OUR BOOK SHELF

La Terre des Merveilles. Par Jules Leclercq. (Paris: Librairie Hachette et Cie., 1886.)

IN this volume M. Jules Leclercq describes a visit made by him a few years ago to the Yellowstone National Park, during which he saw all the sights of this *terre des merveilles*. The writer is already well known in his own country as an accomplished writer of popular books of travel, and accordingly he makes the most of the Yellowstone region and its wonders. His sketches of these are preceded by a very interesting chapter on the early explorations of the territory, from the visits of the first adventurous trappers. There are two maps—one a detailed map of the "Park," the other a general map of part of the United States to show the position of the Yellowstone region. There is also a considerable number of illustrations. The volume is published in Hachette's "Collection des Voyages illustrés," and is a clever, well-written popular account of a district full of natural wonders.

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 Cereals of Prehistoric Times

MY friend Mr. Carruthers in the interesting address delivered recently to Section D of the British Association, makes a remark which I confess surprises me. He says (NATURE, September 9, p. 453):—"It is remarkable that in our own country, with all the appliances of scientific cultivation and scientific farming, we have not been able to appreciably surpass the grains which were harvested by our rude ancestors of 2000 years ago." He mentions in support of this conclusion that "the wheat from lake-dwellings in Switzerland for which I am indebted to Mr. J. T. Lee, F.G.S., are fair samples."

This is certainly a striking fact. The persistence of specific and even of varietal types in a country like Egypt is what we might expect, because the very preservation of the material evidence is a proof that the physical conditions have persisted likewise. But that cultivated plants have remained unaltered since the Stone Age seems a conclusion difficult to accept in the face of every-day experience as to what can be done in modifying them. The data collected by Prof. A. De Candolle ("L'Origine des Plantes cultivées," pp. 284, 285) leads me, I think, to the conclusion that Mr. Lee's specimen must be exceptional. Prof. De Candolle mentions three varieties of wheat as cultivated in the Stone Age; of these he says:—"Aucune n'est identique avec les blés cultivés de nos jours. On leur a substitué des formes plus avantageuses." Two of these have been obtained from lacustrine dwellings. He remarks that the most ancient lacustrine people of Western Switzerland cultivated a wheat with small grains, which Heer has carefully described and figured under the name of *Triticum vulgare antiquorum*. These people he regards as contemporaries of the Trojan war, if not older. The culture of this kind of wheat persisted in Switzerland till the Roman conquest. Unger found the same form in a brick of the pyramid of Dahschûr in Egypt of the date B.C. 3359. The other variety (*Triticum vulgare compositum muticum*, Heer) was less common in Switzerland in the first age of stone, but is the one most frequently found in the less ancient lake-dwellings of Western Switzerland and Italy.

W. T. THISELTON DYER

Physiological Selection and the Origin of Species

HAVING written for the *Fortnightly Review* a full reply to Mr. Wallace's article in that journal, I will not here anticipate what I have there to say. But, seeing that he has repeated in these pages the substance of his criticism, I will here also repeat the substance of my reply. On the present occasion, therefore, it is enough to remark that I have never made the "extraordinary statement that, during his whole life, Mr. Darwin was mistaken in supposing his theory to be a theory of the origin of species." On the contrary, as I shall hereafter show, so far as this matter is concerned, both my opinions and my statement of them are in full agreement with those presented in Mr. Darwin's works.

Without wishing to discuss with Mr. Francis Darwin the meaning of the sentence which he quoted from the "Origin of Species," I feel it is only due to my own understanding to give the following explanation. If any one will turn to the sentence in question (p. 247, 6th ed.), he will find that it constitutes an integral part of an argument showing that sterility between species cannot have been brought about by natural selection. The argument is that, *even supposing sterility with parent forms to be an advantage*, it is an advantage which could not be seized upon by natural selection, and hence that some other explanation of such sterility must be found. Now, so far as I can see, there is here not only no shadow of the theory of physiological selection, but the whole argument is proceeding on totally different lines. For the very essence of this theory is that the sterility in question *need not be supposed to be an advantage*, and therefore that any variation in the way of such sterility *does not require* to be selected through the struggle for existence, being of its own nature a variation which survives. In no part of Mr. Darwin's writings can I find even the most distant allusion to the possibility of this particular variation being thus a variation *sui generis*—itself a cause of specific differentiation, and, as such, independent of natural selection. Least of all can I find evidence of any such allusion in the pasage referred to, seeing that the argument here consists in expressly regarding the variation of sterility as resembling variations in general, and therefore in *not* regarding it as possibly presenting the highly peculiar quality of being survivable *per se*. And, considering how fully Mr. Darwin has given his reasons for rejecting many ideas much less feasible, I confess it appears to me a most extraordinary and unaccountable thing that he should nowhere have so much as mentioned this alternative, had it ever been familiar to his mind. I may add that, if any reasonable ground can be shown for supposing this to have been the case, it would cause me to abandon the whole research.

Mr. J. H. A. Jenner's remark cannot apply to the particular kind of variation with which alone my theory is concerned, because, if so, it would amount to saying that the more sterile the variety is with its parent form the more will this sterility be increased by intercrossing with that form, which is absurd. But with regard to many other kinds of *beneficial* variation the remark of course is true.

I am greatly obliged to Mr. Evershed for directing my attention to Mr. Catchpool's letter in NATURE (vol. xxxi. p. 4). Having obtained a copy of the issue referred to, I find, as he says, that "the theory of physiological selection is very clearly put forward." Moreover, the difficulties against the theory of natural selection on account of inutility and sterility are very clearly stated. I may take this opportunity of requesting any of your readers who may know of any previous publications of the theory—no matter how vague or sketchy—to be kind enough to furnish references.

GEORGE J. ROMANES

Geanies, Ross-shire, September 18

Cooke's "Chemical Physics"

IN your issue of September 2 (p. 405) I find under the cover of a review of Cooke's "Chemical Physics" that Prof. Armstrong has been good enough to quote a passage from my "Lessons in Elementary Chemistry," though without naming the source, concerning Avogadro's law, about which he asks the question, "Could anything be more misleading and inaccurate?" My friend appears to be no exception to the well-known rule as to critics failing to read the books they review, for a note on the same page (55) disposes of the "inaccuracy," whilst the "misleading" statement is explained further on (p. 154). On the other hand, Dr. Armstrong has not followed the usual practice of critics, who, not being authors, escape from the danger of a retort courteous from those whom they find fault with; and hence I feel sure he will forgive me in saying that, whilst fully agreeing with him in the statement that a knowledge of mathematics is advisable for a chemist if he is to understand physics and physical