man upon any matter under their control, more especially when those views are in accordance with systems used in countries of advanced civilization, they are bound to give effect to those views, in preference to any mere expedient, however practical, unless circumstances of a very exceptional character happen to justify its temporary adoption.

ART. XXVII.—*On Anemometry.* By CHARLES ROUS MARTEN, F.R.G.S., F.M.S., M.Sc.M.S.

[Read before the Wellington Philosophical Society, February 24th, 1877.]

1. ANEMOMETRY—the science of measuring wind-force—is a branch of Meteorology which always has attracted much attention, and engaged much inventive ingenuity. The practical advantage, as well as the scientific interest attaching to a knowledge, first, of the actual dynamical force of the wind in severe gales, as experienced in the past, and therefore likely to be sustained in the future; and, second, of the comparative wind-force in different localities, have produced various methods of estimating and comparing that force.

2. Hence we observe a two-fold aim in Anemometry: first, to gauge the actual pressure of the wind on a given vertical plane area; second, to compare its average force as felt in various places. Thus, it is obvious that both accuracy and uniformity are essential.

3. Unfortunately the result of all the efforts in this direction up to the present time is so unsatisfactory that Anemometry would appear to be wholly indefinite and untrustworthy so far as any approach to scientific exactness is concerned. I regret to say that after many years' careful study and comparison of anemometrical observations, I have been unable to arrive at any other conclusion, than that the instruments and formulæ, now in use, not only fail to give an accurate register either of the pressure or the velocity of the wind, but also utterly lack the essential qualification of comparability.

4. The strength of the wind is measured chiefly by three methods: first, by estimating its relative force, either in words, such as "light," "fresh,"

"strong," "a gale," or in figures, as by the scale introduced in 1805, by the late Admiral Sir Frederick Beaufort, and still known by his name—other scales of relative force have been tried, but none have maintained so permanent a hold and extensive a use as the Beaufort scale of 0–12–; secondly, by gauging the pressure in pounds on a plate one foot square, which, acting on a spring, enables the actual apparent pressure to be registered by a simple mechanical contrivance (this is the principle of Osler's anemometer); thirdly, by measuring the velocity of the wind in miles per hour, with an instrument known as Dr. Robinson's anenometer—to be described later.

5. All attempts to reconcile the results of these three methods, so as to institute any trustworthy comparison, have proved futile. And here I would observe that it is necessary to bear in mind the distinction between force, pressure, and velocity, in the following comparisons. The primary object is to ascertain the wind's *force*, but (excepting the rough guess-work *relative* force, estimated by the Beaufort scale,) this is obtained by deductions from the recorded pressure, or velocity, or both. In fact, the modern practice is to observe the velocity by instrument; thence to calculate the pressure, and from that to deduce the relative dynamical force exerted. The Osler anemometer, on the whole, is so unsatisfactory, both from the difficulty of estimating the mean pressure registered by it, and from the liability of its machinery to get out of order, that I fully agree with the Director of the English Meteorological Office, Mr. R. H. Scott, who says in his paper on the subject, "I am convinced that meteorologists on the whole have acted rightly in preferring velocity to pressure as a mode of registering the action of the wind." Taking, then, velocity as our standard of comparison, we find that 49 miles per hour is described by Denham as a "great storm;" while, in the Beaufort scale, as translated into velocities by the Meteorological Office, that velocity only represents force 8, or what is called a "fresh gale; and the maximum force, 12, is stated to represent a "furious hurricane," which is estimated as having a velocity of 85 miles per hour. The result has been rather absurd, for, as anemometers have recorded a velocity of over 100 miles per hour, it has been found necessary in describing the wind's force by the Beaufort scale to add extra numbers, up to 14, or two degrees beyond the wind's greatest possible force! This obvious absurdity caused a re-arrangement of the tables, and the force 12 was made to represent a velocity of 100 miles per hour by anemometer. Even this, however, did not meet the difficulty, for I myself, have registered velocities of 107 and 109 miles per hour: 120 miles has been registered at Holyhead and Liverpool; and in the recent storm at Sydney the observer there recorded the hitherto unprecedented speed of 153 miles per hour.

6. Thus, practically, we may dismiss the estimated-relative-force system as a very rough-and-ready and a thoroughly unscientific and non-comparable mode of anemometry. It remains to be seen whether the scientific systems be more accurate.

7. It has been stated already that the most generally received method is by measuring velocity, and that the instrument used for that purpose is Dr. Robinson's anemometer. To make comprehensible what is to follow, a brief description of this instrument is necessary.

8. It consists of four hemispherical equi-distant cups, forming the terminations of as many arms, disposed horizontally, in the shape of a cross, and revolving on an axis, the cups being so placed that the concavity of one and the convexity of another are always exposed to the full force of the wind, simultaneously acting on their diametrical planes. The revolutions are numbered and recorded by a simple and ingenious mechanism, which it is unnecessary to explain.

9. The circumference of the circle described by the cups in each revolution, and the number of revolutions made in a given time, being known, it is easy to calculate the distance travelled by the cups in such given time. Dr. Robinson calculated that from the resistance offered by the convex cup on the one side, the force of the wind on the concave one on the other side, would be only sufficient to propel it at one-third of the wind's velocity, and that this rule was irrespective of the diameter of the cups, or the length of the arms. His calculations are given in Vol. XXII. of the "Transactions of the Royal Irish Academy." His mode of testing his original anemometer was by fixing it on a pole eleven feet in length, attached to a locomotive engine, which was driven a measured distance, in calm weather, on the Dublin and Kingston Railway, at various speeds, ranging up to nearly 70 miles per hour, the anemometer thus being pressed against the wind, instead of the wind pressing against the anemometer. The distance run by the engine being known, it was only necessary to compare it with the number of revolutions made by the cups, and the consequent comparative distance they had travelled. The result was that they were found always to have travelled one-third of the distance run by the engine; and this "irrespective of their diameter or of the length of the arms." This went to confirm the theory Dr. Robinson had formed on the subject; but it is noteworthy that he apparently regarded that coincidence as somewhat fortuitous. Nevertheless, its correctness has been sustained by many subsequent experiments, including those made in Greenwich Park by Mr. Glaisher, who testing the instruments at various velocities, and under differing conditions, always found the result to corroborate the inventor's theory.

10. I may state here that I entirely accept that result *so far as it goes*, viz., as establishing the rule that the cups once fairly under way the centres will travel at about one-third the wind's velocity, *so long as a steady rate of speed is maintained*, but no longer.

11. It will be perceived that this last qualification implies a most serious possibility of error, inasmuch as the wind's velocity varies almost every moment; however, I am inclined to believe that a really good anemometer will give a very fair *mean* velocity for a lengthened period, such as 12 or 24 hours, or in other words that it will indicate with tolerable faithfulness the total horizontal movement of the air during such periods. This, although not fulfilling the first of our two postulated anemometrical desiderata, as will be shown shortly, would suffice for the second, viz., comparison of the average wind-force experienced in various localities, provided the anemometrical records be themselves intercomparable. Let us now examine whether this be the case.

12. In the year 1871 I made a series of experiments at the Southland Observatory, with the view of ascertaining whether three of Dr. Robinson's anemometers gave approximately identical results, in order, that should this prove to be the case, I might place them in as many different localities to obtain comparative records of wind-force. Of these instruments two were the ordinary double-indexed single-dial anemometers by Casella, with cups three inches in diameter, and registering to 505 miles, and the third was an improved five-dial instrument by Negretti and Zambra, with cups four inches in diameter, and registering to 1,000 miles. A single week's trial sufficed to prove all comparison hopeless. The three instruments were exposed in a precisely similar way, and their individual positions were interchanged. Nevertheless their records varied so widely as to be simply ridiculous and unworthy of preservation, accordingly I relinquished my plan of comparing the respective local wind-forces, and adopted the Negretti anemometer, being apparently the most trustworthy, as my standard. As an instance of the variety in the records of these three instruments, I may mention that in one of my 24 hours' observations, when the Negretti anemometer indicated 517 miles, one of the Casella instruments indicated 370, and the other 283 miles. So much for my own personal experience.

13. About the same time, similar but more elaborate experiments were being carried on in England on behalf of the Meteorological Society by Mr. Fenwick Stowe and by Mr. Robert Scott, the Director of the Meteorological Office. Both have published the results. Mr. Stowe's were the most extended observations, and I extract the main conclusions at which he arrived. No fewer than ten anemometers were thus tested, Nos. 1, 2, and 3, being the same as in my experiment; No. 4, one by Adie with four-inch brass

cups and moderately short arms; No. 5, ditto, with arms only two inches in length; No. 6, ditto, with nine-inch arms; No. 7, ditto, but with eighteen inch arms and large elliptical cups; No. 8, ditto, but with very light *tin* cups and arms; No 9, ditto, but the cups conical instead of hemispherical; and No. 10, a large standard anemometer, with copper cups nine inches in diameter, as recommended by Dr. Robinson. Omitting the intermediate results, which progress by tolerably uniform degrees from minimum to maximum difference, the respective results at the highest and lowest velocities observed were as follows :—

No.	—	At Lowest velocity. Miles.	At Highest Velocity. Miles.	Range of Velocities at which tested. In Miles per Hour.
1	Casella's	833	762	3—35
2 & 3	Negretti's	1,041	748	3—38
4	Adie's	1,120	750	4—30
5	,, Short arms	810	619	11—35
6	,, Long arms	856	822	7—40
7	,, Longer arms and elliptical cups	915	850	9—18
8	,, Light tin cups, 2½in. deep	840	568	5—30
9	,, ,, ,, conical cups, 4in. deep ..	none	661	—41
10	Kew standard, 9in. cups	1,000	1,000	—

On this discouraging result Mr. Stowe remarks,—

"It will at once be seen that the results of these experiments are, in the case of every instrument tried, utterly irreconcilable with Dr. Robinson's dictum, that the centre of each cup travels at one-third of the rate at which the wind moves, and that *this law is irrespective of the size of the cups or the length of the arms.* Anemometers with short arms do *not* agree even approximately with the standard, excepting at low velocities; but there is this peculiarity, that, while those which have the smallest cups relatively to the length of arm, maintain at all velocities a tolerably even percentage of the motion of the standard, those on the contrary which have large cups and arms move at a high relative speed in very light airs, but fall actually below the others when the wind is high. If the standard be assumed correct, the cups of most of the small instruments move through a space scarcely more than one-fourth of that passed over by the wind. Of course it may be asked—which is correct? I do not know with what instruments Dr. Robinson's experiments were made; but I assume that, as he adopted and recommended the adoption of large anemometers, they ought to be taken as a standard, at least till they are proved incorrect, about the probability of which even I have no means of forming an opinion. Only as our confidence has been rather rudely shaken in one respect, we, perhaps, need re-assuring that the relation between the wind and the cups is not equally mythical."

14. The results of these experiments, which are corroborated by those of Mr. Robert Scott, Mr. Glaisher, and Mr. Charles Cator, appear almost conclusively to dispose of the question—whether our present anemometrical returns be intercomparable for the purposes of climatology, by answering that question most decidedly in the negative. I may add that the observations taken at the various New Zealand observatories during the past ten years exhibit such striking and inexplicable discrepancies as strongly to support this unfavourable view.

15. We now arrive at the second point on which anemometry is supposed to furnish us with information, namely, the force of the wind in heavy gales. We seek to ascertain the dynamical force exerted by the horizontal movement of the air on a vertical plane surface, such force to be expressed in pounds of pressure on each square foot of the said surface. It is accepted as an axiom in meteorology that the velocity and pressure of the wind are correlative, and this correlation has been variously formulated. The formula now most generally accepted is that given by Sir Henry James, which is that the pressure in pounds on the square foot equals the square of the velocity in miles per hour multiplied by ·005; or, expressed algebraically, if a be the velocity and x the required pressure, then $\frac{a^2}{200} = x$.

16. From this formula it will be perceived that the relation of velocity to pressure proceeds with increasing velocities by a peculiar mode of progression. The theory when investigated gives some rather remarkable results. For instance, at a velocity of 20 miles per hour, or what is considered a " fresh " breeze, the pressure is 2 pounds per square foot, but at a velocity of 40 miles the pressure is 8 pounds. So at 50 miles the pressure is 12·5 pounds; at 100 miles 50 pounds; at 150 miles, 112·5 pounds. At 200 miles, could such a velocity be attained, the pressure would be 200 pounds; at 400 miles, 800 pounds; and at 1,000 miles, 5,000 pounds. Thus by this formula when the velocity is doubled the pressure is quadrupled. Such at least is the accepted theory.

17. It is true that other tables of relation between anemometric velocity and pressure have been constructed, but apparently they are founded merely on the concurrent records of Robinson's and Osler's anemometers, and not on any definite mathematical theory or investigation of the principle involved. For example, one author represents a force of 9 pounds as equal to a velocity of 49 miles, which he describes as " a great storm." Another describes similarly a velocity of 74 miles, which he gives as equivalent to a pressure of 21 pounds. In a third work a "most violent hurricane" is represented by a velocity of 107 miles, and a pressure of 46 pounds. A fourth describes a pressure and velocity of respectively 49 pounds and 110 miles, as " a hurricane, tearing up trees and throwing down buildings"; while a fifth

gives a much higher force even than this apparent maximum, viz., 58 pounds and 120 miles.

18. It will be necessary to bear in mind these records of wind-force for the purpose of the comparison now about to be made; but as they are obviously deduced from no distinct formulæ, it is needless to analyze the mode of their construction or the theories on which they are based.

19. Let us see now how these estimates of wind-force agree with others made during various severe storms. During the Madras cyclone of 2nd May, 1872, the maximum velocity recorded at the Madras Observatory was 53 miles per hour (14 pounds on the square foot). It happened that Captain Donkin, of the ship "*Inverness*," who was in this cyclone, was caught, six months later, in the English Channel, in the sudden and violent storm of 22nd November. On learning this, the Director of the Meteorological Office wrote to Captain Donkin, asking him whether the force of the Channel gale had been at all equal to that felt during the Madras cyclone. Captain Donkin replied:—"It is my opinion that for two hours only, at Madras, did it blow harder." In each case the force of the wind was recorded as 12 in the ship's log; and the velocity of the Channel gale at the nearest observatory to the ship's position—Falmouth, was 57 miles per hour (16 pounds), or much about the same as at Madras; but, as 70 miles an hour often had been recorded at Falmouth, it was plain that a serious discrepancy existed somewhere. As already mentioned, 36 pounds once was deemed the maximum pressure attainable; and anemometers frequently succumb at even less pressure, as in the great storm of 15th October, 1868, in Southland, when my anemometer yielded to a force of 35 pounds. It began to be found that a 40-pounds pressure often was recorded in hard gales, as at Glasgow, on 24th January, 1868, when 42 pounds was indicated. At the Liverpool and Holyhead observatories pressures of 50 to 60 pounds gradually began to be accepted, and then 70 or even 80 pounds; while at the Bidston observatory, on 9th March, 1871, a pressure of 90 pounds was recorded. In Mr. Blandford's paper on the climate of Bengal, published in the "Proceedings of the Asiatic Society," it is stated that the highest pressure ever registered in Calcutta was 50 pounds; but that was in a storm of no remarkable violence, and one which did but little injury. In the far more severe storms of 2nd November, 1867 and 5th October, 1864, the anemometer was blown way at 36 pounds. The greatest force I ever recorded was in Southland, on 23rd December, 1871, when, in one gust, the Negretti anemometer registered a velocity of 160 feet per second; equal to 109 miles per hour, or nearly 60 pounds on the square foot, and in another gust, 107 miles, or 57 pounds.

20. All these records, however, are completely eclipsed by the results

given by the anemometer at Sydney, during the recent great storm, when the amazing velocity of 153 miles per hour, equivalent to a pressure of 117 pounds on the square foot was registered; being the highest ever yet recorded in any country; while several times during that storm a pressure of 112 pounds was indicated.

21. To appreciate the real meaning of such a tremendous pressure, the following illustration of its practical effect may be given :—A plate-glass window three feet square, a very common size, would receive a blow of 1,053 pounds, or nearly half a ton. A sheet of plate-glass such as those of several shop windows in this city, viz., ten feet by five, would have to sustain a *blow* (not a steady pressure, be it remembered,) of nearly 6,000 pounds, or almost three tons; while the side of a building 50 feet long and 40 feet high—no extraordinary dimensions—would receive a lateral blow of 234,000 pounds, or more than 100 tons.

22. Such would have been the dynamical power of the wind, if (1) its velocity were really that indicated by the anemometer on the occasion referred to; and (2) if that velocity actually represented the pressure deduced by the accepted formula.

23. This latter point I do not purpose to investigate in the present paper, but I shall endeavour on a future occasion to show that there is good reason to doubt the correctness of the accepted ratio of wind-velocity to pressure.

24. I now proceed to explain why I do not believe that the actual velocity of the wind, on the occasions of these extraordinary records being obtained, was that apparently indicated by the anemometer.

25. In the first place, let it be clearly understood that the only point actually recordable by the anemometer is the number of revolutions made by its cups. From this datum simple multiplication gives the distance travelled by the cups. The accepted formula is, that a second multiplication by 3 gives the distance travelled by the wind. Hence, in the Sydney storm, when the apparent velocity of the wind was 153 miles, the actual self-recorded speed of the cups was 51 miles, and the triple velocity was deduced on the adopted ratio of 3–1. But is this ratio applicable to all conditions of the wind, to steady or unsteady breezes, to zephyrs or gales, to increasing or decreasing forces, or to sudden gusts or lulls? If a dead calm were followed by a gust, would the cups at once revolve at full speed in spite of their *vis inertiæ?* Or again, if a gale were suddenly succeeded by a calm, would the cups, at one instant revolving so rapidly, in the next moment become motionless? The reply is obvious: they would not. In the latter hypothetical case, the cups would continue to revolve by momentum at least for several seconds. Proceeding further, suppose the cups

to be revolving at a speed of 20 miles, being propelled by a 60-mile breeze, and that, in a sudden gust, the actual velocity of the wind was increased to 90 miles: would the speed of the cups be augmented only by one-third of the added velocity,—*i.e.*, 10 miles per hour? Clearly, if the cups moved at the same rate as the wind, no possible increase of wind-velocity, superadded to any degree of initial velocity and momentum, could drive them faster than the propelling power. But, as they only move at one-third the speed of the latter, is it not conceivable that successive augmentations of the propelling power, superadded successively to the initial velocity and its attendant momentum, may raise the actual speed of the cups at least to a rate considerably more than one-third of that of the wind?

26. My theory may be illustrated thus: Suppose the wind to be travelling at the rate of 60 miles per hour, and suddenly to fall calm, or, which would amount to the same thing, to be shut off suddenly from the anemometer. In such a case, it would be found that, notwithstanding (1) the resistance of the atmosphere, which at first would be equal to an opposing wind of nearly 20 miles an hour force, (2) the resistance of friction, and (3) the absence of any propelling power, the cups would revolve by momentum at a gradually decreasing velocity for upwards of a minute, in which period they would have travelled perhaps half a mile without the aid of any propelling power but momentum. On the other hand, although, if a dead calm were followed suddenly by a 60 miles-an-hour gust, the cups would not start instantly into full velocity, being retarded by *vis inertiæ*, yet this retardation would be almost inappreciable. Repeated experiments proved to me that a well made anemometer will take considerably less than a second to attain its full velocity, and in fact the cups catch the wind's force so quickly that I found it impossible to arrive at any definite appreciable allowance to be made for *vis inertiæ*. As, too, the best made instrument is that in which the resistance of friction is reduced to the minimum, it follows that the anemometer which is the quickest in attaining full velocity will be the slowest in parting with that velocity. Thus a 9-inch Kew standard instrument or a 4-inch Negretti and Zambra, owing to their delicate construction and admirable balance, will be *found*—*once set in motion*—to retain that motion for a much longer period of time than would be imagined by any one who had not actually tried the experiment.

27. Now let us take another case. Suppose the wind to be strong but squally and unsteady—blowing generally at a speed of 30 miles per hour, and consequently propelling the cups at the rate of ten miles per hour. Next suppose a sudden gust of five seconds' duration to blow at exactly double that strength, or 60 miles per hour, what velocity would be indicated by the anemometer? The theory hitherto has been that the increase in

velocity being merely relative to the previous speed, the motion of the cups would be accelerated only by one-third of that additional wind-velocity, and that their rate of travelling would be $\frac{30}{3} + \frac{30}{3} = \frac{60}{3} = 20$.

28. But from careful observation and experiment I am convinced that this formula is not strictly accurate; that, inasmuch as the cups already are travelling at a speed of ten miles per hour, instead of being at rest, their real velocity in a sudden gust of double strength would be $\frac{60}{3}$, *plus* momentum, *minus* some indefinite amount to be deducted for the resistance of *vis inertiæ*—which would be exceedingly small in such a case—and for the gradual loss of momentum velocity, which also would be very trifling in so brief a space of time. Hence if my theory be correct, the cups in this hypothetical case would travel at a velocity of nearly 30 miles an hour for those few seconds, or almost half the actual speed of the wind, and I can conceive of cases where several successive and rapidly following augmentations of wind-velocity might cause the cups to travel at very nearly the same velocity as the wind. This of course could only be for a few seconds, and under a most exceptional continuation of contributing causes, for after those first few seconds the cups soon would settle down to their ordinary relative velocity. The occurrence, however, of a modified form of the above-mentioned hypothetical case would suffice to account for some of the marvellous statements of the force supposed to have been exerted by sudden gusts, that force being deduced on the ratio of $\frac{a^2}{200} = x$, x representing the required force, and a the velocity, as ascertained by multiplying the speed of the cups by 3.

29. Take, for instance, the great Sydney storm, already quoted in this paper. When the anemometer cups were travelling at the rate of 51 miles per hour, the accepted formula bade the observer record the wind's velocity as 153 miles an hour; but if my hypothesis be correct, the wind's velocity on that occasion may not have been more than 100 miles an hour, or perhaps even less. And supposing that the precise case suggested had occurred, of a suddenly doubled velocity, the theoretical speed of the wind, when the cups were travelling at 51 miles per hour, would be about 102 miles—probably somewhat less, which would represent a pressure of 52 pounds on the square foot instead of 117 pounds as calculated—a most material difference.

30. Now let us see what light Mr. Fenwick Stowe's experiments throw on the problem. Under my theory the heavier and larger the cups the greater the velocity they should indicate at the higher speed, as compared with lighter ones, for the simple reason that the greater weight necessarily would imply greater momentum. Reference to the table already quoted answers this question in a most decided and unmistakeable manner. It

will be seen that the lighter the cups the smaller their comparative velocities at higher speeds, the lightest of all—that with very thin cups of tin—indicating little more than half the velocity registered by the large Kew instrument at the highest speed compared. It is curious that Mr. Fenwick Stowe did not see the conclusion to which this result of his experiments pointed, but it must be remembered that his object was not that of the present paper, but simply a comparison between large and small anemometers, with the view of ascertaining which were the more trustworthy instruments at different velocities, and in these tests the large Kew anemometer always was adopted as the standard of comparison, without any attempt to ascertain whether it might not be, under certain conditions, the less trustworthy of the two. The probability of an excessive amount of atmospheric horizontal movement being registered (by momentum) in case of calms succeeding gusts is noted, but not the other aspect of the case—the possibility of this occurring also with sudden increases of wind force.

31. One explanation has been suggested of the extraordinary pressure exerted by sudden gusts, as indicated by an Osler (pressure) anemometer, but not by the continuous self-registering Robinson (velocity) instrument. It is that such abnormal force may be exerted only in narrow columns of air—in gusts perpaps only a few yards, feet, or even inches in breadth, as is seen sometimes in the case of tropical hurricanes. That such non-uniformity in the wind pressure does exist is unquestionable. Evidence of this may be seen in every gale that blows across our harbour, whose waters frequently are "streaked," as it were, with narrow gusts of excessive violence, which seem actually to tear up the water as they rush over it, yet are often only a few feet in breadth, or even less. Such gusts possibly might strike the concave cups of the anemometer, while the convex cups, less than two feet distant, might be in a comparative calm. Such an occurrence would account for unduly high anemometrical readings, but the probability is rather a remote one.

32. This non-uniformity of pressure—this "streakiness," if I may use the expression—of the wind it is important to consider from another point of view, the one only alluded to in passing in an earlier part of this paper, viz., the possibility of the accepted ratio between anemometric velocity and pressure not being so strictly accurate as is generally supposed to be the case. Of course were the atmosphere a solid body of uniform density, moving with uniform velocity, the dynamical force exerted by its impact on a vertical plane surface of given area always must be in proportion to that velocity. But this is not the case; indeed, the reality is almost the reverse of this supposition. The wind rushes forward in a number of irregular darts or tongues, often curling about in curves and eddies, seldom if ever

striking any plane surface at a fair right angle, but exhibiting infinitely more irregularity in its course than even a body of water; how easily that may be deflected it is needless to state, the fact is obvious to any one who has watched the current of a tide or river. This attribute of water is, *à fortiori*, that of the more elastic fluid—air—and hence it is that I think it open to grave doubt whether, even if we grant that the wind may move at the *immense* velocity asserted, it really exerts that dynamical pressure, on a given plane area, which, *cæteris paribus*, we should be led from such velocity to predicate.

This paper already has exceeded due limit as to length, and I must defer the further investigation of the subject to a second paper.

Art. XXVIII.—*Elements of Mathematics*. By James Adams, B.A.

[*Read before the Auckland Institute, 4th September, 1876.*]

When Peter the Great determined to rouse his subjects to the active life and business habits of the people of England and Prussia, he began by removing impediments. He wished his people to become skilful workmen and mechanics; and it was evident that the Russian of his time, with his long flowing robe and his pendulous beard, could not work at the forge or the bench. To remedy this, Peter stationed men at the city gates, each armed with a pair of shears, who cut off the long skirts and sacred beard of all those who passed through the gates.

This was the first step in giving them a mechanical education, and the effect he produced, in raising his people to the level of other European nations, has always been a subject of admiration.

A similar course was adopted, in the matter of education, after the French Revolution; when the School Commissioners dismissed, in a summary manner, the teachers of the schools and colleges, and flung after them, so to speak, the golden legends, controversial treatises, Aristotle's Ethics, and Euclid's Elements; not that they felt no reverence for these books, but because a *new* era had arrived, when practical knowledge had taken the place of speculative, and when it was of paramount inportance that the students should reach, by the shortest and plainest route, the wide range of learning that was now for the first time opened to the human mind.

The object of education to their mind was to study the nature of things, with the view of adding to the comfort and happiness of man, and not to learn to dispute in the argumentative manner of ancient philosophers.