ported by the observers in the *Daily Weather Report* at 8 a.m. and 6 p.m. It was a difficult matter to consider fogs in relation to the centres of cyclones and anticyclones, as it could not frequently be determined where the cyclone began and where the anticyclone ended. No ships' logs had been taken into consideration, and, in fact, there were very few observations from ships' logs close to the coasts, as many ships did not commence to keep logs until quite clear of the British Islands. A scale of visibility of objects at known distances, for the purpose of measuring the amount of fog obscuration, had been arranged at the Kew Observatory and carried on for two years, but some difficulty, chiefly due to unequal illumination, had been experienced in making observations. The objects observable were also all situated in directions from north-west round by east to south-east; the rest of the horizon being limited by Richmond Hill or by trees. As regards the issue of forecasts of fog for the banks of Newfoundland, it must be remembered that there was no similar condition of ocean currents prevailing in the seas round the British Isles as was the case at Newfoundland. The note "in a gale," to which Mr. C. Harding had drawn attention, was intended to mean that a wind force of 7 or 8 was observed somewhere in the British Islands at the time of the occurrence of the fog. As regarded the use of sea temperatures in connection with this investigation, very few satisfactory and trustworthy observations of sea temperature were available, and he (Mr. Scott) did not anticipate that sudden changes of sea temperature would be found to occur.

SOME CHARACTERISTIC FEATURES OF GALES AND STRONG WINDS.

By RICHARD H. CURTIS, F.R.Met.Soc.

(Plate V.)

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There is probably no branch of meteorology about which so little is really known as wind measurement, and yet there is no other which is of more general importance or which is more far-reaching in its bearings upon other branches of the science. But directly we get beyond the general statement that wind is a result of solar action, and ask more particularly as to the causes by which its infinitely varied movements are determined and governed, we find ourselves in a domain in which theory abounds, but in which ascertained facts are conspicuously rare. We know, for example, that some relation exists between barometrical gradients and wind force, but of the causes which produce those gradients, and modify them, by varying the intensity of areas of high or low pressure, we know scarcely anything at all. And not only is this true as regards the broader features of the subject, but it is so also with respect to details as to which it might have been supposed that meteorologists would long ago have found out all that was to be known. For years we have had all over the country an army of observers, most of whom are enthusiastic and intelligent, and yet after all they have done we still know next to nothing as to the manner in which the wind blows, of its strength or velocity, of the nature of its pulsations, its squalls and its lulls, or of the way in which its dynamic force is exerted upon obstacles opposed to its path.

Or if we look at our instrumental appliances for obtaining information on these points, we find a state of things which may fairly enough be termed chaotic. We still use without comment appliances yielding results which are at least doubtful to a very high degree ; we put our anemometers up on sites which we know are fatal to their integrity; and quietly ignoring ascertained facts respecting them, we still use the results obtained from instruments of different patterns, and known to be in their present state non-comparable, as if they all meant one and the same thing.

I have long felt it to be a fair matter of reproach to meteorologists, that while this is the state of the case so little effort should be made to remedy it. Of course I know perfectly well the inherent difficulties in the way of advance in certain directions—perhaps no one knows them better,—but still there are some branches of the subject in which I venture to think that very much might be done by intelligent observers, even without elaborate mechanical appliances, if only systematic and thoughtful attention were given to them.

I have mentioned these matters, since they afford the reason why I have brought the present paper before the Society, for they appear to me to justify one in making any contribution, however small, to our knowledge of a subject about which so little is known.

In a paper[1] which I read before the Society in 1881, I drew attention to the evidence of very high velocities of wind which was afforded by the traces obtained from the Robinson Cup Anemometers at some exposed stations. I pointed out that although the records of that class of instrument were not well suited for giving velocities for short periods, owing to the great reduction

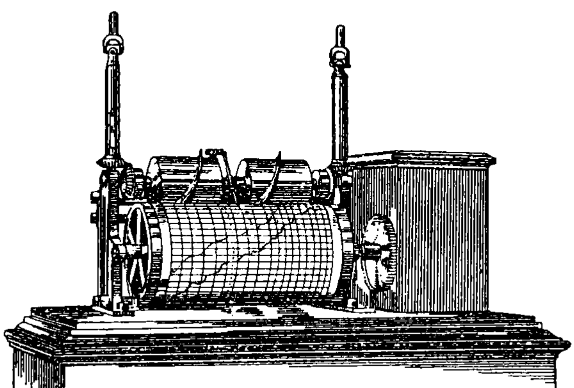

Fig. 1.—Recording portion of the Robinson Anemometer.

which the revolutions of the cups underwent before registration, yet the traces often showed decided angular changes which clearly indicated that large increases of velocity took place, but which, lasting only for a short time, were

[1] *Quarterly Journal*, Vol. VII. p. 205.

not recognisable in the general result obtained when the run of the cups over sixty consecutive minutes was measured, as is usually done in tabulating the records. In the large size Anemometer, with 9 inch cups and arms of 2 feet radius, the recording portion of which is shown in fig. 1, 7,000 revo-

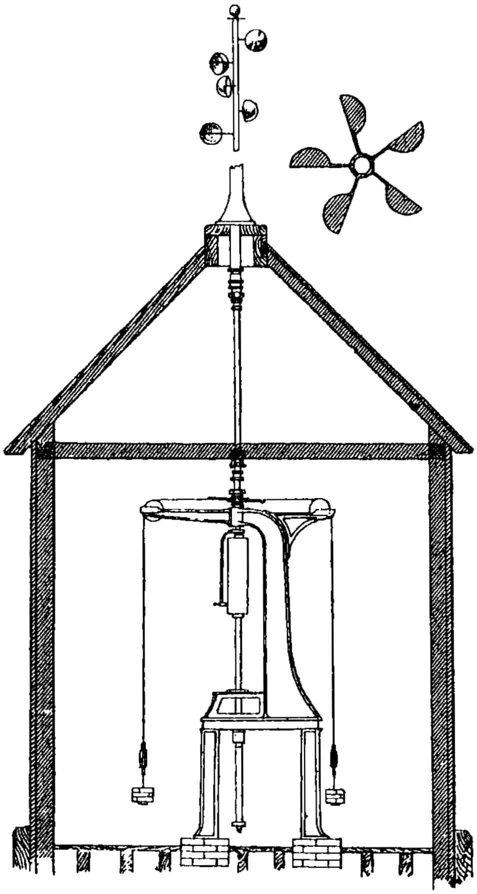

Fig. 2.—Bridled Anemometer.

lutions of the cups are required to produce a piece of trace 2¼ inches long, representing the passage over the instrument of 50 miles of wind. The case is similar to that of a train which makes a run of so many miles in an hour, but which does so by running at varying speeds, slowly up steep inclines and rapidly down others, the slower and the faster rates being merged in the actual nett result for the hour.

Since then I have had opportunities of more thoroughly studying this subject from other points of view, and I wish now to call attention to some conclusions I have obtained, mainly from a study of the records yielded by the Bridled Anemometer, devised by Sir George G. Stokes, F.R.S., and erected by the Meteorological Council at Holyhead.

The construction of this instrument will be understood by a reference to fig. 2. It consists of five hemispherical cups attached by short arms to a vertical spindle, upon which they are arranged spirally and in such a way that no cup shall interfere with the one immediately behind it. The cups and spindle are not free to revolve with the wind, as in the case of the Robinson instrument, for their movement is checked by weights which have to be lifted as the spindle revolves, and the amount of torsion of the spindle is therefore the measure of the wind's force. The instrument, it will be seen, belongs to the class of pressure anemometers ; but since the pressure varies as the square of the velocity, it has been designed so as to give an equal scale of velocity throughout. A large cylinder carries a sheet of paper which receives a record of every movement of the cups, and there is an arrangement by which the time-scale, which is normally seven-tenths of an inch to the hour, can be doubled, or if desired enlarged to about 48 times its normal size, or even more.

With the most open scale it is possible to follow every movement of the wind, and to obtain a good deal of insight into the manner of its working, but with the normal scale the gusts in gales of wind follow each other in such quick succession that the pencil produces a broad band of shading extending over some miles on either side of the average velocity, and it is only the gusts and lulls which extend beyond this belt which can be singled out from the others.

This instrument throws a considerable amount of light upon the question of maximum velocities to which I just now referred. For the reasons I have mentioned it is of course almost impossible to correlate individual gusts with the indications of increased velocities shown on the Robinson curves, but it is possible to do so with *groups of gusts*, and the comparisons of the two which I have made show that the estimates quoted in my paper were not far from the truth, when a proper allowance has been made for the fact that they were based on the assumption that Dr. Robinson's original factor (3), for the relation between the motion of the cups and that of the wind, was correct. I may mention that the scale of the Bridled Anemometer is based upon data recently obtained by Mr. Dines, and it will be remembered that other of his experiments led him to conclude that the factor (3) was about 30 per cent. too large.

The highest force of an individual gust that I have met with on the Bridled Anemometer was registered in December 1891, and amounted to a rate of 115 miles per hour, which with the old factor would be equivalent to a rate of about 150 miles per hour. Gusts at a rate of from 90 to 100 miles per hour have many times been recorded, but the usual limit for gusts in a strong gale may be taken to equal about 80 miles per hour, which on the old scale would be equivalent to about 120 miles per hour. It is necessary to bear in mind these old scale equivalents, because it is with them that the maxima formerly quoted must be compared.

The record obtained from the Robinson instrument will depend very much upon the character of the wind, and for this reason it is not always a very safe guide in forming a judgment as to the intensity of a gale. If a high hourly mileage is registered there can be no doubt that a very strong wind must have prevailed to produce it, and yet it is possible that an even more destructive and dangerous wind should have prevailed with a smaller mileage indicated by the instrument; indeed, I have come to the conclusion that whilst the positive evidence of the Robinson instrument is valuable, its negative evidence must be accepted with the greatest caution, and is indeed worth little. These records are sometimes produced in Courts of Law as evidence when litigants wish to show that a gale did or did not prevail; in the latter case it would be only necessary to ask whether a heavy transient squall could not have occurred without its being indicated upon the trace in order to destroy entirely its value as evidence upon the point.

As a matter of fact, gales differ in character very much, as indeed do also winds which do not attain the strength of a gale. As the result of a prolonged study of their general features, as recorded by the bridled anemometer, I have for my own use divided them into three general classes, within which there are, of course, some fairly wide limits for variety.

In the first class I place those gales which are essentially *squally* in character—in which the gusts constitute the main feature of the gale. In such a case the gusts and lulls alternate very rapidly, and indeed the wind is never constant in force for many seconds together. Usually it oscillates over a range of perhaps 10 or 20 miles at a mean strength of probably 40 miles per hour, but every now and again it bursts out with a velocity of possibly 40 or 50 miles an hour above the average, and after a few seconds sinks to a rate of 10 or 20 miles below it. This is certainly the most usual character of the gales experienced on our coasts, although such gales vary immensely as regards the strength and frequency of the gusts and lulls, and also as to the average velocity of what may be called the "normal of the gale."

The trace which the bridled anemometer would make in such a gale may be thus described. At that part of the scale indicating a velocity of (say) 80 to 40 miles per hour there will be a band of flat shading, caused by the innumerable passages backwards and forwards of the pencil as it ranged over those limits with the oscillations of the wind, this is what I have termed the "normal of the gale;" above this band will be a less dark fringe, passing off into more or less isolated lines, made by the stronger gusts which have carried the pencil beyond the usual limits; while every here and there, at intervals of perhaps a few minutes, lines will start up, generally two or three quite close together, far above any others, marking the extreme limits of the gale. At the bottom of the belt of shading a similar appearance will be found caused by the lulls succeeding the gusts, but generally extending over a much more limited range of velocity. Fig. 1 (Plate V.) is a reduced reproduction of a portion of the record of such a gale, in which, however, the "normal" is much higher, whilst the range of the gusts above it is less, than is commonly the case.

The frequency with which the gusts follow each other varies, as I have said, immensely. I have, however, made an attempt to count them in cases where the use of the most open time-scale would allow of my doing so, and I think I may say that in an average gale the ordinary gusts follow each other at intervals of about 10 to 20 seconds, while the extreme gusts occur at the rate of about one per minute. I only put this forward, however, as an approximation for the case of an average gale.

The range of velocity covered by the squalls is another point upon which gales differ very largely. In those to which I am now referring—squally gales—a range in rate of from 40 to 50 miles per hour above the "normal" is common, and may, I think, be regarded as a fair range for the extreme gusts in a fairly strong gale, while in a similar gale a total range of rate, from the lull to the following squall, of from 60 to 70 miles, or even more, is not unusual.

My second class of gales are those in which the velocity of the wind is tolerably steady, much steadier than in the first. The "normal" of the gale covers a much smaller range, perhaps not more than a rate of five or six miles per hour, and the gusts are far less severe; and I have noticed that generally they are not accompanied by lulls falling much below the "normal," or if the lulls are frequent then the gusts above the "normal" are comparatively few. This is a rather curious feature, but I have noticed it in several cases. Figs. 2 and 5 (Plate V.) are reduced facsimiles of portions of traces produced by gales of this class, the time-scale used in fig. 5 being a very open one.

The trace in this instance would be very different from that I have just described. The band of shading is much narrower, and as a rule far less flat, whilst the isolated lines above or below are much shorter and perhaps less numerous. It does, however, sometimes happen that the "normal" is even higher than in some of the more violent squally gales.

My third class should perhaps have been the second, inasmuch as it is a distinctly squally variety of gale. But the characteristic by which I distinguish it is peculiar. The record made by the anemometer has *two* "normals," connected by a lighter band of shading, as shown in fig. 3 (Plate V.). Indeed the gale appears to be made up of two series of rapidly succeeding squalls, the one series at a comparatively low rate of velocity, the other at a much higher one, the wind-force shifting rapidly and very frequently from the one series to the other. Examples of this class of gale are not very common, but those I have seen have been very well marked.

The trace in this case would show a very broad band of shading, with the upper and lower limits much darker than the centre, and having a good many isolated lines indicating gusts of considerable intensity projecting above the upper limit.

Before leaving the subject of gusts there are two other points to which I would like to call attention, and the first is as to their *duration*. As I have already said, the bridled anemometer is provided with an arrangement by which a time-scale of about ½ an inch to the minute can be used, and when this is in use in a gale of wind the duration of the gusts can be very fairly judged. In many instances they are very sudden in their occurrence; the rate rises to the maximum with great rapidity, remains there but an instant, and falls back again as quickly; the whole thing lasting but a second or so. In many cases, however, it is different. I find with the bridled anemometer the same thing as I have since witnessed, times without number, with Mr. Dines's pressure tube anemometer, and which is the second point I wish to mention. The gust does not rise to its maximum at one bound. The pen or pencil rises some distance, then pauses, and perhaps recedes slightly, it then starts upwards again, and perhaps makes another pause before it finally reaches its maximum. When there it seldom rests long, but falls again as it rose, in stages, and I have measured many cases in which the gust remained at about its maximum, falling and rising 2 or 3 miles only, for several seconds; indeed, in one case, of which I have a note, it did so for more than half a minute, during the whole of which interval the velocity did not vary more than 6 miles. The action of the instrument under the influence of gusts and lulls is well shown in figs. 4 and 5 (Plate V.), in both of which an open time-scale was used.

The only other point to which I wish now to refer is one to which I do not think attention has been called before. On looking carefully over these records I have found, oftentimes very distinctly marked, a long pulsation in the wind force which recurs again and again with more or less regularity at intervals of perhaps 20 minutes or half-an-hour in some cases, and in others at longer intervals of about an hour, more or less. What I have before termed the "normal of the wind force" rises gradually, and as gradually subsides again; the quicker and larger oscillations of gusts and lulls going on all the while and quite independently of this long period oscillation. In some cases this phenomenon is more marked than in others, but it is very common, and I am inclined to think that in many instances where it is not very apparent its absence is due really to the more casual oscillations having in some way obliterated it.

Generally it is best seen on the traces by foreshortening them somewhat, and so getting a more generalised view of them, but in plenty of instances it stands out, as in fig. 6 (Plate V.), evident enough without doing this. The most violent gales are not those in which it is best seen; a moderate gale, or a strong wind which falls just short of the strength of a gale, are perhaps the cases in which it shows itself most evidently. When the gusts and lulls very rapidly succeed each other, and at the same time cover a wide range, it is less apparent without foreshortening. This feature of the wind-force is also shown very distinctly on the curves yielded by Mr. Dines's new pressure tube anemometer; and the regularity with which it recurs throughout the whole continuance of a gale, as well as the fact that it is of such common occurrence, leads me to think that it very probably plays an important part in the general movement of large masses of air, such as are involved in gales of wind, and that it is a phenomenon well worthy of further investigation.

The phenomenon is not to be confused with the passing gust which comes and goes in a few seconds, and in which the velocity probably rises considerably before it dies away again. In the case of the long pulsation the increase in the "normal" is always gradual, and may be very slight, although very evident, and the pulsation is certainly not shown in the hourly measurements of velocity, owing to the fact that its period is fairly uniform, and therefore its effect becomes distributed pretty equally.

In conclusion, I beg to express my thanks to the Council of the Meteorological Office for their permission to make use of the curves of the Bridled Anemometer for the purposes of the paper.

DISCUSSION.

Mr. W. H. Dines said that he quite agreed with everything Mr. Curtis had said with reference to the chaotic state of anemometry, and as an instance he might mention the way in which the hand rail of the ladder by which the observer got up to the cups was sometimes left projecting, so that the eddy from it would just catch one side of the instrument as it turned. He was glad Mr. Curtis had based his results upon the Bridled Anemometer at Holyhead, as he thought that was the only trustworthy pressure instrument from which we had any records in England. He might state that he had constructed a scale for this anemometer at the request of the Meteorological Council. The identical cups now in use at Holyhead had been sent to him, and he had tried them at speeds up to 70 miles an hour upon the whirling machine at Hersham. The velocities quoted by Mr. Curtis were therefore quite independent of any question about the value of the factor of the Robinson cups. He had obtained the pressure upon a square foot circular plate on the same whirling machine, and by precisely similar methods, and hence he could state positively that the highest wind pressure ever recorded at Holyhead was about 36 lbs. per square foot, corresponding to Mr. Curtis's 115 miles per hour, and about 20 lbs. would correspond to the gusts of 80 miles per hour. This was remarkable, since Holyhead was one of the windiest stations in the British Isles, yet pressures of 30 lbs. were quite common at many other stations inland, Bidston particularly, where 90 lbs. per square foot had been registered. He thought it was explained by the fact that the Bridled Anemometer was trustworthy, whereas the records of the pressure plates were utterly unreliable in so far as maximum pressure was concerned, on account of the unsatisfactory way in which the pencil was connected with the plate. Mr. Curtis had found that with an average or "normal' velocity of 40 miles per hour, a maximum of 80 or 90 might be expected in the gusts. At Oxshott he had found that the maximum was generally about double what Mr. Curtis called the normal velocity, and the relation seemed to hold for light as well as strong winds. It was noteworthy, however, that when the temperature was high the wind was always more gusty, also at Oxshott the South-south-west and North-west to North winds were especially gusty, but this was probably due to the fact that to the south-south-west and north-north-west there were trees within 1C0 yards nearly level with the anemometer, while in all other directions there was a very good exposure.