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