developments of the century. Strangely enough, there is in the whole work no mention of that most widely-spread of all electric inventions, the domestic electric bell, nor of its almost forgotten inventor, John Mirand. Prof. Mendenhall has added to the interest of his sketch by supplying a number of illustrative cuts of objects of historic interest, such as Faraday's first magneto-electric machine, and his first transformer or induction-coil. We should have welcomed some account of the great theorists, Coulomb, Laplace, and Weber, who, with Sir William Thomson and Clerk Maxwell, have, by their calculations and mathematical developments, played so leading a part in the progress of the century; but the author would probably have found it impracticable with the plan of his sketch to deal with the labours of these intellectual giants. In his less ambitious aim of popularizing the experimental development of the subject he has succeeded admirably.

OUR BOOK SHELF.

The Fungus Hunter's Guide and Field Memorandum Book, with Analytical Keys to the Orders and Genera, illustrated, and Notes of Important Species. By W. Delisle Hay, F.R.G.S. (London: Swan Sonnenschein, Lowrey, and Co., 1887.)

A FIELD guide and mentor is a welcome companion for the practical botanist, provided it is so compiled as to meet all the requirements of field work, otherwise it is merely "a delusion and a snare." This little volume, unfortunately, belongs to the "otherwise," for it is insufficient, antiquated, and misleading: insufficient, because it includes only a few species under each genus or sub-genus, and these have been selected without manifest reason; antiquated, because, although dated 1887, it is based upon the state of this branch of science in 1871, and might have been published at that date, for all internal evidence to the contrary; and misleading, because the errors of 1871 are not corrected, the illustrative figures are entirely without names of the species intended to be represented, and more important or essential species are excluded than many of those included in the lists.

Under each genus or sub-genus in the volume a list is given of "common or notable species,"—each with its scientific name (but without the authority for the specific name, which any botanist would regard as essential); an imaginary popular name, which is useless because imaginary and not real; a short description, rarely sufficient; and letters indicating esculent or poisonous qualities. As only one or two species are given under a genus or sub-genus which has a dozen or more other British representatives, it should have been stated distinctly that there are so many more species which are not named, any of which the collector might meet with in his rambles. Unfortunately the selection of the species favoured with a place has been made with very little judgment. Some are included which are so rare that they have only been found once or twice in this country, whilst others are excluded which are almost sure to be met with in any moderately successful ramble. The fact is patent that the "Hand-book" issued sixteen years ago is accepted as the authorized record for to-day, whereas it is absolutely out of date, and all the great advances made during the intervening period are studiously ignored. The volume is interleaved with ruled paper for notes and memoranda, and we venture to affirm that this is the only useful and unexceptionable portion of the work. The purchaser must judge whether it would not have been more economical to secure a blank memorandum book, since the numerous figures are valueless without names,

and the analytical keys ought to have been more accurate and better constructed. M. C. C.

My Hundred Swiss Flowers: with a Short Account of Swiss Ferns. By Mary A. Pratten. (London: W. H. Allen and Co., 1887.)

THIS is a very unpretending book, and should be of considerable service to beginners in botany who may wish to carry on botanical studies among the Alps during the month of July or early in August. The writer has selected those Swiss flowers which seem to her "most remarkable, most characteristic of the country, or most commonly seen," and she is, of course, right in thinking that a great many of them will be new to such as make a first visit to the Alps. Her descriptions are clear and sufficiently full, and the illustrations are very good.

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.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Carnatic Rainfall.

MR. H. BLANFORD'S authority is so deservedly high, that I have had some hesitation in writing to controvert the conclusions he has adopted in the paper published in NATURE of July 7 (p. 227), entitled "The Eleven-Year Periodical Fluctuation of the Carnatic Rainfall"; and to state my reasons for thinking that there is no real validity in the arguments he uses in favour of "the very high probability that the apparent undecennial fluctuation is no chance phenomenon."

Mr. Blanford brings forward a series of figures which show the mean annual variation of the rainfall during twenty-two years, at a number of stations in that part of Southern India locally known as the Carnatic, from the mean annual rainfall for the Carnatic generally. From these figures he has inferred the appearance of two complete cycles of eleven years, with a dominant periodical fluctuation.

To test the character of this apparent periodicity he obtains from these figures the two first terms of an harmonic expression that shall represent the observed facts for an assumed eleven-year period; and he finds the mean difference between the observed values and those calculated from the adopted harmonic expression to be ± 3.5 inches, from which the mean probable error of any of the calculated periodical values is found to be ± 0.70 inch.

Now it is apparent that such a series of calculated values has no physical signification whatever. The greater or less degree of difference between the observed and calculated quantities only indicates how far the sums of the terms of the harmonic series employed coincide with the series of observed quantities which the calculated series was designed to represent. It is also obvious that by a sufficiently extended series of terms the calculated quantities might be brought to agree, within any desired degree of approximation, with those observed. No conclusion whatever, therefore, can be based on the amount of the differences above alluded to, so far as any question of periodicity is concerned, and the so-called "probable error" is merely an arithmetical result of the particular form of calculation adopted.

Mr. Blanford goes on to remark that the mean difference between the observed series of values of the annual variation of rainfall and the mean of the whole of them, is ± 5.2 inches, with a probable error of the general average of ± 0.94 inch.

And here again I am unable to see that any weight can be attached to these figures in connexion with the main point at issue. The mean variation of the series of observed values, from the mean of all of them, will of course be greater than the mean variation of those observed values from a series deliberately calculated so as to correspond with them, such as that obtained by aid of the harmonic series. The introduction of the expression "probable error" of the general average is also

likely to be misleading. This too only represents an arithmetical result, and signifies that as in the series of twenty-two observations there is an average departure of ± 5.2 inches from the mean of all the measurements, the probability is that this mean will be within ± 0.94 inch of the truth, so far as those measurements are to be trusted.

For these reasons I am quite unable to follow the arguments by which it is sought to connect the amounts of these two "probable errors," or to see how they can in any way indicate "the relative probability of this particular variation being the result of a periodic law, and of its being a mere fortuitous series of variations from a constant average."

Neither does there appear to be any justification for assuming that the relative probability of the truth of two hypotheses is represented by the inverse ratios of the probable errors of results derived from them. Still less is there any ground for saying that because the particular series of quantities under discussion relates to a period of twenty-two years, the relative probability just alluded to is thereby increased to the twenty-second power of that ratio, or from about $1\frac{1}{2}$ to 1, to 655 to 1. It is no doubt true that if the probability of an event occurring once be represented by the fraction $\frac{1}{x}$, the probability of its recurring n times in succession

will be represented by $\left(\frac{1}{x}\right)^n$; but I fail to see how this affects the question at issue.

RICHARD STRACHEY.

July 11, 1887.

Is Cold the Cause of Anticyclones?

IN a review of Loomis's papers in this volume of NATURE, p. 2, occur the following sentences:—"While all, or nearly all, of the high pressure of anticyclones may be accounted for by the very low temperatures which overspread the same region at the same time along with the resulting upper currents concentrating upon them from adjoining cyclonic regions, it is quite different with the low pressures of cyclones. In the case of cyclones the problem is complicated by the strong winds, the copious precipitation, and the ascending currents, which affect the results in ways which no physicist has yet been able to explain."

This induction of Loomis's, that anticyclones are largely the result of cold, which the reviewer here repeats, is in entire opposition to the deductive views of Ferrel, and I think the discrepancy is to be found in the method used by Loomis in drawing his inductions. In order to investigate the cause of anticyclones, Loomis selected only decided areas of high pressure, and as a consequence his data were almost entirely confined to the winter months, when the temperature accompanying anticyclones is always low. If, however, he had selected more moderate anticyclones, he would have found that in summer anticyclones in the United States are sometimes accompanied by intense heat (90° F. or more). This is especially so in periods of drought. Under these conditions the approach of a cyclone with rain brings a most refreshing cooling. Furthermore, Hahn, attacking the problem by a different method, has obtained results apparently directly opposed to this induction of Loomis. Hahn made a careful study and comparison of the observations obtained last autumn and winter on the Sonnblick and at adjacent mountain and valley stations (see *Meteorologische Zeitschrift*, February and April 1887). One of the most marked results found was that at heights exceeding 1000 metres above sea-level there was an increase of temperature during anticyclones, while a decreased temperature was only found in valleys and near the general level of the earth's surface. Hahn's average results show that the temperature on the Sonnblick, height 3090 metres, rose from an average of $-16^{\circ}4$ C. at the average barometric pressure of 509.1 mm. to $-7^{\circ}7$ C. at the barometric pressure of 529.3 mm.; while at the same time the average temperature at Schafberg, height 1776 metres, rose from $-9^{\circ}0$ C. to $+1^{\circ}4$ C.; but on the contrary at Zell-a-See, height 754 metres, the average temperature fell from $-5^{\circ}9$ C. to $-8^{\circ}9$ C. These results, which show that the larger portion of the atmosphere is warmed instead of cooled within the area of an anticyclone, seems entirely destructive of Loomis's hypothesis that the cooling of the air near the earth's surface is the chief cause of the anticyclone. Hahn's results, however, indicate that the cooling of the air near the earth's surface does increase the pressure somewhat. Thus in October 1886 the barometric minimum occurred at all the stations, both mountain

and valley, on the 17th; while the barometric maximum occurred at all of the stations on the 30th. When the difference in pressure between the mountain stations at the time of barometric minimum was compared with the difference in pressure between the same stations at the time of barometric maximum, it was found almost exactly the same; but the difference in pressure between the valley stations and the mountain stations was about 5 mm. greater at the time of maximum pressure than at the time of minimum pressure. Hahn refers this greater range of pressure at the valley stations to the decreased temperature at valley stations during anticyclones, but this still leaves a range of pressure of nearly 20 mm., which the decreased temperature entirely fails to explain. These results of Hahn are in entire accord with the results obtained at Blue Hill Observatory (640 feet above sea), as compared with Mount Washington and with stations near sea-level, which indicate that the cooling in anticyclones is almost entirely confined to within a few hundred feet of the earth's surface (see *Science*, vol. viii. pp. 233 and 281).

In the light of these facts, it seems more reasonable to assume that the warmth found on mountains and the cold in valleys, accompanying anticyclones, are the result, rather than the cause, of anticyclones. Such researches as those of Langley on the solar heat, and of Hahn on the distribution of temperature pressure, &c., in different planes of the atmosphere, indicate that the effect of the sun's heat on the atmosphere is far more complex than some of our text-books on meteorology would have us believe; and instead of the cause of anticyclones being as simple as the reviewer of Loomis's article states, it seems probable that we shall understand the phenomenon of the anticyclone only when we master the problem of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Boston, Mass., U.S., June 28.

Physiological Selection.

I AM perhaps in a position to contribute something practical to the discussion upon Mr. G. J. Romanes's proposal of physiological selection as an improvement upon Darwin's natural selection.

I failed to meet with Mr. Romanes's paper in the Journal of the Linnean Society, and I confess that I did not gain a very clear idea of what he meant by physiological selection, until I read his article in the *Nineteenth Century* for January. His main difficulty appears to be the intercrossing with parent stock, which he thinks would prevent the survival of any varieties naturally selected to become species. Now, my Australian bush experience of the habits of animals and birds satisfies me that this difficulty is mainly, if not entirely, imaginary, and that Nature amply provides against the supposed intercrossing. Any person who has observed the habits of semi-domesticated stock, such as horses and cattle, can scarcely fail to know that migration is a general practice of one sex, and a frequent one of the other. The old always hunt the young males and strangers entirely away to form herds and families of their own, and thus the supposed intercrossing is by one sex at least effectually obviated. But it is also frequently the case that young fillies and heifers, at the same season, take to wandering, for less evident reasons, far from their accustomed haunts, scores of miles, after which they will stop, and attach themselves to another herd and locality as tenaciously as their parents remain in theirs. This of course further tends to prevent intercrossing with parent stocks.

I cannot but think that Mr. Romanes's anxiety to find a solution of his difficulty has led him into serious mistakes, which vitiate his treatment of the subject. For instance, he says (page 59, *Nineteenth Century* for January), "The hypothesis of physiological selection sets out with an attempted proof of the inadequacy of natural selection, considered as a theory of the origin of species." I was out walking yesterday when I read this, and I wrote in the margin, "The theory of natural selection is one, not of the origin of species at all, but of the preservation of particular varieties." On reaching home, I referred to the "Origin of Species" (4th edition, 1866), and was certainly pleased to find that I had adopted Darwin's precise words repeated in several places (see pages 71, 91, 123, &c.). At page 91 he says:—"Some writers have misapprehended or objected to the term natural selection. Some have imagined that natural selection induces variability; whereas it implies only the preservation of such varieties as occur, and are beneficial under