

calculation. The future must show how far it will be possible to apply to the theory of species the definition of central specific forms, from which varieties calculably diminish in numbers as they depart in type.

E. B. TYLOR

OUR BOOK SHELF

Magnetism. By Sir W. Snow Harris and H. M. Noad. (London: Lockwood and Co.)

THIS is a good book, and we are glad to see the subject of magnetism fully treated in a popularly written text-book. It is a second edition of Sir William Snow Harris's rudimentary treatise, with considerable and important additions by the editor. The part of chief importance which is added is Chapter viii., which deals with the more recent progress of terrestrial magnetism. This chapter consists of thirty pages, and the author has managed to condense into that space a wonderfully large amount of interesting, useful, and accurate information on the subject. In so short a space we must be content with results rather than with particulars, but the matter contained in this chapter, in point of importance, accuracy, and exhaustiveness, places the present treatise, as far as terrestrial magnetism is concerned, much before any similar book with which we are acquainted. The correction of the compass in iron ships is entered into in the last chapter. The telegraph is scarcely touched upon, but this perhaps rather belongs to a treatise on electricity. We have a chapter on theories of terrestrial magnetism. The theory of Gauss should never be classed, as it is here, and indeed as it is generally classed, along with theories like those of Halley or Hanstein, or with such things as electro-magnetic theories and the like. The word "theory" in these cases means quite a different thing from what it means when applied to Gauss's investigations. Hanstein and the like all make some physical hypothesis, which may or may not be the case; but Gauss makes no such assumption at all, except in so far as he supposes that the needle at all parts of the earth's surface is affected by forces due to the same origin, and varying inversely as the square of the distance, which has been experimentally proved to be the law according to which magnetic forces act. He then shows how the effect on a needle can be expressed in terms of an infinite series which is necessarily mathematically convergent and true, and he then uses an approximation to that series, which approximation is justified fully by experiments similar to those made by the late Prof. Forbes at the top and bottom of the Faulhorn. Gauss's theory, then, is a truly scientific theory, inasmuch as it involves no unjustified physical hypothesis, but is a logical deduction from observed facts and established principles, and in this differs radically from the other theories which are too often classed with it. Dr. Noad has been so successful in Chapter viii. that we cannot help wishing he had introduced a chapter also on this subject.

JAMES STUART

The Amateur's Flower-Garden: a Handy Guide to the Formation and Management of the Flower Garden and the Cultivation of Garden Flowers. By Shirley Hibberd. Illustrated with coloured plates and wood engravings. (London: Groombridge and Son, 1871.)

MR. HIBBERD is a practised writer on gardening subjects, though his books have not much claim to be considered as scientific treatises, but rather as pretty gift-books to lie on the drawing-room table and give to its furniture a quasi-scientific air. That they have their use cannot be doubted, but it is not a very high one. The worst part of this book is the illustrations. From the letter-press may be doubtless culled some useful hints as to the planting and management of a flower-garden,

though we do not think it equal in this respect to some other works, such as those by Mr. Robinson, which are less under the trammels of time-honoured prejudices and superstitions. But many of the illustrations, including some of the woodcuts and nearly all the coloured plates, are simply atrocious. The drawings of a show pelargonium (p. 80), pansy (p. 45), ranunculus (p. 156), carnation (p. 117), and some others, are mere caricatures, and unworthy of a place in any work which bears the least pretensions to a scientific character.

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 Survival of the Fittest

I HAD designed sending a note to you, critical of the abstract of my paper on "The Laws of Organic Development," republished from the *American Naturalist* in one of your recent issues, before I read the remarks of Mr. Spencer in your number of February 1.

If Mr. Spencer will examine the Essay itself (for sale by McCalla and Stavely, 237, Dock Street, Phila., or Naturalists' Book Agency, Salem, Mass. *) he will find that I have there exclusively employed his phrase "Survival of the Fittest." The expression "Preservation of the Fittest," not used by Mr. Spencer, was inadvertently introduced in writing the abstract. This was done hurriedly between the sittings of the Amer. Assoc. Adv. Sci. for a reporter of the *New York Tribune*, and was subsequently printed by the *Naturalist* while I was absent on the Plains of Kansas. It therefore contains several obscurities, the result of an attempt to abridge, and a number of typographical blunders. The essay will be found to be free from these.

There being no misrepresentation of Mr. Spencer's views on this point, I notice the second objection he makes. Where, in the sentence regarding the Survival of the Fittest, I say that "this neat expression no doubt covers the case, but it leaves the origin of the fittest entirely untouched," Mr. Spencer regards my language as an "indirect statement that I" (Mr. S.) "have done nothing to explain the origin of the fittest."

It is plain enough that my remark does not apply to Mr. Spencer or to his writings, but exclusively to the doctrine of Natural Selection, and to Mr. Spencer's terse phrase, "which no doubt covers the case," *i.e.* Natural Selection (not the whole theory of Evolution). I cannot see that this language can be tortured into the interpretation Mr. Spencer places upon it, but Mr. Spencer's language decidedly implies that my statement is literally correct.

I am, however, well aware that Mr. Spencer has done more than any living man to explain the "Origin of the Fittest," and on this account in particular his name does not appear in my criticism. Another reason for its omission is that I have taken the liberty not to read his work, "The Principles of Biology," because I have suspected, from my reading of other works of this philosopher, that it is in advance of other treatises on the subject. I postponed it until, by investigation "in the shop," I should have attained to some definite views based on reasoning uninfluenced by the opinions of others, hoping to use "The Principles of Biology" thereafter in such a way as its merits and justice to its author should require.

EDWARD D. COPE

Philadelphia, Feb. 20

Ethnology and Spiritualism

THERE is only one point in Mr. Tylor's communication (*NATURE*, Feb. 29, p. 343) on which it seems desirable that I should say a few words, in order that I may not be supposed to assent to what I conceive to be a most erroneous view. Mr. Tylor suggests that the phenomena that occur in the presence of what are called mediums, are or may be of the same nature as the subjective impressions of persons under the influence of a powerful mesmeriser. Five and twenty years ago I was myself

*Under the title, "The Method of Creation of Organic Types."

a practised mesmeriser, and was able to produce on my own patients almost the whole range of phenomena which are exhibited in public as illustrative of "mesmerism" or "electro-biology." I carried on numerous experiments in private, and paid especial attention to the conditions under which the phenomena occur. During the last seven years I have had repeated opportunities of examining the phenomena that occur in the presence of so-called "mediums," often under such favourable conditions as to render trick or imposture simply impossible. I believe, therefore, I may lay claim to some qualifications for comparing the mesmeric with the mediumistic phenomena with especial reference to Mr. Tylor's suggestion, and I find that there are two great characteristics that broadly distinguish the one from the other.

1. The mesmerised patient never has doubts of the reality of what he sees or hears. He is like a dreamer to whom the most incongruous circumstances suggest no idea of incongruity, and he never inquires if what he thinks he perceives harmonises with his actual surroundings. He has, moreover, lost his memory of what and where he was a few moments before, and can give no account, for instance, of how he has managed to get out of a lecture-room in London to which he came as a spectator half an hour before, on to an Atlantic steamer in a hurricane, or into the recesses of a tropical forest.

The assistants at the *séances* of Mr. Home or Mrs. Guppy are not in this state, as I can personally testify, and as the almost invariable suspicion with which the phenomena are at first regarded clearly demonstrates. They do not lose memory of the immediately preceding events; they criticise, they examine, they take notes, they suggest tests—none of which the mesmerised patient ever does.

2. The mesmeriser has the power of acting on "certain sensitive individuals" (not on "assemblies" of people, as Mr. Tylor suggests), and all experience shows that those who are thus sensitive to any one operator are but a small proportion of the population, and these almost always require previous manipulation with passive submission to the operator. The number who can be acted upon without such previous manipulation is very small, probably much less than one per cent. But there is no such limitation to the number of persons who simultaneously see the mediumistic phenomena. The visitors to Mr. Home or Mrs. Guppy all see whatever occurs of a physical nature, as the records of hundreds of sittings demonstrate.

The two classes of phenomena, therefore, differ fundamentally; and it is a most convincing proof of Mr. Tylor's very slender acquaintance with either of them, that he should even suggest their identity. The real connection between them is quite in an opposite direction. It is the mediums, not the assistants, who are "sensitives." They are almost always subject to the mesmeric influence, and they often exhibit all the characteristic phenomena of coma, trance, rigidity, and abnormal sense-power. Conversely, the most sensitive mesmeric patients are almost invariably mediums. The idea that it is necessary for me to inform "spiritualists" that I believe in the power of mesmerisers to make their patient believe what they please, and that this "information" might "bring about investigations leading to valuable results," is really amusing, considering that such investigations took place twenty years ago, and led to this important result—that almost all the most experienced mesmerists (Prof. Gregory, Dr. Elliotson, Dr. Reichenbach, and many others) became spiritualists! If Mr. Tylor's suggestion had any value, these are the very men who ought to have demonstrated the subjective nature of mediumistic phenomena; but, on the contrary, as soon as they had the opportunity of personally investigating them, they all of them saw and admitted their objective reality.

ALFRED R. WALLACE

Development of Barometric Depressions

IF I have misrepresented Mr. Ley's views, the misrepresentation was certainly unintentional; but after fairly considering his letter in NATURE of February 29, I am unable to see that I have misrepresented his views, so far as they are exposed in his "Laws of the Winds prevailing in Western Europe." Part II., of course, I ignored. It is not yet published; for aught I know, is not yet written; and as I have not the pleasure of a personal acquaintance with Mr. Ley, it is difficult to understand how I could be expected to express any opinion on a book which is still in the womb of the future. But as to the present work,

Part I., which I read and reviewed, it is mainly occupied with instances, ingeniously worked out, in illustration of the rule which he distinctly enunciates, that revolving storms are due to the depression of the barometer caused by a heavy rain over a large area. Perhaps, in the same way, Part II. is to be mainly occupied by an examination and discussion of the still more numerous instances in which revolving storms have not followed heavy rain over a large area; and if so, I shall be glad in due time to give it my best attention. But for the present, having before me merely the author's existing work, I repeat what I have, in effect, already said, that the occasional or even frequent sequence of rain and storm does not establish between the two a relationship of cause and effect.

A very casual examination of our own registers, and those of Western Europe generally, would show that instances of rainfall quite as great as any that Mr. Ley adduces, happen very frequently without any storm following; and clearly if Mr. Ley's rule is sound, it must apply to all instances which cannot be claimed as exceptions, and that not only in our own latitudes, but in other parts of the world, and especially in those parts where the rainfall is excessive. It was certainly not "necessary" to travel to Khasia for instances of the failure of this rule; but its failure was exhibited in the most full and clear manner by a reference to that extraordinary rainfall.

Mr. Ley speaks of some "fact" relative to the Himalayas which "may be denied." I do not quite understand what fact he means. The facts I have spoken of are the "heavy and long-continued precipitation," and a very great depression of the barometer." If it is either of these that he wishes to deny, I can only say that his doing so confirms my former statement that he has confined his investigations too exclusively to Western Europe. But when I spoke of the one as causing the other, it was not as stating a fact, but as suggesting a probability; whilst whether there is or is not "a region in which Ballot's rules are contravened" I am unable to say; if there is I have not discovered it, and I don't know where it is, but it is not near the Himalayas, where, so far as we know, the circuit of the wind is quite in accordance with Buys Ballot's Law, though on a scale of extreme magnitude—of such magnitude indeed that our observations do not extend to the end of it. It is curious that an author who, like Mr. Ley, writes sensibly within his professed boundaries, should have limited his studies so closely as he appears to have done; but as the remark to which I have just referred shows pretty conclusively that he has not examined into the range of the barometer in India, so the remark which he makes about the advance of cyclones "in the West Indies, *e.g.*," shows that he is strangely in the dark as to the variations of temperature in the tropical Atlantic.

The columns of NATURE are not the place to discuss at length such well-worn subjects as either Buys Ballot's law or the influence of the earth's rotation, and certainly whether the earth's rotation does or does not produce the effect attributed to it, was quite beyond the scope of my former allusion to it; but I said and repeat that its influence is not "obvious," that an argument based on it is not a "truism," and that to apply these words to a point that is at any rate doubtful is both objectionable and improper.

J. K. L.

Solar Intensity

I HAVE read with interest the criticism in your last number of Padre Secchi's Solar Intensity Apparatus. With reference to the single point of the discordant results obtained by thermometers with bulbs of different size, I would observe that I encountered a similar difficulty some years ago in investigating the adaptability of the instrument invented by Herschel, commonly called the "black bulb *in vacuo*," to regular comparable meteorological observations. I found that the large bulbs always gave a higher reading than the small bulbs. I supposed this to proceed from the colder stem depriving the blackened bulb of its heat, the larger bulb, of course, losing less than the smaller, and I overcame the difficulty entirely by having about an inch of the stem as well as the bulb coated with lamp-black. I am not sure, however, that this would answer so well in a non-exhausted chamber. Probably a small bulb will always be cooled by convection more rapidly than a large one.

In the excess of the temperature indicated by the improved instruments I have referred to over the temperature of the air, at the same height—usually 4ft.—above the soil (which is also very