was brought prominently to light during the time that the normal sidereal clock for Greenwich was under trial. That clock had been fitted with a heavy mercurial pendulum, and it was found that the rod got warmer or colder some time in advance of the mercury; of course the compensation *failed* for such interval of time.

The following form of pendulum was afterwards substituted. The expansion of zinc is, as you see, nearly double that of brass, and consequently a good deal less of it is required to compensate a pendulum. To the extremity of an internal steel rod (see Fig. 12) a collar is fastened, and a zinc tube inclosing the steel rod rests upon it. To the summit of the zinc tube is attached a steel tube, which in turn incloses both it and the rod, and the pendulum bob is fastened midway of its length to the extremity of this tube. The outer steel tube is cut away at its sides, and holes bored in the zinc in order to let in changes of temperature rapidly.

The action of the combination is similar to that of the gridiron pendulum, the expansion of the zinc upwards exactly neutralising and destroying the expansion of the steel downwards. It is important (as suggested by Mr. Buckney) that the bob should be suspended at its centre, because otherwise it would also operate as an expansion length, and although its effect could be counterbalanced by shortening the zinc tube, yet owing to its bulk it would be sure to lag behind the rest of the compensation, and cause such an error as I have referred to.

Barometric Compensation.

When you aim at the very highest time-keeping, barometric compensation becomes necessary; that is to say, compensation against the disturbance to the pendulum due to changes of atmospheric pressure. For instance, when there is any rise in pressure, when the atmosphere becomes denser, our clock will lose, and will gain when the atmosphere becomes more attenuated, the variation in the Greenwich clock having been at about the rate of ·3 of a second a-day for a difference of one inch in the barometer.

The following compensation (see Fig. 13) is one designed by Sir George Airy:—

C is a lever moving around an axis at A. One arm of the lever carries a horse-shoe magnet, *b*, and the other a float, *e*, supported upon the mercury in a barometer cistern. Two bar magnets (the front one, *a*, only is shown, the other being behind the bob) are fastened upon the pendulum bob, the north pole of one pointing upwards, and of the other downwards (in order to render the combination astatic).

The poles of the horse-shoe magnet face the opposite poles of the two bar magnets, and attraction goes on between them. When the barometer rises the mercury in the cistern falls, and with it the float. The other arm of the lever, therefore, rises, bringing the poles of the horse-shoe magnet closer to the poles of the two bar-magnets, and increases the attraction between them, which is a force acting in the same direction as gravity. The pendulum consequently moves faster (for we increase the pull upon it), the tendency to go slow arising from the increased atmospheric pressure is by this means compensated. Dr. Robinson, at the Armagh Observatory, effected the same correction by attaching a barometer to the pendulum rod. He also noticed that changes in atmospheric pressure would disturb a mercurial pendulum to a very considerable extent if there were air-bubbles in the mercury.

(*To be continued.*)

CROOKES'S RADIOMETER

I HAVE recently made a few experiments with this instrument which may not be uninteresting to the readers of NATURE.

The radiometer used had discs of aluminium polished on one side and blackened on the other; it was more than usually sensitive, and would sometimes continue its rotation for twenty minutes after the sun had set in the sea.

The instrument being in a room in which the radiation was far too feeble to cause the arms to move, I grasped the bulb with both hands, so as still further to exclude it from light. The vanes immediately began to revolve briskly, the polished sides first. Removing my hands after two or three minutes, the movement soon stopped; and then, after a very brief interval of rest, began in the opposite direction, and so continued for several minutes.

I now placed the instrument in a room, near to a window through which the light of the full moon in a clear atmosphere was shining. The arms of the radiometer did not move. By means of a large lens the moonlight was then concentrated about 200 times, and allowed to fall full upon the blackened side of one of the circular discs, in such a way as to cause the intensely brilliant image of the moon to nearly cover the disc. Not the slightest movement occurred, although the concentrated light impinged upon the disc for a quarter of an hour.

As is well known, the light of the moon contains, for a given luminosity, far less heat rays than does light from any terrestrial source, no matter how much the latter may be strained through intranscalent media; in fact it require Lord Rosse's 6-feet reflector clearly to demonstrate the excessively feeble thermal power of the lunar rays.

These experiments show, firstly, that light is not necessary to the movement of the radiometer; secondly, that light only contributes to the movement in so far as, by its absorption, it is transformed into heat; and thirdly, that the motion is due to the unequal heating of the two sides of the discs, the cooler surfaces always preceding the warmer; for when the instrument was grasped by the hands, the blackened surfaces of the discs rapidly absorbed the heat rays, whilst the polished surfaces reflected them. Thus the surfaces of the blackened discs remained warmer than the metal beneath, but gradually communicated their heat to the latter. On removing the hands from the bulb, the thermal condition of the discs would soon become reversed; the black surface—a good absorber and also a good radiator—would cool much faster than the opposite surface, which being of polished metal was an exceedingly bad radiator.

The blackened surfaces, therefore, now became the coolest, and preceded the polished ones, in other words, the direction of rotation became reversed.

October 17 E. FRANKLAND

THE GEOLOGY OF ENGLAND AND WALES[1]

THE well-known volume of Conybeare and Phillips, entitled "Outlines of the Geology of England and Wales," which was published in 1822, and was based on an earlier and slighter work of the second-named author, has long held an honourable place among geological classics. It has served, indeed, to supply to some extent the want so universally felt of a descriptive memoir or handbook to William Smith's Geological Map, a work which "the father of English geology" could never be prevailed upon to write himself. The "Outlines," however, is but a fragment, the second part of the work, which was to have dealt with the oldest rocks and with questions of Economic Geology, never having been published; and more than half-a-century of research, carried on in connection with a science which appears to have as

[1] "The Geology of England and Wales: a Concise Account of the Lithological Characters, Leading Fossils, and Economic Products of the Rocks; with Notes on the Physical Features of the Country. By Horace B. Woodward, F.G.S., of the Geological Survey of England and Wales. (London: Longmans, Green, and Co., 1876.)

yet lost none of the vigour and elasticity of youth, have of course rendered much of the information contained in it obsolete. The only work of more recent date which occupies somewhat the same ground is D'Archaic's "Histoire des Progrès de la Géologie," which aimed at doing for all those portions of the globe which had been geologically explored, what Conybeare and Phillips had attempted for England alone. This work is one of the very highest order of merit; its author being equally distinguished for his industry in the compilation of materials, his skill in arranging them, and his boldness and originality in generalising from them. But such a design as that of the "Histoire des Progrès" was perhaps too ambitious to be within the compass of the efforts of any single individual; at all events, after the portions relating to the Tertiary and Secondary strata had appeared in a series of eight volumes, between the years 1847 and '60, the work, which had up to that time been published by the Geological Society of France under the auspices of the Minister of Public Instruction, was finally abandoned.

It will be seen, therefore, that Mr. Woodward's handy volume, the title of which is given above, appears very opportunely; and, supplying as it does a real need of the geological student at the present time, it is certain at once to take its place as the most useful general work of reference on English Geology which exists. After a careful perusal of it, we find scarcely anything calling for qualification of those terms of high commendation in which we are constrained to speak of its general accuracy and excellence of arrangement; of the happy way in which the mean has been hit between conciseness of description and fulness of detail; and of the manner in which the work has been made to include the latest results of geological research.

At the time when Conybeare and Phillips wrote, many portions even of those Secondary strata of England, the successful classification of which had been the chief among

FIG. 1.—The Cheddar Cliffs.

the triumphs of William Smith's genius, were as yet almost unknown to geologists; the labours of Sedgwick and Murchison, which were destined to replace the confusion that reigned among all the older deposits, by the clear succession of the Cambrian, Silurian, and Devonian systems, had not then commenced; and as yet the palæontological studies of Lyell and the stratigraphical researches of Prestwich had not dispelled the almost equal obscurity which prevailed concerning the order of the Tertiary formations. There are perhaps few ways in which the strides made during the last fifty years in our knowledge of the geology of this country can be more vividly realised than by a comparison of the sketch-maps prefixed to the volume of Conybeare and Phillips, and to that of Mr. Woodward respectively. Such a comparison will render strikingly apparent the great advances which have been made in developing the true structure of the country, both through the researches of private individuals and the labours of the National Survey; and it will equally serve to demonstrate the necessity of such a work as that which Mr. Woodward has now given to us.

The avoidance by the author of this work of all references to the equivalent formations on the continent of Europe, or even to those in other parts of the British Islands—although perhaps a necessity dictated by the limits he had set himself—creates some serious difficulties, which are more especially felt when questions of classification come to be treated of. It is altogether vain to hope that such problems can be decided by an appeal to the English representatives of the formations alone. To discuss, for example, the question of the classification of the Silurian, Devonian, and Permo-Triassic (Poikilitic) formations, without any reference to the typical developments of these strata in Bohemia, the Eifel, and Central Germany respectively, is surely a most unsatisfactory and inconclusive proceeding.

In adopting Sedgwick's classification of the Cambrian and Silurian strata instead of that of Murchison, the

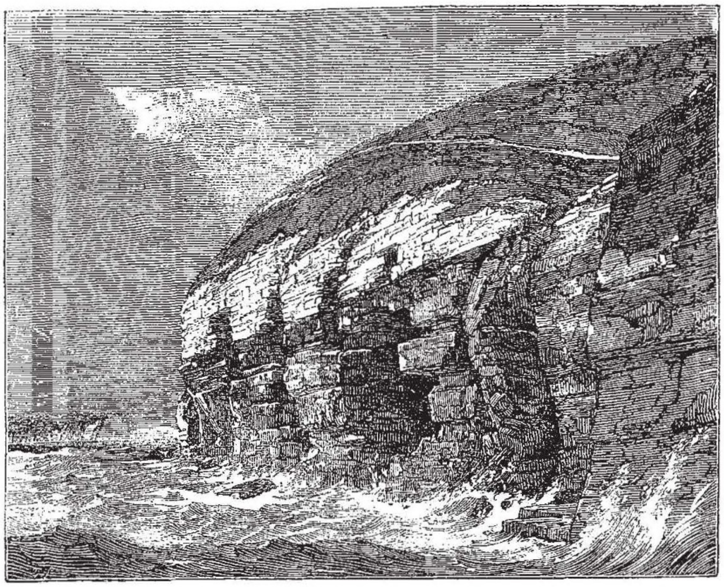


FIG. 2.—Purbeck and Portland Beds at Tilly Whim, near Swanage; the former being the light-coloured strata above, the latter the darker strata below.

FIG. 3.—Section at Writtle, near Chelmsford.—2. Chalky Boulder Clay (Upper Glacial). 1. Sand and Gravel (Middle Glacial).

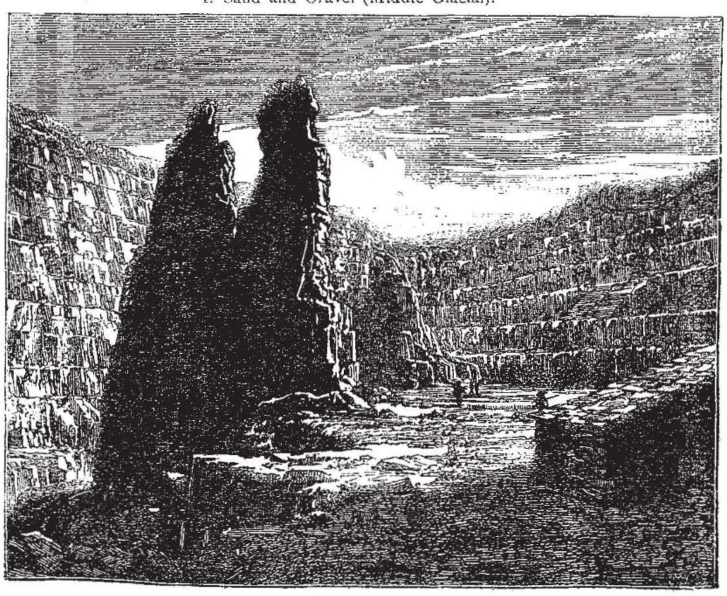


FIG. 4—Penrhyn Slate Quarry.

author may possibly have been actuated by the conviction that unless the pendulum of opinion, which has so long been firmly held at one end of the arc by official influences, were allowed to rebound to the extreme limit in the opposite direction, there would be little chance of its finally attaining a position of stable equilibrium between them. Looked at from any other point of view, we must confess that we cannot regard the attempt here made totally to revolutionise the classification in question with much satisfaction. We had hoped that the day had long since gone by when the divisions between geological periods were to be regarded as governed by anything more than convention, or as serving any other purpose than that of convenience of reference. Breaks, whether stratigraphical or palæontological, in the series of formations, are *purely local phenomena;* and it is certain that if stratigraphical geology had taken its rise only so far away as in Eastern instead of in Western Europe, the divisions of the great systems, and even of those larger periods (which Mr. Woodward calls "cycles") would have been wholly different to that which has been actually adopted. But although the classification of the geological periods is a purely artificial one, yet it has its uses, and nothing but confusion can result from attempts to unsettle its landmarks without sufficient cause. Such being the case, we are surely entitled to ask what useful purpose can possibly be served by including, as our author does by his own showing, considerably more than one-third of the whole thickness of British sedimentary deposits under the name of Cambrian? Is not a Cambrian system, enlarged beyond all reasonable proportions, equally objectionable with an overgrown Silurian? This question has passed beyond the stage when it can be regarded simply as a battle-ground for the partisans of rival reputations. Now that Sedgwick and Murchison have both passed away, let us rather seek to be guided by the principles which determined the action of the greatest of their contemporaries in respect to this controversy; gladly availing ourselves of that which is good and true in the splendid work of both the observers, let us build it into our geological system, there to stand as the noblest monument of their genius; and for their mistakes, let these pass into the oblivion which awaits the memory of the injustice and animosity which were unworthy of either of them.

There are one or two other points which we would venture to suggest for the author's consideration in the event of his being called upon, as we hope he will be, to prepare a second edition of this work. As the different formations or groups of strata belonging to the same system which occur in different parts of the country are treated of consecutively, although in many cases they were doubtless formed contemporaneously, it would be well to keep the latter fact as prominently before the mind of the student as possible; and this, we think, might best be accomplished by prefixing to each chapter diagrammatic sections of the succession of strata, exhibiting their equivalences in different parts of the country. Again, although we recognise with the author the impossibility of quoting in such a work as the present the authority for every statement, yet we think that a well selected series of references to those original memoirs, in which fuller details concerning each formation may be found, would greatly add to the value of the book without materially increasing its bulk.

We cannot but commend the manner in which Mr. Woodward has resisted all attempts at fine writing, and has sought rather to produce a work characterised by accuracy and soundness than by showiness and superficiality; in this respect following the example of his father, the late Dr. Samuel Woodward, to whose memory the work is dedicated. We anticipate for the "Geology of England and Wales" a sphere of usefulness not less extended than, and a reputation as enduring as that which has been attained by, the "Manual of the Mollusca;" and higher praise it would scarcely be possible to award to it.

It only remains to add that the work is illustrated, not only with a very clear chromo-lithographed map prepared by Mr. Griesbach, but by woodcuts of such excellence (as will be manifest from the specimens we give of them) that we can only regret that they are so few in number.

J. W. J.

SUMNER'S "METHOD AT SEA"

IN reference to our review of Sir William Thomson's work on this subject (vol. xiv. p. 346), our attention has been called by Sir G. B. Airy to the following paper in the *Proceedings* of the Royal Society, vol. xix. p. 448:—

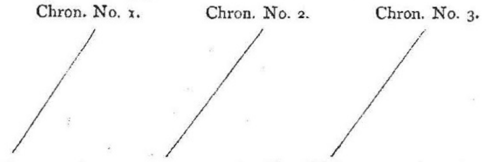
"Remarks on the Determination of a Ship's Place at Sea." In a Letter to Prof. Stokes. By G. B. Airy, LL.D., &c., Astronomer-Royal.

Royal Observatory, Greenwich, S.E., 1871, April 5.

MY DEAR SIR,—In the last published number of the *Proceedings* of the Royal Society (vol. xix. p. 259), there are remarks by Sir William Thomson on the proposed method for determining the *locus* of a ship's place at sea, by making one observation of the sun's (or other body's) altitude, and founding, on this, computations of longitude with two assumptions of latitude; and there are suggestions, with a specimen of tables, for solving the spherical triangles which occur in all similar nautical observations, on the principle of drawing a perpendicular arc of great circle from one angle of a spherical triangle upon the opposite side.

In regard to this principle and the tables which may be used with it, I may call attention to the employment of a similar method by Major-General Shortrede, in his "Latitude and Declination Tables," pp. 148 and 180. In p. 150, line 11 from the bottom, it will be seen that the "column" gives the trial-value of the perpendicular arc by which the two right-angled triangles are computed. This is not the same (among the various elements which may be chosen) as Sir William Thomson's; but it is so closely related that in some instances the tabular numbers are identically the same as Sir W. Thomson's, though in a different order. General Shortrede's object was "Great Circle Sailing," in which the trigonometrical problem is the same as in the nautical observation. I think, however, that Sir W. Thomson deserves thanks for calling attention to the application of this method to time-determinations.

In regard to the problem of the "locus," allow me to point out the geometrical circumstances of the case. If, upon a celestial globe, an arc of small circle be swept with the sun's (or other body's) place for centre, and the observed zenith-distance for radius, the ship's zenith will be somewhere in that curve; and if, with the pole for centre, arcs of parallels be swept with the two assumed colatitudes for radii, the intersection of these two curves with the first drawn curve will give the ship's zenith on the two assumptions; and if within the celestial globe there be placed a small terrestrial globe, and if these zenith-points be radially projected upon the terrestrial globe, the terrestrial places of the ship on the two assumptions will be marked. But the practical application of this requires that the position of the terrestrial globe, or of the earth, be known in respect of rotation—that is, it requires that the Greenwich sidereal time, or solar time, be known; in other words, it requires a perfect chronometer. Now the experience of Capt. Moriarty, cited by Sir W. Thomson, does not apply here. Capt. Moriarty received time-signals from the Royal Observatory through the cable every day, and he had therefore a perfect chronometer. But other ships have no such perfect chronometer; and though the *direction* of a *locus*, as determined above, may be sufficiently certain, yet its *place upon the earth* will be uncertain, by a quantity depending on the uncertainty of the chronometer. Thus three chronometers may give the following positions for the locus-curve:—



And the question now presents itself, which uncertainty is the greater—the uncertainty of latitude, which it is the real object of this problem to remedy? or the uncertainty of the chronometric longitude, which must be used in attempting to find the remedy? I do not doubt the instant reply of any practical navigator, that the chronometric longitude is far more uncertain than the latitude; and if it be so, the whole method falls to the ground.

I fear that a publication like that which has been given to this method may do very great injury among navigators who are not accustomed to investigate the geometrical bearings of such operations, and may lead them into serious danger.

I am, my dear Sir, yours very truly,

G. B. AIRY.

Prof. Stokes, Secretary of the Royal Society.

[From a general recollection of a conversation I had with Sir W. Thomson before the presentation of his paper, I do not imagine his object to have been exactly what the Astronomer-Royal here describes, but partly the saving of trouble in numerical calculation, partly the exhibition, for each separate observation of altitude at a noted chronometer time, of *precisely what that observation gives, neither more nor less*, which introduces at the same time certain facilities for the determination of a ship's place by a combination of two observations. Of course the place so determined is liable to an error east or west corresponding to the unknown error of the chronometer; and doubtless, under ordinary circumstances, this forms the principal error to which the determination of a ship's place is liable. This remains precisely as it did before; and it is hard to suppose that the mere substitution of a graphical for a purely numerical process could lead a navigator to forget that he is dependent upon his chronometer, though perhaps the general tone of Sir W. Thomson's paper might render an explicit warning desirable, such as that which Mr. Airy supplies.—G. G. STOKES.]

NOTES

WE hear with sincere regret of the death of the eminent French meteorologist, M. Charles Sainte-Claire Deville. We hope next week to give some details of his life and work.

WE publish on another page an abstract of the Rev. Mark Pattison's forcible and outspoken address at the Social Science Congress, Liverpool, on the state of our universities. Many other valuable papers were read, but they were for the most part too special for notice in our columns. We should, however, mention the remarks of Mr. W. H. James, M.P., in connection with the discussion of the question of incorporating a professional and technical training with a sound system of general education. Mr. James traced the history of the City Guilds of London, showed how enormously wealthy they must be, how this wealth is totally misspent, and maintained that the country had a perfect right to ask an account of their stewardship, and appropriate the funds, if necessary, for educational purposes. He proposed that