that the small sum of 1,000*l.* annually entrusted by Government to the Royal Society for miscellaneous experiments, is administered in a most praiseworthy manner; and if Government would be ready to grant, and the Royal Society willing to undertake, an extension of this trust, it would, I think, be a great point gained for this class of physical experiments. (I speak only of experiments, but the encouragement of experimenters is a point of equal importance.) But when we come to experiments and observations requiring great time, the case is very different. Certain experiments, whether from the great time they require or the great expense they demand, cannot be well performed in a college; while routine and long-continued observations such as those connected with the various branches of cosmical physics are of such a nature as to require a central establishment to superintend their organisation and reduction. There is thus, I think, the necessity for a central establishment of some kind devoted to that class of experiments and observations requiring great time, great space, and great expense for their completion.

Referring more particularly to Cosmical Physics, I feel convinced that meteorology should be pursued in connection with terrestrial magnetism and solar observations; and were a well-considered scheme for solving this great problem fairly introduced, I am sure that scientific institutions and individuals throughout the country would do all that they possibly could to promote this most important branch of physical research.

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

Barometric Predictions of Weather.—Mr. F. Galton, F.R.S. It has long been an established custom to consult the barometer to learn what the weather is likely to be. Now, I propose to investigate the value of this form of barometric authority by showing that it is possible to make strict use of the rules of prediction, notwithstanding the vagueness with which they are enunciated, and then, by comparing a series of carefully-made predictions with facts, to measure the degree to which they correspond. There is another form of barometric authority about which I do not propose to say anything here, namely, where the barometer is consulted in connection with the daily Weather Report. Owing to the new data thereby introduced, an inquiry into the value of those predictions would have to be conducted along an altogether different line to that which I am about to follow.

My comparisons between predictions and facts will be based upon the tracings of the continuously self-recording instruments at Falmouth, established by the Meteorological Committee appointed by the Royal Society, which have been published for the first quarter of the year 1869. It is, however, right to add, that some years ago I made an elaborate inquiry into the Dublin observations during a much longer time, which led, so far as it went, to the same conclusions as now.

I did not publish those inquiries, because I had a misgiving which was never wholly removed until I had the opportunity now afforded by the above-mentioned publication of studying the *continuous* records of instruments in large numbers. It is said that instrumental changes commonly occur in sweeps so large and steady that future changes in them may be to some extent predicted by a knowledge of what has occurred. An analysis of the Dublin observations, made at intervals of three hours, contradicted this assertion, but I felt they might be held insufficient to dispose of it. It might fairly be said that three hours was too long an interval between the observations, and that if the instruments had been read off more frequently, I should have been led to different conclusions. It was necessary to settle this doubt, because, as there is certainly some correspondence between the barometer and the weather, it followed that if it be possible to predict the movements of the former, we shall also, as a matter of course, be able in some degree to predict the latter. I therefore examined the tracings which represent the continuous records of the barometer and other instruments with great interest and care, and soon convinced myself that the irregularities of the barogram and thermogram were far too great to enable us to predict their course from moment to moment. We have only to place a paper upon them, so as to hide what follows any given instant, and to expose what precedes it, and to move the paper forward, step by step, guessing beforehand what we are to see, to be convinced of the vanity of our expectations. This basis of weather prediction is so manifestly unsound, that I shall not take any further notice of it.

We all know that the weather with which the barometer sympathises, is considered to consist of three independent variables—the velocity of the wind, its temperature, and its dampness. It is a question how far the direction of the wind need be reckoned as a fourth distinct influence. We also know that the velocity of the wind is the most important; it is said that when the two other variables are unchanged, and the velocity of the wind alone varies, the barometer may range through two inches, but that it can only range through a quarter as much when either the temperature or the damp are the sole variables. I therefore feel at liberty to begin by simply comparing the changes of the wind's velocity with those of the barometer, in order to obtain a provisional idea of the manner in which they go together.

Two things are very clear at first sight—the one is that the wind's velocity passes through numberless tumultuous variations of which the barometer takes no cognizance, and the other is that a connection decidedly exists between periods of storm and of fine weather, with barometric falls and rises. What, then, do we mean by *periods* of storm? How long is the period during which the velocity of the wind should be summed up and averaged, in order to be made to accord most closely with the barometer?

I made several trials, and protracted the results on the same time-scale as the corresponding barograms. The ordinates of the different points whose position I calculated represented the average velocity of the wind during a definite period at the moment indicated by the point; then I connected the points by a line drawn with a free hand. In this way I constructed a curve, every point of which represented the average velocity of the wind during the space of one hour, being half an hour before and half an hour after the instant corresponding to that point. In another curve, a three-hour period was adopted, and so on. Below all these I copied the barogram.

There could be no doubt, on inspecting those lines, that a one-hour period is far too short, that a three-hour is better, a six-hour better still, and that a twelve-hour is as good as can be obtained. Any period between twelve and sixteen hours seemed equally suitable for adoption, some parts of the curve improving in correspondence as the period was lengthened, and others falling off; but, after a sixteen-hour period, the curve of wind velocity became less varied than the barogram, and the maximum of correspondence was passed. Finding the twelve-hour system the most convenient to employ, I have adopted it here, leaving it to be understood that a different period might be taken within the limits named, without sensibly affecting the results.

The correspondence between the wind curves thus obtained and the barograms is respectably close, there being hardly a dip or rise in the one which has not a counterpart in the other; but they are far from being exactly alike. Neither do the changes, of course, in the two curves, bear an invariable relation in point of time to one another; but, as neither of them lags habitually behind, they must be considered *on the average* simultaneous.

I do not find the correspondence sensibly affected by making broad allowances for the neglected variables. Thus, on marking the epochs of cold and dry polar winds in one way and those of warm and moist equatorial winds in another way, little or no new light was thrown on the reason of the want of coincidence of the two curves. It seemed to me, from this trial, that the influence of temperature, damp, and wind's direction, is considerably less than was commonly believed.

The parallelism of the curves was as close in extreme positions as in mean ones, which confirms the common statement that we must look to differences of barometric height and not to the absolute height for signs of changing weather.

All this is easily compressed into a formula: h_1 h_2 are two successive barometric heights a few hours apart; v_1 (12) v_2 (12) are the corresponding twelve-hour averages of wind velocity; m is a simple factor to be determined by trial, then

$$h_1 - h_2 = m\left\{v_1(12) - v_2(12)\right\}$$

+ a function of temperature and another of damp, neither of which is of much importance.

m is strictly constant only for the same season, because the range of the barometer is wider in winter than in summer, for the same latitude because its range is smallest at the equator, and for the same locality because the wind's velocity may be checked by geographical conditions. Bearing this in mind, the value of m for the first quarter of the year, at Falmouth, as derived from about 100 selected equations of the form $\frac{h_1 - h_2}{v_1 - v_2}$ is -2, when the barometer is reckoned in hundredths of an inch and the velocity in miles per hour. In selecting the equations, I omitted all cases of abrupt change in any of the variables. Consequently our equation becomes

$$h_1 - h_2 = 2 \left\{ v_2 (12) - v_1 (12) \right\}$$

+ the functions of temperature and damp.

It may now be very reasonably asked how it is possible for the barometer to be affected by past and coming conditions of wind. Its sympathy with such considerable periods as six hours before and six hours after the moment of observation, cannot be accounted for on the hypothesis of each new phase of weather regularly making its first appearance high in the atmosphere, because, if it did so, each phase would necessarily disappear from above before it disappeared from the earth's surface, and, consequently, the barometric change would invariably precede the change of average wind velocity, which, we have already seen, it does not. What, then, is the explanation of the curious phenomenon, of the barogram corresponding with the average velocity of the wind, according to the system of twelve-hour periods?

The answer to this question will best be conveyed by a consideration of what we should expect the movements of the mercurial column to be if a suitably made barometer were plunged into troubled water. Its movements would not correspond to each ripple that passed vertically above its cistern, because it would be affected by all the disturbances in an area of surface water whose radius is a function of the depth of immersion. If it were plunged to the depth of many fathoms the mercury would wholly cease to oscillate, because the average level of the large area with which it sympathised would be constant, however much its surface might be broken up into undulations. If it were immersed to a suitable depth, the mercury would foretell the advent of each wave of exceptional size, before an exceptional height of water had arrived vertically above the barometer. It is easy and interesting to make an experiment to the same effect, by dipping a glass tube, open at both ends, straight into a pan of water, and disturbing the water with the hand. When the tube is dipped but a short way in, the water it encloses harmonises in its oscillations with the water that surrounds it, but this harmony is diminished, and the oscillations in the tube become more sluggish, as the tube is immersed more deeply, and at length they disappear altogether. In precisely the same way I believe the mercury in the barometer sympathises with atmospheric disturbances throughout a wide circle. Its height represents the average value of them at the moment of observation, and when a great atmospheric disturbance sets in, as is wont, from the westward, the barometer is affected some time before the arrival of the locus of greatest disturbance. The diameter of the circle which affects the barometer may admit of being determined in more than one way, but I am not now concerned with its linear measurement. What I am immediately in search of is, what the diagrams have already told me, that its diameter in relation to its usual rate of movement is such, that it is commonly twelve hours in passing over an observatory.

It appears to follow that the twelve-hour period for averages must apply not only to the wind but to all other elements of atmospheric disturbance, such as temperature and damp. Therefore the undetermined portion of our equation will be functions of $t\,(12)$ and of $d\,(12)$.

Without professing to decide the precise nature of those functions, we may be sure that it does not differ materially from a simple proportion, within the limits of meteorological records. The inferior importance of these functions makes a small error of still less consequence. I therefore assume the undermentioned portion of the equation to be

$$p \left\{ t_1 (12) - t_2 (12) \right\} + q \left\{ d_1 (12) - d_2 (12) \right\}$$

Calculating on the basis of the already quoted statement, that temperature and damp, unaided, may respectively affect the barometer to the amount of half an inch, p and q may both of them be considered equal to -1, when t is reckoned in degrees Fahr., and d is the vapour tension expressed in hundredths of an inch. For reasons already mentioned, I disregard the direction of the wind. Consequently the formula becomes

$$h_1 - h_2 = 2 \left\{ v_2 (12) - v_1 (12) \right\} + \left\{ t_2 (12) - t_1 (12) \right\} + \left\{ d_2 (12) - d_1 (12) \right\}$$

and I now proceed to utilise it, in making a series of predictions for comparison with facts.

Let h_1, h_2, be separated by an interval of six hours, which I will distinguish by the letter b; similarly let a represent the six hours that preceed h_1, and c the six hours that succeed h_2.

Now the average wind velocity during the twelve-hour period $a + b$ is half the sum of the average velocity during the six-hour periods a and b,

or $$v_1 (12) = \tfrac{1}{2} \left\{ v (a) + v (b) \right\}$$

also $$v_2 (12) = \tfrac{1}{2} \left\{ v (b) + v (c) \right\}$$

$$v_2 (12) - v_1 (12) = \tfrac{1}{2} \left\{ v (c) - v (a) \right\}$$

similarly for t and d.

Therefore our equation becomes

$$h_1 - h_2 = v (c) - v (a) + \tfrac{1}{2} \left\{ t (c) - t (a) \right\} + \tfrac{1}{2} \left\{ d (c) - d (a) \right\}$$

and

$$v (c) = h_1 - h_2 + v (a) + \tfrac{1}{2} \left\{ t (a) - t (c) \right\} + \tfrac{1}{2} \left\{ d (c) - d (a) \right\}$$

Using this simple formula, I selected all the periods during which the changes of the barometer had been abrupt or otherwise characteristically marked, and I calculated the values of $v\,(c)$ during those periods, obtaining in this way a total of 106 predictions. Comparing these with the actual facts, I obtained a mean error of ten miles per hour. Next, in order to procure a standard whereby to ascertain the importance of this error, I obtained and took the mean of a series of differences between the same observed values of $v\,(c)$ and $v\,(b)$; in other words, I calculated what the mean error would be supposing it was invariably asserted that the average wind velocity for the next six hours would be the same as during the six hours just elapsed. The mean error in this case was only 7·7 miles per hour. This extraordinary result made me curious to learn whether the co-efficients of t and d might not be altered with advantage; so I first made them both $= 0$, in fact, ignoring the influences of temperature and damp altogether. The mean error again came out ten miles per hour, the gains and losses due to the correction having balanced one another. Secondly, I made the co-efficients each $= -2$, that is to say, I doubled the importance originally given to temperature and damp, and the mean error rose to 11·3 miles per hour.

The result of all this is, that, judging by the experience of 106 well-marked instances of change occurring at Falmouth during the first quarter of 1869, it is more unwise in the ratio of 10·0 to 7·7 to be guided by the barometer, than to say off-hand that the weather will continue as it has been. Also that there can be no gain and may be further loss, if the wet and dry thermometers be consulted as well.

It is, no doubt, possible that the errors I have assigned may be qualified in a trifling degree by other calculators. They may adopt periods of average and numerical co-efficients, somewhat differing from my own; also, their data as measured off from the instrumental tracings, may be more accurate than those that I made, but I feel satisfied there is no mistake in the broad truth of my results. After more tentative analysis than I care to describe, I believe it impossible to substantially improve these predictions. and the experience I gained from the Dublin observations makes me doubt whether a more extended examination would lead to different conclusions. The barometer, when consulted by itself, without a knowledge of the weather at adjacent stations, can claim but one merit, namely, to guide us in a form of storm which does not occur once a year in the British Isles, of a fall in the mercury outstripping in an extraordinary degree the increasing severity of the weather; and I believe it to be on account of this rare phenomenon here, and of the reports of sailors from hurricane latitudes, where it is much more frequent, that the fame of the instrument has been so widely spread. But for use in ordinary English gales, and still less in ordinary English weather, this inquiry shows the barometer to be one-third worse than no guide at all. It is better to base our expectations upon what has occurred, than also to take the barometer into our counsel. We easily see the reason of this to be, that the theory of prediction involves many postulates, every one of which must be strictly fulfilled in order that the result may be correct. But they are only true on the *average* and not in the individual case. The area with which the barometer sympathises is never exactly twelve hours in passing over us; the six-hour radius of that area, which has already gone by, is not an accurate sample of the demi-area of which it forms the central strip; neither is it at the moment of observation in the same condition as when it passed over us. Precisely the same may be added in respect to the six hours of weather which are the subject of prediction. It must also be especially borne in mind that whatever error may affect the twelve-hour average is necessarily doubled in the six-hourly prediction, because the difference between what was expected of the whole twelve hours, and what has been fulfilled in the first half of it, must be heaped on to the second half, which has therefore to bear an additional load of error, equal in amount to its rightful share. Thus, if 100 miles of wind had been expected, and eighty miles really came, in the twelve hours, the error of the expectation would be one in four; but if forty miles of wind had come in the first six hours, the prediction would assign sixty miles to the next six hours, whereas the fact would show forty miles, making an error in the prediction of two in four, or double the original error of expectation for the whole twelve hours.

SECTION B.—CHEMICAL SCIENCE

On the Utilisation of Sewage, with Special Reference to the Phosphate Process.—Mr. David Forbes, F.R.S. &c. The processes at present employed for the treatment of sewage were classed under two heads: the purely mechanical and the mechanico-chemical. The former, which at best only effected a mere filtration of the sewage, have everywhere failed to effect any such purification of the sewage water as was necessary in order that it might be allowed to flow directly into the streams. Sewage irrigation was included in the latter class, not because any direct chemical treatment was employed, but for the reason that, whilst the soil acted mechanically as a filter to separate the solids, the chemical action exerted by the vegetation decomposed and assimilated the organic matter, ammonia, and other available substances in the liquids also. The more purely chemical processes, such as the treatment by lime alone, or in combination with chloride of iron, alum, sulphate of alumina, and, lastly, the so-called A B C process, were next alluded to, but regarded as failures, since the evidence brought forward not only proved that the affluent water had not been sufficiently purified, but also that the sewage manure obtained was, by the admixture of the materials employed in these processes, rendered of so low an agricultural value as to preclude its employment elsewhere than in the immediate neighbourhood of the sewage works.

Admitting that sewage irrigation was to be recommended wherever the local circumstances were favourable to its employment, it was maintained that under many circumstances it was neither applicable nor advantageous, and that in these cases it was preferable to employ a chemical treatment, which had the advantage of not requiring any large outlay for pumping or distributing machinery, or the purchase of large areas of ground for sewage farms. For this purpose an entirely new process was recommended, called the phosphate process, based on the property which hydrated phosphates have of combining with organic matter, whilst the ammonia also can be precipitated in the condition of the double phosphate of ammonia and magnesia.

The process was shown experimentally with Liverpool sewage, and consisted merely of adding a solution of certain phosphates, chiefly of alumina, in sulphuric or hydrochloric acid to the sewage, and afterwards a little milk of lime barely sufficient to neutralise the acid and give a faint alkaline reaction to the sewage; even if tinctorial matters of great intensity (ink was added in the experiments) were present, the liquor became immediately discoloured, the supernatant liquor resting quite clear above a precipitate of the phosphates along with all the insoluble matter and a large portion of the soluble organic matter and ammonia originally contained in the sewage. The authors of this process, Messrs. A. Price and D. Forbes, although they did not pretend to have extracted the entire amount of the ammonia and other matter valuable for agriculture from the sewage, or to effect an absolute purification of the affluent water, believed that, as the water so purified was free from any nauseous taste, so that it could be drunk without repugnance, was devoid of smell, and did not putrefy or emit any disagreeable odour even when left standing in an open vessel during the whole of the preceding hot summer, that it had been sufficently purified by the phosphate process as to permit of its being directly run off into rivers without detriment to the fish in them or the health of the inhabitants on their banks.

A most particular feature of this process when compared with the other processes of precipitation was, that the substances employed in effecting this purification were not detrimental to the agricultural value of the precipitated manure, but, on the contrary, added so much to its value as to enable it to bear the cost of transport to distant parts of the country, and thus showed some hope that the value of the manure might be sufficient to pay for the expense of treating the sewage.

SECTION C.—GEOLOGY

Remarks on newer Tertiary Fossils in Sicily and Calabria. Mr. J. Gwyn Jeffreys, F.R.S. In the last deep-sea exploring expedition in H.M.S. *Porcupine*, in the Bay of Biscay, and along the Atlantic coasts of Spain and Portugal, Mr. Jeffreys procured at considerable depths, and especially from 994 fathoms, many species of mollusca in a living or recent state, some of which had previously been regarded as fossil only, and extinct, and all as belonging to the newer tertiaries of Sicily and Calabria; and he believed that a record of the fact might lead to the further discovery of the geological phenomena which had caused the fossilisation of such species in that limited area. Several of these species inhabit northern, and even Arctic, seas; such are *Terebratula cranium*, &c. Other species now found in a living or recent state, are *Terebratula spheroidea*, &c. One of the last species, in the last list or category (*Fiparesepla papillesa*) was also dredged by Mr. Jeffreys last autumn, at Dröbak, in Norway; and he was of opinion that our knowledge of the Arctic marine fauna was very imperfect. The newer Tertiary fossils of Sicily and Calabria had been to a great extent investigated by Dr. Philippi, formerly of Cassel, Professor Seguera of Messina, the Abbé Brognone of Palermo, and Dr. Tiberi of Resina near Naples; and their collections had been examined by Mr. Jeffreys. Two suggestions or questions were submitted by the author of the present paper. 1. Have not all the deep-sea species of European mollusca originated in the north, and spread southwards in consequence of the great Arctic current? 2. Inasmuch as the pliocene division of the Tertiary formation is now ascertained to contain scarcely any extinct species, and the future explorations may reduce the percentage of such species to *nil*, may not that artificial division hereafter merge in the quaternary formation, and the tertiaries be restricted to eocene, miocene, and pliocene?

The President and Sir Roderick Murchison spoke of the great importance of this communication, and the latter hoped Mr. Jeffreys did not share the opinion of his colleague Dr. Carpenter, that their discoveries tended to upset modern geology.

Professor Duncan confirmed Mr. Jeffreys' statement with respect to the specific identity of corals from deep-water with those of the South-Italian tertiaries.

The Rev. H. W. Crosskey also addressed the Section as to the glacial fossils of Scotland being quaternary and not tertiary.

Mr. Jeffreys, in reply, begged to assure Sir Roderick, as one of the parents of English geology, that he need not be under any apprehension for his offspring, so far as the deep-sea explorations were concerned.

Modern and Ancient Beaches of Portland.—Mr. W. Pengelly. The Chesil Bank having been described, the author stated that he had found amongst the flints of which it was chiefly composed, specimens of Budleigh Salterton pebbles, some containing the fossils occurring in these pebbles. Some granite pebbles were probably from the valley of the Teign. From these specimens it was concluded that the transportation along the coasts of South Devon and South-west Dorset is up Channel, that is, in the direction of the prevalent winds. The Raised Beach of Portland Bill consists of 7 feet of yellow clay, the same of pebbles, sand and shells from the Raised Beach, and 50 feet of rock resting at sea-level on a shingle beach. The shells are species now living on the shore. The beach was held to indicate an elevation of the coast of not less than 50 feet; and the pebbles showed that at the time of their deposition the direction of transportation was the same as now. Portland was then an island.

On the Occurrence of Seams of Hard Sandstone in Middle Drift of East Anglia.—Mr. J. E. Taylor. This sandstone was composed of 66 per cent. of siliceous sand, cemented by 33 per cent. of carbonate of lime. It occurs immediately below the upper or chalky boulder-clay. Formerly it was employed at Norwich in building, the Castle being built of it. Even in beds later than the boulder-clay, specimens of indurated sandstones had been found by the author, proving, as he believed, that the older rocks