to address such an audience, either with the pen or the voice, and gave as an illustration a great meeting convened by his fellow-citizens to welcome him back to Canada after he had been knighted. He was, of course, expected to say something of himself and of his visit to Europe. He tried his best, he said, but soon grasping a long pointer, turned round to some maps and diagrams illustrative of the geology of Canada, and only recovered his peace of mind and command of language when he found himself once more among Laurentian, Huronian, gneiss, limestone, and the rest of his beloved rocks. Nevertheless, he kept copious journals of his various expeditions, and illustrated them with most admirable pen-and-ink sketches. A selection from these could hardly fail to be of great interest, both in relation to the man himself and to the way in which geology has to be carried on amid the wild life of the backwoods.

By those who were privileged with his friendship, Sir William Logan will be affectionately remembered as a frank, earnest, simple-hearted man, ever gentle and helpful, enthusiastically devoted to his profession, and never happier than when discussing geological questions in a *tête-à-tête*, full of quiet humour, too, and showing by many a playful sally in the midst of his more serious talk, the geniality and brightness of his sunny nature. Peace to his memory! He has done a great work in his time, and has left a name and an example to be cherished among the honoured possessions of geology.

ARCH. GEIKIE

TREVANDRUM MAGNETIC OBSERVATIONS

Observations of Magnetic Declination made at Trevandrum and Agustia Malley in the Observatories of his Highness the Maharajah of Travancore, G.C.S.I., in the Years 1852 *to* 1869. Vol. i. Discussed and edited by John Allan Broun, F.R.S., late Director of the Observatories. (London: Henry S. King and Co.)

WE have heard a great deal lately about the native rulers of India, and the worst features of one of them have been brought very prominently before us; but it is a pleasing reflection that they are not all like the potentate of Baroda, while some of them might even read a lesson to the paramount power. Let us hear what Mr. J. Allan Broun, a magnetician of great eminence, has to say of the late ruler of Travancore.

"The Trevandrum Observatory," he tells us, "owed its origin in 1836 to the enlightened views of his Highness Rama Vurmah, the reigning Rajah of Travancore, and to the encouragement given to them by the late General Stuart Fraser, then representing the British Government at Trevandrum. His Highness, desirous that his country should partake with European nations in scientific investigations, sanctioned the construction of an observatory, named Mr. Caldecott its director, and gave him power to furnish it with the best instruments to be obtained in Europe."

The peculiar position of Trevandrum, not far from the magnetic equator, induced Mr. Caldecott, with the Rajah's permission, to procure from Europe a complete equipment of the best instruments for magnetic and meteorological observations, and to build a magnetic observatory, which was completed in 1841.

Mr. Caldecott died at Trevandrum in 1849, and the observatory was in January 1852 placed under the direction of Mr. John Allan Broun, who had previously directed with well-known success the observatory of Sir T. Brisbane at Makerstoun, in Scotland.

Mr. Broun began his office with the conception of an interesting and important problem in terrestrial magnetism, which he was determined as far as possible to work out. This would render it necessary that the observations should not be limited to a single station. He wished, among other things, to determine how far the physical constants of terrestrial magnetism and their various changes depend on differences of height, of latitude, and of longitude.

The Agustia Malley, the highest mountain in the neighbourhood, was chosen as affording the best means for determining the effect of height, and accordingly Mr. Broun resolved to erect an affiliated observatory on this nearly inaccessible rocky peak, surrounded by forests, the inhabitants of which were elephants and tigers. These and all other difficulties connected with this formidable undertaking were, however, completely vanquished, and the Agustia Observatory was completed in 1855.

We learn from Mr. Broun that his labours were not entirely confined to these two observatories. "Other observations," he tells us, especially of magnetic declination, were made simultaneously "during short periods at different stations in Travancore, as nearly as possible on the magnetic equator, 90 miles north of Trevandrum, and also 40 miles to the south. Observations connected with meteorological questions were also made simultaneously to the east and west, and about 5,000 feet below the Agustia peak, on the peak itself, and at Trevandrum; while on one occasion hourly observations were made during a month at five different stations, varying gradually in height from the Trevandrum Observatory (200 feet) to 6,200 feet above the sea-level, in which fifteen observers were employed."

In this first volume Mr. Broun has confined himself to the magnetic declination, and one of the chief objects sought has been to determine every possible action of the sun and moon upon the magnetic needle. The observations extend from 1852 to 1870, and embrace in all nearly *three hundred and forty thousand readings.*

A considerable portion of the introduction is devoted to the discussion of a question which has, we think, been somewhat too much overlooked. When a magnet is suspended by a thread and enclosed in an appropriate box, it does not necessarily follow that all its movements are due to magnetic causes, for changes in temperature and humidity may affect the zero of torsion of the thread, and thus cause slight changes in the position of the suspended magnet. It is perhaps unlikely that such changes could seriously affect the character of the daily variation, but it has been thought that they might perceptibly affect the annual variation, since in this case the magnetic change is comparatively small, while the range of temperature and humidity is generally great.

Mr. Broun overcame this source of error by observations of an unmagnetic brass bar suspended in the same way as the magnet, which thus afforded him the means of estimating, and hence eliminating, the error due to these causes.

Besides all this, several declinometers were used and

compared together, and the result of all these comparisons tends to impress the reader with the fact that we have in this volume a series of observations of the magnetic declination of a thoroughly accurate and trustworthy nature.

The following passage from Mr. Broun's magnetic diary may be quoted as exhibiting the sources of error to which magneticians are exposed, as well as the care bestowed in avoiding them—

"1855, Dec. 4d. 9h. A sudden vibration of Grubb's magnet through thirty scale divisions was observed, and the difference of Adie's and Grubb's instruments, which had previously been − 0′·05, became suddenly + 3′·50. It was supposed that either the suspension thread was breaking, or that a spider had got within the box.

"Dec. 4d. 22h. The boxes were removed, and an exceedingly small spider was discovered and removed. This was the only occasion in which a spider succeeded in entering Grubb's declinometer boxes between 1852 and 1870. Every care was taken when the boxes were removed, before replacing them, to hold them for some time over the flame of a lamp, so that spiders, even invisible to the naked eye, must have been dislodged or destroyed."

It remains now to give our readers a summary of the most important results obtained by Mr. Broun from the reduction of his observations.

In the first place, the *secular variation* is found to be irregular, but the observations seem to indicate that after a certain interval the acceleration or retardation of the secular movement has equal values. *This interval is estimated at* 10·5 *years.* In order to find the *annual period*, the variations which form the secular and decennial inequalities have been eliminated. The observations then indicate a twofold inequality, one of which corresponds to a single oscillation in a year, with a minimum in March or April, and a maximum in September or October, while the other represents a double or semi-annual oscillation with maxima in March and September.

Mr. Broun was also led to suspect a *period of forty-four months*, which was repeated four times successively in his observations, although no cause is known which could produce an inequality of this duration.

The next inequality noticed is *the twenty-six day period*, which Mr. Broun is inclined to attribute to solar action with more confidence than the longer period of ten or eleven years. Our readers will remember that the period was re-discovered by Dr. Hornstein, director of the Prague Observatory. Mr. Broun thinks that there are traces of a double oscillation of the twenty-six day period.

Coming next to the important *solar diurnal variation*, the chief features of which are tolerably well known, Mr. Broun finds this to consist of one marked maximum and one marked minimum of easterly declination in each month of the year, and of one or more secondary maxima and minima.

The principal maximum occurs in the six months of April to September at about 7 A.M., and the principal minimum about twenty minutes past noon in the same months. Nearly the inverse of this happens in the four months of November to February. The results obtained by Mr. Broun appear to him to indicate the action of opposite forces belonging to the two hemispheres, which mainly destroy each other in March and October at Trevandrum, but one of which is preponderant in the other months of the year; and of these forces he remarks that those of the northern hemisphere seem to have a greater effect on the variations of the whole globe than those of the southern hemisphere.

The daily range was a minimum in 1856 and a maximum in 1860. It is a minimum in March and October, and a maximum in August and December.

In considering the *lunar diurnal variation*, Mr. Broun begins by showing that the results relating to the variation to be obtained by him are really due to the lunar action, and not to any portion of solar perturbation remaining uneliminated.

The following very singular results have been obtained:—

1. The mean lunar diurnal variation consists of a double maximum and minimum of easterly declination in each month of the year.

2. In December and January the maxima occur near the times of the moon's passages of the upper and lower meridians; while in June they happen six hours later, the minima of easterly declinations thus occurring near the times of the two passages of the meridian.

3. The mean of the ranges of the lunar diurnal variation shows (like the solar diurnal range) a minimum in 1856 and a maximum in 1860.

4. The action of the moon on the declination needle is greater in every month of the year during the day than during the night.

5. There appears to be a remarkable change in the lunar action connected with the rising and setting of the sun, especially with the former.

We now come to a part of the reductions where we feel compelled to differ from the eminent magnetician as to what may be termed the scientific policy which he has pursued. We allude to the question of disturbances.

There can, of course, be no doubt that a strictly mathematical discussion of a series of observations will indicate the various periods of action of the influential forces. We know that this method served to indicate many important astronomical periods long before the mechanical nature of the astronomical forces was recognised.

We might, for instance, take a body of meteorological observations and treat them in a strictly mathematical manner, and we should no doubt be led to a yearly and to a daily period, even if we were not acquainted with the existence of the sun. But who would pursue this method? We take advantage of the knowledge derived from other sources of the exact length of these two periods to begin with, and do not think of endeavouring to obtain these by means of the observations themselves.

Furthermore, in meteorology, with the general consent of all engaged in it, we have gone even further than this. There is unquestionably a distinct daily and yearly fluctuation of the meteorological elements brought about by the sun, but besides this there are other phenomena ultimately due to the sun, though not in the same way, which meteorologists have agreed to consider apart by themselves.

We allude to cyclones, which, when examined separately, are found to obey very different laws from those which regulate ordinary atmospheric changes. Thus these laws have been discovered by agreeing to separate certain observations which were unmistakably abnormal,

and to discuss those by themselves, and the result has been the most interesting and important discovery of the law of storms. And if it be asked what right meteorologists had to separate a body of disturbed observations, the reply will obviously be that they are justified by their success. Deny the right, and a cyclone becomes an altogether false and illegitimate scientific conception.

Now, a large and increasing number of magneticians are of opinion that the phenomena of terrestrial magnetism can bear a similar treatment. They believe that the sun has a daily and yearly influence on the magnetism of the earth just as it has upon its meteorology, and they also believe that it is the cause—the indirect cause, it may be—of an abnormal magnetic influence, just as in meteorology it is the indirect cause of the cyclone. Some even go so far as to say that these two abnormal influences, the one in magnetism, the other in meteorology, are intimately connected together. This assertion, however, is not now the point in question. The point is that we have in magnetism certain abnormal disturbances which may be compared to abnormal meteorological disturbances. Now, it is held by Sir E. Sabine and those who share his views, that it is expedient to separate out these disturbed magnetical observations, just as we separate out the meteorology of a cyclone. This school assert that we may thus arrive at a series of phenomena obeying very different laws from those of the undisturbed observations, and that we are therefore justified in making the separation, inasmuch as we are thereby led to a clearer knowledge of the various ways in which the sun affects the magnetism of the earth. And they insist very strongly upon the point that both these magnetic actions of the sun have diurnal and annual variations different from one another, so that if treated together we obtain a result much more complex than if they be treated separately.

We have little doubt of the policy of this method of treatment, and we cannot, therefore, but regard it as a misfortune that Mr. Broun has not unmistakably adopted it. He has, however, given us all the individual observations, so that, if it be thought desirable, those magneticians who advocate a somewhat different method of reduction may make it for themselves. We need only add, in conclusion, that the appendices will be found to be very interesting reading, and that all who are interested in terrestrial physics must look with great interest to that magnificent series of researches of which the volume before us forms the first instalment. B. STEWART

OUR BOOK SHELF

Chapters on Sound, for Beginners. By C. A. Martineau. (London: The Sunday School Association; Manchester: Johnson and Rawson, 1875.)

WE have read this little book with great pleasure. Its object, the author tells us, is to teach a few of the simpler facts in acoustics in such a way that the learner shall not be deterred by unnecessary difficulties, either in the use of technical language or in having to provide expensive apparatus. Most successfully has the author attained the end he had in view. It is just what a child's book on science should be. Written in a simple attractive manner, without any silly childishness, it conveys a great deal of information, and that in the best kind of way. For the learner, by a series of simple experiments, is made to lay firmly the groundwork of his knowledge on this subject. All the apparatus the author requires is a toy fiddle, one or two small tuning-forks, a couple of finger-glasses, a clamp, a square and a round piece of glass, a gimlet, a tall jar, silk thread, and some solitaire balls. With such homely instruments really good elementary teaching is given. The chapter on strings made to vibrate in time with tuning-forks is capitally done, and will give the learner more knowledge than he could gain from many a pretentious text-book. We should like to suggest to the author a few additions to his simple experiments, but in the limits of this notice we cannot do more than direct his attention to the Instructions in practical physics given to the science teachers at South Kensington, and printed for their use by the Science and Art Department. There is of course nothing new in the way of experimental illustration in these chapters on sound; it is the good use the author has made of what has been done by others that is the merit of this little book. We gladly recommend it to all girls and boys who will honestly go through what is to be done as well as what is to be read.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

On the Temperature of the Human Body during Mountain Climbing

THE account of Dr. Forel's laborious and carefully conducted observations on the temperature of the body during mountain climbing, given in NATURE, vol. xii. p. 132, has recalled to mind the results of a few observations which I made shortly after the publication of Dr. Lortet's and Dr. Marcet's experiments. As my results are in the main confirmatory of those of Dr. Forel, they may not be without interest as a contribution to what, until the appearance of Dr. Forel's memoirs, was regarded as the heterodox side of the question.

Before joining the party of observers sent out to Sicily to see the solar eclipse of 1870, I provided myself with a set of delicate clinical thermometers with a view of repeating the observations of Drs. Marcet and Lortet, should any opportunity occur of getting up Etna during our stay in the island. On Christmas-day a number of us attempted to make our way up the mountain, and with the aid of Mr. Fryer I made a number of observations of body-temperature on myself during the ascent. The temperature of the mouth was taken, as in the observations of Marcet and Lortet. The thermometer employed was carefully selected so as to get the maximum amount of displacement in the column for a thermal disturbance with a minimum bulb-capacity. As regards sensitiveness, it left little to be desired. Some weeks before the start a number of preliminary observations were made with the view of ascertaining the best manner of placing the thermometer and of determining the length of time required for the column to attain a position of rest. By repeated trials it was found that fully five minutes were needed after placing the thermometer in position before the level of the mercury became approximately constant, both during repose and after a rapid run. Any subsequent variation seldom exceeded $\frac{1}{20}$ of a degree F. The following readings taken from among a number of similar observations will serve to show the extent of the changes from minute to minute after placing the thermometer *in situ*:—Time, 7·30 P.M.; condition, rest. After first minute: Temp., 96°·4; second, 97°·9; third, 98°·4; fourth, 98°·5; fifth, 98°·5. That there is nothing in the rate of change peculiar to the individual is evident from the results of a similar series made at the same time upon another person: first minute, 96°·4; second, 97°·0; third, 97·5; fourth, 97°·8; fifth, 97°·8.

On the day of the attempted ascent we set out from Catania at 5·30 A.M., and drove to Zaffarana. Mouth-temperature before starting, 98°·4. In the carriage, 98°·3; time, 9h. 10m.; pulse, 78. At Zaffarana, 98°·4; pulse, 83. As Zaffarana lies at a considerable elevation above the sea-level, the observations so far serve to confirm Dr. Marcet's statement that the rarefaction of the air is without influence on the temperature of the body. After a stiff walk of thirty-five minutes, during which the