high. Reference had been made to the Saltash bridge as being in an exposed position; and certainly if wind-stresses had to be dealt with on Mr. Bender's principles this bridge ought to have tumbled down long ago.¹ He might also be permitted to refer to Landore viaduct, built by the late Mr. Brunel, thirty years ago. It was 110 feet high, with a span of 110 feet. A wooden truss rose about 16 feet above the level of the rails, and the whole of the wind-stress on the pier came upon a raking strut, 36 feet long and 9 inches square. No one would think of putting a strain of more than 2 cwt. per square inch on a pine strut 9 inches square and 36 feet long—a ratio of 48 to 1. But calculating wind-pressure according to the new rules of the Board of Trade, there would be one of 20 cwt. to the square inch; so that that bridge which had been standing thirty years had but one-tenth of the strength required by the new regulations. He had had the curiosity to calculate the stability of certain buildings erected in accordance with the Metropolitan Building Act. It was a singular fact that, having regard to steady pressure only and not blows, windows were, as a rule, the strongest part of a

¹ Taking the wind-pressure, as on Mr. Bender's 400-foot span, at 1,610 lbs. per lineal foot, the total pressure on Saltash 455-foot span = 327 tons, and the bending moment = 18,600 foot-tons. The moment of resistance of the 16 feet 9 inches $\times$ 12 feet 3 inches elliptical top tube which alone had to sustain this bending moment = 1,400; hence the stress per square inch = 13.3 tons. Adding the direct compression = 4.2 tons, the total stress = 17.5 tons per square inch, or $\frac{1}{3}$ more than the ultimate resistance to crippling of the tube.

house; the walls would go down with much less force than the windows. He had taken a two-story building and an eight-story building, erected in accordance with the Building Act, and had found that one would be blown down at $10\frac{3}{4}$ lbs. per square foot, and the other at 11 lbs. The windows, as a rule, would stand 40 lbs., and occasionally, according to his experiments, 70 lbs. pressure per square foot. Nevertheless from the photographs of the Walmer storm, to which reference had been made, it would be seen that in many cases the windows were smashed; and yet the walls which ought to go down with far less pressure stood, showing that the window, or the small area of a pane of glass, got a blow which did not affect the whole area of the building. That, possibly, might account for the fact that wind-pressure boards sometimes registered very high pressures, and yet weak structures in the neighbourhood did not fall down. Before any useful Paper could be written on the design of structures to resist wind-pressure, he thought that experiments should be conducted on a different scale, specially arranged to elucidate the matters which engineers had to take into consideration, and he was of the opinion that the experiments of a hundred years ago should not again be exhumed. A little was now being done by Mr. Fowler and himself in the way of experiment at the Forth bridge, and if everyone would help a little, some useful data might be obtained. They were putting up a pressure board 20 feet long and 15 feet high, and by the side of it there would be ordinary pressure boards of small sizes (1 foot square), so that they would be able to ascertain the relationships between the large and the small area. Mr. Bender thought that the large area would offer more resistance per square foot than the small, but Mr. Baker, for reasons based upon the ascertained unsteadiness of fluid motion, thought it would be less. If leaves and other light objects floating in an apparently perfectly steady current were watched it would be found that certain leaves would shoot forward at an increased velocity of 25 per cent. and upwards as compared with the mean velocity. Over a width of 20 feet at the centre of a wide and steady current the mean velocity might thus be constant, whilst over some particular width of 1 foot it might be momentarily fully 25 per cent. higher, and in the case of wind-pressure 25 per cent. increase of velocity meant more than 50 per cent. increase of pressure. It was quite possible therefore that the large pressure board might register a notably less pressure than the small boards, and might afford a clue to the reason why railway carriages were not upset when travers-

Mr. Baker. ing lofty and exposed viaducts. In a few months they would know all about it. The large board and one of the small boards would be fixed square to the known direction of the heaviest storms in the Forth. One of the small boards would swivel to face the wind as usual, and a comparison of the relative pressures registered by the fixed and moving small boards would enable a conclusion to be formed as to what the pressure on the 300-foot board would have been had it faced the wind in the same manner as the swivelling board. When they had finished with the plain board, he proposed to put a ledge round it, to see what effect it would have, turning it into a box, and then by putting some vertical divisions he intended to make it represent the side of a complete girder. Afterwards the board would be taken away, and a portion of lattice girder in wood put in, to see what that would give in equivalent square feet. They would also try one surface behind the other, so as to get, if possible, some absolute facts as to the pressure on sheltered surfaces. There were other ways in which engineers could help, such as by measuring the bridges themselves. In the case of large bridges with horizontal wind bracing, nothing could be easier than to measure the extension of the bars due to a gale. Taking a length of 10 feet, and laying a piece of hoop iron along it with a graduated scale, they could easily read to the thousandth of an inch. As the bar extended in proportion to the strain, they could read the strain on the wind-bracing to within 2 cwt. per square inch, so that the exact measure might be ascertained at very little cost. The maximum movement of the scale could be read after every storm or at any time in the same way as maximum and minimum temperatures were read by thermometers. The bridge would be made to speak for itself, showing what the strain was when wind of moderate or intense pressure was blowing. Dutch engineers had made experiments of an analogous kind with a bridge of 426-feet span. There were twenty-four observers, who measured simultaneously the extensions of the principal members of the bridge, so that they could check their calculations, and, when a train passed over, see what the extension of each member was. If that were done with the wind-bracing, useful results would be obtained. Many other ways would suggest themselves to engineers. Where, as in the case of Tay Bridge, girders were lying idle, it would be easy to sling a girder with springs at the side, and measure the absolute pressure of the wind. Although the report of the wind-pressure commission was not so satisfactory as it might have been in the way

of experiments they were not worse off than others. American Mr. Baker, engineers (of whom Mr. Bender was a representative) had spent a thousand millions sterling in building railways, but, as far as public evidence went, they had not spent a dollar in making a single experiment on wind-pressure. And in the Paper of Professor Gaudard (who represented continental engineering) the only experiments alluded to were old and familiar ones. On the question of admissible stresses, he thought that Mr. Bender took a rather exaggerated view of the requirements, his calculations being at discord with all existing work. He had stated that the stress should not exceed 5 tons per square inch on wind-bracing calculated in the manner advanced by him. Working that out according to his figures it would be found that a bar 8 inches wide and 1 inch thick would be necessary for a comparatively small bridge; and probably on looking at an existing bridge of the same size it would be found that it had not any bar there at all. One very important point arose in connection with compression members. According to the Board of Trade regulations, the same strain was admissible in compression as in tension. As the ultimate resistance of the metal in the top flange of a girder was much smaller than that in the bottom, that meant, of course, that a smaller factor of safety was allowed with metals in compression than in tension, and he thought it quite right that it should be so. He had broken a great many girders in testing machines, and he had also had girders fail under actual work. He had found that girders of ordinary proportions invariably failed by flexure of the top flange in the testing machine; but in actual working it was the reverse; they always failed by tension. A girder of ordinary proportion generally failed by fatigue of some part of the metal, and fatigue affected the parts under tension a great deal more than the parts under compression. The practice of the Board of Trade was justifiable in that respect, because a smaller factor of safety was admissible. They would allow 5 tons per square inch on both the top and bottom flanges of wrought-iron girders, although the bottom flange of the girder would stand 20 tons, which gave a factor of 4, and the top flange 12 or 14 tons, a factor of less than 3. With regard to struts, some interesting experiments had been made by him during the previous week. A steel column 8 diameters long failed by flexure with a stress of 29 tons to the square inch. A column of similar material 30 diameters long failed with 16 tons. Another column made of the same steel, also 30 diameters long, which he had tested a quarter of an hour afterwards, stood the original stress of 29 tons. After they were

Mr. Baker. tested as struts, the columns were tested for tension. They all had exactly the same tensile resistance, and the same elongation; yet one failed as described with 16 tons compression and the other stood 29 tons. He had not hardened or tempered the steel in any way, but had raised the ultimate resistance from 16 to 29 tons in less than ten minutes, simply by putting a severe end pressure on the steel before testing it as a long column. The squeeze did not reduce the tensile strength or the elongation, which was 19 per cent., but it had the marvellous consequence of increasing the strength, as a column, 80 per cent., without affecting the steel in any other way. He thought that was a most suggestive experiment from which results of great practical importance would follow. A column of the same size in wrought iron or cast iron would have failed with about 12 tons, but the steel column, though far more ductile than either, and with a modulus of elasticity practically the same as wrought iron, stood 29 tons. He did not think that a column of the ratio of 30 to 1 had ever reached that point before, and honour was due to Dr. Siemens whose metal it was.

Mr. Langley. Mr. A. A. LANGLEY stated that he was at the present time constructing the large goods station of the Great Eastern Railway at Bishopsgate. Last October a piece of brick walling was blown down, which led him to take observations as to the pressure of the wind at this place, which was in a most exposed situation, being higher than any other building for a considerable distance. He accordingly made an anemometer (Fig. 12) and placed it in the position objected to, namely, over a roof at a height of about 60 feet above the level of Bishopsgate Street. This was in December last, since which time there had been some heavy storms; but the instrument had never registered more than 40 lbs. on an area of 4 square feet, or 10 lbs. on the square foot. He regretted that the disk was not larger; it was square and measured 2 feet each way. The disk was connected in the centre with a sliding rod which revolved horizontally upon a vertical metal pipe. When the disk was pressed forward by the wind it acted upon a chain which passed over a wheel and down the centre of the pipe, and was attached to a strong spring balance, the needle of which pushed forward a pointer, which registered the maximum pressure on the disk. A long box in which the rod and chain worked acted as a weathercock, and kept the disk always facing the wind. He had tested the gauge by weights so as to pull the disk in, and had found it registered correctly to within 15 per cent. This error, he thought, was in consequence of the friction of the parts, but the momentum of the disk and rod might possibly counterbalance this.

The horizontal movement of the disk when pressed by a strong Mr. Langley. wind was about 1 inch. He thought that the great pressures talked of, 60 lbs. per square foot and more, were never felt over

FIG. 12.

WIND-PRESSURE GAUGE.

Longitudinal Section.

Scale 1 inch = 1 foot.

any considerable area. Half London would be blown down with any such pressure. Indeed he thought that a wind-pressure of 20 lbs. would destroy a great many buildings and works.

Mr. T. SEYRIG observed that in discussing the effects of wind Mr. Seyrig. upon structures or on railway plant, the accidents which had happened on the Southern Railway of France were often mentioned. Mr. Bender probably referred to them when he said that "it was not known whether any number of carriages were overturned by

Mr. Seyrig. the wind when formed into a train." A few details concerning the facts relating thereto might consequently be of interest. The first in date were the occurrences on February 27th, 1860, which took place very nearly at the same instant. At Salces a passenger train was caught by a gust of wind and nearly entirely overturned. It was made up of four carriages and two brake vans, one in front and one behind, besides the engine and tender. The front van and the four carriages were overturned, the tail van not upsetting, but being dragged across the line. A close examination into the weights and dimensions of the vehicles gave the following results, which indicated the effort necessary to overturn them :—

No.	—	Weight.		Necessary Side Pressure.		Observations.
		In kilograms.	In lbs.	In kilograms. per sq. metre.	In lbs. per sq. foot.	
1	van	6,015	13,257	165	33·8	Overturned.
2	carriage, 1st and 2nd class	7,360	16,221	147	30·2	"
3	" 3rd "	7,360	16,221	147	30·2	"
4	" " "	6,865	15,130	149	30·6	"
5	" " "	7,614	16,781	166	34·0	"
6	van	6,915	15,240	242	49·7	Not upset.

The same day, at Rivesaltes, a freight train made up of fourteen wagons had five of them upset, three thrown off the line, and the remaining six left on the rails. The occurrence had particularities enough to deserve special mention, which would be best tabulated.

No.	—	Weight.		Necessary Side Pressure.		Observations.
		In kilograms.	In lbs.	In kilograms. per sq. metre.	In lbs. per sq. foot.	
1	Van	6,220	13,933	251·8	51·5	Left on the rails.
2	Truck	4,360	9,610	2,753	56·4	Thrown off the track.
3	"	14,360	31,650	9,073	185·8	Left on the rails.
4	Covered wagon	5,700	12,563	130	26·6	Upset.
5	"	9,270	20,431	212	43·5	Left on the rails.
6	"	6,640	14,635	151	30·9	Two wheels off the track.
7	"	5,700	12,563	130	26·6	Upset.
8	"	10,595	23,351	242	49·7	Left on the rails.
9	"	5,700	12,563	130	26·6	Upset.
10	"	5,700	12,563	130	26·6	"
11	"	5,700	12,563	130	26·6	"
12	Van	11,875	26,173	417	85·4	Left on the rails.
13	Truck	14,325	31,573	9,043	185·0	" "
14	"	4,325	9,533	2,731	56·0	Thrown off the track.

The last known accident on the same railway occurred at Fiton, on the 5th of December, 1867, a few miles north of the preceding

stations. There again it was a passenger train, made up of six Mr. Seyrig. carriages and a van, which was nearly entirely overthrown. The van alone remained standing, but was thrown off the rails and placed crossways over them. The numerical data concerning them were:—

No.	—	Weight.		Necessary Depressions.		Observations.
		In kilograms.	In lbs.	In kilogs. per sq. metre.	In lbs. per sq. foot.	
1	Carriage, 1st and 2nd class	6,280	13,845	126	25·7	Overtured.
2	„ 3rd class . . .	5,874	12,949	143	29·2	„
3	„ 1st and 2nd class	6,767	14,917	136	27·8	„
4	„ „ „	6,767	14,917	136	27·8	„
5	„ 3rd class . . .	6,770	14,925	148	30·3	„
6	„ „ . . .	6,770	14,925	148	30·3	„
7	Van	6,954	15,330	252	51·7	{ Thrown off the rails.

A few points of interest might be gathered from these facts. The maximum pressure, in the occurrences at Salces and Fitou, certainly exceeded 30·6 lbs. per square foot, and most probably exceeded even 34 lbs., the carriage No. 5 requiring that pressure being also upset, notwithstanding its connection with the following van which remained standing. At Rivesaltes the necessary side-pressure was generally somewhat less, being about 26·6 lbs. for the overturned wagons. But here occurred the singular fact of two wagons (Nos. 4 and 7) upset while those to which they were coupled in front and behind remained standing. Probably, therefore, the pressure was considerably greater. It would seem that No. 6 had been on the point of upsetting, the heavy one preceding it (No. 5) keeping it down, a pressure of 30·9 lbs. being again reached as in the other accidents. No plausible explanation however could be given now, nor was given at the time, for Nos. 2 and 14 leaving the track. It must be remarked over what space the heavy gusts which caused these accidents were spread, at the very least. The length of the passenger trains was from 125 to 150 feet, while the length of the goods train was about 300 feet. It might be of use to bear this in mind when the way in which heavy gusts of wind manifested themselves was discussed. No locomotive had ever been overturned on any French railway by the wind. The way in which the wind acted seems still open to much investigation. All calculations of its effect upon fixed structures supposed a steady flow with a certain velocity producing

Mr. Seyrig. a certain pressure. But never perhaps was this the real mode of action during storms. Observation of a dusty road, or of a space covered with dry leaves, when a storm arose showed at once that the currents were extremely variable, so long at least as they were near the surface of the earth, however flat and uniform it was. Objects floating in the air at the same height seldom, if ever, moved along together. Balloons having the same starting-point always separated and travelled with very different velocities, to say nothing of the varying directions at slightly different heights. It might be easily observed in crossing a bridge in an exposed position how some parts were hardly affected by a gust, while others were at the same moment subject to its most violent efforts. Even at sea this unequal distribution of the currents was nearly constantly observable. Under such circumstances it was easy to conceive of a girder bridge with two continuous spans, being pivoted on the pier, where the greater part of the weight lay. This question of the distribution of currents of air over any given space was, however, an open one. It appeared of vital interest to the designer, and it ought to be a subject for experiment. Ought not an appeal be made to engineers who had charge of lofty structures, such as viaducts or large bridges, with a view of establishing thereon apparatus for observing simultaneously in different points the velocities or pressures of the wind? The currents of air in different points of the cross-section of a valley could surely be investigated as easily as the velocities in a large river. Why not choose such places as the Britannia and Conway bridges for observations of this sort? If the economy in future structures which possibly might be the result of such observations was not thought an object of sufficient interest, to ensure the safety of existing engineering structures ought to warrant a little painstaking and some expense in the matter. But not only the extent of the gusts, their frequency also deserved observation. A repetition of efforts was noticeable in a storm, which might act, to a certain degree, like a series of waves on a beach. Stone structures might not be affected by such repetitions, but metallic structures, more especially high piers, on the top of which the heaviest lateral effort acted, would be under the special influence of such repetitions, an oscillation being produced which might double the strain in the metal, if not more, should its period of oscillation coincide with the repetition of the gusts.

The question could be left in the hands of physicists as to what formula best expressed the relation between velocity and pressure. But if it were possible to make an instrument which

would correctly register the pressure of the wind, the practical Mr. Seyrig. difficulty which engineers had to contend with would be solved. One of the most recent efforts in that direction had been by Mr. Bourdon. He had contrived an anemometer, the principle of which was not new, but which had never found any practical application. It was that of the tube of Venturi. This physicist, knowing that the outflow of water from a reservoir increased when a divergent mouthpiece was added, applied his researches to the determination of that form which should increase the outflow in the largest proportion. He hit upon the form represented in Fig. 13, in which

FIG. 13.

the divergent cone had its length equal to nine times the smallest diameter, the angle of the sides being about 5° . Applying pressure-gauges to different points of the divergent tube, he found that all along the cone suction was occasioned by the flow of water, and that this suction was greatest at the summit of the cone. Mr. Bourdon's experiments had led him to recognise that a current of air produced exactly the same effect as a flow of water. In a tube having the above-mentioned form, if two small water-gauges were inserted at A and B, it would be seen that a difference of pressure existed; and H , H_1 , being the heights of the water columns which measured the pressures at B and at A, then

$$H_1 = K_1 H.$$

If a second tube was placed inside the first tube, having the same form as the first, but much smaller in dimensions, and so situated that the extremity of the divergent cone corresponded to

FIG. 14.

the narrowest part of the first cone, a new effect would be produced. The velocity of the current at A would set up suction in the second divergent cone, and, considering this second one, then

$$H_2 = K_2 H_1$$

Mr. Seyrig. H_2 being the difference of pressure (or suction) corresponding to the velocity at C; combining the two equations $H_2 = K_1 K_2 H$.

A third multiplication could easily be made, augmenting anew the difference between H_3 and H . In measuring the velocity of

FIG. 15.

wind the pressure H was the atmospheric pressure; and one measurement, that of the suction at C, gave at once H , from which, if necessary, v could be deduced.

The point of interest was the determination of the coefficients K_1 and K_2 . They depended on the form of the instrument, and must be determined by experiment. A very complete series had been carried out by Mr. Bourdon, and repeated for verification by Mr. Murgue, who applied this anemometer to the ventilation of a mine at Bessèges. Mr. Bourdon had obtained the following results. The results were translated into English measurement:—

Velocity V in feet per second.	Theoretical Pressure $H = \frac{v^2}{2g}$ in Inch of Water.	Depression H_1 at A.	$K_1 = \frac{H_1}{H}$.	Depression H_2 at C.	$K_2 = \frac{H_2}{H_1}$.	$K = \frac{K_1 K_2}{\frac{H_2}{H}}$.
18.7	0.079	0.236	3.00	0.945	4.00	12.00
22.6	0.118	0.394	3.33	1.457	3.70	12.32
26.2	0.158	0.551	3.50	1.969	3.56	12.46
33.5	0.286	0.787	3.07	2.953	3.75	11.51
37.0	0.315	1.063	3.37	3.940	3.70	12.47
41.6	0.394	1.378	3.50	4.910	3.57	12.49
51.1	0.590	2.087	3.53	7.470	3.58	12.64
55.5	0.708	2.245	3.16	8.660	3.85	12.13
58.7	0.787	2.559	3.29	9.840	3.84	12.48
61.6	0.866	2.795	3.22	10.630	3.80	12.24
65.6	0.984	3.268	3.32	12.400	3.79	12.58
Maximum			3.50	..	4.00	12.58
Mean			3.29	..	3.74	12.30
Minimum			3.00	..	3.56	11.51

It must be remarked that the velocities were here given by deductions from the theoretical formula, not from direct measurement. The pressures therefore were alone exactly comparable with each other. As would be seen the variations from the mean appeared proportionately high, but these variations were most

marked under low velocities, the higher ones, from 40 feet upwards Mr. Seyrig. being nearer the mean than those under 40 feet. Considering the rather rude make of the instrument these results might certainly be considered very satisfactory. Mr. Murgue had repeated the experiments in two ways. He first placed the instrument in a wide iron tube, through which a current of air was blown by a steady-going ventilator, which current was measured by a Combes anemometer. He next placed the anemometer in one of the galleries of the mine, where the velocity of the air current was well known, having often been ascertained by divers means, as a standard. The results were tabulated below:—

I. EXPERIMENTS in an IRON TUBE.

Velocities in Feet.	Corresponding Pressures $\frac{v^2}{2g}$ in Inch of Water.	Total Depressure H_2 in Inch.	$K = \frac{H_2}{H}$.
5.93	0.0080	0.046	5.85
9.65	0.0209	0.192	9.20
12.70	0.0358	0.413	11.57
16.06	0.0574	0.712	12.32
18.70	0.0783	1.055	13.46
21.61	0.1047	1.374	13.11

II. EXPERIMENTS in the GALLERY of a MINE.

Velocities in Feet.	Corresponding Pressures $\frac{v^2}{2g}$ in Inch of Water.	Total Depressure H_2 in Inch.	$K = \frac{H_2}{H}$.
7.52	0.0126	0.099	7.87
9.86	0.0217	0.214	9.90
12.40	0.0342	0.349	10.19
14.15	0.0444	0.476	10.70
17.20	0.0657	0.787	12.01
18.50	0.0763	0.960	12.53
20.15	0.0905	1.220	13.49
20.55	0.0937	1.209	12.88
22.20	0.1102	15.390	13.99

From these results it would appear that the values of H_2 increased very regularly with the velocities. They could be arranged in formulæ, which the two series of experiments gave as followed :

$$1. H = 0.0415 (0.093 v^2 - 0.457 v + 0.562) \quad . \quad (A)$$

$$= 0.00386 (v^2 - 4.91 v + 6.05)$$

$$2. H = 0.0425 (0.093 v^2 - 0.518 v + 0.723) \quad . \quad (B)$$

$$= 0.00395 (v^2 - 5.57 v + 7.79)$$

Mr. Seyrig. But it must be observed that these expressions were based upon the knowledge of v , which had in the experiments at Bessèges been measured as exactly as possible, while Mr. Bourdon, in the first series mentioned above, obtained by direct measurement the pressures H and H_2 . For observations on wind-pressure this direct measurement would evidently be of great advantage. Like all machines or apparatus for obtaining evaluations of stress which were not direct weighing machines, this one necessitated experiment with a view of determining the coefficients or constants which were

FIG. 16.

- A Venturi tube with vanes, capable of turning with the wind.
- B Second Venturi tube, placed inside the anterior part of the first.
- C Pivot of the entire system.
- D Tube from the neck of the second Venturi tube to the interior of the hollow pivot, transmitting the variations of pressure.
- E Supports of the pivot.
- F Cap over the bearing.
- G Surface of roof on which the apparatus is erected.
- H Cock for transmission of pressure to the lateral tube, during the rotation of the pivot.
- K Clockwork fastened at the lower end of the pivot, causing the pencil L to advance from the centre to the circumference in twenty-four hours.
- L Pencil for registering the direction on the horizontal paper disk.

BOURDON'S ANEMOMETER.

required in its use. They were, however, of so simple a nature, so independent from any mechanical function, and so invariable, that there could be no fear in using them when once accurately determined, much less than in the use of an aneroid barometer or a Robinson anemometer. The instrument might therefore be regarded as most reliable. For anemometric observations, registration would be necessary in most cases. This problem had also been solved by Mr. Bourdon in connection with his instrument. The tube being mounted on a pivot (Fig. 16) which passed through the roof of the

observatory, its lower end carried a small clockwork, actuating a Mr. Seyrig. pencil, which was thus moved away from the axis of the pivot to a certain distance within twenty-four hours. It traced a line on a fixed disk placed beneath its point, and it was easy to conceive how a combined record was thus kept of the time and of the direction of the wind. The registration of the pressure was more complicated. As had been shown, the essential point was the measurement of the

FIG. 17.

- A Watertank.
- B Inverted glass bell.
- C End of tube from the anerometer causing the rise and fall of the bell according to variations in the pressure.
- D Clockwork giving motion to the plate E.
- E Plate with sheet of registering paper.
- F Supporting column.
- G Pulleys on which runs the cord for balancing the pressure bell.
- H Registering pencil fixed to the lever M.
- I Balancing spring for keeping the cord N under tension during the movements of the pressure bell.
- K Bracket fixing the lower end of the spring I.
- M Movable arm, pivoting on O and carrying the pencil H.
- N Cord fixed to the bell B, causing the pencil H to register the movements due to variation of pressure.

FIG. 18.

FIG. 19.

BOURDON'S REGISTERING APPARATUS.

Scale $\frac{1}{10}$.

partial vacuum produced at the neck of the smallest tube. For this purpose the tube was there separated into two parts, which were surrounded by a small closed tube, inside which the same pressure existed as at the neck. This was made to communicate with a gauge tube D leading to the point where the actual measurement should take place. In the disposition adopted by Mr. Bourdon this tube was connected with the pivot C of the anerometer, which

Mr. Seyrig. was hollow, and passing through it went down to a cock H near its lower end, from whence again it went to the pressure-gauge. In the first apparatus (Figs. 17, 18, 19) this was a water-gauge or bell,

FIG. 20.

NEWEST FORM OF REGISTERING APPARATUS FOR BOURDON'S ANEMOMETER.

Scale $\frac{1}{16}$.

- a a'* End of the pressure tube from the anemometer.
- a a* Manometric case, expanding or contracting under varying pressures.
- b b* Supports of transmitting levers.
- d f* Levers transmitting motion to the registering pencil.
- i* Registering pencil.
- g g* Levers causing the pencil *i* to move in a straight line.
- h* Drum revolving in twenty-four hours and carrying the registering paper.
- r* Baseplate on columns, supporting the manometric case.
- A* Pivot of the anemometer with vanes.
- B* Eccentric cam causing displacement of the levers *C C*, one of which is also lifted by the cam, the other one only marking at the time.
- C C* Levers carrying the registering pencils.
- D* Connection of levers, ensuring their parallel movement.
- E G* Pencil marking a zero line on each registering paper.
- H* Drum on which the registering paper is stretched, revolving in twenty-four hours.
- R* Baseplate with supports, over a box containing the clockwork.

the movement of which was registered on a revolving disk, turning once in twenty-four hours. This gave satisfactory results, but the inverted bell over water produced several inconveniences, which

Mr. Bourdon's last apparatus seemed to have overcome, Fig. 20. Mr. Seyrig. The tube was now connected with one of his manometric cases *a*, which expanded or contracted as the pressure varied. The movements of the tube were easily transformed by a series of levers *f, g*, into the vertical displacement of a pencil, which traced its marks on a cylinder of paper *h*, turning with a uniform velocity, by means of clockwork. It might be feared that the volume of air to be expanded by the suction at the neck of the anemometer would be so great as not to permit of rapid registration of the gusts of wind. Practice had shown these fears to be unfounded, and the sharpness and rapidity of the marks proved that the rarefaction of the transmitting air took place almost instantly. An elegant solution had been given at the same time to the registration of the direction of the wind. The pivot of the anemometer was connected with a cam *A* leading two levers, *C C*, with pencils attached to them, marking their position upon a paper which passed horizontally beneath them; but owing to the variable thickness of the cam, one of the pencils only marked at a time, although they were displaced together. It then happened that one pencil marked all winds *S*, and the other pencil marked all winds *N*. The registration was thus more easily read than it otherwise would be. It might be asked what advantage this instrument had over already known anemometers? It might be termed an advantage to possess a simple instrument, which had, in itself, no movable parts liable to deterioration. The registering part must be considered apart from the anemometer, and in this the absence of all mechanical combinations was certainly advantageous. Again, the amplification of the pressures, allowing of a more precise measurement, would be useful, more especially when low velocities, such as those in mine galleries were to be measured. For higher velocities, as strong gusts of wind, its simplicity, and its immediate adaptation to the existing pressure, would be esteemed. There was no inertia of moving parts, and no friction to overcome, as in the different systems of revolving or pressure anemometers. When the registering portions of the apparatus were submitted to the action of the suction, these frictions or other retarding forces would have less effect and less inconvenience, owing to the multiplication of the pressure which there produced movement. It would seem, therefore, that for the measurement of wind-pressures, which more especially interested the engineer, rather than velocities, this instrument possesses qualities which might be of great use.

Mr. J. B. REDMAN said that in 1875, at Chatham, he had taken Mr. Redman. some pains in collating the experimental knowledge relating to

Mr. Redman. wind, and certainly the results were so extraordinary, comparing one with the other, that a reference to them might not be out of place. He then referred to Mr. Scott's analysis of those varying estimates. In the Royal Charter gale 16 lbs. had been quoted by Mr. Burder. According to Smeaton, a velocity of 50 miles gave 12 lbs., 60 miles 17 lbs., 70 miles 24 lbs. pressure per square foot. The old estimate by Smeaton was 1 ton to the square foot of roofing, which was 22·40 lbs. per square foot. Tredgold gave a maximum velocity of 159 feet per second, or 108 miles per hour, and a pressure of 57 lbs. per square foot. These varying results indicated the necessity that existed for a more complete set of experiments.

Mr. am Ende. Mr. M. AM ENDE observed that the present mode of dealing with the question of wind-pressure on structures seemed to him to be characterized by a desire to obtain accurate figures for the maximum pressure, rather than to establish a sound rule for the calculation of the areas of structures. No doubt the idea was that the pressure must first be found before establishing any such rule; but wind-pressure was so uncertain and so complicated a thing that he doubted whether it was possible to arrive at any definite result in that way. What he proposed was to establish a rational method of taking the areas of structures, and to try it on existing structures which had been known to withstand very high winds. In this way a figure for the wind-pressure would be found which a given structure had never experienced during its existence. If the method were tried also on structures which had been blown over, it would yield a wind-pressure which must have been reached at least on one occasion. From a number of results of both kinds it would be possible to ascertain a value useful as a basis for calculation more surely than from experiments with anemometers. Occasionally statements were met with like this—that the hurricane which destroyed the bridge over the Tennessee in 1876 must have had a force equal to 100 lbs. to the square foot, but such statements were not of much use if the mode of calculating the area were not stated, since these modes differed very much from each other. Mr. Bender's mode of calculating the area seemed to be to take every part by itself and multiply it with a coefficient according to its shape, and those coefficients varied from $\frac{1}{4}$ to $\frac{3}{4}$. That might be a very accurate way, but it was impracticable, because no one would ever take the trouble of ascertaining every part for itself. The probability was that, for structures of a certain class, such as bridges, an average coefficient existed, for roofs another, for chimneys another, and so on. In Professor Gaudard's Paper there were two methods stated for taking out the area, and he begged to

FIG. 21.

HALF ELEVATION OF RAILWAY BRIDGE OVER THE RIVER PACUARE, COSTA RICA.
Scale $\frac{1}{16}$ inch = 1 foot. Area of elevation, 850 square feet. Area of projection, 2,040 square feet.

Mr. am Ende. mention that he had himself had them both under consideration. The first method he had explained in "Engineering," in 1880, p. 173. He had stated a series of four members, because he had assumed that the structure treated there was one of four surfaces. Professor Gaudard's coefficient k was in this case assumed to be $= 1$. This could be done because k approached unity if $\frac{\sigma}{s}$ was large, and $\frac{\sigma}{s}$ in this case was $\frac{3}{4}$; besides, even when $k < 1$, although it then had a great influence on the sum of a series of two members, it would almost have lost it on one of four members. The other method he had stated in an article on the choice of the coefficients for wind-pressure (Centralblatt der Bauverwaltung, 1881, p. 259). It was to take the most unfavourable projection of the structure on a plane at right angles with the wind-pressure. That was probably the most practical method. For surfaces with identical openings placed one behind the other the projection would be equal to the sum of the series already mentioned, but the projection gave a very simple account also of different surfaces placed one behind the other, as in the case of a railway train, or the platform intervening. In a bridge represented by Fig. 21 he had assumed the wind-pressure acting at an incline of 1 in 4 upon the structure, the ordinary elevation of which was indicated in dotted lines. The projection was shown in continuous lines. It appeared to be a simple structure, but nevertheless the area represented by the continuous lines was about $2\frac{1}{2}$ times as large as that represented by the dotted lines, while according to a rule proposed by the Commission on Wind-Pressure $1\frac{3}{4}$ time, and according to an older one $1\frac{1}{2}$ time, would be assumed for this case; in many structures the projection was 3 or more times larger than the ordinary elevation. The bridge just referred to had two girders 255 feet long and 43 feet high. The greater part of it was only 14 feet 6 inches wide, but at the supports it was 21 feet wide. He had made the design and calculation of stability in 1878 (*i.e.*, before the fall of the Tay Bridge) on the method just described, taking 70 lbs. as the unit pressure. This was also the figure for the extreme pressure on structures he still assumed for calculations. The bridge had been in existence only two years in a tropical climate, and could not therefore give evidence of the sufficiency of the assumption. Considering how little extra expense as a rule was required to obtain extra stability, he doubted whether it was necessary to stipulate that different figures should be used for different localities. Another question discussed in the Papers was how, having ascertained the force of the wind, and the forces acting on the structure,

to ascertain the strains; but he thought that it was not a suitable Mr. am Ende. subject for discussion at this Institution. There was another point of interest, viz., the choice of coefficients for calculating the strength of structures, and upon that point very little had been said in the Papers. It was now considered rational to allow different coefficients for different kinds of loads, one coefficient for the fixed load, because it was permanent, one for the moving load, because it was frequently recurring, and one more might be allowed for the wind-pressure, because it was rarely recurring. If, for example, it were desired to make 6, 4, and 8 tons per square inch the coefficients for the strains arising from these loads, and if f , m , and w were the corresponding strains in any part, the coefficient for that part would be

$$\frac{6f + 4m + 8w}{f + m + w}.$$

This, however, would be applicable only in the case of the strains acting in the same sense.

Sir W. G. ARMSTRONG, President, desired to express his strong Sir W. G. opinion of the urgent necessity for experimental investigation in Armstrong. connection with wind-pressure. The important point to ascertain was whether the extremely high pressures that had been undoubtedly recorded in various parts of the country by reliable instruments, really operated over large surfaces, or only on very small surfaces. That, he thought, might be easily tested by having a line of anemometers representing the length of a large structure—say a large bridge—so as to find whether the maximum pressures operated at the same time throughout the whole length, or only at particular points of the structure. If a current of wind could be seen in the same way as a current of water, it would probably be found very different from what had been imagined. Owing to impediments on the surface and other causes, it would probably be of a very irregular character. There would be streams flowing with different velocities at different parts; those different velocities, and the obstructions upon the ground generating numerous eddies, travelling along with a progressive motion, combined with a rotative motion; and it was easy to see that when those eddies struck upon an impediment, that side of the eddy which had a rotative motion in the same direction as the progressive motion, would cause an impact represented by the progressive force, plus the rotative force; whereas on the opposite side there would be the progressive force minus the rotative force; therefore within a short distance there would be a great difference in the recorded

Sir W. G. Armstrong. pressure. His impression was that the very high pressures recorded did not operate upon large surfaces. He did not see, for example, how the Crystal Palace, exposed as it was, could stand with anything like the pressures he referred to operating on a large surface. As to Bidston Observatory, he did not know what sort of a building it was, but it was absolutely impossible that it could stand a pressure of 90 lbs. per square foot. A series of experiments to determine the matter would save an enormous unnecessary expenditure, and he thought it was well worth consideration, whether the Institution could not take measures for securing a proper series of observations to determine that important question.

Mr. Airy. Mr. WILFRID AIRY desired to draw attention to the statement in Mr. Bender's Paper. "The indications of all these instruments are unreliable, because the wind changes its direction so rapidly, and because it is not a uniform stream of air which has to be encountered, but a succession of gusts." He wished to ask in what respect Mr. Bender considered the indications of the instruments unreliable. The object was to record the force of the wind, whether it blew in a succession of gusts, as it commonly did, or in a uniform stream of pressure. He held that the registrations were quite correct; and he had never talked with any one on the subject who was able to say in what respect they were incorrect. He thought that the extreme pressures registered, 50, 60, 70, 80, or 90 lbs., were reliable. It was an undoubted fact that the period of time during which the extreme gusts lasted was very small, and of course, a question might arise as to whether the shortness of the time affected the applicability of those pressures to the stability of structures. He thought the duration was sufficient. In the case, for instance, of a bridge or viaduct supported on cast-iron columns, it would not take such a column half a second to snap, and he thought that that was plenty of time for the pressure of the wind to be brought to bear upon it. It might also be remarked that successive gusts of wind caused a swaying motion in a structure, and if it fell it was not from the effect of a single gust, but from the accumulated effect of several. On all grounds, therefore, he held that the pressure applicable to structures was the extreme force of the gusts. Mr. Bender, in fact, seemed to admit as much, for he accounted for the accident that happened to a bridge by attributing it to a gust of whirling and upward motion. He had looked at the data upon which Mr. Bender based the coefficient of 33 lbs., and he thought they were insufficient for the purpose. Professor Gaudard adopted 55 lbs. and upwards as the coefficient applicable to structures,

and he had quoted pressures of 60 and 70 lbs. registered at Liver- Mr. Airy.
pool which he appeared to regard as the pressures to be taken
as affecting structures. Professor Gaudard also stated, "Certain
structures may be liable to be wholly overturned by a gust of
wind." It was therefore clear that he took gusts of wind as the
ruling pressures applicable to structures, and that he was not at
one with Mr. Bender as to the coefficient which should be adopted.
What then was the lateral extent of surface over which those
extreme pressures might be supposed to act simultaneously? The
extreme pressures recorded had been registered with instruments
having a pressure-plate of 1 square foot or 2 square feet in area,
and the question was whether the extreme pressures recorded on
those small surfaces were applicable per square foot to structures
several hundred yards in length. Until that question had been
settled the coefficient that ought to be applied in formulæ con-
cerning the stability of those structures was absolutely undeter-
mined, and he held that neither 33 lbs. nor 55 lbs., since that
question had not been settled, could be considered as reliable.
The question was an extremely difficult experimental one, and he
was not aware that anything had been done towards its solution.
He thought the best evidence was that of seafaring men, who
were accustomed to note carefully the effect of the wind in the
open sea; and he should like to quote the experience of a captain
who, when sailing on the Atlantic, observed that at the moment
when his ship was struck by an extreme gust, and had her sails
carried away, another ship about a mile distant was also struck
and had her sails carried away. That, he thought, was conclusive
to show that the direction of the wind was perpendicular to the
line joining the two vessels, and that the crest or face of the
gust was of the full length of a mile. He had no evidence of
his own to offer on that point, but from the evidence that he had
been able to collect he had come to the conclusion that in open
firths and bays, and in the open sea, the lateral extent of heavy
gusts was considerable. With reference to the formula in the
report of the Wind Pressure Commission connecting the probable
maximum force of the wind with the maximum hourly run of the
wind during the same storm, it was clearly enough stated that it
was entirely empirical, and was formed from a comparison of a
great number of recorded facts. A paragraph in the report clearly
stated the scope of the formula.

"For some stations the only published information is the run
in miles of the wind during each hour. There can obviously be
no more than a general accordance between this and the greatest

Mr. Airy. pressure experienced during the hour. . . . In the case of high winds, with which alone we have to deal, it was found that the greatest pressure recorded in an hour was tolerably well proportional to the square of the mean velocity during the hour, and that the empirical formula $\frac{V^2}{100} = P$, where V = maximum run in miles of the wind in any one hour, and P = maximum pressure in pounds on the square foot at any time during the storm to which V refers, represented very fairly the greatest pressure as deduced from the mean velocity for an hour."

It was very natural to assume that a wind of a high mean velocity should contain within it high extremes or gusts. It was probable, for example, that a wind whose maximum hourly run was 60 miles an hour should contain gusts of higher pressure than another wind whose maximum hourly run was only 40 miles, and this general sort of reasoning was found to be fully borne out by the observed facts. He thought the formula might be regarded as useful and reliable, and he certainly considered that a formula that had the approval of such men as Professor Stokes of Cambridge, Professor Grant of Glasgow, and Professor Stone of Oxford, might be regarded as a very tolerable makeshift, at least until some more accurate and scientific formula had been deduced by meteorologists, whose peculiar province it would seem to be. His name had been mentioned by Mr. Baker with reference to the anemometers to be constructed in connection with the Forth Bridge. Mr. Baker had fully explained the scope of the experiments that were to take place, and he could only say that he believed that some very useful information might be obtained by means of them.

Mr. Douglass. Mr. JAMES N. DOUGLASS remarked that Professor Gaudard had given some comparative calculations relative to wave-pressure and wind-pressure on exposed sea towers. He thought, however, that no correct general estimate could possibly be formed of the wave-pressure on such structures. Nearly every case required special consideration. It was necessary first to consider the geographical position and the extent of ocean fetch; secondly, the depth of the adjoining sea; thirdly, the gradient from the sea bottom; and fourthly, the elevation of the foundation with regard to high and low water. He thought the maximum wave-pressure would be found to be considerably in excess of that given by Professor Gaudard, owing to the greater height at which the pressure was exerted on those structures than that on which the calculations had been based. Many instances might be cited of

heavy wave-pressure at considerable elevations, where the conditions were favourable. At Longships lighthouse, off the Land's End, at an elevation of 86 feet above high water, panes of glass 2 feet square and $\frac{3}{4}$ -inch thick, had been driven in by the force of the sea. At Bishop Rock lighthouse, off the Scilly Isles, a fog-bell had been torn from its brackets at an elevation of 100 feet above high water. At Smeaton's Eddystone Lighthouse the granite gallery, with the lantern and the optical apparatus, weighing 61 tons, had been fairly lifted by the sea; and the pressure exerted there, at an elevation of 60 feet above high water, could not have been less than 1 ton per square foot. At Calf Rock lighthouse, on the south-west coast of Ireland, during a storm of last winter, the iron tower was broken off at a level of 86 feet above high water. So that it might be seen that heavy wave-strokes occurred at very considerable elevations. In nearly all cases they were far in excess of any pressure that could be exerted by the wind on such structures. There was certainly great difficulty in arriving at any general estimate of the wave-force exerted on structures of that kind; while in the case of wind the force could be fairly estimated.

Mr. ROGERS FIELD said it seemed to be pretty generally admitted that the question of wind-pressure was in an unsatisfactory state, but he thought there were two or three important points which, if properly understood and thoroughly grasped, would help very much to clear matters up. Foremost amongst them was the question of the increase of wind-pressure with elevation above the ground, which appeared to him to have been much neglected. This had been known in a general way, and it had been stated in meteorological works that at the top of Mount Washington, for instance, there might be nearly a gale blowing, while it was calm at the base. Mr. Bender had also quoted an instance of a balloon travelling 70 miles an hour while it was calm on the ground. He was not, however, aware of any precise data on the subject before those of Mr. Stevenson which Mr. Symons had brought forward, and which appeared to be of the greatest value. It would be seen from Figs. 11 and 32 that the greater the elevation above the ground, the greater became the velocity, and that the increase was tolerably uniform. Looking more closely into the details, it would be noticed how reliable the experiments were. On referring to the records given in the Journal of the Scottish Meteorological Society, he found that the anemometers were tried thus: a tall pole was set up in a field sown with oats, and the anemometers were fixed on this pole at different heights. At the commencement of the

Mr. Douglass.

Mr. Field.

Mr. Field. observations no grain was growing in the field, but in the series V. and VI. the grain was 12 inches high, and in VII., VIII., and IX., it was 3 feet high. It would be seen, on looking at the curves VII., VIII., and IX., that they had a flatter inclination at the bottom than any of the other curves, showing clearly the effect of the grain in retarding the velocity near to the earth's surface. Engineers, however, wanted statistics of pressure more than velocity, and he had therefore taken curve V.—the one that gave the highest velocity—and reduced it to pressure. Different formulæ had been promulgated for reducing velocity to pressure, but most writers on the subject agreed with the formula given by Mr. Hawksley, in his Paper read before the British Association last year, in which he made the pressure proportional to the square of the velocity. This could not of course be strictly accurate, because other factors must enter into the question, such as the density of the air (which would be affected by the temperature of the air and the height of the barometer); but for practical purposes it was sufficient. In Fig. 22 he had not given the actual pressures, but the ratios of the pressures as more convenient for use in other cases. The lowest point shown and datum (100) was $3\frac{1}{2}$ feet above the ground, because that was where the observations began, except in the case of one or two scattered ones. It was also about the average height of the pressure experienced by a man walking on the ground against a wind. He had shown by a strong black line the curve, as far as the observations of Mr. Stevenson went, and by a dotted line the general trend of the curve above it. Assuming the datum of $3\frac{1}{2}$ feet, it would be seen that at 23 feet above the ground there was twice the pressure; at 60 feet, three times; and at 120 feet, four times the pressure. Of course $3\frac{1}{2}$ feet was a small elevation, and one that would not generally be taken into consideration, but any other could be easily adopted from the diagram. At 10 feet above the ground as the datum, the pressure would be doubled at 72 feet, and trebled at 160. At 15 feet as the datum, the pressure would be doubled at 95 feet, and so on. Fig. 22 would also reconcile many apparently contradictory data. For instance, Mr. Bender quoted as evidence of the maximum pressure of the wind that in extraordinary gales men could hardly remain upright, or were driven backwards, and calculated that a man in an upright position could only bear 12 lbs. per square foot, and in a stooping position $22\frac{1}{2}$ lbs. Fig. 22, showed that a wind-pressure of 12 lbs. per square foot against a man walking on the ground was equivalent to a pressure of 48 lbs. at 120

feet high, and that $22\frac{1}{2}$ lbs. against the man was equivalent to Mr. Field. 90 lbs. at 120 feet high. The above data, therefore, instead of invalidating the high pressure recorded by anemometers, confirmed their accuracy for high elevations. The question of increase of pressure with elevation had not been sufficiently considered by engineers, and it was clear from the curve, even regarding it only as a very general indication of what took place, that no pressure could be assigned which was suitable for all elevations. Assuming a pressure of 56 lbs. per square foot to be right for an elevation of 200 feet, there would only be a pressure of about 20 lbs. at 15 feet. It was therefore a radical error to talk

FIG. 22.

INCREASE OF WIND-PRESSURE WITH HEIGHT.

about a particular pressure that a bridge or any other structure ought to bear, without stating the height of the structure. Another point that wanted to be clearly understood was with reference to Robinson's cup anemometer. The pressures quoted were very frequently not measured directly by pressure-plates, but calculated from velocity measurements, and it was therefore of great importance to know how those measurements were taken, and how far they were reliable. A specimen of the standard (Kew pattern) instrument used in the Observatories, was exhibited to the members. There was a distance of 4 feet from centre to centre of the cups, and the cups themselves were 9 inches in diameter. The cups revolved

Mr. Field. at a less velocity than the wind, and the rate was multiplied by a certain factor to find the velocity. It might appear to be rather a startling statement, but he should be able to show that it was strictly true, that nearly every observation by that instrument of a high velocity which had been quoted in England was about 20 per cent. wrong. The instrument was invented more than

FIG. 23.

ROBINSON'S CUP ANEMOMETER, KEW PATTERN.

Crystal Palace, 1872

Robinson, 1878

Dohrandt, 1878

thirty years ago by Dr. Robinson, who stated that the wind moved at three times the velocity with which the centre of the cups moved, so that if the cups travelled 1 mile the wind might be reckoned as going 3 miles. That rule was adopted by all the instrument makers, who made a dial at the foot of the instrument, registering three times the velocity of the cups, and called the

registration of this dial the velocity of the wind. At all the Mr. Field. Observatories, and notably at Kew, where the anemometers were sent to be tested, the calculations were made on that assumption; but it was now admitted by almost all who had studied the subject that the multiple of three was about 20 per cent. wrong. He had prepared Fig. 23, showing the results obtained by experiment compared with those calculated by Dr. Robinson's rule.¹ The first important series of experiments were those made on a steam merry-go-round at the Crystal Palace in 1872, by Mr. Jeffries, and by Mr. Whipple of the Kew Observatory. They showed that the multiplier 3 was wrong; but these experiments had only recently been published. In 1874, Dr. Dohrandt, of St. Petersburg, made a long series of experiments, and found that the ratio 3 to 1 was wrong.² On the publication of his results, Dr. Robinson found great fault with them, and said he would try a series of experiments of his own to show that Dohrandt's results were wrong. He obtained a grant from the Royal Society, and tried an extensive series of experiments at Dublin; and the results, as would be seen by the diagram, almost exactly agreed with those of Dr. Dohrandt's, who had in the meantime carried out another series of experiments.³ The results obtained at the Crystal Palace in 1872 were subsequently given to Professor Stokes to reduce, and his conclusions had been published in the Proceedings of the Royal Society last year,⁴ and agreed almost precisely with the experimental results of Drs. Dohrandt and Robinson. It would be seen from Fig. 23 that the experimental results all showed that Dr. Robinson's rule was wrong for high velocities. For instance, a velocity of 15 miles per hour of the cups represented, according to the experimental results, about 36 miles per hour velocity of the wind instead of 45 miles per hour as assigned by Dr. Robinson's rule. If 36 miles an hour (wind) was divided by 15 miles an hour (cups) it gave a quotient 2.4 instead of 3, showing that the factor 3 was about 20 per cent. too high. For low velocities, Dr. Robinson's rule was nearer the experimental results, but no constant factor would apply for low velocities as the friction of the instrument then affected the results. The error of 20 per cent. in the velocities recorded at Observatories was a serious matter, as it meant an error of about 35 per cent. in the pressure calculated from the velocities, the

¹ Quarterly Journal of the Meteorological Society, vol. v. (1879), p. 212.

² Repertorium für Meteorologie, vol. iv. Bestimmung der Anemometer-constanten, von F. Dohrandt.

³ Repertorium für Meteorologie, vol. vi., 1878.

⁴ Vol. xxxii., p. 170.

Mr. Field. pressures being as the squares of the velocities. He did not consider it creditable that an error of such magnitude should be allowed to exist, but as long as it did exist engineers should be aware of it and allow for it. The above remarks only referred to the Kew pattern. If the length of arms or size of cups were varied, the multiplier varied. The last point to which he desired to refer was the Report of the Wind Pressure Commission. He considered the rule there given for calculating the maximum pressure was a very risky one, and he had prepared a Table which would show unmistakably that it was so.

RATIO OF MAXIMUM to MEAN PRESSURE OF WIND.

—	Hour.	Mean Pressure during the Hour.	Maximum Pressure during the Hour.	Ratio of Maximum to Mean.
		Lbs. per Square Foot.	Lbs. per Square Foot.	
Glasgow, January 23rd, 1863 .	4—5	13·8	36·2	2·6
„ „ .	5—6	15·5	38·8	2·5
„ „ .	7—8	13·5	31·0	2·3
„ „ .	10—11	9·8	30·8	3·1
„ „ .	2—3	11·3	40·0	3·5
„ December 28th, 1879 .	6—7	19·0	35·0	1·9
„ „ .	7—8	24·5	37·0	1·5
Greenwich, Jan. 16th & 17th, 1872	1—2	2·8	30·0	10·7
„ „	2—3	2·9	30·2	10·4
„ „	4—5	3·4	42·0	12·3
„ „	5—6	3·8	37·6	9·9
„ „	6—7	3·6	42·0	11·6
„ „	8—9	4·0	42·0	10·5
„ „	10—11	3·6	41·6	11·6
„ „	11—12	4·0	36·0	9·0
„ „	12—13	3·8	45·0	11·8
„ „	15—16	3·2	43·0	13·4
„ Aug. 28th & 29th, 1878	19—20	2·2	35·0	15·9

The formula given in the Report was $\frac{V^2}{100} = P$, in which V represented the mean velocity of the wind in miles during an hour of maximum run as registered by the cup anemometer, and P the maximum pressure in lbs. on the square foot at any time during the storm to which V referred. From the mean velocity it was no

doubt practicable to calculate the mean pressure, but that was Mr. Field. quite a different thing from calculating the maximum pressure, and unless the maximum bore some tolerably constant ratio to the mean pressure, it would not be practicable to calculate it from the mean. The usual rule for calculating the pressure corresponding to a given velocity was $\frac{V^2}{200}$, so that on the face of it the formula of the Wind

Pressure Commission, $\frac{V^2}{100} = P$, meant that the maximum pressure

was double the mean hourly pressure. This was, however, not quite so, as V in the formula represented the velocity as recorded by the cup anemometer, which, as previously shown, was about 20 per cent. too high for the Kew pattern. After making an allowance for that, the formula in the Report signified that the maximum pressure was about three times the mean. In order to test the matter he had prepared the Table giving the ratio of maximum to mean pressures. He had taken the diagrams given at the end of the Report of the Wind Pressure Commission, and had taken out a number of the mean pressures and the maximum pressures corresponding to them, including only those above 30 lbs. to the foot. He had had some difficulty in compiling the information. He should have liked to take the Bidston pressures, on which the formula was based, but, unfortunately, the diagram of the Bidston pressures, as given in the report, was wrongly drawn, and he had therefore been obliged to take those of Glasgow and Greenwich. With regard to Greenwich, there were some curious discrepancies. It was stated in the report that the instruments did not record pressures greater than 42 lbs. to the square foot, and that the maximum pressures exceeded this, and both in the Tables and Diagrams for January 16th and 17th, 1872, the pressures were shown as more than 42 lbs. In the printed official records of the Observatory, on the other hand, it was stated that the maximum pressure on the same day was 30 lbs. per square foot. Which was right he was unable to say. The matter required clearing up; but the natural supposition was that the official records were right. Returning to the Table of the Ratio of Maximum to Mean Pressure of Wind, it would be seen that the ratio of the maximum to the mean hourly pressure was by no means constant. In a few cases the ratio was that assumed in the formula adopted by the Wind Pressure Commission, but it varied immensely—in fact, from $1\frac{1}{2}$ to nearly 16, so that it was impossible to form any rule on that basis. Until the matter had been further investigated it would be unwise to calculate the pressures by the rule given in the Report.

Prof. Unwin. Professor W. C. UNWIN said his study of the action of wind-pressures on structures dated a long time back. He believed that the action of the wind on structures was first treated as a perfectly simple fluid pressure normal to the surfaces acted on in some lectures at Chatham in 1868.¹ At that date he took as the probable maximum intensity of wind-pressure, for which structures should be designed, 40 lbs. to the square foot, a limit not very different from the most modern accepted view on the subject. He also disintombed at that time the old experiments of Hutton and others, made for gunnery purposes, on the relation of pressure on inclined surfaces to that on surfaces struck normally by the wind, in order to find out some rule for determining what ought to be allowed as the pressure on the inclined surfaces of roofs. Shortly afterwards his rule for finding the proper allowance for wind-pressure on a very large roof was tried in the case of a riding school. The engineer who was most responsible for it thought that Professor Unwin must be mistaken as to the relation of pressure on an inclined surface to that on a vertical surface, and he was obliged to seek additional evidence for the rule which he had obtained from Hutton. He mentioned the matter because he had found some experiments which had not been quoted in that connection, but which appeared to be most valuable in relation to the pressure of fluid on inclined surfaces. Just before the time to which he referred, Mr. Spencer Browning and Mr. Wenham made some experiments in Messrs. Penn's Works at Greenwich, for the Aeronautical Society. They took a plane 2 feet square, and placed it opposite the mouth of a large blowing fan, and they arranged to measure the two rectangular components of the pressure on the plane, the pressure in the direction of the wind, and that at right-angles to it. All the earlier experiments measured only one component, from which the normal pressure on the surface had to be inferred. Combining the components measured by Mr. Wenham and Mr. Spencer Browning, the resultant pressure on the surface was found to be, as it should be, almost exactly normal to the surface. The results on an inclined surface, down to an angle of 15° , agreed almost exactly with Hutton's rule, or, still better, with a rule he should now prefer, mainly on the ground that it was very simple, the rule of Colonel Duchemin.² He mentioned it because he observed in Mr. Bender's Paper the statement that, for pressure on inclined surfaces it was not possible to obtain any correct mathematical

¹ Memorandum on the effect of wind-pressure on roofs. By Professor W. C. Unwin. Inst. C.E. Tracts, 8vo., vol. 245.

² J. V. Poncelet. *Traité de Mécanique Industrielle*.

expression. At all events, for practical purposes, Hutton's rule or Prof. Unwin's the other was quite adequate. He thought that when engineers heard that on the anemometers at Holyhead, Bidston, and other places, pressures of 60, 70, 80 and 90 lbs. per square foot had been registered, they were inclined to think that there was some error. Structures which, in their belief, would not stand such pressures, had stood for many years, and hence he believed they were inclined to explain the discrepancy by abusing the anemometers. It might, therefore, be well to look for a moment at those respects in which the pressure registered by the anemometer was likely to be different from the pressure experienced by an engineering structure. In the first place, an anemometer had an exposure such as very few engineering structures were likely to have. Any one who knew the difficulty of finding a good site for an anemometer, would know that the cases would be rare in which engineering structures had what would be called a good site for that purpose; one might, therefore, expect that engineering structures would generally be exposed to less pressures than those registered by the anemometer. In the next place, anemometers were placed at exceptional heights. Mr. Field had given data upon that point, and he would therefore say nothing more about it. In the third place, the dimensions of an anemometer were probably small, compared with the size of the eddies of wind, while engineering structures were probably large in comparison with the size of those eddies, and he thought that was the reason why there should be a somewhat lower pressure on engineering structures than that registered by anemometers; on the whole, he thought that too much had been made of the question of gusts, because there was ample evidence of winds blowing for a period of at least an hour, with a velocity high enough to give a very large pressure; and the wind blowing for an hour with a mean velocity of 82 miles was not likely to have any very narrow lateral spread. He thought there must be winds blowing steadily for sensibly long periods, over a large extent of surface, which would give tolerably high pressures. Nevertheless, an engineering structure would in general be exposed to an average pressure on its surface, which in the worst conditions would be lower than the greatest pressure recorded by anemometers. Of course where the exposure of a structure was exceptional, the engineer, to be on the safe side, must assume a high value for the wind-pressure. There was one other point to which he wished to refer. In the pressures recorded on a pressure-plate anemometer, or in the pressures which were deduced from a velocity-anemometer, two

Prof. Unwin. separate things were mixed up; there was a front pressure on the front of the plate, and a negative pressure behind the plate. In the case of a great many engineering structures, while they were subject to the full front pressure which would be sustained by an anemometer similarly exposed, the complex form of the structure and the obstacles behind the surface on which the wind acted broke up the vacuum behind the surface, and there was not in such structures so large a negative back pressure as on a simple exposed plate. To show how much influence the negative or back pressure had, he had drawn a diagram from Mr. Berthon's careful experiments on pressures at different points of a cylinder exposed to a current of fluid (Fig. 24). The results were plotted to scale. The radius vector of the curves measured the observed pressure, positive on the front, negative for the rest of the surface. The direction of stream lines round a cylinder was shown in Fig. 25. Where the stream lines were concave to the surface, the pressure must be less than in the undisturbed fluid. It was obvious from Figs. 24 and 25 that if, in an engineering structure, the negative or

FIG. 24.

NORMAL PRESSURE ON CYLINDER. BERTHON.

FIG. 25.

back pressure was not fully developed, there would be a much less total pressure than if it were fully developed. In the case at all events of many engineering structures, the full negative back pressure that would be obtained on an anemometer-plate, was wanting. With regard to the relation between pressure and velocity, some time ago he was consulted about a weir to be built across a river in India where the water falling from the weir would at certain times in the year impinge on the base of the weir from a height of 70 feet. It was questioned whether the rock at the base would stand the impact of water from that height, and he was asked what was the probable maximum intensity of pressure at the base. He wrote to

say that the maximum pressure could not be greater than the pressure due to the height corresponding to the velocity. While his letter was going to India, Mr. Beresford, who designed the weir, made some interesting experiments. He allowed a jet of water to impinge normally on a plate, and he obtained the pressure curves. For an ordinary small jet the pressure at the centre of the jet was almost exactly the pressure due to the height corresponding to the velocity, that was the weight of a column of water equal to the height from which the water descended. The pressure extended in a decreasing way over an area four times the area of the jet. The way in which the pressure decreased from the centre was shown in Fig. 26. The normal acceleration of the water due to

FIG. 26.

FIG. 27.

FIG. 28.

the curvature of the stream lines produced an accumulated maximum pressure at the centre of the jet. The ordinates of the dotted curve in Fig. 27 gave the pressures at different distances from the

Prof. Unwin. centre of the jet, the pressure at the centre being very nearly equal to the head of water over the plate. If, now the size of the jet was increased relatively to the plate (Fig. 27), the variation of the pressure was less, and when the section of the jet was as large as the plate (Fig. 28), the mean pressure was not much less than the maximum. There was, in fact, over the whole area of the plate, a pressure nearly corresponding to the height due to the velocity. If the plate were supposed to be put in an indefinite fluid, there would be over the whole of the plate a pressure still more nearly corresponding to the height due to the velocity, and from that could be deduced the relation between the pressure and the velocity. If V was the velocity of the wind in miles per hour, then $1.466 V$ would be its velocity in feet per second. The velocity height would be $\frac{(1.466 V)^2}{2g}$, and the limit to the value of the pressure on a flat surface struck normally would be $G \frac{(1.466 V)^2}{2g}$ where G was the weight of a cubic foot of air. Putting $G = 0.0807$, the front pressure on plate = $0.003 V^2$. No

FIG. 29.

theory could give exactly the back pressure, but it could not in any case exceed the front pressure. Hence the theoretical limit to the whole pressure on the plate was $0.006 V^2$. According to the rule adopted by Mr. Scott, the pressure on a plate was $0.005 V^2$, or only one-sixth less than the limiting pressure found by a simple theory. Fig. 29 indicates the pressure corresponding to different velocities calculated by the rule $P = 0.005 V^2$, and also the part of the whole pressure which was probably

front pressure. The curves showed the rapid way in which the pressure attained great values when the velocity increased beyond 100 miles per hour. It was worth while to refer to the matter, because it would permit the discussion of one point in Mr. Bender's Paper. Mr. Bender evidently looked with favour upon a rule which he had found in some of the older experiments, which seemed to suggest that the pressure per square foot on a surface increased with the area of the surface; that on a large surface there would be a greater pressure per square foot from a given wind than on a smaller surface. He thought that as the pressure on the ordinary pressure-plate anemometer was found to reach $0.005 V^2$, and as the greatest pressure to be expected, if the front and back pressure were equal, would be $0.006 V^2$, there was no room for any large increase of pressure on the surface with the increase of area. He imagined that the pressure per square foot would be much the same as that obtained on a larger surface. One point which appeared to him to be novel was that Mr. Bender seemed to think it possible to divide an engineering structure into small parts—round, square, flat, channel shape, and so on—and calculate the pressure on each of them separately, then adding the pressures together to find the total pressure on the surface. He thought the way in which wind acted upon such structures would hardly permit of that course being adopted, and that sufficient allowance had not been made for the very great degree in which each member of a structure would affect the direction and velocity of the impact of the wind upon the members surrounding it. He imagined that a round bar standing amongst a great number of other bars would have the direction of the current impinging so affected, that it would not give quite the same pressure as a round bar taken away from the structure and exposed to the wind. He did not think a point had been arrived at at which engineers could take a structure, and give different coefficients to each part according to its form, and then calculate the whole pressure on the surface. It ignored the fact that each member of the structure affected in some degree the direction of the currents on other parts, and that especially the parts behind the part struck affected the development of the negative pressure behind. In illustration of how far that process of cutting up a structure might go, he would call attention to a remark in Mr. Bender's Paper about ventilators on the top of a railway carriage: "The ventilators increase the wind-surface, not only because of their height, but also on account of their wind-catching form, and thus considerable additional wind-surface occurs just where it most impairs the

Prof. Unwin.

Prof. Unwin. steadiness under side-pressure." The ventilators of a railway carriage he supposed were on the top, and some way back from the face of it, and instead of catching the full impact of the wind they lay almost completely in a wind shadow. The wind in striking the face of the carriage was driven upwards, and probably the ventilators were never subjected to any pressure at all. That was an extreme case, in which the cutting up of structures led to considerable error. It would be admitted that there was a great deficiency of data. For some years past he had been thinking in what way engineers could get data more adapted to engineering purposes than those obtained by anemometers, and he thought that observations should be made at a great number of points on structures. For engineering purposes it was quite as necessary to know the distribution of the pressure as its maximum intensity. To make observations on structures, at a great number of points, it would be necessary to have some cheap kind of wind-pressure measuring apparatus; and as it would be impossible to employ a man to wait for a gale, so as to be able to make observations just when the pressure was highest, some kind of self-registering anemometer would be required. He did not know that he had found any solution of the question, but he ventured to throw out a hint which might be useful, and he mentioned the matter because he had not now very good opportunities for trying experiments of that kind, and others might have better means of doing so. The simplest measuring apparatus was the old water siphon, which had been used more or less frequently for measuring the pressure of wind. The only thing necessary to make it useful for getting pressure at a number of points of a structure was to make it self-registering. The way in which that could be accomplished was the following:—Having a small mouthpiece which could be placed anywhere, through slits in a roof or on the face of a bridge, and desiring to get the pressure on the mouth of the plate, a flexible tube could be taken from it to a siphon-gauge containing water, and the rise of the water would measure the pressure on the face of the structure. To render it self-registering it had occurred to him that, instead of using clear water, water with a small amount of bi-chloride of platinum might be used. A clean brass wire with an adjusting screw was fixed vertically in the siphon tube, and as the platinum solution rose it would blacken the brass wire, and so a register might be obtained of the maximum pressure to which the gauge was subjected.

Mr. Peggs. Mr. J. W. PEGGS exhibited diagrams of three factory chimney-shafts that were blown down in the gale of October last. He had

at that time collected a number of cases of structures that were destroyed, and he had endeavoured to ascertain the pressures of the wind from the failure of such structures.¹ Some of the most interesting examples were stacks of house-chimneys, generally erected above the ordinary house-level, in London, the pressures found ranged from 17 lbs. to 20 lbs. per square foot. The chimney-shafts, as might be expected, developed much higher pressures. He had been much struck by the observations with regard to the increase of pressure according to elevation. In many engineering structures that was a very important matter to be considered. Where there was a very tall factory chimney-shaft the pressure should not be taken at one figure; but an increasing pressure in going towards the top should be allowed for. One example was a small shaft built into the structure. By calculating the weight of the brickwork he found that the pressure developed was 31·4 lbs. per square foot; in another case the shaft was rather higher and the pressure was increased to nearly 35 lbs. per square foot. In the case of a chimney-shaft 127 feet 6 inches high, which was much higher than the other two, the pressure was 40·5 lbs., showing clearly that the pressure increased according to elevation. During a series of experiments that he had carried out at the Kew Observatory, he noticed that above the dome of the Observatory the anemometer was always running at a greater velocity than the one on the experimental shed. Even when the latter was almost at a stand-still, there would be a motion of the former. With reference to the lifting action of whirlwinds, he might be permitted to refer to a Paper in the Minutes of Proceedings of the Institution, in which Colonel Pasley mentioned a certain roof at Chatham that was lifted up and carried away.² It was very important for engineers to know that upward pressure was often in excess of horizontal pressure. He did not think that the roofs of the Cannon Street and Charing Cross railway stations would be carried away. Being so open at the ends with apertures along the sides the vacuum would be rapidly filled up and the roofs would stand. Of course, in the case of a dwelling-house, or a house fairly wind-tight, the upward pressure might lift the roof. With all the anemometers at present in use the horizontal component of the wind could be measured. It was well known that at certain times the wind did not blow in horizontal lines, but might be inclined upwards, or downwards, and he thought that in any anemometer

¹ Quarterly Journal of the Meteorological Society, vol. viii. (New Series), p. 29, and plate iv.

² Vol. i. (1839), p. 33.

Mr. Peggs. for registering pressure or velocity the inclination of the wind should also be taken into account.

Mr. Pearsall. Mr. H. D. PEARSALL said that Mr. Bender had stated in his Paper that the extreme lifting power of the wind did not probably exceed 40 lbs., but Mr. Pearsall thought that there were many instances in which the pressure had been greater, and that 40 lbs. was certainly too low an estimate for the maximum. In one case that had come under his observation the lifting power of a whirlwind was clearly demonstrated to be over 136 lbs. per square foot. A small iron lattice-girder bridge, weighing 9 tons, was then lifted *en masse* and carried a considerable distance. The bridge was over a canal at Arrab, near Sasseram, Bengal. When the wind struck it there was no platform, and the actual area on plan exposed to the wind was only 160 square feet. Dividing the weight of the bridge by that area, there was a lifting power of 136 lbs. per square foot on the whole area. The circumstances very clearly showed that it was a lifting power and not a horizontal pressure. The bridge was carried away 25 feet in a horizontal direction before it fell as much as 12 feet, and of course a horizontal force which could accomplish that would have been enormous—very much greater than the lifting power required. On the stone abutments on which the lattice girders rested directly, without bed-plates, there was not the smallest abrasion, and no other effects of the violent wind were evident in the line in which the bridge moved. In a nearly vertical direction some damage was done. Of course the mean pressure of 136 lbs. implied a much greater pressure on parts of the structure, and the maximum pressure must have been of very great intensity. He did not know that such high pressures had been recorded in this country, but several cases of equally high pressures in the United States were on record.¹ With reference to the maximum pressure to be allowed for, it appeared to him that those accidents were quite as important factors as the records of any anemometer.

Mr. Barlow. Mr. W. H. BARLOW, Past-President, said that, with regard to the formula in the report of the Wind Pressure Commission, which appeared to have been traced to some kind of relationship between the pressures on anemometer boards and the mean velocity of the wind in the hour, the decision arrived at was in no way dependent

¹ "The Iowa and Illinois Tornado of May 22, 1873." By James Mackintosh, M.A. The Quarterly Journal of Science, vol. iv. (New Series), p. 339. And "Wind Pressure upon Bridges." By C. Shaler Smith. Transactions of the American Society of Civil Engineers, vol. x. (1881), p. 139.

upon the published Table, which was merely put in as showing Mr. Barlow. the kind of coincidence appearing from previous observations. As to the mode of measuring pressure, it had been pointed out by Professor Stokes, in his evidence at the Tay Bridge inquiry, that there was a marked difference in the apparatus used. Receiving pressure upon the aperture, and measuring it by water, was what he had termed "standard pressure," which represented pressure upon the actual surface; but in dealing with the pressure on the board there was not only the back pressure referred to by Professor Unwin, but there was friction round the edges, and there were other causes of resistance. Professor Stokes assigned a very large amount to these; and he considered the relation between the pressure on the board and the pressure derived from an instrument of that sort, as showing a difference of from 40 to 80 per cent. That was a very important point to bear in mind, because there had been a good deal of misapprehension in the minds of engineers as between the pressures recorded in different ways. There was an utter confusion in the Tables in regard to the pressures recorded by water-gauges and those recorded by boards. The greatest pressure by water-gauge had been recorded by Dr. Lynn, the inventor of the original instrument, in a storm at Edinburgh—about $6\frac{1}{2}$ inches of water, or 34 lbs. pressure. There was some confusion even with regard to the term "anemometer." The whirligig exhibited to the members was called an anemometer, and the flat board with a spring on it had the same designation. The disk anemometer recorded occasionally as much as 80 or 90 lbs. pressure. It was certainly an important fact to notice, that, while the greatest recorded result in one instrument was 34 lbs., 80 or 90 lbs. had been recorded with other instruments. Even at Greenwich, a sheltered place, as much as 40 or 50 lbs. pressure had been noticed. While the standard pressure recorded by the aperture was considerably below that which an engineering structure would offer to the wind, the results obtained at Bidston and other observatories were quite as much above. It had been stated that the pressure or velocity increased with the height above the ground, and within certain limits that was true; but it was not true where an anemometer was placed on the top of an observatory, in such a manner as to be liable to create eddies and whirls, which actuated the instrument itself. If the wind was blowing 50 miles an hour, and an eddy was occasioned revolving at 20 miles an hour, the eddy travelled along with the wind, and there might be 70 miles an hour on one side of the scale, and only 30 miles on the other. He believed it was those spurts of wind, in the nature of eddies, which produced

Mr. Barlow. the remarkable effect upon anemometers; and he thought that Bidston Observatory was more subject to that drawback than any other. It was for this reason that results had been obtained which were inconsistent with practical experience. He was glad to hear a reference by Mr. Peggs to the question of chimneys. He had looked a good deal at that question, and he thought that chimneys were very good practical anemometers. At Glasgow there was a chimney 454 feet high, on the top of a hill, exposed to as much wind as Bidston Observatory, or any other place. It had been carefully calculated by Professor Rankine, and the strength was estimated to sustain 55 lbs. per square foot of area. If that was correct, there was certainly a limit to the force of the wind there, at any rate for many years. Mr. R. H. Scott, at the Tay Bridge inquiry, was asked how it happened that that chimney was still standing, and his reply was that its time had not come. He apprehended that, although great pressures might be correctly enough found upon small anemometer boards so placed as to collect and register eddies, such effects would not be obtained upon large structures; but he certainly thought it was possible to obtain higher pressures than 33 lbs. It was very difficult to get hold of real facts, and very difficult to estimate them aright when obtained. He observed that Professor Rankine, in his calculation of 55 lbs. for the Glasgow chimney, had allowed nothing for the tenacity of the mortar. On the other hand, he had taken into his calculation that the pressure upon a round surface was only half that upon a square surface. Nor had he taken into consideration the fact that tall chimneys of that kind swayed about a great deal in the wind. Probably putting one thing against another his figure was a fair result; but a great deal had to be taken on faith. In the neighbourhood in which he lived, not far from Blackheath, the top of a church steeple was blown off in October last. He had calculated, as far as he could, the pressure required to overturn it, and he found it to be 30 lbs. per square foot. A curious thing came out, that whether it was a cone or a solid cylinder, it took the same pressure to overturn it. He could not help thinking that engine chimneys were practically very good wind-gauges; they had nothing to do except to admit of a draught going up through them; they had only to keep up, and not be blown away. The result had been arrived at by long experience. If a chimney was found to be weak, and was blown down, the next would be made a little stronger; and at last regular rules were arrived at and published. He had calculated some of the pressures required to upset them when they were 100 or 150 feet high, and had found that they ranged from 46 to 56 lbs. per square foot.

Railway carriages had been upset that required only a pressure of Mr. Barlow. 30 lbs., and he thought that was the lowest. Thousands of engine chimneys were standing which would certainly go down at 56 lbs. He thought that the pressure lay between those two limits. It was important to consider not only the effect upon the first surfaces but upon secondary surfaces. A formula had been given for that in Mr. Gaudard's Paper. He seemed to think that if the openings constituted three-quarters of the solid surface, the pressure upon the second girder should be three-fourths of the pressure on the first. Mr. Barlow would rather say that the velocity was three-fourths, which would make a material difference. Sir William Armstrong had suggested that certain experiments should be made, and Mr. Barlow would be extremely happy to give any assistance he could in whatever experiments might be needed to throw further light upon the subject. Notwithstanding all that had been said, and all the wisdom of the Institution, engineers were not at present very far advanced in their knowledge with regard to wind.

Mr. E. A. COWPER desired to mention two chimneys, the time of Mr. Cowper. one of which had come, but of the other, not. One was a chimney 406 feet high at King's Norton, near Birmingham. When he first saw it, he felt certain that the chimney would come down some day, although it had been standing four or five years; and in the course of about four years more, during a storm, 120 feet came off from the top. Another chimney, 309 feet high, had a greater batter and did not come down. It was lowered 60 feet, at his recommendation, merely to get a larger area. In the one case there was too little batter and 120 feet came off, while the other chimney was standing to the present day. Reference had been made to the Brighton Chain Pier. In 1836 his father wrote an article upon the subject of the pier and of the storm which caused its failure. It was not a very violent storm, but it was gusty, and the gusts came at regular intervals, causing the chains to swing backwards and forwards, and at last the platform was broken. He mentioned the matter to show that sometimes the oscillations of an engineering structure occasioned a great deal of damage when the wind was gusty. With reference to the experiments proposed by Mr. Baker with a wind-pressure plate, 20 feet long by 15 feet deep, he did not think the question was going to be settled by making boards rather larger or smaller; what was wanted was to find out whether the gusts of wind were 100 feet or 1,000 feet long. It was not a question of whether the gusts were 10 or 12 feet or 1 foot square, but whether they reached over

Mr. Cowper. the whole span of a bridge. In his opinion they were often 300 or 400 feet long, but that was a matter that ought to be ascertained experimentally. He would suggest that a number of anemometers might be placed in some windy place high above the ground in a line; a dozen or twenty might be arranged in a circle. The wind on one side would catch a certain number, and if they were connected electrically it would be known at what instant of time each was struck. It would then be known whether the wind approached a large gust, or whether it came with small hammer-blows on small parts of the structure. He did not believe in the danger of small gusts. There were small gusts, but they were not what tried a bridge; it was the gusts 200 or 300 feet long that a structure ought to be able to stand. The Crumlin Viaduct, the Tay Bridge, Bidston Hill, or any such exposed place, would be suitable for experiments. Engineers wanted definite information on the subject. Almost every speaker had confessed his ignorance with regard to it. A number of anemometers should be employed, and when a storm came, the attendant should set the recording apparatus going rather faster than usual, so that it might be seen what shape the gusts were—whether they were simply hammer-blows, or whether they had round tops and bottoms, or in other words, whether they lasted an appreciable length of time. The experiments of Mr. F. Osler showed distinctly that the wind did not come with sudden hammer-blows, but, as indicated by the rounded form, lasted an appreciable time. If engineering structures had to be placed in positions such as those favourable for anemometers, all he could say was, that they were very unfavourable positions for engineering structures. But it should be ascertained in those positions, what was the extreme pressure the structures would have to bear, and then make them accordingly.

Mr. Bender. Mr. C. B. BENDER in reply remarked that, concerning the subject of wind-pressure proper, his Paper was based upon the following studies:—

1. Experiments since the time of Newton on the resistance of air to falling bodies.
2. Experiments on the resistance of air to projectiles discharged from cannons or small fire-arms.
- 3 and 4. Information on the application of wind to the motion of windmills and sailing vessels.
5. Direct experiments as regarded the relation of velocity to the force of the wind, made with whirling machines in England, and with whirling machines, small windmill instruments, and falling plates in France and in Germany.

6. Records of storms and cyclones, more especially by De Foe, Mr. Bender, Colonel Reid, FitzRoy, Piddington, and Theodor Reye, of Strassburg (1880).

With respect to Mr. Symons' criticism, it would be found that he only spoke of a barometric depression in the centre of great storms amounting to 180 lbs. per square foot. In the centres of such storms there was hardly any wind—which, however, acted in its greatest force in the vicinity of the centre. De Foe gave, as the lowest pressure in the North of Scotland in the great storm in 1703, 27·45 inches, or about 2·6 inches less than normal pressure. That the low barometric pressure could not cause any lifting was evident, for the bodies to be lifted were within the low barometric pressure.

Whirling machines were invented by Robins before 1741; the experiments by St. Loup, in 1879, were the latest, and were considered the best made with an instrument on Robins' principle.

He had been charged with having consulted the experiments of those whom Mr. Baker had termed their great-grandfathers. This he admitted; he had learnt that the instruments used to-day in meteorology were in principle exactly those used of old. He had not drawn conclusions from small plates, or from balls thrown from St. Paul's Cathedral, but from the experiments of a highly scientific French officer, General Didion, with falling plates of 10·7 square feet area. The pressures were the lowest results derived from all such experiments, and had been described in General Didion's work on gunnery.¹ From these experiments, for a velocity of 111 miles per hour (50 metres per second), pressures of 40 lbs. per square foot were deduced from Didion's formula; whilst the old Smeaton-Rouse formula would have furnished 61·6 lbs. per square foot, or about 50 per cent. more. He considered that any attempt to make a theoretical formula was futile. He said this because Professor Unwin seemed still to adhere to the experiments of Hutton and the Smeaton formula, $0·005 v^2 = \text{pressure}$. Didion's coefficient would be 0·0036, and St. Loup's formula gave a lower coefficient still. The conclusions to which Professor Unwin arrived from his theoretical deductions as to the possibility of increase by back pressure, or impossibility of influence of great size of plate, did not agree with the above results of experiments. On the contrary, Hutton and Edgeworth with whirling machines, and De Borda with falling plates, agreed in the fact that the greater size of plates indicated a higher pressure. It required some time

¹ "Traité de Balistique."

Mr. Bender. for air to flow off the plates, and more time with large plates than with small plates, so that the partial vacuum behind varied, and there was some reasonable argument to endorse the results of the three experimenters named. With thin plates, the pressure again was greater than with prisms (Thibault's experiments), and projectiles rounded behind experienced very much smaller pressures. He had applied D'Aubuisson De Voisins' formula, derived from De Borda's results, to Didion's large plates, and had not adopted the results before ascertaining that reasonable figures were obtained. Undoubtedly this formula could not be used *ad infinitum*; but within the proportions of the experiments it held good, and it was not used beyond those proportions of test-plates. Should the influence of size not be considered, the pressure of 40 lbs. per square foot, on plates of 10.7 square feet area, should be substituted for 33 lbs. If it were considered for boards 10 feet high (railway carriages), the maximum pressure of $52\frac{1}{2}$ lbs. would be obtained, while for 1-inch bars it would sink to 20 lbs. These pressures were considerably smaller than those advocated by the Tay Bridge Commission, or adopted by Mr. Nordling, and he should respectfully draw attention to this fact. Objection had been raised by Professor Unwin to his method of considering the form of bridge-members for two reasons; first, because the disturbances of the air flowing from one member might influence the pressure on another member. But modern bridges were very open, with high trusses, great panels, and bars of small dimensions, so that the disturbance was too trifling to be considered; and inasmuch as cups, parachutes, and hollow bases undoubtedly caused double the pressures, the form must be considered to obtain anything like a correct result. Secondly, he objected that the ventilators of carriages fell in the shade of the wind. So undoubtedly they did, if the pressure came sufficiently from below, but not if the wind struck horizontally, or came in an inclined direction from above. Professor Unwin perhaps went too far if he compared the effect of a stream of air with that of a stream of water, for the former was elastic, and the other inelastic.

He agreed with Mr. Baker that the extraordinary high pressures of wind sometimes observed should not be taken into account as extending over great areas, and in this regard he was willingly led by the late Astronomer Royal, Sir G. Airy, who considered that such high pressures were momentary, and arose from irregular whirlings of the air, extending to no distance comparable with the length of a bridge. It would be observed that such cases were not discussed in the Paper.

That high pressures, such as were the consequence of velocities Mr. Bender of, say, from 90 to 100 miles per hour, did extend over great surfaces had been already proved. The Grand River Viaduct, in Mauritius, weighed 2,000 lbs. per lineal foot, and was only 9 feet 4 inches high, and yet it was blown away during a storm, which also overturned an empty carriage at another part of the island. And there was the communication of Mr. Seyrig relating to trains upset in the South of France, which had also been mentioned by Mr. Gaudard. He had seen, in the interior of Germany, the effect of a great storm where a road had been broken through an extensive forest. About eight years ago it was reported that a part of the Omaha iron bridge had been blown off and landed on the bank of the Missouri, and that the foundation cylinders were much twisted; and in 1872 he was shown, in the north of the State of New York, the debris of a wooden town bridge which had been taken bodily from its piers and carried into a field.¹

Some experiments had been promised by Mr. Baker on an extensive scale; but these experiments, to be made with boards with an area of 300 square feet, would be attended with various difficulties. In the first place, as great storms were rare, and it was not known when and where they would act with their maximum force, it would be necessary to have such instruments set up in various parts of the country, and to appoint experienced observers to attend to them—a process expensive and of long duration, which after all might lead to nothing. Next there must be a vane in proportion to the size of the board; a perfect turning apparatus, and the whole machine must be made to stand freely in the air. It would be larger than a windmill. Then, as the old principles of anemometry were rejected by Mr. Baker, he would have to discover new ones. In this regard, he might be permitted to refer to the address of Mr. J. K. Laughton, President of the Meteorological Society, at a recent meeting of that Society, who said that what was wanted was, not so much a new and improved apparatus for registering or recording the wind, as some radical improvement in the instrument itself, or in the theory which translated its action. Exactly the same defect had been pointed out in Gehler's *Physicalisches Wörterbuch*. Gusts caused a sudden action or impact, and it was required to know what would be the pressure caused by such a quantity of mechanical labour upon a structure which did not yield appreciably. Weak springs in instruments

¹ Quite recently (June 1882) an entire goods train in one case, and twenty carriages and half a town in another, had been blown over during a tornado in the United States of America.

Mr. Bender. received too great a momentum ; and strong springs placed behind a plate did not give sufficient motion to be measured. It was the same with the quadrant and Lind's wind-gauge. It was an easy experiment to try the effect of wind on a T square, such as used with drawing boards. When the wind was steady this oscillated but little, but when a gust came it oscillated quickly and violently. If the gust struck the blade when it moved with the wind, the deflection was much greater than if it happened to strike the blade returning against the wind. With rhythmically repeated impacts the blade might break under wind which, without the previous motion, would only deflect it a little. It was clear, therefore, that the instrument would sometimes show higher pressures than were due to the velocity ; whilst the pressure on a wall or on a bridge, which on account of its mass did not quickly vibrate, would be smaller. It was from these considerations that he proposed an instrument which not only was simpler than the older instruments, but which was more similar to the condition of a bridge. It had been argued by Mr. Baker that wind could not be very strong, because houses did not fall more frequently, and he gave the results of his calculations on the strength of houses ; but he should like to learn how the calculations were made. As regarded the strength of window-panes, which were stated to bear pressures of 40 lbs. per square foot, he thought it would make a difference whether the glass panes were large or small, whether the glass was thick or thin, and the degree of brittleness of the glass, and the way in which it was inserted in the frame would affect the question.

As regarded the strains for wind-bracing, he had specified a factor of safety of 3 ; while the Tay Bridge Commission in their report, which was published after his Paper had been written, and without considering strains in the bracing as arising from movable loads, specified a factor of safety of 4, so that the difference was not very great, and in fact this tallied with some of his own calculations.

It had been further objected by Mr. Baker that the dimensions of wind-bracing would become enormous. Now he had lately furnished bridges in which he had embodied the principles advocated in the Paper. One of them was a viaduct with girders 15 feet deep, which was designed to carry a wind-pressure of 1,000 lbs. per lineal foot ; the floor being at the top, 800 lbs. of the pressure would come upon the top floor ; but, owing to the top and bottom boom sections being in proportion as 3 to 2, 600 lbs. pressure were neutralised by the top lateral bracing, and 400 lbs. by the bottom

lateral bracing. The three spans of the viaduct rested upon two Mr. Bender. end piers and two iron trestles, placed upon two substantial blocks of masonry. The girders were bolted to the trestles, and the trestles and the end spans were tied to the masonry with twenty-four $1\frac{3}{8}$ -inch bolts, 14 feet long. Posts and diagonals were pin-jointed, and all mathematical centre-lines of the various members, which were of symmetrical section, except the wind-diagonals and struts, intersected in the true centre-points. The wind-bracing consisted of stiff Andrew crosses, both horizontally and vertically, between the uprights of the girders; the girders were 10 feet apart, and the trestles, being 21 feet high, spread to 20 feet at the base, where they were connected by 8-inch by 4-inch rolled beams serving as ties. The inconvenience of unusual dimensions of bracing did not only not arise, but it was found desirable, for the sake of practical execution, to make many sections stronger than required by the specification. In case of larger bridges, of course a deduction would be made from the boom sections equal to about $\frac{1}{3}$ of the areas of the lateral diagonal braces. The general stability of the Saltash bridge under high winds, according to his calculation, was sufficient.

He did not agree that want of knowledge of the action of the wind was a disgrace to the engineering profession, but engineers were deserving of no little blame if, notwithstanding the accidents to the Omaha, Ashtabula, Mauritius and Tay bridges, the many railway accidents by wind in America, and those in France, they were led by mere opinions to design insufficiently braced structures.

As regarded Mr. Max am Ende's remarks, there was no difficulty, nor any great labour, in taking into consideration the size, form, and position of the members; otherwise a great part of the means of neutralising the effects of wind efficiently and economically would be sacrificed. But if singularly shaped bridges were to be adopted, it would become tedious to calculate the wind-area. Such girders or arches, however, after a little examination, were found to possess no merit over forms simpler and more easy of execution, and which admitted of much better design. The proposal to draw rules for the size of wind-bracing from long-standing examples was an idea which had also occurred to Mr. Bender, but he had dismissed it, as being simply crude empiricism. Mr. Max am Ende still had to explain why it was improper to show how to determine the extra strains in wind-bracing arising from movable loads, as such strains were generally neglected and not treated in books. On the other hand, it was expected that he would propose to apply the empirical formulæ of certain mathematicians to the

Mr. Bender. question of strains in wind-bracing. But these formulæ expressed nothing but conjectures upon an incomplete series of experiments, and they were made by men who had never built bridges, and who did not possess sufficient practical experience of the material of which they were to be made.

In conclusion, he ventured to assert that though undoubtedly fresh experience would accumulate, yet engineers already could ascertain near enough the maximum velocity and force of wind to be expected in a given locality. The cost of engineering structures would not be much increased even if that standard were assumed a little too high. The stiffness given by substantial bracing was absolutely necessary for other reasons, and bracing a little stronger than was absolutely wanted must not be considered waste, because the wind-bracing also provided for the irregular movements of trains; it was a security against accidents in case of derailments of trains and collisions, and bound together the compressional boom pieces in one solid column of the length of the bridge. It would therefore be seen that the wind-bracing had other important offices besides the mere neutralisation of wind-pressure. Now in the United States, where for the sake of economy these conditions were much underrated, during the eight years from 1873 to 1880 there were one hundred and seventy-three accidents from broken railroad bridges, and last year, up to the month of May, there had been sixteen more, including a viaduct more than a mile long. He was satisfied that many of these bridges broke owing to deficient bracing, and engineers should profit by this experience.

The consideration of wind-bracing of engineering structures in many instances decided the choice of the general plan; further, it influenced greatly the main proportions of width, depth, and length of panel to the length of the span; and finally it led to the best sectional form of the members, and to the most effective design of the details of a structure.

Correspondence.

Mr. Atkinson. Mr. W. ATKINSON remarked, that although winds coming from the sea were, under ordinary circumstances, the strongest, in a country like Spain, with a high tableland, the winds from the land to the sea were the most violent. This was so well recognised in the country that it was a common saying, that the most violent winds were those which were born in the mountains and died in the sea. Thus, at Santander, the south wind was sometimes strong enough to blow the people down—coming over the lowest summit