the Indian botanists a firm platform to work upon, and it is very gratifying to find that so much has been done within so short a time, and that the working out of the new material has fallen into such competent hands.

The Colonist's Medical Hand-book. By E. A. Barton. (London: Cassell and Co., 1890.)

THE author explains that this little volume has been "written expressly for the use of colonists and squatters, who are entirely out of reach of medical assistance." A more suitable book of the kind could not be at their disposal. They will readily understand his directions, and in recommending appliances for the treatment of emergencies he has taken care to refer only to such as are likely to be found in any squatter's hut.

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 intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Dr. Romanes on Physiological Selection.

AS Dr. Romanes now declares that the essence of his theory of physiological selection is "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species," and that the occurrence of infertility among the members of an undifferentiated species is secondary and comparatively unimportant, I ask leave to quote one or two more of his original statements, in addition to the four emphatic passages quoted in my communication of November 27.

(1) "When accidental variations of a non-useful kind occur in any of the other systems or parts of organisms, they are, as a rule, immediately extinguished by intercrossing. But whenever they happen to arise in the reproductive system in the way here suggested, they must inevitably tend to be preserved as new natural varieties, or incipient species. *At first the difference would only be in respect of the reproductive system; but eventually, on account of independent variation, other differences would supervene, and the new variety would take rank as a new species*" (NATURE, vol. xxxiv. p. 316).

The words I have italicized show clearly that variation in fertility only was what Dr. Romanes then claimed as essential to his theory. Again, after referring to variations in the season of flowering as a "well-known and frequently observed cause" of isolation, he adds:—

(2) "But it is on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection" (*l.c.*, p. 337).

The meaning of this is still further enforced by other passages. After discussing the supposed causes of infertility, he says:—

(3) "Why should we suppose that, unlike all other such variations, it can never be independent, but must always be superinduced as a secondary result of changes taking place elsewhere? It appears to me that the only reason why evolutionists suppose this is because the particular variation in question happens to have as its result the origination of species" (*l.c.*, p. 339).

And again:—

(4) "It appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that *the cases where it has been superinduced by the secondary distinctions are comparatively few in number*" (*l.c.*).

Notwithstanding the passages I have now quoted, emphasizing eight times over, in different ways, that the theory is essentially one of variations as regards fertility and sterility alone, Dr. Romanes now says that, even if all this is wrong, "the principle of physiological selection, as I have stated it, is not thereby affected." If this is not an a solute change of front, words have no meaning; and it is further shown to be so by the fact that Dr. Romanes acknowledged that Mr. Catchpool had "very clearly put forward the theory of physiological selection." But Mr. Catchpool clearly distinguished between the old theory that species arise *first* by variation in form and structure, and only

gradually become mutually infertile, and the new theory that they arise "by spontaneous variations in the generative elements, and are in this case *originally* mutually infertile, but only *gradually* become otherwise divergent" (*l.c.*, vol. xxxi. p. 4).

That this was the essential and original "physiological selection," that was claimed as supplying the missing link required to make the origin of species by natural selection a reality, is yet further shown by the repeated statements that physiological "selection" is a powerful preservative agent. Besides the statement already quoted, that variations in fertility "cannot escape the preserving agency of physiological selection," we have the assertion, quoted above, that such variations "must inevitably tend to be preserved as new natural varieties or incipient species," and the following still more emphatic assertion:—"Neither are we concerned with the degrees of sterility which the variation in question may in any particular case supply. For whether the degree of sterility with the parent form be originally great or small, the result of it will in the long run be the same: the only difference will be that in the latter case a greater number of generations would be required in order to separate the varietal from the parent form."

Now my contention has always been, and still is, that there is no principle at work which can accumulate or even preserve the variations of infertility occurring in an otherwise undifferentiated species, and that the term physiological "selection" is therefore a misnomer, and altogether misleading. If Dr. Romanes will carefully work out numerically (as I have attempted to do) a few cases showing the preservative and accumulative agency of pure physiological selection within an otherwise undifferentiated species, he will do more for his theory than volumes of general disquisition or any number of assertions that it *does* possess this power.

My next contention is, that this is the only new part of his theory—as he himself shows by his reference to the ordinary view, of sterility following other changes, as that which "evolutionists suppose." All the rest is to be found more or less fully discussed in Darwin's works; and I myself claim only to have carefully studied Darwin's facts, and his brief but most suggestive discussion of them in his chapter on "Hybridism" (vol. ii. of "Animals and Plants under Domestication"), and by arranging them more systematically to have shown that they do really give a fairly consistent and sufficient solution of the problem. The only part of my work I claim as a distinct addition to the theory is the proof that, under certain conditions that appear to me probable, natural selection is capable of increasing incipient infertility between distinct races or varieties; and the same view was submitted to Darwin twenty years ago.

Lastly, I totally and emphatically deny that any portion of my facts or conclusions on the subject were derived from Dr. Romanes's writings on "physiological selection." The only two sentences he has quoted from my book to prove that I have done so merely express what he himself has declared to be the common opinion of evolutionists, and which is also the direct outcome of the facts collected by Darwin. If this is "the whole essence of physiological selection," then physiological selection is but a re-statement and amplification of Darwin's own views, since he certainly assumed, and proved, that "some amount of infertility" characterized "some varieties" of animals and plants, and that this infertility, when it occurs, is of some use in preventing the swamping effects of intercrossing. I feel sure that if *this* had been stated, at the outset, to be what was termed "physiological selection," no discussion would have arisen as to the principle involved, but only as to its novelty and as to the appropriateness of the name given to it.

If now, notwithstanding his repeated and emphatic statements that variation as regards fertility in otherwise undifferentiated species was what constituted the basis of his theory of physiological selection, Dr. Romanes continues to assert that I have adopted that theory "purely and simply, without any modification whatever," it will show that our respective standards of scientific reasoning and literary consistency are so entirely different as to render any further discussion of the subject on my part unnecessary as regards myself and useless as regards Dr. Romanes.

ALFRED R. WALLACE.

A Large and Brilliant Fire-ball Meteor.

ON Sunday night, December 14, between 9h. 40m., and 9h. 45m. G.M.T., I had the good fortune to witness the display of a most magnificent fire-ball meteor. It rose rapidly with a bright blue trail from an altitude of 6° above the horizon,

at a point 7° south of west, and in about 7 seconds of time attained a culminating altitude of 55° at a point 19° north of west. Two large trees interlaced prevented me from seeing the head of the meteor till shortly before its culmination, but the light given out by it soon after its first appearance was equal to that of the full moon, and at culmination it much surpassed the light of a full moon in a cloudless sky. The ball seemed to be of a most dazzling bluish-grey colour, and it had a diameter of at least three-quarters of a degree. The disk presented a nebulous appearance with radiations within it as from a centre, but was well defined, except on its lower edge. The glare was almost too much for eyesight, and although the night was very frosty, calm, and clear, all the stars in the west became invisible. I turned to look again very shortly after, and at 5 seconds from culmination found the meteor had become a small yellow ball only one-twelfth of a degree in diameter, and was dropping ruddy sparks. It then disappeared at an altitude of 23° towards a point about 51° north of west. My impression was that this meteor was at no great distance from this place in any part of its course (lat. $51^{\circ} 20' N.$, long. $3m. os. E.$). I noted the positions relatively to trees and tall shrubs, and measured them exactly with a theodolite this morning. To-day I hear that the fire-ball was seen to fall by a man in Chestnut Street, a hamlet on the Maidstone road, and then appeared quite close to him. The direction of fall accords fairly well with my own observation, and would make it descend about two and one-third miles from me. At culmination I should say it seemed very much nearer to me, considering especially its great apparent size at that time. I heard not the slightest noise either of rushing or bursting.

A. FREEMAN.

Murston Rectory, Sittingbourne, Kent, December 15.

ON Sunday, the 14th inst., about 9h. 45m. p.m., I was entering my house by the back door, when the whole place was so brilliantly illuminated that I momentarily supposed there had been a flash of lightning. That erroneous impression was at once removed by the continuance of the light. Wheeling round, I saw a splendid meteor of the fire-ball type, descending obliquely through the sky. Though the Monday newspapers reported serious fog in London on the previous day, yet the night sky at Loughton was perfectly clear; and it was easy to see that the meteor in its descent was passing a little to the right of the constellation Gemini, in a direction nearly, but not quite, parallel to a line joining Castor and Pollux. The head, which was downwards, was a large oval mass of light. The tail was not a mere thread of silvery radiance, like those of November 1866; it seemed broad, irregular on the edges, and sending out sparks. The fire-ball had not descended far when it vanished among a shower of sparks, which also very speedily disappeared. I heard no rushing sound during its course, and no noise of an explosion when it came to its end.

As the time available for observation extended to only a few seconds, it is possible that there may be some error of detail in the foregoing statements. I shall not, therefore, call them observations, but mental impressions of what took place.

ROBERT HUNTER.

Forest Retreat, Staples Road, Loughton, Essex,
December 16.

Attractive Characters in Fungi.

IN the communications which have recently appeared in your journal on this subject, it has been taken for granted that, in the development of *spores* into *mycelium*, the former must necessarily pass through the body of an animal host. We have no scientific evidence of this. I am inclined to think that the theory is a remnant of the old superstition that toadstools are the result of the excrement of toads, and that we must seek for more natural processes of fertilization if we are to solve the mystery. Unbelief is sometimes the nearest road to a right faith. Scepticism is often the gate to truth. It is at least desirable that those who are investigating the subject should approach it unfettered by a theory which is yet destitute of proof, and should direct their researches to ground which is not littered with what may be only the fragments of an exploded superstition. My own observations tend to convince me that germination of the spore and development of the mycelium are alike dependent upon conditions of soil, modified by atmospheric

influences, fertilizing agents, &c., &c.. It may be added that the solution of the problem is rendered more difficult by the apparently inexplicable fact in Nature, so strikingly exemplified in the field of mycology,

"that of fifty seeds
She often brings but one to bear."

The countless millions of spores which never reproduce their kind must be transmuted into other conditions of being, to form part of the "living whole" of the universe, in which nothing can be lost.

J. S.

Glamis, December 5.

Some Habits of the Spider.

IT would be strange indeed, if, as your correspondents infer, there is no record of the gyrating habit of a species of geometric spider so common as even to be well known to Londoners who have a garden. It may be that Kingsley referred to this species, but his "Water Babies" is not found in scientific libraries. It occurred to me, last September, whilst amusing myself by making some of these spiders gyrate, by blowing on or gently touching them, that the instinct, in this kind, is in a decadent state; it does not appear well suited to a heavy-bodied, sharp-legged species like this one, and is certainly much less perfect than in other species possessing a similar habit.

Some years ago, when describing the habits of some Argentine spiders, I mentioned a species of *Pholcus*, abundant in La Plata, with legs of extraordinary length, in colour and general appearance something like a crane-fly, but double the size of that insect. When approached or disturbed in any way, it gathers its feet in the centre of the web, and swings itself round and round with the rapidity of a whirligig, so that it appears like a very slight mist on the web, and offers no point for an enemy to strike at. Here the correspondence between structure and habits is very perfect; the slinness and great length of the legs causing the creature, at the moment the swift revolutions begin, to seem to disappear from sight; and, owing to the string-like form of the legs, the fatigue experienced is probably very much less than the action would cause in a stout short-legged spider like the English species. At all events, it can revolve for fifteen or twenty seconds at a stretch; and, if the cause of alarm continues, it will perform the action no less than three times before quitting the web. The English spider exhausts itself in a few seconds.

Those of your readers who are interested in the habits of spiders will find the paper referred to in the *Gentleman's Magazine* for 1884. Some of my observations contained therein, I find, have been served up—after a fashion, and (of course) without acknowledgment—in a spider article in the current number of *Longmans' Magazine*.

W. H. HUDSON.

"Nowhere can Mathematics be learned as at Cambridge."

THESE words are ascribed to Dr. Hopkinson in his speech at the Royal Society on behalf of the medallists; and as they are calculated to sustain a belief which, Heaven knows, is already widely enough prevalent among even tolerably well-informed people in England—the belief, namely, that no one but a Cambridge Wrangler is worth thinking of as a teacher of mathematics—perhaps you will allow me to enter a protest.

In the department of applied mathematics, thanks to the work of Thomson, Tait, and Clerk Maxwell, Cambridge is supreme, and so far Dr. Hopkinson is right. The weakness of the Cambridge system has always been on the side of geometry; and I am sure that those who studied this subject under Hamilton, Salmon, and Townsend (to go no farther back), will agree with me in saying so. It is a weakness apparent in almost every work emanating from Cambridge. What would the subject of conic sections, for example, be, if Salmon had not shown how it should be treated? The answer is easily supplied by some of the Cambridge works on the subject which are still in extensive circulation in England. As a result of the belief which Dr. Hopkinson seeks to strengthen, the schools and Colleges throughout the whole country are becoming dominated by Cambridge methods exclusively, all appointments in them being virtually filled with Wranglers and no others; and hence we shall have, in time, a dead-level of sameness of method and thought which will partake of whatever shortcomings may characterize the Mathematical Tripos. Is this result desirable?

GEORGE M. MINCHIN.

R.I.E. College, Cooper's Hill, December 11.