Nature. Vol. II, No. 51 October 20, 1870

London: Macmillan Journals, October 20, 1870

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THURSDAY, OCTOBER 20, 1870

ON THE COLOUR OF THE LAKE OF GENEVA AND THE MEDITERRANEAN SEA

THROUGH kindness for which I have reason to feel both proud and grateful, I have had placed in my hands two bottles of water taken from the Mediterranean Sea, off the coast of Nice. To my friend M. Soret I am also indebted for two other bottles taken from the Lake of Geneva. The friendly object in each case was to enable me to examine whether the colour of the water could in any way be connected with the scattering of light by minute foreign particles, which is found so entirely competent to produce and explain the colour of the sky. In the open Lake of Geneva, Soret himself had studied this question with considerable success,* and my desire was to apply to it other methods of examination.

The bottles, as they reached me, and with their stoppers unmoved, were placed in succession in the convergent beam of an electric lamp. Water optically homogeneous would have transmitted the beam without revealing its track. In such water the course of the light would be no more seen than in optically pure air. The cone of light, however, which traversed the liquid, was in both cases distinctly blue, the colour from the Lake of Geneva water being especially rich and pure. Something, therefore, existed in the liquid which intercepted and scattered, in excess, the shorter waves of the beam. The longer waves were also scattered, but in proportions too scanty to render the track of the beam white. The action, in fact, was identical with that of the sky. Viewed through a Nicol's prism the light was found polarised, and the polarisation along the perpendicular to the illuminating beam was a maximum. In this direction, indeed, the polarisation was sensibly perfect. A crystal of tourmaline placed with its axis perpendicular to the beam was transparent; with its axis parallel to the beam it was opaque. By shaking the liquid larger particles could be caused to float and sparkle in the beam. The delicate blue light between these particles could be quenched by the Nicol while they were left shining in the darkened field. A concave plate of selenite, placed between the Nicol and the water, showed a system of vividly coloured rings. They were most brilliant when the vision was at right angles to the beam, just as they are most brilliant when the blue sky is regarded at right angles to the rays of the sun. In no respect could I discover that the blue of the water was different from that of the firmament. The colour presented by the Mediterranean water was a good sky-blue, while that presented by the Geneva water matched a sky of exceptional purity.

My interest was long ago excited by the attempts made to account for the colour of the Lake of Geneva, and continued observation in 1857 impressed me more and more with the notion that the blue was mainly that of a turbid medium. Soon afterwards I wrote thus regarding this colour:—

"Is it not probable that this action of finely divided matter may have some influence on the colour of some of the Swiss lakes—on that of Geneva for example? This lake is simply an expansion of the river Rhone, which rushes from the end of the Rhone glacier. Numerous other streams join the Rhone right and left during its downward course, and these feeders being almost wholly derived from glaciers, carry with them the fine matter ground by the ice from the rocks over which it has passed. Particles of all sizes must be thus ground off, and I cannot help thinking that the finest of them must remain suspended in the lake throughout its entire length. Faraday has shown that a precipitate of gold may be so fine as to require a month to sink to the bottom of a bottle five inches high; and in all probability it would require *ages* of calm subsidence to bring all the particles in the Lake of Geneva to its bottom. It seems certainly worthy of examination whether such particles, suspended in the water, do not contribute to that magnificent blue which has excited the admiration of all who have seen it under favourable circumstances." *

Through the observations of Soret, and through those here recorded, the surmise of thirteen years ago has become the verity of to-day.

But though in the action of small particles we have a cause demonstrably sufficient to produce the blueness referred to, it is not the only cause operative. In the Lake of Geneva we have not only the blue of scattering by small particles, but also the blue arising from true molecular absorption. Indeed, were it not for this, the light *transmitted* by a column of the water would be yellow, orange, or red, like the light of sunrise or sunset.† Not only then is the light mainly blue from the first moment of its reflection from the minute particles, but the less refrangible elements which always accompany the blue are still further abstracted during the transmission of the scattered light. Through the action of both these causes, scattering and absorption, the intense and exceptional blueness both of the Lake of Geneva and the Mediterranean Sea I hold completely accounted for.

During the year 1869, M. Lallemand communicated to the Paris Academy of Sciences some interesting papers on the optical phenomena exhibited by certain liquids and solids when illuminated like the actinic clouds in my experiments. I also, in 1868, had examined a great number of liquids in the same manner, and a brief reference to these experiments will be found towards the end of a paper on the blue colour of the sky and the polarisation of its light, published in the Proceedings of the Royal Society for the 16th of December, 1868. M. Lallemand supposed the scattering of the light to be effected not by foreign particles but by the molecules of the liquids with which he experimented. M. Soret, on the other hand, contends against this novel view, maintaining that the scattering of the light is an affair of particles and not of molecules. While admiring the skill and learning displayed by the young French physicist, I am forced to take the side of Soret in this discussion M. Lallemand assumes a purely hypothetical cause while a true cause is at hand. He bases his case mainly on clear glass and distilled water. But the clearness is that observed in ordinary daylight, which is a very deceptive test. Glass exhibits the phenomena

* See his letter to me, Philosophical Magazine, May, 1869.

* Glaciers of the Alps (1860), p. 261.

† In fact, we have a dichroitic action of this kind exerted by glacier water when the subsidence is less complete than in the Lake of Geneva.

of scattering in every degree of intensity. Exceedingly fine examples of dichroitic action on the part of this substance are to be seen in Salviati's window in St. James's Street.* By reflected light the dishes and vases there exposed exhibit a beautiful blue—by transmitted light, a ruddy brownish yellow. The change of colour is very striking when, having seen the blue, a white cloud is regarded through the glass. Where the opalescence is strongest, the transmitted light, as might be expected, is most deeply tinged. From these examples, where the foreign ingredient is intentionally introduced, we may pass by insensible gradations to M. Lallemand's glass. The difference between them is but one of degree. Many of the bottles of our laboratory show substantially the same effect as the glass of Salviati. We can hardly ascribe to molecular action, which is constant, an effect so variable as this. It is also a significant fact that, in the case of pellucid bodies—rock salt, for example—where the powerfully cleansing force of crystallisation has come into play, M. Lallemand himself found the scattering to be *nil*. Under severe examination, rock salt itself would probably be found not altogether devoid of scattering power. I have examined many fine specimens of this substance, and have not succeeded in finding a piece of any size absolutely free from defect. A common form of turbidity exhibited by clear rock salt, when severely tested, resembles on a small scale "a mackerel sky." Nor have the specimens of Iceland spar that I have hitherto examined proved absolutely wanting in this internal scattering power.

In relation to this question, which is one of the first importance, the deportment of ice is exceedingly instructive. As a rule the concentrated beam may be readily tracked through ice, at least at this season of the year, when the substance shows signs of breaking up internally. In some cases the sparkle of motes, which are evidently spots of optical rupture, reveals the track of the beam. In other cases the track appears bluish, though rarely of a uniform blue. By causing a previously sifted beam to traverse lake ice in various directions, we are soon made aware of remarkable variations in the intensity of the scattering, and we find some places where the track of the beam wholly disappears. The convergent beam is sometimes divided by a transverse plane, one half of the cone being visible and the other invisible. In other cases the cone is divided by a plane passing from apex to base, one half shining with scattered light, and the other showing the darkness of true transparency. Now, if the scattering were molecular, it ought to occur everywhere, but it does not so occur, therefore it is not molecular. The scattering is, perhaps, in most cases due to the entanglement in the ice, when the freezing is rapid, of the ultra-microscopic particles abounding in the water. It is only by excessively slow freezing that such particles could be excluded from the ice. Purely optical ruptures of the substance itself, if minute and numerous enough, would also produce the observed effect.

The liquids which I examined in 1868 all showed in a greater or less degree the scattering of light, to which was added in many cases strong fluorescence. In no respect did the deportment of the non-fluorescent liquids which showed a blue track differ from that of the blue actinic clouds with which I was then occupied. I examined water from various sources and found it uniformly charged, not only with particles small enough to scatter blue light, but with far grosser particles. Tested by the concentrated beam, our ordinary drinking water presents a by no means agreeable appearance; some of the water with which London is supplied is exceedingly thick and muddy. Nor does distillation entirely remove the suspended matter. Soret vainly tried to get rid of it; he diminished its effect, but he did not abolish it. I was favoured a few days ago with specimens of distilled water from four of the principal London laboratories. Looked at in ordinary daylight the liquid in each case would, in ordinary parlance, be pronounced "as clear as crystal," but when placed in the concentrated beam of the electric lamp, the notion of purity became simply ludicrous. No one who had not seen it would be prepared for the change produced by the concentrated illumination. There were differences of purity among the specimens, arising, doubtless, from the different modes of distillation, but to an eye capable of seeing in ordinary light what was revealed by the concentrated beam each of the specimens would appear as muddy water. I also examined a specimen of extra purity distilled from the permanganate of potash and liquefied in a glass condenser. It contained a large amount of foreign particles; not of those which scatter blue light, but grosser ones. Such must ever be the case with water distilled in the laden air of cities and collected in vessels contaminated by such air. These facts amply justify the language applied by Mr. Huxley to the statement that solutions without particles can be obtained by the processes hitherto pursued. Such a statement could only be based upon defective observation. In the number of this journal for the 17th of March, an experiment is described in which water was obtained from the combustion of hydrogen in air, the aqueous vapour arising from the combustion being condensed by a silver surface of unimpeachable purity. In this case, though the floating particles of the air were, in the first instance, totally consumed, the water was still well laden with foreign matter. The method of obtaining water here referred to had been resorted to by M. Pouchet with a view of utterly destroying all germs, and my especial object in repeating the experiment was to reveal the dangers incident to the inquiries on which M. Pouchet and others were then engaged. But the warning was unheeded. It is not for the purpose of adding to the weight of calamities, already sufficiently heavy, that I allude to this, but rather to advertise the adventurous neophyte, who may be disposed to rush into inquiries which have taxed the skill of the greatest experimenters, of some of the snares and pitfalls that lie in his way.

JOHN TYNDALL

Royal Institution, Oct. 18

* Mentioned to me by a correspondent.

NICHOLSON'S MANUAL OF ZOOLOGY

A Manual of Zoology. By H. A. Nicholson, D.Sc. Vol. I. Invertebrate Animals. (Blackwood and Sons.)

A BOOK such as this aims at being was wanted—one which should give a little more of systematic detail than is to be found in Professor Huxley's "Outlines" or

Professor Rolleston's introductory chapters of his "Forms." It required, however, a man of considerable knowledge of the subject to write such a book worthily, and we doubt whether Dr. Nicholson, though he deserves credit for enterprise, was quite the man to undertake it. He excuses himself for shortcomings in plan and execution in his preface, on the score of leading a busy life. Now is it, we would ask, for men who lead lives devoted to other objects than the pursuit of zoology, to bring out educational works on that branch of science?

Having once determined not to aim at originality, Dr. Nicholson has really performed a service in reproducing so much of Professor Huxley's lectures, both the later ones and those published in the *Medical Times and Gazette* in 1856-7, as he has here, with due acknowledgment, done; for it is not everyone who can now get at those lectures for themselves. Most of the illustrations in this book appear to come from these lectures also, but this is not acknowledged. Other figures of Turbellarians we recognise as copied from an article in the *Popular Science Review*. All figures not original should be acknowledged in a work of this kind, and if it were done in this case we should find that Dr. Nicholson has seldom gone to an original source—but has copied the copies of other people. We may make exception, however, in respect of the figure of *Rhizocrinus* on page 134 and a few others which have not before made their appearance in any handbook or manual of our acquaintance; figs. 47, 67, 68, 73, which we pick out at random, are as old as the hills. It is not only as to figures that we have to regret that the author does not quote at first-hand. On page 4 the student is told that Professor Huxley has applied the name of "protoplasm" to the physical basis of life. No one would resent more than Professor Huxley this statement, seeing that Mohl's term for vegetable "Schleim" was extended to animal "Schleim," and adopted in the broad sense of the protoplasm theory by Max Schultze and Kuhne more than seven years since. Dr. Bastian's name is quoted (p. 154) in connection with the remarkable history of *Ascaris nigrovenosa*, which we really owe to Leukart and Mecznikow. The history of a science is really of sufficient importance to render it desirable to keep to it as truly as possible, and the compiler of such a work as this should not so entirely confine himself to appropriating the gleanings of others, but should shift for himself, and give us the result of inquiries among the original writers.

The account of the Annelida is not very full, nor is it accurate. The leeches are made to form an order of the group, and the common horse leech and medicinal leech are the only members of it mentioned. Oligochæta is given as a synonym for Terricola, and is divided into Lumbricidæ and Naïdidæ. The latter group is said to include *Tubifex*, and then it is said that nonsexual reproduction characterises it. The fact is that Oligochæta includes Terricola = Lumbricidæ and Enchytrœidæ, and Limicola = Sænuridæ and Naïdidæ; *Tubifex* belonging to Sænuridæ and not to Naïdidæ, and exhibiting no reproduction by budding, which is confined to the genera *Nais*, *Chætogaster*, and *Æolosoma*. The fluid of the pseudo-hæmal system in *Aphrodite* and *Polynoë* is stated to be yellow, but Claparède's researches show these worms to be ananzian, with the doubtful exception of *Aphrodite aculeata*, so that they have not this fluid at all, either yellow or of any other colour. Whilst no mention is made of Haeckel's proposal to connect sponges with Cœlenterata—perhaps a judicious omission at present—we find them actually classed as Rhizopoda, which is to us a new step in the opposite direction, and not a justifiable extension of Dujardin's class. In defining the Annuloida, Professor Allman's words, relative to the Echinodermata, only are quoted as though applying to both Echinodermata and Scolecida, into which two groups Dr. Nicholson follows Professor Huxley in dividing the sub-kingdom.

The Mollusca are treated by the aid of the late Dr. Woodward's manual, due prominence being given to the homological views of Professors Huxley and Allman. Mr. Hancock is not, however, mentioned, nor are Duthier's views on *Dentalium*, nor again are the exceedingly important facts of the resemblance of development of the nervous system and chorda dorsalis of the Tunicate larvæ and of Vertebrata, established by Kowalewsky and Kupffer, alluded to.

Perhaps a greater omission is that of all reference to the Monera of Haeckel. Whatever he thinks fit to do with them, the genera *Protamœba*, *Protogenes*, *Protomyxa*, *Myxastrum*, *Protomonas*, and *Vampyrella*, deserve recognition on Dr. Nicholson's part; so also do the Labyrinthuleæ of Cienkowski. Another omission is that of the fresh-water forms of Radiolaria, described of late years by Archer, Focke, and Greef; whilst under Myriapoda, we find no mention of Sir John Lubbock's curious aberrant form *Pauropus*; no mention of *Cecidomyia* larvæ under Insects; no mention of *Rhabdopleura* under Polyzoa, though it is alluded to in connection with Graptolites, the solid axis of the latter being supposed to resemble the chitinous rod of Prof. Allman's new order of Polyzoa (*Quarterly Journal of Microscopical Science*, January 1869)—a resemblance, if admissible, not confirmatory of Sertularian affinities, by the way. Interesting transition forms, such as *Echinoderes*, *Rhabdosoma*, *Myzostomum*, are not spoken of. The curious low forms of Arachnida, belonging to the genus *Pentastomum*, are said to occur in *some* vertebrates. It should be stated definitely that a species infests man, and that one body out of every five has been said to contain specimens.

It is not an agreeable task to point out so many deficiencies; at the same time a book destined for educational purposes can least of all be excused in these matters. For all that it contains of reference to recent zoological literature, this manual might be dated ten years back, when the same condensing process applied to the same educational books would have produced much the same result.

There is a glossary to this book which should be useful; we have only looked at its first page, and did not venture further into what promised to be a new series of mistakes. There is no Greek word "aktin," a ray—though Dr. Nicholson says there is.

As a rule, the definitions of both large and small groups are well given by Dr. Nicholson, and the most original part of his manual appears to be in those paragraphs which give the geological range of the various groups; these are well and carefully done, though there are exceptions—the geographical distribution is not given with equal care.

E. R. LANKESTER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Evolution of Life: Professor Huxley's Address at Liverpool

BELIEVING that readers of NATURE can feel no interest in the extended personalities with which Prof. Huxley almost fills his letter this week, and believing also that such matters are little worthy of occupying your columns, I shall only allude to that part of his letter which contains statements having a scientific bearing.

The distinct issue raised in my experiments was, were *living* things to be found in the fluids of my flasks? If so, such living things must either have braved a higher degree of heat than had been hitherto thought possible, or else they had been evolved *de novo.*

The effect of the very high temperature upon pre-existing living things, which were purposely exposed thereto, was shown by their complete disorganisation in an experiment which is recorded in NATURE, No. 37, p. 219, and to this I would especially direct Prof. Huxley's attention.

Prof. Huxley advances an explanation of the mode of origin of the distinct fungi, bearing masses of fructification (NATURE, No. 36, figs. 12, 14, and 17) and of the inextricably tangled coils of spiral fibres (figs. 13 and 15) found in my flasks after exposure to temperatures at and beyond Pasteur's standard of destructive heat; his theory is entirely novel, apparently extemporised for the occasion, and is very startling. He says, and in justice to Prof. Huxley I quote the passage in full, "Any time these six months Dr. Bastian has known perfectly well that I believe that the organisms which he got out of his tubes are exactly those which he has put into them; that I believe that he has used impure materials, and that what he imagines to have been the gradual development of life and organisation in his solutions is *the very simple result of the setting together of the solid impurities, which he was not sufficiently careful to see when in their scattered condition when the solutions were made.*"

Now, although it was quite true that minute portions of *Sphagnum* leaf were found in two unpublished experiments, it seems very marvellous that on this slender foundation Prof. Huxley should hazard such a purely imaginative and unprecedented hypothesis as to the mode of production of fungi.

I have, moreover, not been able to see why the occurrence of the incident to which he refers should make him repudiate a number of experiments in which *unmistakeably living things* were found in fluids from hermetically sealed flasks after these and their contents had been exposed to temperatures higher than those which living things are known to be capable of resisting.

Following a precept more honoured in dialectics than in science, Prof. Huxley has attacked his opponent rather than the arguments which he affects to destroy. He objects to only one passage in my "Reply," and this he thinks was not worthy of the special type in which it was printed; and yet, notwithstanding its special type, I can only conclude from his reply that Prof. Huxley has failed to appreciate its meaning. My words were: "Living things may and do arise as minutest visible specks, in solutions in which, but a few hours before, no *such* specks were to be seen." The word which now alone stands in italics was ignored by Prof. Huxley. I had no wish to tell him that certain refractive particles, or foreign bodies, might not be visible in the thin film of fluid to which I referred. I alluded to the gradual and equable development of living specks throughout a fluid containing no apparent germs. His retort that some unobserved visible germs might have become centres of development is a *contre-sens.* It does not apply to the gradual appearance of myriads of *equally diffused* motionless particles in a motionless film of fluid.

The very authoritative tone which Prof. Huxley has lately assumed in his remarks concerning Brownian movements and those of living organisms, fails to impress me very much. His knowledge about these movements, as I have good reason to know, is of quite recent growth. Movements which, in the month of March of the present year, Prof. Huxley did not regard as Brownian, he now does believe to have been of this nature. If he is now right, what value is to be set upon his knowledge of Brownian movements six months ago; and what guarantee have we that in another six months Prof. Huxley may not again take a different view?

Let me assure Prof. Huxley, however, that the duty which he is "credibly informed" he owes to the public remains still undischarged. I protested against his "Address" on scientific grounds which are fully stated, and those who have read my protest will see that Prof. Huxley cannot dispose of the question really at issue by recounting any mistakes of mine, whether real or imaginary. If, as I believe, he has failed to give any worthy or serious view of the question, this could have been in no way necessitated by a disbelief, however strong, in my experiments. The labours of Profs. Wyman, Mantegazza, and Cantoni had already taken the question into regions never attained by M. Pasteur, and therefore they demanded a fair consideration. Is Prof. Huxley, in his capacity of President of the British Association, warranted in ignoring their labours, and therefore in misrepresenting the present state of science on the subject, because, owing to two errors among my many experiments, he declares himself to have altogether lost faith in my skill or capacity as an investigator? The answer cannot be doubtful. Is it, again, consistent with his high responsibilities that he should pervert the real issues, and should do a grave injustice to others, in order that he might preserve a "silence" which should be his "best kindness" to me? Let me tell Prof. Huxley that I repudiate such "kindness," as any honest man would who is simply seeking after truth, and relegate it to the same regions as I would that indescribable air of restrained omniscience whereby he endeavours to crush arguments and facts, to which he altogether fails to reply.

H. CHARLTON BASTIAN

University College, Oct. 17

Aurora Borealis

A BRILLIANT display of the Aurora Borealis was visible here on Sept. 24th. I submit the following statement of my observations, which probably may be acceptable to your readers.

About 8.30 P.M. a broad white streak of light was seen due north, extending from the horizon towards the north star. This streak was soon accompanied by several others, rising and streaming upwards from the horizon between the north-west and north-east points, and converging towards the zenith: at the same time all the northern portion of the sky became illuminated.

At 9 P.M. the coruscations of the Aurora became more brilliant and active, streaming with great rapidity towards the zenith, though none could be seen to reach that point, assuming very beautiful colours, which were generally of light pink and reddish hues.

About 10 P.M. the colouring and brilliancy of the Aurora attained its fullest effect, the coruscations varying with yellow, pink, and almost crimson hues, the intermediate spaces of the sky in the background appearing to be light green. The brightest streamers were confined to the north and north-westerly directions. The coruscations continued in rapid action with surprising grandeur up to 10.30 P.M., when a somewhat indistinct purple arch was visible, with its culminating point north and about thirty-five degrees above the horizon. Shortly after this exhibition, the display of coruscations and luminous streamers gradually ceased action, but the northern portion of the sky remained illuminated, resembling twilight after sunset.

The sky was quite clear at the commencement of the Aurora; it became partially obscure about 10 P.M.; the clouds then, especially those in the north-east, reflected very beautiful roseate tints from the Aurora, which, in combination with the varying colours of the coruscations, produced a most striking and grand spectacle.

Three shooting-stars were observed between 9 and 10 P.M., and several were seen on the previous night.

The barometer was at 30·4, and has not read below 30·3 during the past week. The temperature has been very regular for the same time; the maximum by day was 64°, and the minimum at night 51°. Winds have been constant from the east, and the sky free from cloud.

The northern region of the sky remained illuminated throughout the night, producing a strong reflection on the sea, and rendering the rocks and vegetation around me quite perceptible. Even at 3.30 A.M. the light from the northward produced all the effect of early dawn.

I must add, that during all my Arctic voyaging I never witnessed in any Aurora the same conditions of varied colouring as were displayed on this occasion.

It may also be of interest to state that on the 21st inst. I observed a bright meteor to shoot in a path from near the Pole Star to Capella, vanishing near the latter after discharging a brilliant light-green flash.

From Ilfracombe the view to the north commands an extensive range of the sea horizon.

ERAS. OMMANNEY, Rear-Admiral

Ilfracombe, Devon

Natural History Museums

WHILE admiring and agreeing with the main features of Dr. P. L. Sclater's plan for the exhibition of the National Natural History collections in the new Museum at Kensington, I must beg to be allowed to enter a protest with regard to the geological portion of the scheme, which was all but completely overlooked by those who took part in the discussion of his suggestions and by Dr. Sclater himself.

The only words which Dr. Sclater devoted to the subject were to the effect that the palæontological collections should be merged into the general zoological and botanical ones. This arrangement is, however, one which will not serve the purpose of the geologist. We can fancy the utter misery of the man who wishes to study the fossils of some special formation, when he finds that to do so he will have to rush from case to case, amid stuffed and spirit specimens of all kinds, all over the place. The naturalists who deplore the loss of time incurred by them under the present order of things at the British Museum, have no thought for the geologist whom their proposed arrangement would condemn to far worse wandering in the new building. But in this particular I, as a geologist, sincerely trust that the project will not be carried out without some revision, and that a palæontological collection, quite independent of any other, and arranged *stratigraphically*, will form a by no means inconspicuous part of the Museum. The advantages of this system may be seen at a glance in Jermyn Street, where the British fossils are thus arranged.

In a collection as large as that of the National Museum must be, it may very likely be found impossible to exhibit specimens of every species in the show cases, and indeed it is by no means desirable that this should be done. It would be quite sufficient, and as far as the general public is concerned infinitely better, to limit the *exhibited* specimens to those belonging to species characteristic of each formation, and to keep all others, which would interest students only, in a cabinet drawer, where they would be at once handy to the specialist and not wearying to the sight-seer. Duplicates enough could doubtless be spared to complete the zoological and botanical series in the lower galleries, but unique and rare specimens should, to my mind, most decidedly be kept for the geological series. In conjunction with the latter, it would be highly desirable to establish a lithological collection, the absence of which, with the solitary exception of the small one in the Geological Museum, is every day more and more to be wondered at in such a country as ours.

Not only should appropriate rock-specimens accompany the fossils of each fossiliferous bed, but they should be so arranged that the organisms and the matrix in which they are found embedded, could be examined side by side. Typical examples of unfossiliferous strata should be placed in their regular order of succession, including specimens of contemporaneous rocks from other parts of the world. Clear geological sketch-maps, boldly coloured and not over-burdened with names, showing the distribution (so far as it is known) of the various equivalent groups of beds, should be placed in conspicuous places at intervals in the room or gallery, and thus, or in some closely-resembling manner, could a homogeneous whole be arrived at, combining the greatest amount of instruction to the public and the greatest convenience to the student.

Of course the geological collection shadowed forth above would by no means take the place of another more detailed lithological collection, which should, if possible, be added to the mineralogical one, containing all the igneous rocks, &c., and, like the minerals, arranged *chemically*.

G. A. LEBOUR, F.G.S. &c.,
of the Geological Survey of England.

Changes of Level at Pozzuoli, referred to in the "Apocryphal Acts of Peter and Paul"

IN the well-known description which Sir Charles Lyell gives of the changes of level of the shores of the Bay of Baiæ in the 30th chapter of his "Principles of Geology," there occur at pages 172-174 (vol ii. 10th edition) the following statements.

Speaking of the so-called "Temple of Serapis," and its adornment by the Emperor Alexander Severus between A.D. 222 and 235, he says: "From that era *there is an entire dearth of historical information for a period of* 12 *centuries*, except the significant fact that Alaric and his Goths sacked Pozzuoli in 410, and that Genseric did the like in 440 A.D." Again: "The period of deep submergence was certainly antecedent to the close of the 15th century"—a statement which he goes on to prove by a quotation from Loffredo referring to the year 1530, and a reference to documents cited by A. di Joris, one of which, dated 1503, speaks of land "where the sea is drying up."

Still more recently Professor Phillips, in his interesting volume on Vesuvius, speaking (p. 244) of the "Temple of Serapis," observes that, at the time of its adornment by the Emperor, "early in the 3rd century, it must have been in its original, or else in its second stage—perhaps we may adopt the latter view—there may have been a depression of 5ft., a new floor, restoration, and adornment. Nothing is absolutely known of any further change of level till the early part of the 16th century."

Thus both the authorities cited appear to agree that from the middle of the 3rd to the end of the 15th century there is a *total* absence of information. You may therefore possibly think that the following extract from the "Apocryphal Acts of Peter and Paul" possesses some interest, even though it may be difficult to agree on the approximate date of the writer. Your columns would scarcely be the place, nor am I competent, to discuss this last point, but as one of the MSS. collected by Professor Tischendorf for his edition of the Greek original is said to be of the end of the 9th century, it appears to me that we have here not only a rather quaint explanation of the immediate cause of the changes of level of the land at Pozzuoli and in its neighbourhood, but a distinct reference *at least* to six centuries before the Italian writers already quoted, not merely to the fact, but also to the extent, of the movement in question. Notwithstanding its suspicious legendary framework, the statement that "Pontiole sunk into the sea-shore about one fathom; and *there it is until this day*, for a remembrance, under the sea," has an air of *vraisemblance*, a ring of truthfulness, about it which I hope will justify my bringing the matter under the notice of those so much more competent than I am to assign to it its true value, and to whom it may possibly be new.

I quote from the translation by Mr. A. Walker, forming a portion of Vol. xvi. of the "Ante-Nicene Library," published by Messrs. Clark of Edinburgh, pp. 257–8.

F. FOX TUCKETT

Frenchay, near Bristol, Oct. 3

"And when Paul came out of Mesina he sailed to Didymus, and remained there one night, and having sailed thence, he came to Pontiole (Puteoli) on the second day. And Dioscorus the shipmaster, who brought him to Syracuse, sympathising with Paul because he had delivered his son from death, having left his own ship in Syracuse, accompanied him to Pontiole. And some of Peter's disciples having been found there, and having received Paul, exhorted him to stay with them. And he stayed a week in hiding, because of the command of Cæsar (that he should be put to death). And all the toparchs were watching to seize and kill him. But Dioscorus the shipmaster, being himself bald, wearing his shipmaster's dress, and speaking boldly, on the first day went into the city of Pontiole. Thinking, therefore, that he was Paul, they seized him and beheaded him, and sent his head to Cæsar. . . .

"And Paul, being in Pontiole, and having heard that Dioscorus had been beheaded, being grieved with great grief, gazing into the height of the heaven, said: 'O Lord Almighty in Heaven, who has appeared to me in every place whither I have gone on account of Thine only begotten Word, our Lord Jesus Christ, punish this city, and bring out all who have believed in God and followed His Word.' He said to them, therefore, 'Follow me.' And going forth from Pontiole with those who had believed in the Word of God, they came to a place called Baias (Baiæ), and looking up with their eyes, they all see that city called Pontiole sink into the sea-shore about one fathom, *and there it is until this day, for a remembrance, under the sea*.

"And those who had been saved out of the city of Pontiole, that had been swallowed up, reported to Cæsar in Rome that Pontiole had been swallowed up with all its multitude."

Hereditary Deformities

IN the number of NATURE for Sept. 3, a letter from Mr. William Field appears with the title "Hereditary Deformities," commenting on certain alleged facts quoted in an ethnological article in *Cassell's Popular Educator*, from Dr. Theodor Waitz's "Introduction to Anthropology," translated by Mr. Collingwood. Mr. Field justly remarks that facts of such a character, "if substantiated, would introduce *Accidental Distortion* as a coworker with Natural Selection in the modification of species." But he puts the question—"Do these stories rest on a good foundation?" Personally, I do not know. All I can say is, that Dr. Waitz, whose scientific authority is unimpeachable, published them without expressing any doubts of their accuracy. They may be found with some alleged facts of analogous character, at pp. 83 to 85 of his first volume, as translated by Mr. Collingwood. Speaking of animals, he says:—

"Mutilations also are sometimes transmitted. Williamson* saw in Carolina dogs which have been deficient in tails for three or four generations, in consequence of one of their ancestors having accidentally lost it. A cow, three years old, which had lost by suppuration her left horn, produced three calves, which, instead of the left horn, presented only a small protuberance on the skin. Dogs and horses whose tails or ears are clipped, as the draught dogs in Kamtschatka, often transmit these deficiencies to their offspring." (p. 83.)

Referring next to man, he considers that there are "cases in which deformities have shown themselves hereditary" (p. 84.) He says:—

"Instances of hereditary blindness and deafness, and of alternating dumbness, so that every second or third child was deaf, are given by Lucas. Harris communicates a case of hereditary blindness in one eye, and of a double thumb on the right hand."

I omit other instances. He continues again:—

"Instances are not wanting of mutilations that have been transmitted from parents to children; such, however, occur less frequently. According to Blumenbach, the children of an officer, whose little finger had been cut across and become crooked, possessed an analogous defect. Gosse cites the case of an officer wounded in the battle of Eylau, who transmitted to his offspring a scar on the forehead. Other instances of hereditary deformities are found in Wagner" (p. 85).

As Waitz, it will be perceived, quotes Blumenbach, it may be mentioned that the last-named author has a paragraph headed, "Problem proposed. Can mutilations and other artifices give commencement of native varieties of animals?" After showing that some have answered the question in the affirmative and others in the negative, he adds, "I have not at present adopted as my own either the affirmative or negative of these opinions." See p. 203 of his Anthropological Treatises, translated by Mr. Bendyside.

New facts, capable of being severely tested, would be of great value.

THE WRITER OF THE ETHNOLOGICAL PAPERS IN "CASSELL'S POPULAR EDUCATOR"

* More specific reference to the authorities on which Waitz rested may be found in the foot-notes to his work. Some of the volumes he quotes are foreign, and not to be found in the British Museum Library.

The Stability of Turret Ships

SINCE the loss of the *Captain* an opinion has rapidly gained ground, not only amongst unscientific men, but even amongst those who from their education should have acquired some of the most simple laws of statics, that that noble ship toppled over on account of her being "top-heavy"—that the *Captain*, an armour-plated ship with a low freeboard, was more "top-heavy" than a broadside ship, with more than twice as much out of water! The fact is, that her weights were lower and not higher than those of other vessels, and therefore that her fault was not "top-heaviness."

In looking at the stability of a vessel we take two points—the "centre of gravity" and the "centre of buoyancy;" the former being a certain point at which, if the attraction of gravity impressed a single force equal in intensity to the sum of all its separate actions on the component parts of the body, the ultimate effect would be the same as it is under the system of separate actions which really exists. The latter is the centre of gravity of the volume of water displaced by the ship, and may be regarded as the pivot on which she would turn on heeling over. The vessel being in a vertical position, the centre of gravity is immediately over the centre of buoyancy, and she is in a state of unstable equilibrium, *i.e.* she is in the same fix as a walking-stick standing on end. So far in favour of "top-heaviness."

Suppose now that she heels over to one side, what will the effect on our centres be? Immediately she begins to heel over the centre of buoyancy travels outwards to the side towards which she heels, and the centre of gravity being fixed, there will come a point when it will exert a force to overcome the heeling over pressure. This travelling outwards of the centre of buoyancy depends wholly on the shape of the vessel, and will appear perfectly plain by drawing sketches of a ship lying at different angles. The more a vessel heels over, the further outwards does the centre of buoyancy travel, and the greater is the resistance offered to the heeling over pressure until she approaches a certain point, then the centre of buoyancy moves out at an increasingly slower rate, and finally reaches the position corresponding to that of her maximum statical stability.

Before this, if by any disturbing cause, such as the alteration of the wave slope, the ship were inclined beyond her position of maximum stability, the resistance to heeling would become less the farther she went, until she reached a position at which her moment of stability would be the same as before the disturbing force began to act. And in this position she would remain in unstable equilibrium if the disturbing forces were removed. But if she should pass this position before the disturbing forces, and the angular velocity caused by them, cease, the ordinary movement of the heeling over force would then be greater than the resistance offered by the stability, in any position through which she would pass, and she would be turned over.

Now the difference between a high and a low freeboard ship as regards stability under sail is this:—The position of maximum stability is reached soon after the immersion of the edge of the deck; and as a high freeboard ship does not immerse her deck until she has attained a large inclination, while a ship of low freeboard will immerse hers at a very much less angle, it follows that, in the latter case, the position of maximum stability and then of unstable equilibrium is reached at a comparatively small angle of heel, and a ship of this construction is much more likely to be capsized than one with a high freeboard. Of course, in a low-sided ship, the centre of gravity may be brought so low as always to be on the right side of the centre of buoyancy, but this is not practicable in an armour-plated turret ship.

From what we have here stated it will be seen that the error in design that made the *Captain* so much heavier than was expected, and draw six feet instead of eight, was not adding so much to her stability but was in reality lessening it, and, perhaps, was the cause of her loss; and that if we are to have armour-plated turret ships, they must either be built of low freeboard, to be propelled by steam alone, or of high freeboard sufficient to give stability for sails.

The feud between naval architects and the advocates of the turret system has been going on for ten years back—the former contending that a high side was necessary in a rigged sailing ship, and the latter, that if they put their guns in turrets, they could have low-sided ships, or, in fact, ships with no sides at all, after a certain amount of inclination.

That the loss of the *Captain* has resulted from a preventible cause is quite evident, and we have shown what that cause is; it, therefore, only remains that if we are to have sailing turret ships, we must have high freeboard for the sake of stability.

T. BELL LIGHTFOOT

Newcastle-upon-Tyne

The New Postal Act

AT page 474 Mr. Reeks complains as to the working of the new Postal Act. It seems intended to obliterate the old parcel post. He says, "Herbarium specimens are not excluded." Perhaps so; but they are not included. The provisions of the new Act are limited to books, written and printed matter, genuine trade samples and patterns, so far as regards the two ounces for a halfpenny. All parcels other than books, &c., as described above, go at the letter rate of one penny for each half-ounce.

The postal card is the thin end of the wedge that will hereafter open to us a regular letter rate of a quarter of an ounce for a halfpenny. For instance: we may now send ordinary business communications up to two ounces, thus embodying the matter of twenty postal cards, for the halfpenny, if folded in a paper wrapper. An ordinary business communication of half-ounce weight goes for the halfpenny, if folded as a letter but left unsealed.

I ask, If Government will now take an open letter for a halfpenny, why not take it closed at the same rate? Common sense will hereafter equalise this disparity. The parcel post, however, is at present discontinued. P. N. Row

October 14

Science and the Government

THE reason that the Government has refused to aid "the expedition to observe the approaching eclipse" is, that it is perfectly assured that "men of science and culture" are nothing but a set of lying impostors, and would swindle the public out of thousands of pounds to take an observation which might be done for 10*l*. and much less. The nation is fast beginning to perceive that astronomy is a monstrous cheat—and the Transit of Venus has no more to do with the distance of the sun than it has with the number of fingers on my hand.

JOHN HAMPDEN

[We congratulate Mr. Lowe on his ally, merely remarking that the same Government which has refused the Eclipse Expedition has granted 20,000*l*. for observations of the Transit of Venus. Perhaps Mr. Hampden can explain the cause of this inconsistency on the part of the Government.—ED.]

Insects upon a Swallow

G. H. H. mentions in NATURE of Sept. 22 his having found on a swallow in the month of August two slate-coloured insects. On the 15th of July last, when hauling a seine net in Studland Bay, near Poole, with some fellow members of the Linnean Society Club, I picked up at the foot of the low cliff a young sand-martin, which had seven of these parasites (*Ixodes plumbea*) affixed to the skin of the head, giving it the appearance of having a slate-coloured fleshy crest. The poor little bird exhibited the symptoms described by your correspondent. I found it panting and apparently exhausted on the ground, and it remained stationary without making any effort to escape. The fishermen of our crew informed me that they often pick up martins with these ticks adhering to them.

The Waldrons, Croydon, Sept. 26 HENRY LEE

Aurora Borealis

A FINE aurora was observed here on the evening of the 14th inst., between $8^h 30^m$ and $9^h 40^m$, which, in spite of bright moonlight, nearly equalled the splendour the display of September the 24th. During the early part of the evening the eastern quarter of the sky was covered with bands of light *cirri*, which had a general direction of E.N.E. to W.S.W., or nearly at right-angles to the magnetic meridian; it was among these clouds and in the N.E. that the beams of the aurora were first seen. At $9^h 0^m$ a magnificent rose-coloured ray was noticed in the N.N.W., extending from the horizon through *Vega* towards the zenith. The three stars, ζ, ϵ, and δ, of Ursa Major were for more than ten minutes enveloped in the crimson glow of the aurora.

Bedford, Oct. 18 THOS. G. ELGER

NOTES

WE are glad to be able to state that Dr. Wyville Thomson has entirely recovered from the attack of gastric fever which prevented his taking part in the *Porcupine* expedition this summer. He is at present going over the zoological collection brought home in that vessel, at the University of London, with Dr. Carpenter, and he reports some very remarkable additions to his new group of vitreous sponges, mainly from the coast of Spain and Portugal. These, with some others procured by Mr. Saville Kent, in Dr. Marshall Hall's yacht, will nearly double the number of known forms referred to the order. They are no pigmies. One of them forms a lovely lace-like vase upwards of three feet in diameter at the lip!

WE have to add to the list of candidates for the Regius Professorship of Natural History in Edinburgh the name of John Anderson, M.D. Edin., F.L.S., director of the Imperial Museum of Natural History, Calcutta. Dr. Anderson was attached as Naturalist to a recent expedition through the north of Burmah, and he is, we understand, now on his way home, bringing with him detailed accounts of the important additions which he is known to have made to science in that expedition.

MEMBERS of the University of Cambridge, and all who are interested in the study of physical science, will hear with pleasure of the munificent offer made to the University by its Chancellor, the Duke of Devonshire, contained in the following extract of a letter to the Vice-Chancellor:—"I find in the report, dated Feb. 29, 1869, (*sic*) of the Physical Science Syndicate, recommending the establishment of a Professor and Demonstrator of Experimental Physics, that the buildings and apparatus required for this department of science are estimated to cost 6,300*l*. I am desirous to assist the University in carrying this recommendation into effect, and shall accordingly be prepared to provide the funds required for the building and apparatus, so soon as the University shall have in other respects completed its arrangements for teaching experimental physics, and shall have approved the plan of the building."

AT the Commencement held on the 12th inst. of the Queen's University in Ireland, the Most Honourable the Marquis of Kildare, the newly-appointed Chancellor of the University, presided. After expressing the deep regret of the University at the death of their first Chancellor, the Earl of Clarendon, to whom the University and Colleges were indebted for much of their prosperity, the Chancellor mentioned that the Senate had decided to establish a special curriculum, in which science should have the predominance, and that the degrees of Bachelor and Doctor in Science would be given to those who passed the examinations in the subjects to be hereafter enumerated. He also referred to the examinations for women carried on by the University—the first of these was held in June last in Belfast and Galway—and mentioned that of thirty-three candidates who presented themselves, twenty-one acquitted themselves to the satisfaction of the examiners.

MR. M. R. PRYOR has been elected to a Fellowship of Natural Science in Trinity College, Cambridge. The examination was conducted by Prof. Liveing, Prof. Michael Foster (the new Prælector of the College), Mr. Trotter, and Mr. Hort, Fellows of the College, was open to all the University, and was on a par with the examinations in classics and mathematics, held at the same time for Fellowships. It is the first occasion that this has been done, and the first time that a Fellowship has been offered in Cambridge for competition in Natural Science. We sincerely trust the plan will be continued in Trinity, and that the example will be followed in other Colleges. It would probably contribute more than any other single thing to promote the study of Natural Science in the University, and give an impetus to it in the various schools throughout the kingdom. We sometimes think the Colleges are scarcely conscious of the power they are capable of exercising in this way, and of the responsibility which necessarily attaches to such power. The questions were of a very high order, and we understand the answers evinced so much power as well as knowledge, that the examiners would gladly have elected more than one candidate. We sincerely congratulate Mr. Pryor on having thus worthily won the highest competitive reward for Natural Science hitherto given in this country.

THE Senate of the Queen's University in Ireland has conferred on William King, M.D., Professor of Geology and Mineralogy in Queen's College, Galway, the honorary degree of Doctor of Science, in consideration of his eminence as a geologist.

THE following lectures in Natural Sciences will be delivered in Trinity and St. John's Colleges, Cambridge, during the Michaelmas term, 1870. On Electricity: Mr. Trotter, Trinity. On Chemistry: Mr. Main, St. John's. Instruction in Practical Chemistry will also be given. On Geology—(1) Palæontology; (2) Lyell's Principles of Geology; (3) Elementary Lectures: Mr. Bonney, St. John's. (Students of other Colleges can be admitted to these Lectures by arrangement with their college tutor.) On Elementary Botany: Mr. Trotter, Trinity. On Physiology: The Trinity Prælector of Physiology (Dr. M. Foster) at the New Museums.

MERTON COLLEGE, Oxford, announces an open Natural Science scholarship of the value of 80*l.* for five years, and one or more exhibitions of the value of 25*l.* for three years, to be awarded in December next. The examination will be in chemistry, physics, and physiology, and an opportunity will be given for showing a knowledge of practical work in chemistry and physiology. They will be awarded either for special excellence in one subject or for excellence in two out of three subjects, but no candidate will be examined in more than two subjects. There is no limit to age, but members of the University must not be over six terms standing.

THE Natural Science Scholarship at Lincoln College, Oxford, has been awarded to Mr. Schofield, of Owens College, Manchester. The National Science Demyship of 80*l.* a year at Magdalen College, Oxford, has not been filled up, there being no candidate up to the required standard.

THE Dublin University have just published the regulations for the examinations for women for 1871. They will be held in Dublin in March 1871, one for senior and one for junior candidates. The latter must not be above eighteen years of age. Examinations will also be held at any place where a ladies' superintending committee shall be constituted, and at least twenty candidates guaranteed to present themselves.

WE understand that Dr. Schimper, the palæontologist, escaped from Strasburg the day before the final closing of the gates at the commencement of the siege. Fears are entertained as to the safety of his fine collection.

IN addition to the literary prospects of the approaching season already announced, Mr. Murray's list of forthcoming books includes the following:—"On the Descent of Man, and on Selection in Relation to Sex," by Charles Darwin; with illustrations, 2 vols. crown 8vo. "The Student's Elements of Geology," by Sir Charles Lyell; with numerous woodcuts, post 8vo. "Scrambles among the Alps, 1860–69," by Edward Whymper, including the First Ascent of the Matterhorn, with Observations on Glacier Phenomena in the Alps and in Greenland; with 100 maps and illustrations, medium 8vo. "A Visit to High Tartary, Yarkand, and Kashgar (formerly Chinese Tartary), and Return Journey over the Karakorum Pass," by Robert Shaw; with map and illustrations, 8vo. Second and cheaper edition of "The Music of the Most Ancient Nations," by Carl Engel; with 100 illustrations, 8vo. A third edition of Kerr's "The Gentleman's House; or, How to Plan English Residences, from the Parsonage to the Palace;" with illustrations, 8vo. Among Mr. Bentley's announcements are—"Travels in the Air; a Popular Account of Balloon Voyages and Ventures, with recent attempts to accomplish the navigation of the air," by J. Glaisher, of the Royal Observatory, Greenwich; royal 8vo., with 133 illustrations. "The Marvels of the Heavens," from the French of Flammarion, translated by Mrs. Lockyer; crown 8vo., with numerous illustrations. A new and cheaper edition of "The Heavens," an illustrated handbook of Popular Astronomy, by Amédée Guillemin, edited by J. N. Lockyer, F.R.S.; demy 8vo., with 200 illustrations.

MR. M. C. COOKE's Hand-book of British Fungi will be published in the course of the ensuing season by Messrs. Macmillan and Co. Although the first volume is already printed, its issue will be delayed until the whole work is ready. It will be a complete *vade mecum* for the British Fungologist, and will contain descriptions of all the species, and illustrations of all the genera. We are glad to see that the University of St. Lawrence has recognised Mr. Cooke's services to botanical science by conferring on him the honorary degree of M.A.

MR. VAN VOORST has in preparation "The Mollusca of the European seas," by Mr. Gwyn Jeffreys, F.R.S., in continuation of his work on "British Conchology."

AN English edition of M. Taine's "De l'Intelligence," revised with additions by the author, is in preparation, and will shortly be published by L. Reeve and Co.

WE have received a prospectus of a very energetic society located in the north of London, called the Hackney Scientific Association, which is now commencing its fourth session. It numbers among its officers many men of high scientific standing, and the following is its programme of papers for the season:—Recent Progress in Astronomy—W. T. Lynn; Comets—Henry T Vivian; Observations on Solar Eclipses—William J. Dyer; Water—C. W. Stidstone; The Predicted and Observed Meteorology of 1870, and the Probable Weather for 1871—Frederic Pratt; A Catalogue of variable Stars, with Remarks upon their Physical Constitution—Albert P. Holden; The Calendar—Henry T. Vivian; The Methods used in endeavouring to determine Man's Antiquity—William H. Davis; Infusoria—William West; The Diseases prevalent in certain Geological Formations—H. W. Emons; Micro-Photography—C. W Stidstone; The Habitable Condition of other Worlds, as affected by Temperature—Frederic Pratt; The Origin and Constitution of the Minor Planets, called Asteroids—Albert P. Holden; Inorganic Chemistry—George C. Fearn.

WE are glad to find in the number of our able contemporary, the *Academy*, for October, an extension of the scientific department. The paper will be published in future regularly on the 15th, instead of the second Saturday in each month.

WE learn from the *American Entomologist and Botanist* that the botanists of New York have formed themselves into a club, named, after one of the most distinguished botanists of America, the Torrey Club. The club publishes a monthly *Bulletin*, the object of which is "to form a medium of communication for all those interested in the flora of this vicinity, and thus to bring together and fan into a flame the sparks of botanical enthusiasm at present too much isolated." It is many years since a similar botanical club or society has existed in London.

MR. G. P. BIDDER has written to the *Times* describing a remarkable example of parhelia or mock suns observed by him on the 7th inst. at Canterbury:—"A portion of a circular arc was seen considerably above the halo and convex to it and the sun. The centre of the circle of which it formed part was, as nearly as I could estimate, close to the zenith. This arc had all the prismatic colours pure and distinct, and about 80° or 90° of it were visible. The following rough approximation represents the general arrangement of the whole: Altitude of the sun, about 12°; radius of the halo, about 25°; interval between the nearest points of halo and rainbow arc, about 25°; radius of rainbow arc, about 25°."

THE new buildings of the Taunton College School were formally opened during the past week. After a sermon from the bishop of the diocese and the inevitable luncheon, some very good speeches were made, among which we may especially mention those of the head-master, Rev. W. Tuckwell, Mr. J. G. Fitch, and Mr. E. B. Tylor. Mr. Tuckwell thus announced the programme of the system of instruction given at the school: "I may be allowed to say one word upon the nature of the education which we are striving to carry out, because to this is chiefly due the impulse in our favour which has so greatly altered our position. All who have been concerned with us are aware that in our curriculum we have departed widely from the ancient systems. We refuse to restrict our boys, as my own contemporaries were restricted, to the exclusive study of Greek and Latin; but while we give to these only a portion of our time, and find room for the higher mathematics, for physical science, for geography and history, for French and German, and for a careful study of English literature and language, we assert that by improved methods, by dexterous timing, and by greatly increased personal labour, we teach these new subjects thoroughly, and by no means neglect the old ones."

Engineering records the death, on the 16th inst., at New York, of Mr. Thomas Ewbank, the well-known writer on hydraulics. He was born at Barnard Castle, England, in 1792, and, after being apprenticed to a tin and coppersmith, came to London, where he spent all his spare time in scientific study. In 1819, being then a member of several learned societies, he emigrated to New York, where he was engaged for seventeen years in business as a manufacturer of lead, tin, and copper tubing, which occupation he relinquished in 1836 for purely scientific work. Besides contributions to scientific journals, and labours on various Government scientific committees, Mr. Ewbank was the author of "A Descriptive and Historical Account of Hydraulic and other Machines for Raising Water, both Ancient and Modern;" "The World a Workshop, or the Physical Relation of Man to the Earth;" "Life in Brazil, or the Land of the Cocoa and the Palm;" and numerous smaller publications.

THE United States steamer *Kansas*, now fitting out at the Washington Navy Yard for duty on the Tehuantepec and Nicaragua Expedition, was, according to *Engineering*, put in commission about a fortnight ago. The *Kansas* will be the principal vessel of the expedition. The survey in Nicaragua will embrace the route for a canal advocated thirty years ago by the Emperor Napoleon. That in Mexico by the Tehuantepec river possesses less interest, owing to the length and difficulties of the route.

THE first quarterly number of the *Journal of the Iron and Steel Institute* is announced to appear with the new year.

MR. ADOLPH HUBNER read an interesting and valuable paper on scientific observation in the interior of Port Natal, at a meeting of the Natural History Association of that colony on the 20th of August.

ON the 8th August Mr. S. Vincent Erskine read a paper before the Natal Natural History Society—Mr. John Robinson in the chair—on the Tsetse Fly. Mr. Erskine severely criticised Dr. Livingstone's statements, and denied that the fly was destructive to the life of the ox, horse, or dog. He affirmed that death was to be attributed more to change of grass or climate. The same evening Mr. Morant read a paper on the Entomology of the Free State and the Trans Vaal, particularly with regard to the butterflies.

THE expedition of Yale College students, under the leadership of Prof. O. E. Marsh, to which we referred last week, spent several months in the Rocky Mountain regions, investigating its flora and fauna, and collecting for the Yale Museum as fine collections as possible of the extinct animal remains found in such abundance in the tertiaries and cretaceous deposits of Nebraska, Dakota, and Wyoming. Leaving this region they will visit California, and after investigating the geology of the Pacific coast, will return through Colorado and Kansas, reaching New Haven, if possible, in November.

COCOA

COCOA is a valuable article of food that is becoming more and more in use in this country, and judging from the increased importations during the past three or four years, and the constant average of the coffee imports during the same period, it seems that cocoa is, in a measure, displacing coffee as a popular beverage.

The plant producing the cocoa of commerce is a tree seldom growing to a greater height than 17 or 18 feet. It is known to botanists as *Theobroma Cacao*. It bears an oblong fruit, ribbed longitudinally, measuring from six to ten inches in length and four to five inches across, and, when ripe, is of a yellow colour, changing to brown in drying.

It contains from fifty to one hundred seeds, and these seeds, after being washed, thoroughly dried in the sun, and roasted, form the cocoa-nibs of commerce.

Linnæus must have had a high appreciation of cocoa when he gave to the genus the name Theobroma, which is derived from *theos*, god, and *broma*, food, signifying it as food fit for a god. Cocoa contains a large amount of nutritive matter. In this respect it differs in a marked degree from tea and coffee; for while they are taken only in infusion and are used as refreshing beverages, cocoa is usually taken more in substance, and, as such, may be considered both as food and drink.

It was used in very early times in Mexico, whence it was introduced by the Spaniards into Europe about 1520. Humboldt tells us that it was extensively cultivated in the time of Montezuma, and the seeds were commonly used as money by the Aztecs. At the present time the cocoa-tree is largely grown in the West Indies, more especially in Trinidad, and over a great part of tropical America. Numerous varieties of the cocoa tree exist, some producing longer, or broader and some thinner or thicker skinned fruits, others producing larger, longer, or broader seeds, as the case may be. The seeds also vary in quality, according to the variety producing them or the place of their growth: thus Caraccas and Trinidad seeds are considered the finest, and some manufacturers use the names of the best districts as a recommendation to their wares.

The seeds are brought into this country in a dried state, and are roasted in revolving metal cylinders, the heat causes them to shrivel slightly so that the husks or skins are left loose and are removed by fanning. It is said that large quantities of these husks are imported from Italy under the name of "Miserable," and are used in Ireland by the poorer classes. The roasted seeds, after the husks are removed, are known as cocoa-nibs, but they are never seen in commerce in their whole form. The seed naturally divides by its two cotyledons, and in the process of winnowing each cotyledon gets broken into two or more pieces. To obtain the nibs and boil them in the old-fashioned way is certainly the surest way of getting genuine cocoa.

Some trouble, however, attends the preparation of the beverage in this form, the nibs requiring to be boiled an hour or two to extract their valuable properties. To obviate this, and to supply the public with a more convenient article, powdered cocoas, which require simply mixing with cold milk, boiling water being afterwards added, were introduced. These prepared cocoas opened a wide field for wholesale adulteration, the public, by using them, sacrificing purity for convenience in the preparation for the table.

These powdered cocoas are "prepared" by reducing the seeds to a fine paste by grinding them under heavy heated rollers—starch, flour, sugar, molasses, and, in the cheaper kinds, other ingredients less wholesome being added; after which, the whole mass is reduced to powder, packed in different forms, and sold under various trade terms, such as "Homœopathic Cocoa," "Soluble Cocoa," &c. Each manufacturer's individual preparation varies perhaps in flavour, according to the proportion or character of the ingredients added. The numerous forms of cake chocolate are prepared in the same way, vanilla being largely used in the flavouring, and the pasty mass being pressed into moulds instead of being reduced to powder. Rock cocoa and Flake cocoa are likewise prepared in a similar way, but are not so highly flavoured.

Few articles are more liable to adulteration than cocoa; and so many forms or qualities are known in trade, varying in price from 6*d.* up to 4*s* per lb., that it is not surprising that in the cheapest forms the adulterants themselves should be of the commonest and worst description. If people would only trouble themselves to think that cocoa-nibs, which are simply the roasted seeds without any preparation, are retailed at 1*s.* 4*d.* per lb., how can they expect to obtain an equally genuine article in a finely-pulverised state, and packed in tinfoil and a showy outward cover, at the same price? which is what the so-called "Homœopathic" and similarly prepared cocoas are sold at. Expensive machinery in the first place, and the constant wear and tear of the same, the consumption of fuel in the steam apparatus, and the expense of packing, have all to be paid for by the consumer, not by charging him a directly higher money price, but by increasing the bulk or weight of the article by adding foreign substances of a much cheaper description, and, which is frequently done in the commoner kinds of cocoa, bad or damaged seeds themselves. There is one thing to be said in favour of our principal cocoa manufacturers, that they seldom advertise these powdered cocoas as genuine; they either leave out that important word altogether or call them "prepared" cocoas, and this word should be borne in mind by those who wish to avoid the prepared and to obtain the real article, and are consequently ready to pay a fair price for such. If it is impossible to procure genuine powdered cocoa at 1*s.* 4*d.* per lb., still more impossible is it at 6*d.*, which is the price paid by the poorer classes for an article called "Soluble Cocoa," sold in ¼ lb. packets at 1½*d.* each, and largely consumed by them. The very fact of its low price ought to be sufficient to tell us pretty plainly that a very small quantity of cocoa, and that of an inferior description, is to be found in such a packet. It contains a large amount of common fat, the presence of which can be detected by smearing a little on a piece of glass, and can be still more clearly seen on a glass slide under a microscope. The addition of fat adds to the weight, while, to increase the bulk, a very large quantity of starch is added, which is the cause of the thickening of the beverage in the cup. If a little of this so-called cocoa be placed on the tongue and rubbed

FIG. 1.—Section of Cocoa-Nib as seen under the microscope

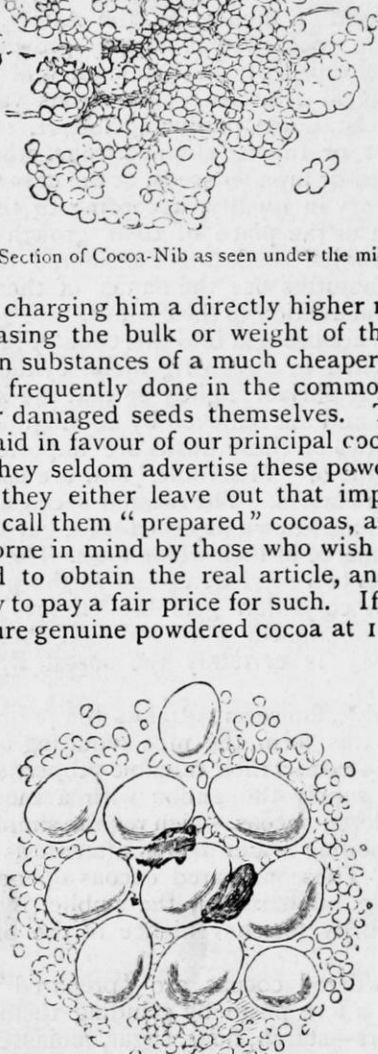

FIG. 2.—"Soluble Cocoa" as seen under the microscope

against the roof of the mouth, it will be found to grate against the palate, and, moreover, to have a decidedly chalky or earthy flavour. The spoon also grates against the sediment at the bottom of the cup, clearly showing the presence of mineral matter.

Until within the last few years, all these powdered cocoas were more or less "prepared," so that pure cocoa could not be obtained in this convenient form. An article, called "Cocoa Essence," recently introduced, has, however, dispelled this notion. We all know that the cocoa-seed naturally contains a large quantity of butter or fat (about 50 per cent.) which makes it too rich or heavy a beverage for many persons, and this more especially when we consider that other elements of nutrition, such as albumen, are also present. To deprive it entirely of its butter would be to take away one of its valuable principles; but it is possible to have too much of a good thing; therefore, by taking away about two-thirds of the butter the cocoa itself is not only improved in a dietetical point of view, but the addition of sugar, arrowroot, &c., is rendered unnecessary to take up or balance the fatty portion. Those who wish for pure cocoa in a convenient form should therefore obtain the "Cocoa Essence." It is sold in 3oz. packets at 6*d.* each. A small spoonful is sufficient for one cup, and, unlike the "Homœopathic," "Soluble," and other similar cocoas, it is not mixed with milk, but with a little boiling water, and stirred for a second or two until it is dissolved into a fine paste, the cup is then filled with boiling water, and milk and sugar added to please the taste. As no sugar is used in the manufacture of this article, it requires the addition of a larger quantity than any of the so-called prepared cocoas, and as no starch enters into its composition, the beverage is as clear as a cup of well-strained coffee. It is quite as portable as any of the packet cocoas, and as easily mixed. Its extra cost, in the first instance, is fully compensated by its purity, and by the fact that a smaller quantity is required for each cup. It is, moreover, a proof of the extensive adulteration of those kinds which are retailed at 1*s.* 4*d.* or 1*s.* 6*d.* per lb.

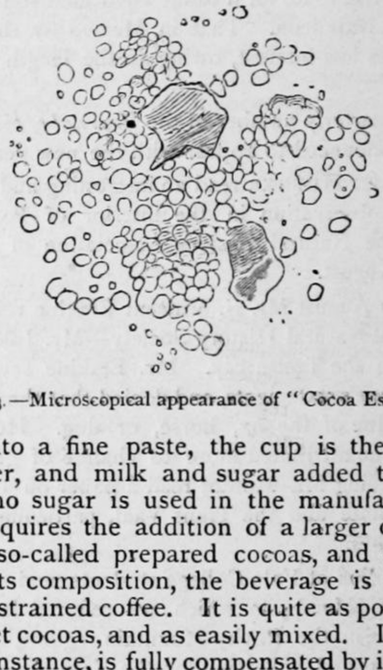

FIG. 3.—Microscopical appearance of "Cocoa Essence"

To such an extent has the public palate been led to prefer the flavour of many adulterated articles to that of the genuine, that we believe a great proportion of those who take cocoa really do prefer the thickened soup-like preparation made from the highly-flavoured and doctored sorts to an infusion of the pure seeds. If such people would think for one moment why and for what purpose they take this or that kind of food, and what are the properties and effects on the system of the articles they are supposed to consume, and what those of the articles they actually do consume, a much better state of things might be brought about, for, pending the appointment of a public analyst, the head of every household might make himself analyst to his own family, and so see that he does not get cheated either in pocket or health.

JOHN R. JACKSON

THE RECENT DEVELOPMENTS OF COSMICAL PHYSICS*

THERE are two conflicting theories with regard to education. The extreme partisans of one of these would have us believe that the great object of education is not so much to inform as to discipline the mind—the subject taught in a seminary may not in itself increase the student's real knowledge, yet if it tend to discipline his mind, it has proved its value in their eyes as a branch of education.

According to the upholders of this theory, our object in going to school is to leave it with a mind enlarged in its capacity for acquiring knowledge rather than stored with knowledge itself—having trained the soldier well they would send him without scruple into the enemy's country, not only to fight his own way but to find his own weapons.

But there is another and opposite class of theorists who regard education not as an agent for training the mind so much as a means of storing it with knowledge. The extreme partisans of this theory would teach the student nothing but what is of apparent and immediate use; above all things they would teach him the sciences, both in their principles and also in the various details of their applications to the industrial arts of life.

The mind of the student who has undergone a training of this kind carried to its extreme, resembles the inmate of a house which is not so much well-furnished as filled full of furniture. In the accumulation of mere material, anything like plan or principle has been forgotten. It ought to be remembered that the value of a fact lies not in its existence somewhere in the mental storehouse, but in the readiness with which the mind can find it when required.

Now between these two extreme theories it is surely possible to steer a middle course—it is possible to avoid grounding on Scylla without being swallowed up in the vortex of Charybdis. In the material world, what would be said to a man who insisted upon developing bodily strength by a course of gymnastics without reference to food, or of another who insisted upon doing the same by a course of diet without reference to exercise? But is the separation more natural in the mental world? Is not that mind most perfectly disciplined which is at the same time most perfectly informed? The student who has been disciplined by only one branch of knowledge is, I venture to think, the possessor of a mind only partially disciplined, as it is only partially informed. He may not easily perceive his deficient discipline, but in after life he must often have cause to regret his deficient information. Nor is he whose mind is inordinately stored with scientific details one whit better off. Facts in education ought to be strictly subordinated to principles. A sound principle of science clearly understood will embrace a great multitude of facts, just as a simple rule of arithmetic will enable us to obtain a thousand products, each of which we should have to remember were it not for the rule. And in other branches of science, if the triumph of principle be not so apparent, it is only because we have a less accurate knowledge of its fundamental laws. It would be difficult indeed to say how many of the failures in the various walks of life are due to the neglect or ignorance of some principle which has been either omitted or dismissed from our calculations. From our leaders downwards we are a nation systematically ignoring principles, and in the minds of many a high esteem for fact is held to be quite consistent with a contempt for theory. A theory is not regarded as the very sap and life-blood of the tree of knowledge, but rather as the mildew that blights its leaves, or the worm that gnaws its root. Facts and theories are esteemed by this class of men to be sworn foes to each other, and the philosopher is supposed to live in a world of his own, rather hostile than otherwise to the world around him.

The existence of the two extreme educational theories to which I have alluded would thus seem to indicate the wisdom of a middle course. We ought to start from a platform as comprehensive as possible. Literature and science ought to go hand in hand in producing the well-trained and well-informed disciple. And while there ought to be a broad basis of instruction common to all, there should be raised upon this common basis several distinct departments, so that the student may have the opportunity of attaining proficiency in that of his choice.

Professor Stewart next touched upon the subject of energy.

* Extracted from a Lecture delivered at Owens College, Manchester, introductory to the Session 1870-71, by Professor Balfour Stewart, LL.D., F.R.S.

If an egg be allowed to rest on its shorter axis, it will remain stationary, and any attempt to alter its position will be resisted by the egg. But the case will be different if we succeed, as perhaps we may, in causing it to stand on its longer axis, for in this position the slightest force will cause it to topple over. In the first case the egg is in stable, but in the second case it is in unstable, equilibrium.

If it stand upon its longer axis at the very edge of a table, we cannot tell whether the first slight breath of air will cause it to fall inwards upon the table or outwards over the table, to be dashed to pieces on the floor. It is a case where a very slight cause may determine a very considerable issue as far as energy is concerned. Whether the egg will retain its energy of position by falling on the table, or whether it will convert this into the energy of motion, and ultimately into heat, by falling upon the floor, is an issue that depends upon a cause too minute to come within the scope of our calculations.

Now we have two types of machines, and in one of these we take advantage of the principle of stability, while in the other we use the principle of instability. A clock is a very good instance of a machine of the first kind. When a good clock has been wound up, we are perfectly sure that at noon to-morrow both its hands will stand at twelve, and that its weight will have fallen through a distance which we can calculate with the utmost exactness, if we take the trouble, all its movements being capable of the most rigorous calculation. On the other hand, a mine that is about to be fired by means of an electric battery is a machine or combination in which advantage is taken of nature's unstable arrangements. The powder which is about to explode represents chemical instability, just as the egg on its longer axis represents mechanical instability. The slightest percussion, the smallest spark will rouse the imprisoned giant which it contains into volcanic energy. This spark has to be sent from a distance by the galvanic battery, and to do this we must complete the circuit. We cause the two wires to approach each other until they are only a very small fraction of an inch apart, but the contact is not yet complete—another touch, so slight as to be imperceptible, and the current passes, the powder is ignited, the mine explodes, and the fortress is hurled into the air. In such machines, great results, great transmission of energy, are due to the most trivial disposing causes. It depends at last upon the smallest conceivable movement of the wires conveying the current whether or not the fortress is to perish.

Nature also employs these two varieties of mechanism. In the solar system we have a clock on a large scale, only very much more accurate than any time-piece we can produce. The movements of every planet in the solar system are susceptible of the most rigorous calculation, and we have only to point our telescope properly in order to tell to the fraction of a second when a given planet will cross the field of view.

But in the living forms with which this world is so plentifully endowed, we have machines, which, viewed in their relation to matter, belong to the second order we have described. The object here is not regularity, but rather freedom of action. The motion of an animal is not like that of a planet—the latter yields to calculation, but the former defies it. Now it is probably somewhere in the mysterious brain chamber that the delicate directive touch is given which determines our movements, just as the slightest possible touch to the wire in the battery chamber explodes the distant mine. That mysterious thing we call life is not a bully who swaggers out into the open universe, upsetting the laws of energy in all directions, but rather a consummate strategist, who, sitting in his chamber before his wires, directs the movements of a great army.

While we are thus led to confine the directive action of life to the very boundary of the universe of energy, it must not, however, be supposed that we have solved the problem as to the nature of life. We have only driven the difficulty into a border land of thick darkness, into which the light of knowledge has not yet been able to penetrate. If there be truth in these statements, and if we see in a living being a machine in which great results are produced by an exceedingly small primeval impulse, are we not led to expect that the unstable forms of nature will here be largely made use of? We must not therefore be surprised that the materials of our bodily frames are eminently liable to decay, or that the very intensity of our life is to be measured by the rate of change taking place in the tissues of our bodies, so that possibly those parts which have during life the noblest and most delicate offices to perform are the very first to perish when life is extinct.

But this unstable matter, which is so wonderfully worked into our frames, is derived from the food we eat. This food does two things for us: it gives us energy in the first place, and in the second it furnishes our frames with a quantity of delicately organised tissue. But food is ultimately derived from the vegetable kingdom, and that kingdom derives it from the sun, so that we are led to regard our luminary as the ultimate material source not only of our energy, but also of our delicacy of construction.

To come now to our own luminary—very remarkable strides have lately been made in our knowledge of its physical constitution. It is difficult to say when and by whom the existence of sun spots was first remarked. Galileo, however, was the first to use them as the means of determining the elements of the sun's rotation. Besides these black spots on the sun's surface, equally mysterious forms have been seen to surround the sun on the various occasions of a total eclipse—these generally went by the name of red flames or red protuberances. Mr. Warren De La Rue was the first to prove that these phenomena were attached to the sun himself, and that the only office of the moon during an eclipse was to subdue the general light sufficiently to render them visible to the eye. While the red flames thus became objects of cosmical interest, Schwabe in Germany and Carrington in this country had both done much to increase our knowledge of sun spots. Schwabe, by a patient course of forty years' observations, had proved the existence of a well-marked periodical fluctuation in the amount and frequency of sun spots, the period of which was about eleven years. Carrington, again, had shown that the region of spots was a somewhat limited one, extending to about 20° or 30° on either side of the solar equator, so that a spot never appears at the sun's pole, and he had also detected a proper motion of spots. Schwabe and Carrington had, however, confined themselves to mapping down accurately what they saw; but De La Rue, by the introduction of celestial photography, was enabled to obtain autographs of the sun which might be studied at leisure with an absolute certainty of their being correet. A large number of such pictures has been already obtained, and they are in the process of examination by Mr. De La Rue, and those associated with him in this research.

Some of the preliminary results of this examination have already been published, and they seem to point to a connection between the behaviour and frequency of sun spots and the positions of the chief planets of the system.

Our acquaintance with the red flames has extended as rapidly as our knowledge of sun spots. It was discovered independently by Janssen and Lockyer, that these strange protuberances yield to the spectroscope under ordinary conditions of the sun, and without the necessity of waiting for a total eclipse. They exist, in fact, always round the sun, but their brightness is quenched in the diffused light which surrounds the sun's border. When, however, we apply a sufficiently powerful spectroscope, the diffused light—consisting of ordinary sun light, and therefore containing a great variety of rays—is drawn out into a long spectral ribbon, and has its brightness scattered or diffused over the various parts of this ribbon, while on the other hand the light from the red flames, consisting only of one or two kinds, appears in the spectroscope as one or two bright lines not having their intensity weakened by the scattering action of the spectroscope. They, therefore, stand out in the field of view, while the ordinary light disappears. Lockyer has found, by this means, that there is an envelope of incandescent hydrogen surrounding the glowing surface of the sun, into which there are frequent injections of heated matter from beneath, and in which there are violent huricanes often moving at the rate of 100 miles a second. By the laboratory labours of Frankland and Lockyer, taken in connection with the solar observations of the latter, there is, I think, a probability of our ultimately ascertaining the pressure and the temperature as well as the chemical composition of the atmosphere of our luminary.

Descending now from the celestial bodies to our own earth, there is some reason to suppose that we are knit to our luminary, and possibly through him to the other members of our system by some other bond, besides that usually recognised. General Sir E. Sabine appears to have proved that disturbances of the earth's magnetism take place most frequently in those years in which there are most sun spots. This is confirmed by the experience of the present year, during which we have had a great number of sun spots, and frequent and large disturbances of the earth's magnetism.

I have already alluded to a possible connection between the behaviour of sun spots, and the positions of the planets; to which we may add, that Schwabe and other observers imagine that they have discovered traces of a periodical variation in the appearance of the planet Jupiter. All these observations would appear to indicate the existence of some unknown bond between the different members of the solar system.

But that department of cosmical physics which is of most immediate interest to ourselves, is undoubtedly the meteorology of our globe; and here it is of great importance to know whether the earth's climate and atmosphere are influenced in any way by the changes taking place in the atmosphere of the sun. Such a connection has not yet been traced, but it has hardly yet been sought for in a proper manner. Recent observations discussed by Baxendell, lead us to think there may be some connection between the daily changes in the earth's magnetism and the daily motions of the air. Coupling this with the fact that the frequency of terrestrial magnetic disturbances would appear to be connected with that of sun spots, we are led to contemplate at least the possibility of some connection between meteorology and sun spots.

If these remarks are of any value, they tend to indicate the probable union of the various branches of observational inquiry into one great cosmical research, and point to the wisdom of a very close union between the workers in the cognate fields of meteorology, terrestrial magnetism, and celestial physics.

At the present moment the prospects of meteorology are more hopeless than those of the other two branches. Our knowledge of the motions of the various components of the earth's atmosphere is extremely limited, and yet without this knowledge it is impossible to connect meteorology with the other branches of cosmical inquiry. If we endeavour to analyse the causes of this backward state of meteorological research, the first and most apparent is the magnitude of the problem.

We are too intimately associated with the earth and its atmosphere to be easily able to tell its motions. Strange to say, the meteorology of the sun is more easily studied than that of the earth, and we know already as much about the strength of solar storms as we do about that of terrestrial hurricanes.

But another cause of the backward state of Physical Meteorology arises from the fact that there are two branches of science, each of which may be furthered by meteorological observations. There is first the physiological branch of meteorology, the object of which is to trace the influence of climate upon animal and vegetable life; and there is next the physical branch, the object of which is to study the physics of the earth's surface, and more especially the motions of its atmosphere.

It is now high time that a separation should be made in the mind of the observer between these two branches of research. If he would rather pursue the physiological inquiry, let him say so definitely, but if he wish to pursue physical meteorology, let him clearly keep before his mind the object of all his labours. He should ask himself the question, what is the best system of observation, and what is the best method of reduction, to advance the great object of physical meteorology—a knowledge of the motions of the earth's atmosphere, and of the causes thereof? He should not fix upon a system of observations and a method of reduction that may possibly, but upon one that must necessarily, further this great object.

I have thus endeavoured in a few words to bring before you the recent advances in cosmical physics. Besides this, there are two other no less important branches of physical inquiry. We have the physics of organised beings, and we have also molecular physics. But there is this difference between these two branches and that of which I have now spoken:—To forward physiology or molecular physics we chiefly require experiment, but to forward cosmical physics we chiefly require observation. You are all aware that at the present moment a Royal Commission is inquiring as to the relation between Science and the State; and perhaps, therefore, you will permit me the opportunity of stating my views as to the manner in which this very necessary assistance may best be given. I think that those branches of science which demand for their extension experiments not requiring very great time may be furthered with much advantage in institutions such as Owens College. I believe it to be advantageous to bring the highest class of physical teaching into contact with research. If Government be disposed to grant pecuniary aid to such researches, an extension of the allowance made annually to the Government Grant Committee of the Royal Society would appear to be a very legitimate way of accomplishing this object.

The scientific professors of a college would thus have to state the aim of their research to a committee of the Royal Society entrusted with the disposal of Government means, and the requisite funds would be forthcoming. No one, I think, can doubt 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 $74\frac{1}{2}°$ Fahr. on the 14th March, the air being but $37°$; and in June of $79\frac{3}{4}°$, 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 southeast 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. Lloyd. 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 Bedford, writes: "Observers of solar phenomena have seldom an opportunity of witnessing such a fine outbreak of spots as that which took place during the last fortnight of August. After the disappearance of the large group observed in the S. hemisphere (about July 31), a comparative lull in solar activity ensued, lasting thirteen days; the spots which appeared during this interval presented no remarkable features, and were mostly confined to the S. hemisphere. On the 17th, in the N. hemisphere, a large scattered group was observed, which a few days before had consisted of a congeries of minute specks; on the 18th it was 2′ 55″ in diameter, and was followed by another group, 2′ 26″ in length; both these groups diminished very rapidly after the 19th. On the 20th the two largest groups on the disc were nearly central; one of them 36″, the other 54″, in diameter. Cloudy days intervened between the 21st and 24th. On the latter date the first indications of the approaching outburst were remarked. At 4^h 30^m there were three immense groups in the N. hemisphere, extending from the centre of the disc to the E. limb; the preceding group, which was made up of very light and ill-defined penumbræ, enclosing upwards of sixty separate black spots, measured 3′ 10″ × 1′ 49″ The second group was 1′ 20″ in length, the third was too near the line to be satisfactorily measured. From the 26th to the end of the month the north maculose zone was completely crowded with groups and isolated spots, while the corresponding S. zone contained only punctures and small clusters. The following are the lengths of the three largest groups observed on the 29th: 3′ 6″, 2′ 26″ and 1′ 57″. The spotted zone could be seen with the naked eye, protected by an ordinary dark glass at noon on the 28th; it had the appearance of a dusky belt parallel to the sun's equator. Fresh groups observed in the sun's N. hemisphere during August = 11; ditto observed in the sun's S. hempishere = 15. Maximum number of groups on disc = 13 (Aug. 29, 21^h 18^m); minimum number = 4 (Aug. 20, 4^h 15^m).—Mr. William F Denning, of Bristol, observed the sun with his 3in. refractor, on Aug. 28, and reports that on this date four large groups of spots were visible in the northern hemisphere. In the N.E. quadrant two large groups were perceptible lying just above the equator. In the N.W quadrant an irregular scattered group was seen near the limb, and another group near the centre of the disc was very conspicuous. The S.E. quadrant contained three small groups, while the S.W. quadrant was entirely free from visible spots.

Aurora Borealis.—Mr. H. Michell Whitley writes that on August 20 he observed a brilliant aurora. From 11^h 30^m to 12^h it was very well defined. Straight beams of light shot up from the N. horizon to an altitude of about 35°. "These streamers faded and reappeared in other places." Mr. Henry Ormesher, of Manchester, also witnessed this phenomenon. He says, "I first observed it at 11^h 40^m, but from its appearance it must have been visible for some time previous. I determined the extent of its base to be as far as W. by N. to N.E. by N. From between these points streamers shot forth in rapid succession, to a very considerable altitude, a great many of them reaching to the zenith of my place of observation. Some of these streamers were very brilliant, particularly one which at 11^h 50^m shot forth from a point just beneath the Pointers in a direction towards the polar star, and onwards to the zenith. I should think this stream of light to have been of about five minutes' duration, during which time its colour changed from a dark straw to a yellowish tinge. At 12^h 10^m there was quite an auroral arch, whose centre was towards the magnetic pole, and extending from the before-mentioned points to an altitude of at least 40°. The brightness of this arch increased until about 12^h 14^m, when it was exceedingly brilliant. During the whole of the time the sky was very clear, with the exception of a reddish glow, of which the aurora was the cause.

Meteors.—Very few meteors appear to have been observed on about August 10. Mr. Edmund Heison saw nine on the 10th, three on the 11th, and two on the 12th. The Rev. S. J. Johnson watched the sky from 10^h 45^m to 11^h 46^m on the same date, and only detected one. Mr. H. Michell Whitley, of Penarth, witnessed the appearance of two meteors on the evening of August 29. The first was visible at 10^h 25^m, and was accompanied with a faint train. It passed downwards below Corona Borealis. The second was seen at 10^h 30^m to the W. of Aquarius. Both were equal to a first magnitude star. On the 30th the Rev. S. J. Johnson observed the train of a very brilliant meteor. From the appearance of this train it was evident that the meteor must have become visible a degree or so to the W. of *k* Draconis and have ended a degree or two to the E. of *a* Draconis.

NEW ZEALAND

Wellington Philosophical Society, July 10.—The value of the New Zealand Flax was fully discussed, and Dr. Hector exhibited the operation of the machine he is employing in testing the strength of the various fibres for the Commissioners who have been appointed to investigate the subject. The result, as far as yet obtained, tends to prove, that while the flax of the ***Phormium tenax*** dressed in the native manner greatly exceeds in strength either Russian hemp or Manilla; yet, when dressed by the machines in ordinary use, it is much inferior. The few samples of the fibre prepared by retting or carefully applied chemical processes, however, gave much better results.

July 17.—Mr. T. H. Potts described an egg of the Great Auk which is in his possession.

New Zealand Institute, July 23.—Anniversary meeting, his Excellency Sir G. F. Bowen, G.C.M.G., in the chair. The president, in adverting to the transactions of the Institute and affiliated societies during the past year, drew attention to the number of contributors on a great variety of subjects to the last issued volume, as proving that a large amount of intellectual activity and practical zeal exist among the associates, although debarred by the geographical circumstances of the colony from achieving frequent meetings. The address was chiefly directed to the necessity for practical scientific instruction; and he stated that the Board of the Institute, having been applied to, the Government had recommended that a course of lectures shall be established in connection with the Museum and Laboratory, on natural history, geology, chemistry, and mineralogy. In proposing the thanks of the meeting to the president, the Hon. Mr. Fox, Premier, stated that the scheme which his Excellency had propounded would be favourably entertained by the Government, who were very desirous of assisting the diffusion of sound scientific instruction, as it was an essential step towards developing the resources of the Colony.

BOOKS RECEIVED

ENGLISH.—The National History of Commerce: John Yeats (Cassells and Co.).—The Tr to e: a Method of Harmony and Modulation: G. Green (Novello).—The Forces of the Universe, part I.: G. Berwick (Longmans).—The Adventures of a Young Naturalist: L. Biart (S. Low, Son, and Co.).

FOREIGN.—(Through Williams and Norgate)—Abhandlungen der mathematisch-physikalischen Classe der k. bayerischen Akademie der Wissenschaften 10er Band.—Flora der preussischen Rheinlande: Dr. Wirtzen.—Lehrbuch der Ingenieur- und Maschinen-Mechanic: Dr Weisbach.—Das Naphthalin und seine Derivate: M. Ballo.—Anleitung zur Ausmittelung der Gifte: Dr. R. Otto.—Leopold von Buch's gesammelte Schriften 2er Band.

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