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

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

+ 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 owed their compactness more to the cementing material than to heat or pressure.

On the Palæontological Aspects of the Middle Glacial Formation of the East of England, and on their bearing upon the Age of the Middle Sands of Lancashire.—Messrs. Searles V. Wood, and F. Harmer. The authors gave a list of 65 species of shells obtained from the middle glacial sand in the neighbourhood of Yarmouth, of which a large proportion, about 20 per cent., are not now known to be living, and one of them, *Erycinella ovalis*, a coralline crag form, being almost generically extinct; among which they had found five or six shells apparently new. The formation altogether presents a decidedly southern aspect, with strong affinities to the crag, only two species (with the exception of the new forms) being unknown to the crag beds. On the other hand, the middle sand of Lancashire, as at present described, did not appear to have yielded any shell not now to be found in the seas of the immediate neighbourhood. They pointed out that the mere fact of the middle glacial sands of the east of England, and the middle sands of Lancashire being both of them underlaid and overlaid by boulder-clay, was altogether inconclusive, and urged that all the evidence we had at present before us would relegate the latter to a much more recent position in the glacial sequence. They suggested tentatively that the Lancashire sands might possibly prove to belong to the Hessle series, as there seemed to be a close resemblance between the list of shells from Kelsey Hill, believed by them to belong to the Hessle series, and those from Blackpool.

In the discussion which followed, Sir Charles Lyell was understood to say that he was inclined to accept the conclusion of the authors.

Mr. Hughes did not think there were as yet sufficient data to correlate the drifts of the East and West of England. But little was known of the relations of the drifts of the West to one another. We must first trace the included fragments to their origin in the mountains, and not form any theory to account for the origin and succession of the wide-spread drift of the lower country, which will not also account for the phenomena observed in the hills. As the result of his own observations, he described three drifts occurring in the district north of Liverpool, which might be roughly distinguished from one another.

1. A stiff blue clay with included fragments of the rocks of the immediate neighbourhood. This drift occurs at the highest levels up to 2,100 feet. The fragments are striated when the rock is of such a character as to preserve the scratches.

2. The ordinary stony clay drift which occurs along the valleys and runs up the hill-sides to about 1,800 feet. In the *Fell-top drift*, No. 1, the matrix is a very uniform stiff lead-coloured clay, no matter what it rests on; while the included fragments may have come from rocks about the same level and close at hand. In the *Hill-side drift*, No. 2, the matrix varies more according to the rock on which it rests; while the included fragments, which are more numerous than in the *Fell-top drift*, No. 1, are derived from higher up in the same drainage area; and, where different kinds of rock occur on opposite sides of the valleys, the drift on either side is chiefly derived from the rocks on the same side, as if it were the lateral moraine of a glacier coming down the valley.

3. In the lower valleys false-bedded sands and gravel, such as might be produced by the action of the sea at the end of the receding glaciers, overlap the clay drift No. 2, and are almost continuous with the great mass of gravel drift which is so largely developed on the lower ground of North Lancashire. But while these divisions are tolerably clear in a large way, in detail it is difficult to draw a line between them; and when we try to group all the drifts in and around the mountain districts of Wales and the North of England under one of these heads, or to fit all the observed phenomena into any scheme of regular increase and decrease of cold—any uniform submergence or elevation, we find many exceptions and complications. The false-bedded sands and gravels usually occur along the larger valleys at low levels, but sometimes we find similar sand gravel dovetailing into the boulder clay at various heights; in another place occurring along terraces 1,000 feet above sea-level. In the absence of organic remains, we cannot yet say which of these should be referred to marine action and which to fresh-water streams and ponds in and near the melting ice. Flints commonly occur in the lower gravels, but once a large unworn flint, about eight inches in diameter, was found in the re-sorted surface of the highest drift at about 1,900 feet above the sea. In the case of the Shap granite boulders, to the mode of distribution of which Professor Harkness has devoted much attention, the difficulties are more obvious, as the rock is so marked. Boulders cannot be formed except when the rock from which they are derived is above the ice and water. This limits the submergence and depth of the ice as to the maximum. But according to the view that the boulders were transported over Stainmore into East Yorkshire on floating ice, the south end of the Pennine range must have been submerged. This limits the submergence as to minimum. What was the line of transport of the Shap boulders *before* the submergence of Stainmore? Again, in the drifts on the top of Stainmore, we find not only boulders of Shap granite, but also fragments of the Permian brecciated conglomerate which can have come only from the bottom of the valley more than 1,000 feet below. Can we believe that these have been lifted by shore ice from time to time throughout that long submergence, or have we evidence of older drifts of very different origin being washed, sifted, and sorted by the encroaching sea. Again we find, even on the north side of steep mountain ranges, where we should have expected the glaciers to have lingered longest and to have ploughed out the old drift, that even boulder-clay has travelled up the hill from the lower ground, and must therefore be referred to a period when its transport was irrespective of the present valleys and mountain slopes.

In fact, there is much evidence to show that the land ice has often ploughed across and transported marine deposits, and the sea has often washed and re-sorted the *debris* brought down by land ice, and thus the drift has been used up over and over again. That might be the reason why we so often find fragmentary and rolled shells associated with perfect though delicate shells, which seem to be of the age of the deposit in which they are found. He quite agreed with Mr. Searles Wood as to the derivative character of some of the shells in the gravel drift recently described by Mr. Jamieson.

But even frequent oscillations of level would not alone be sufficient to account for the manner of occurrence of the marine drifts, especially when the palæontological evidence is considered. The agencies which produce the warm Western Ocean currents must have been in operation throughout the long period under notice, but the circumstances which determined the direction of those currents must have varied with the changes of level. He asked what would be the effect upon the Gulf Stream of a submergence or an elevation of a large part of the bed of the Atlantic to the amount of 2,000 feet or more? The shells of Moel Tryfaen, though of less Arctic character, might well be referred to the period of greatest general cold, provided the form of the sea bed and distribution of land turned a warm ocean current on that part of the western coast.

He would, therefore, urge the expediency of adopting the method always taught by Professor Sedgwick to his pupils—first, to establish clearly the relation of the beds in each separate area, and to avoid obscuring an already complicated question by adopting prematurely in the West the local nomenclature of the East of England.

On Certain Glacial Phenomena in the Central District of England.—Rev. H. W. Crosskey. The author had determined the existence of glacial striæ in the central plateaux of England, and covering these markings on true boulder-clay, physically corresponding to the older "till" of Scotland. The clay with granite boulders in the midland counties was of marine origin. A succession of drift beds was established from an actual section showing a boulder-clay resting on the Bunter sandstone; second, sands and gravels with false bedding; third, a clay with pebbles; fourth, a bed of sand mixed with clay.

On some Thermal Springs in the Fens of Cambridgeshire.—Mr. F. W. Harmer. In several farm-yard wells near Chatteris, of the depth of about ten feet, the author had found water of the temperature of 74½° Fahr. on the 14th March, the air being but 37°; and in June of 79¾°, the air being then 70°. An analysis of the water by Mr. F. Sutton showed that the heat was not due to chemical causes. The fens being below the sea-level, and therefore permanently saturated with water at the depth of ten feet, and the phenomenon described being apparently continuous over an area of ten miles, and no doubt further, the cause producing the heat would not be an insignificant one. Mr. Judd, of the Ordnance Survey, affirms that the secondary rocks of this neighbourhood are extensively faulted, and may thus afford a communication with the central heat of the earth.

On the Matrix of the Gold in the Scottish Gold Fields.—Dr. Bryce. The author had found gold in the fragments of granite, and tracing it to the native rock, he obtained the crushing of a sufficient portion to prove that it was distinctly, though not remuneratively, auriferous.

Some Remarks on the Denudation of the Oolites of Bath.—Mr. W. S. Mitchell. The author held that there was no proof establishing the continuity of the Oolites. He thought they were accumulated locally, and, as it were, in patches, with currents sweeping in between. The sedimentary matter which followed filled in the spaces forming the Bradford clay. Denudation subsequently came into play, and the ready-yielding clay formed the valley.

On an Antholite discovered by Mr. C. P. Peach.—Mr. W. Carruthers. Various estimates of the position and of the structure of this fossil had been formed, but the specimens found by Mr. Peach established that the bud sprang out of the axil of a bract, and consisted of several scales supporting three or four flowers, having fruits which had been described as species of *Cardiocarpum.*

On the Sporangia of Ferns from the Coal Measures.—Mr. W. Carruthers. These organs were found in what were called coal-balls, from the beds of coal at Bradford and Halifax. They exhibited the structure, form, and attachment of the sporangia of some recent hymenophyllaceous genera.

SECTION D.—BIOLOGY

Department of Zoology and Botany

On the Larval State of Molgula, with description of several new species of simple Ascidians.—Mr. A. Hancock. The author described the tadpole larvæ of *Molgula complanata*, and referred to the Amœba-like form of larva described by Lacaze Duthiers, as found in *M. tubulosa*; but it may be doubted whether this species is a *Molgula*, and reasons for believing it to belong to a new genus *Eugyra* were given. Many new species were described belonging to the genera *Ascidia* (11), *Carella* gen. nov. (2), *Ciona* (1), *Molgula* (3), *Eugyra* (1). Many of these species were sent to Mr. Hancock by Dr. Bowerbank, Rev. A. Norman, and Mr. A. G. More.

On the Structure of the Shell in the Pearly Nautilus.—Mr. H. Woodward. After referring to the great interest attaching to the *Nautilidæ* on account of their vast geological and geographical range, the author proceeded to describe the structure of the shell with its septa and siphuncle, the latter structure being only found in the Cephalopoda and nearly confined to the Tetrabranchiate division of the class. The camerated structure, however, is found both among the Bivalves and Gasteropoda, and the author suggested that if any incipient character could be found leading up as it were to the siphuncle, we might fairly infer that that structure was only a more highly-differentiated form of shell-growth. Such incipient structure occurs in the *Ostreadæ* and *Spondylus*, in which the shell-muscle dips down from layer to layer, offering a rough similarity to the siphuncle in *Aturia* and some other *Nautili.* Mr. Woodward described the structure of the shell, and showed by actual dissection that no vascular system exists between the shell and the animal by means of the siphuncle. The siphuncle proves only to be a pearly tube, within which is another composed of an extension of the periostracum, and quite destitute of vascular or cellular structure. Shell structure proves, when once formed, to be dead matter, destitute of change, and can only be repaired when in contact with the mantle of the shell.

On a New Species of Coral.—Mr. W. S. Kent. In 1869, when examining the collection of Madrepores in the Paris Museum, the author found a worn specimen having a close general resemblance to *Alveopora fenestrata*, Dana. A superficial examination at once showed it to be quite distinct from that species, and the presence of numerous irregularly-disposed but perfect and well-developed tabulæ demonstrated its close relationship to the ancient genus *Favosites*; the cretaceous genus *Koninckia* forming the immediate connecting link. To this remarkable form he gave the name *Favositipora deshayesana.* Diagrams were exhibited showing the structure of this new coral, as also photographs of the original specimen.

The Secretary read a letter from Dr. J. E. Gray, of the British Museum, in which he described a new genus, *Callisphæra*, for the beautiful sponge from Setubal, described by Mr. Kent as *Pheronema grayi*, and which he believed differed in several particulars from the genus *Pheronema*. He also provisionally referred the *Holtenia pourtalesii* of Schmidt to the genus *Vazella*, and the same author's *Tetilla polyura* to the genus *Polyurella*.

Mr. J. Gwyn Jeffreys, who had just returned from the south of Europe, after having accomplished his part of this year's deep-sea exploring expedition in H.M.S. *Porcupine*, stated that in this cruise he had dredged across the Bay of Biscay, and along the coasts of Spain and Portugal to Gibraltar. The weather had not been favourable; but the depth reached was 1,095 fathoms. A large collection of Mollusca, Echinoderms, Corals, Sponges, and Hydrozoa, had been made. Half-a-dozen specimens of a beautiful new *Pentacrinus* (*P. wyville-thomsoni*) had been taken in 795 fathoms depth, between Vigo and Lisbon. Both Northern and Mediterranean species of shells were met with.

On the Vegetable Products of Central Africa.—Colonel Grant.

On the Parasitic Habits of Pyrola rotundifolia.—Mr. Gibson. In the discussion which followed this paper, the opinion was expressed that the parasitism of this species was not yet proved.

On the Desert Flora of North America.—Dr. C. Parry.

Note on Ribes spicatum.—Professor Lawson.

Mr. E. Birchall exhibited a beautiful collection of Hybrid Sphingidæ and other Lepidoptera.

Department of Ethnology and Anthropology.

Account of the Exploration of the Victoria Cave, Settle, Yorkshire.—Mr. Boyd Dawkins.

Account of certain remarkable Earth Works at Wainfleet, Lincolnshire.—Rev. C. Sewell.

On Ancient Sculptures and Works of Art from Irish Cairns.—Dr. Conwell. The author gave a brief account of his discoveries and researches during the summer of 1865, among the ruined remains of thirty-one cairns, extending along "the Loughcrew Hills," in the county of Meath, about two miles south-east of the town of Oldcastle. So far had these ancient remains escaped all public attention previously, that it was only during his investigation of them that an officer was sent from the Ordnance Department to insert them upon a map, which is now zincographed and sold by the Ordnance publishers. Revered and sacred in former ages as must have been these resting-places of departed splendour, standing out conspicuously on the peaks of a range of hills rising to the height of nearly a thousand feet above the level of the sea, in the proverbially flat country of Meath, it is very remarkable that the site has not yet been identified with any description, reference, or allusion, in the ancient annals of the country. The unroofed chambers, and the fragments of broken urns, afforded practical evidence that these ancient tombs had been plundered at some previous period, and this fact gives additional interest to the miscellaneous collection of articles of stone, bronze, iron, amber, glass, bone, &c., found in them.

In the remains of fourteen of the cairns, the large upright stones, to the number of 115, which formed the interior chambers of the cairns, were found inscribed with devices, almost entirely novel, sometimes *punched*, and at other times *engraved*, the diversity of which may be inferred from the fact, that out of all this number there are not two alike. No key has yet been found for reading or interpreting these devices. A series of drawings were exhibited of the symbols on thirty-one stones from *a single cairn* at Loughcrew, being exactly the number of inscribed stones in the two well-known cairns of Dowth and New Grange, taken together in the same county.

The following are some of the more remarkable objects of art which were found:—Several small stone balls of various colours, one of syenite beautifully polished, and nearly three inches in diameter, and another somewhat larger, of brownish red hæmatite or iron-stone; an oval object of jet-like appearance and polished; two pendants and a bead, all of different colours, evidently portions of a necklace of stone; a ring; probably part of an ear-ring, made of jade, and nearly worn across in one place; one polished flint nodule; one leaf-shaped flint arrow-head.

Of bronze, were found several small open rings, and a very perfect bronze pin, with ornamented head.

Of iron, as might be expected, in a much corroded state, some fragments of uncertain use, two small iron rings, a piece of iron resembling one leg of a compass; another an iron chisel or punch.

Of amber, some small beads of different shapes and sizes.

Of plain glass, some small beads differing in shape and colour; and one "*double bead*," imperforated, and of a soft green hue, and some glass fragments.

Of bone, were obtained two bone beads, some bone pins—one with ornamented shaft and a metallic rivet, apparently for attaching a bead; a collection of the remains of nearly 5,000 worked bone flakes of various sizes and patterns, in a few instances preserving their original polish, as if quite modern; and upwards of 100 of these ornamented in a very high order of art, with various circles, curves, ornamental puncturings, &c., of which no description could give an adequate idea. On one was found the representation of an antlered stag: but what may have been the uses to which these bone flakes were applied, Dr. Conwell expressed himself unable to come to any conclusion. As the accounts are very meagre of any articles of "historical value," having ever been extracted from cairns, the collection now brought under notice is probably the most curious and remarkable which has ever been found joined together under similar circumstances.

On some forms of Ancient Interment in County Antrim.—Dr. Sinclair Holden.

On a Discovery of Platycnemic Men in Denbighshire.—Mr. Boyd Dawkins and Prof. Busk. Mr. Dawkins explained that the remains to which he referred were found at a place in Denbighshire which rejoiced in the name of Perthi Chwaren. They were in a cave in the mountain limestone, and the explorers found from twenty to twenty-five human skeletons, and a large quantity of the remains of animals. The skeletons were interred differently from those of modern times, in that they were lying in confused heaps, which clearly showed that the people had been buried in a sitting posture. The cave (he said) was inadequate to contain such a large number of human corpses at one time, so it followed that it was used at different times, probably as a family mausoleum. There were also found bear's bones, fragments of mussel shell, a specimen of cockle, and a tusk, one of the largest he (Mr. Dawkins) had ever seen. There was likewise discovered some pottery, fragments of coal, and a splinter of iron which was not oxydised. The only evidence as to the antiquity of the cave was a fragment of flint. Flint was used by the Romans and Egyptians, and the discovery pointed to the fact that at one time flint was the only material in use, but it did not show that this deposit was of the date when no metals were known. Mr. Dawkins thought all the evidence went to show that the cave was of the Neolithic age.—Prof. Busk next gave his conclusions with regard to the skulls which were found in the cave. He said that the people whose remains had been discovered were of low stature, the skeletons being only from 5 feet to 5 feet 6 inches in height.

On Carved Stones recently Discovered at Nithsdale, Scotland.—Dr. Grierson.

On a Quartz Implement from St. George's Sound. — Mr. H. Woodward. This crystal of quartz (having its terminal planes preserved at both ends) was found by his colleague, Mr. Davis, among a number of other minerals in the British Museum, forming part of the Old Sloane Collection. On examination it was found to have written on it in ink, "St. George's Sound, N.W. coast of America, Captain Cook." The crystal had been used as a flint implement, one end being sharp and the other notched. It had an attachment for a wooden handle, which would admirably fit it for picking holes in the ice, through which the Esquimaux might fish.

On a Flint-flake Core from the River-gravel of the Irwell, Salford, Manchester.—Mr. John Plant. The upper valley of the Irwell, the author said, was overspread with silt and sandy layers. Terraces of above 200 feet in elevation were very distinct in places. The river now flowed over the beds of New Red sandstone, having contracted its bed for at least a mile to about sixty yards. The upper terrace was composed of sand and gravel of older age than the silts which fringe the banks. The pebbles of gravel were mainly derived from the pebble beds and eroded silt, others were flattened pebbles from the coal measures. Throughout these pebbles there were no flints, but bits of chert only from mountain limestone. The weapons of Lancashire were neolithic in character, so that the occurrence of a flint-flake was remarkable from its site in the barren desert of gravel and sand in the Irwell. Mr. Plant thought the specimen he exhibited belonged to the time when the east of England was in the occupation of the early palæolithic people of Europe.

Remarks on Stone Implements from Western Africa.—Sir John Lubbock, Bart.

Section E.—Geography

The Lines for Ship Canals across the Isthmus of Panama.—Gen. W. Heine. The author said that in his various explorations, extending over twenty years, he had often found the reports made by other explorers differed from existing facts. In cases where elevations of only 150 feet had been reported, 900 feet were found to exist; and few explorers seemed to have taken into consideration the geological formation, which often consisted entirely of porphyry and basalt, and was almost as hard as cast steel. After giving an account of the line proposed across the Isthmus of Tehuantepec, Honduras, Nicaragua, and nine lines proposed across the Isthmus of Darien, he came to the conclusion that only two lines were of a kind to deserve consideration, because the expense of constructing and working the canal would not be out of proportion to the benefit derived from it.

These two lines are—1. From Aspinwall along the line of the railway to Panama, with an extreme elevation of 269 feet, a length of thirty-five miles, through rocks of porphyry and basalt, and with but middling ports of entry. 2. From the Gulf of Darien through the rivers Atrato, Casarica, Paya, and Tingra, to the Gulf of San Miguel, with an extreme elevation of 186 feet, length 52 miles, through a soil composed of alluvial deposit with some thin ranges of greyish sandstone or schist, and with very good ports of entry.

The survey of the first line was very perfect, that of the second line less so, and a more exact level was desirable. Of the nineteen expeditions undertaken, twelve were of American origin, four were undertaken by Frenchmen, one by a native Columbian, and only two by Englishmen. Considering the vast interests England had at stake in shortening a marine passage to Australia, the west coast of America, the islands of the Pacific Ocean, including Japan, he was astonished at the lack of energy, especially as the very moderate expense of 1,000*l.* to 1,500*l.* would suffice for all necessary exploring purposes.

Section G.—Mechanical Science

Hydraulic Bucketting Engine for Graving Docks and Sewerage.—Mr. Percy Westmacott, C.E. This was a short paper describing the mechanical appliances devised by Mr. Westmacott, at the suggestion of Mr. George Fosbery Lyster, dock engineer, for the purpose of emptying the Herculaneum Graving Docks, Liverpool. They were devised with the view of working in conjunction with the system that the dock engineer had resolved to adopt for working the gates, capstans, &c., and thus save the erection of another steam engine and plant for the special purpose of emptying the docks. The system adopted was the hydraulic system. The essential feature of Mr. Westmacott's engine is a scoop-shaped bucket, constructed of wrought iron, and capable of lifting 14½ tons of water. It is undesirable to give the constructive details of the engine without the drawings by which the paper was illustrated; but the following facts may be mentioned:—The minimum lift at the high level discharge is 7 feet, and the maximum 23 feet, and about 5 feet more stroke is required for tipping up the bucket. About 3 feet per second is the usual average speed of the bucket in plunging or lifting. The filling is effected in 5 seconds, but the emptying occupies from about 12 to 15 seconds, owing to a contraction that had to be made at the front end of the bucket to suit the existing masonry in the well. This latter operation, with a free mouth to the bucket, should not require any more time than the filling, if even so much. The coefficient of effect obtained by this engine is as follows:—At 7 feet (minimum) lift, ·4; at 23 feet (minimum) lift, ·6—average, ·54; which will be found to compare not unfavourably with other appliances under the same conditions of working; but the loss occasioned by the choking of passages and gagging of valves or paddles is altogether avoided by this system, which, for this reason, is peculiarly well adapted for sewerage purposes.

The President, Mr. R. B. Grantham, C.E., and Mr. Oldham expressed approval of the engine. Mr. J. F. Bramwell, C.E., F.R.S., said that for short lifts the engine would be economical, but for very high lifts he thought it would be inapplicable. In reply Mr. Westmacott said there would be no difficulty in making a 40-feet lift, while beyond that height there might be two or three buckets each above the other.

On Appliances for the Production of Heavy Forgings.—Lieut.-Colonel Clay. In this short paper the author mentioned, under the following three heads, the improvements recently introduced into the manufacture of large forgings, as illustrated by his experience at the Birkenhead Forge:—1. Improved heating by the use of Siemens's regenerative gas furnaces. 2. Facilities for handling and removing large masses of wrought iron from the furnace to the hammer, and for moving them when under the hammer. 3. Improved hammers, with a clear unfettered fall, and with such width of standards as to give the workmen all the comfort and convenience possible in executing the necessary operations of shaping, forging, and cutting the material to the required form.

On Hammering and Stone-Dressing Machinery.—Dr. J. H. Ll yd. The author claimed to have devised machinery which was particularly applicable for cutting, sawing, chiselling, drilling, and dressing stone and other substances, for forging and hammering metals, and for working the tools in general by motive power, so as to supersede hand-labour. The invention has not yet been applied; indeed, the improved machinery as yet only exists in the state of a model. The paper was illustrated by numerous drawings.

REPORTS OF COMMITTEES

REPORT OF THE RAINFALL COMMITTEE

This report was read by Mr. G. J. Symons, the secretary of the committee. It commenced by referring to the steps taken last year to secure uniformity in the registration of rain by the observers throughout the country, and to the acceptance by the General Committee of the recommendation of the Rainfall Committee that additional observers should be obtained in parts of the country where at present such observers are far from one another. Dartmoor was last year quoted as an illustration; thither after last meeting Mr. Symons proceeded, and the result is that the number of stations in that district has been doubled. There are, however, still two parts of the moor where no one lives, and no one has yet been found willing to superintend a gauge. Reference is next made to other steps taken by the committee to secure returns from various other districts, and to the success of these efforts. The committee close this portion of their report by pointing out that to keep up an amateur staff adequate to the requirements of the subject, say from 1,500 to 2,000 observers, it is indispensable that a number of new ones be enlisted each year to supply vacancies caused by deaths and removals, and they therefore intimate their desire to receive through their secretary (Mr. G. J. Symons, 62, Camden Square, London) offers of assistance from parties willing to provide themselves with the inexpensive and simple gauge now generally in use. The report then proceeded to mention that the secretary has during the past year visited and examined the gauges in use at upwards of one hundred stations. By this personal intercourse greatly improved accuracy and uniformity of procedure is secured. The committee regret that through want of funds they have been unable to make any progress with the collection of old returns during the past year. The report then proceeds to describe certain experiments carried out at Calne, in Wiltshire, by Colonel Ward, with a view to determining the difference in the amount of rain collected at various heights above the ground, not so much with a view to determining the cause of this variation as its amount, and therefrom the possibility or otherwise of reducing observations made with gauges at different heights above the ground to what they would have been at some uniform datum. This portion of the report commences by a brief notice of the experiments made by Prof. Phillips at York in the years 1832-35, then pass on to illustrate the necessity for the determination of these corrections; thence to a description of the instruments employed, and their position; and then follow a heavy batch of tables of the calculations and the results which it is impossible to abbreviate. Part of the conclusions were exhibited in the form of diagrams representing the total rainfall on the surface of the ground, and its decrease at various altitudes above it, one diagram giving the mean annual decrease, and a series of twelve others the monthly curves; from these it was perfectly obvious that the difference between a gauge on the ground and one 20ft. high is in winter nearly three times as great as in summer, and hence it becomes evident that the mean annual correction is applicable to the total fall in one or more years only, and not to individual months, for each of which separate corrections are given. The report then proceeds to consider the most suitable height for the orifice of gauges to be above ground, and gives various reasons *pro* and *con*, finally concluding that 1ft., as hitherto adopted, be still recommended. The report next refers to the tables in an appendix giving the monthly fall of rain at about 300 stations during the years 1868-69, and to various calculations in different states of progress. The report concludes by pointing out the great work being done by the voluntary and entirely gratuitous services of nearly 2,000 observers, and suggests that it would be alike graceful and an economical act on the part of the Government were they to offer to relieve the observers from the cost of reducing and publishing the observations which are now by their accuracy and completeness accepted as a type by foreign countries and our own colonies, and which are found yearly more and more useful in relation to our manufacturing and commercial interests. The committee conclude with the following words:—"A few hundreds annually would probably suffice to hold together a body of practised observers which has no equal in the world, and which once broken up, could not be replaced, since, irrespective of the difficulty of training the new observers, the continuity of the observations would be destroyed."

SCIENTIFIC SERIALS

THE *Geological Magazine* for September (No. 75) opens with an important article by Mr. E. Ray Lankester, describing a new species of *Cephalaspis* (*C. dawsoni*) from the Devonian sandstones of Gaspé, in Canada. This fish is figured, as also a spim, *Machairacanthus sulcatus*, which was found associated with it. Mr. Lankester also describes the characters of *Scaphaspis knerii.*—Mr. Davidson continues his descriptions of Italian tertiary Brachiopoda, which he illustrates with two fine plates containing a great number of figures.—Mr. Alfred Marston contributes a paper on the transition beds between the Silurian and Devonian rocks; and Mr. Lankester describes and figures a supposed new species of *Terebratula* (*T. rex*), obtained from East Anglian drifts, but probably derived from beds of Portlandian age. The remaining articles in the number are a catalogue of mammalian fossils which have been discovered in Ireland, by Mr. R. H. Scott, and a reply by Archdeacon Pratt to some remarks by M. Delaunay on Mr. Hopkins's method of determining the thickness of the earth's crust.

THE *Journal of Botany* for October commences with some Observations on Willows, by the Rev. J. E. Leefe. Dr. Hance contributes some carpological notes on Chinese plants; and Mr. A. W. Bennett his paper on the relative period of maturity of the male and female organs in hermaphrodite plants, read at the Liverpool meeting of the British Association, of which an abstract has already appeared in our columns. Dr. Ferdinand von Müller has a note on some interesting plants gathered near Lake Barlee during Mr. Forrest's recent expedition; and among the borrowed abstracts is one of Mr. Bailey's useful paper on the natural ropes used for packing cotton bales in the Brazils, read before the Literary and Philosophical Society of Manchester.

THE two longest articles in the *American Naturalist* for September are a reprint of Mr. Darwin's memoir on the movements and habits of Climbing Plants, and a highly favourable review of Wallace's "Contributions to the Theory of Natural Selection." Prof. Cope contributes an article on the Fauna of the Southern Alleghanies, and Dr. C. C. Abbott one on Mud-living Fishes. One of the most interesting papers in the number is a very short one by Dr. William Stimpson on the Deep-water Fauna of Lake Michigan, containing a short account of a series of dredging operations which has been undertaken in this lake during the present year by the Chicago Academy of Sciences. At a distance of eighteen miles from Chicago, where the depth was fourteen fathoms, the sandy or gravelly bottom was found to be nearly barren of life. Between the distances of twelve and twenty-two miles from off Racine, the average depth was forty-five fathoms, and the bottom generally a reddish or brownish sandy mud. This bottom was found to be rather densely inhabited; the captures including a *Mysis* allied to Arctic forms, which led the author to refer its original entry into the lake to the cold period of the quaternary epoch, two species of *Gammarus*, a small white *Planaria*, and a new species of *Pisidium*. The investigation of the materials obtained by the dredging parties of the Academy is now in progress, and the results will be published in full with illustrations at an early period.

SOCIETIES AND ACADEMIES

BRISTOL

The Observing Astronomical Society.—Report of Observations made during the period from Aug. 7 to Sept. 6, 1870, inclusive.—*Solar Phenomena*:—Mr. T. G. E. Elger, of Bed-