physiology himself as far as he goes. Occasionally he gives a useful illustration or a detailed explanation which is not to be found in the Elementary Lessons, and there are not many bad blunders. The account of a cell at p. 14 is obsolete, though too often found in the minds of compilers of manuals and of examiners. Arteries are not lined with mucous membrane. The account of long and short sight is inexact. The *corpora quadrigemina* can be seen without removing the cerebellum, and do not consist of the olfactory and optic lobes. On the other hand the "*hold* of nervous system on the arteries" is a very happy expression, and the plasma of the blood exuding through the capillaries is well compared to "a stream lost in the sand." The experimental illustrations at the end of the chapters are good, and it would have been well if this part of the plan had been more fully carried out, together with some practical hints as to dissection and microscopic observation. Unless these practical studies are undertaken, the study of physiology is a mere cramming of statements, and is quite unworthy of a place in any scheme of education. If it is to be generally taught, the most important thing is to show teachers how they must set about it, and for this purpose directions can scarcely be too minute.

The questions in the appendix are excellent, though it was a pity to give only one specimen of an examination paper. They of course presuppose dissection of a sheep's head and viscera, and acquaintance with some simple physiological experiments. The woodcuts are very rough, but most of them answer their purpose.

On the whole it is not likely that any shorter or simpler manual than Huxley's "Lessons" can be written, that will be of use for the serious study of the elements of physiology by those who do not intend to go further. It would cost much more time and trouble to go through it than through the "popular" substitutes of which this is an example, but, for that very reason among others, the result would be far more valuable. P. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Our National Herbarium

IT is with as much pain as surprise that I notice in your impression of last Thursday a most unfair as well as ungenerous attack upon the botanical establishment at Kew under Dr. Hooker. It is not within my province to discuss the inaccuracies upon which the insinuations of bad cultivation are founded, nor yet the extraordinary statement that the herbarium which the constant experience of a long life has proved to me to be an indispensable adjunct for the efficient working of a national botanical garden—that this "collection of dead plants," as your correspondent contemptuously terms it—has interfered with the proper care of the garden. There is, however, one of the facts mentioned which my long and intimate acquaintance with the herbarium of the British Museum and its successive keepers, Mr. Brown and Mr. Bennett, calls upon me to deny.

Foremost amongst the "examples of scientific work at the London Herbarium" is given the "Prodromus Floræ Nova Hollandiæ." That great work, which at once placed Robert Brown at the head of the botanists of the age, was published in 1810, many years before the so-called "London Herbarium" was in existence.

I would add that the second of the works named as an example of scientific work at the London Herbarium was published in 1818, two years before the death of Sir Joseph Banks, the subsequent transference of whose herbarium formed the nucleus of the "London" or "Metropolitan Herbarium."

Nov. 11 GEORGE BENTHAM

PERMIT me to correct some errors of detail into which Dr. Hooker has fallen in his reply to Prof. Owen, printed in a recent number of NATURE.

1. Prof. Owen has no official relation to the Botanical Department, and, consequently, is not acquainted with the particular arrangements between the Trustees and the officers of that department.

2. This department is open in summer from 9 to 6, and in winter from 9 to 4.

3. The officers are required to be in attendance for six hours daily, but as this does not include an hour for dinner, the official hours are the same as at Kew.

WM. CARRUTHERS,
Keeper of the Botanical Department

British Museum, Nov. 4

The Beginnings of Life

ON reading a review of my recently-published work, "The Beginnings of Life," in the last number of the *Academy*, written by Mr. H. N. Moseley, I could not help feeling considerable surprise at many of the statements which it contained. That such apparent ignorance of the facts should have been shown, and that such an inadequate statement of the case should have been made by a distinguished pupil of Prof. Rolleston, I was not prepared to expect. My first resolution was to pay as little attention to the statements of the reviewer as they seemed to deserve. It has, however, been strongly represented to me by friends whose opinion I value that some of the statements ought not to be allowed to pass without comment or contradiction.

Referring for a moment to the reviewer's opinion that known facts seem to warrant the notion that organic matter can only be formed "by a series of gradations brought about by a succession of complex conditions" (the process referred to in my work at vol. i. p. 94), I may remark that many facts bearing against this being the only possible mode of formation of organic matter are stated in vol. ii. pp. 27-33, and 36. Protoplasm (existing as *Bacteria*) is capable of growing indefinitely in a solution of ammonic tartrate; and, to say the least, we at present know nothing concerning the existence of any long series of intermediate conditions between the ingredients of the saline solution and the protoplasm which rapidly grows therein. As I have said (vol. ii. p. 28), "The most simple not-living or mineral constituents coming into relation with one another in the presence of pre-existing protoplasm, appear, for aught we know to the contrary, to fall at once into those subtle combinations which constitute the basis of living protoplasm. The rapidity of the process mocks and defies all theoretical explanation. Here at all events there seems to be no laborious process of synthesis—no long chain of substitution compounds before the final product is evolved." It has been commonly assumed that the process of "origination" is intrinsically different from the process of "growth," so far as living matter is concerned. One of the principal objects of my investigation, however, was to endeavour to ascertain whether this assumption was warranted by the facts. If experimental evidence seemed to show that an independent elemental origin of living matter was possible, we should have a very fair right to assume that the process of "origination" was not much more gradual or protracted than the process of "growth."

Turning now to the question of the nature of the evidence concerning the origin of living matter, it appears that the reviewer is content to admit what I have so frequently stated, (NATURE, No. 35, p. 171, and No. 47, p. 412; "Modes of Origin of Lowest Organisms," p. 32), viz. that Bacteria develop in solutions, or parts of solutions, in which no particles can be observed with the microscope. It is true that the reviewer even says nothing about my having ascertained such a fact; he assents to it (notwithstanding the objections previously urged by Prof. Huxley), apparently because my friend and colleague, Dr. Burdon Sanderson, has since been compelled to come to a similar conclusion (*Thirteenth Report of the Med. Off. of Privy Council*). *Bacteria* appearing in such a manner in a solution, must either be the developed representatives of invisible germs thrown off from some pre-existing form of life, or they must be developed representatives of invisible germs on nuclei which had been engendered *de novo* (vol. i. p. 297). Experimental evidence alone can enable us to decide whether the latter of these equally legitimate though rival hypotheses is at all tenable. Fortunately, however, the experiments to which we are compelled to resort may be of the simplest description (vol. i. pp. 311, 337, and 350). Suitable fluids require to be boiled for a time in a glass vessel, the neck of which, if not many times bent or plugged with cotton wool, must be hermetically sealed in the flame of the blowpipe before the process of ebullition has entirely ceased.