

males. The forms of ant in nests Nos. 1 and 2 are as follows: (a) queen (b) male (both winged, but the queen loses its wings after marital flight), (c) large workers, (d) small workers, and (e) nurses. In nest No. 3 I have not yet seen the queen or male, but it possesses—(a) soldier, (b) larger workers, (c) smaller workers, and (d) nurses; but these are different in form to those of nests No. 1 and No. 2. Probably we might add a third form of worker, as there are several sizes in the nest. . . .

"It is curious that in No. 1 nest, from which the queen was removed on August 30, new queens and males are now being developed, while in No. 2 nest, where the queen is at present, nothing but workers have been brought out, and if a queen larva or pupa is placed there it is at once destroyed, while worker larvæ or pupæ are amicably received. In No. 3 all the eggs, larvæ, and pupæ collected with the nest have been hatched, and no eggs have since made their appearance to date. There is no queen with this nest. . . . On November 14 I attempted to prove by experiment how small a number of 'parasol' ants it required to form a new colony. I placed two dozen of ants (one dozen workers and one dozen nurses) in two separate nests, No. 4 and No. 5. With No. 4 I placed a few larvæ with a few rose petals for them to manipulate. With No. 5 I gave a small piece of nest covered with mycelium. On the 16th these nests were destroyed by small foraging ants, known as the 'sugar' or 'meat' ant, and I had to remove them and replace with a new colony. My notes on these are not sufficiently lengthy to be of much importance. But I noted four eggs laid on the 16th, or two days after being placed in their new quarters; no queen being present. The experiment is being continued. I may mention that in No. 4 nest, in which no fungus was present, the larvæ of all sizes appeared to change into the pupæ stage at once for want of food [a fact corresponding with the fact I have named as observed by myself sixty years ago in the case of wasp larvæ]. The circumstance tends to show that the development of the insect is influenced entirely by the feeding it gets in the larvæ stage.

"In nest No. 2 before the introduction of a queen there were no eggs or larvæ. The first worker was hatched on October 27, or fifty-seven days afterwards, and a continual succession has since been maintained, but as yet (November 19) no males or queens have made their appearance."

In a letter accompanying the report, Mr. Hart says:—

"Since these were published, my notes go to prove that ants can practically manufacture at will; male, female, soldier, worker, or nurse. Some of the workers are capable of laying eggs, and from these can be produced all the various forms as well as from a queen's egg.

"There does not, however, appear to be any difference in the character of the food; as I cannot find that the larger larvæ are fed with anything different to that given to the smaller."

These results were obtained before the recent discussion of the question commenced, and as they agree with the results reached by Grassi in the case of the *Termites*, it can now scarcely be doubted that the various forms or classes among the social insects are wholly determined by the treatment of the larvæ.

St. Leonards, December 2.

HERBERT SPENCER.

#### "Acquired Characters."

I DO not think we are in any way bound by the terms of the law enunciated by Lamarck. Those laws may be shown to be erroneous in all but the suggestion of a principle which may possibly be developed into an important and far-reaching doctrine, and if so the importance of the doctrine will be in no wise diminished by the crudity of the early suggestion. There is scarcely any scientific generalisation which does not require an amended enunciation in each generation if it is to be in accordance with the contemporary state of knowledge. Nevertheless it seems to me that the second law of Lamarck does not state that a character acquired by individuals for the first time is inherited, or "alters the potential character of the species." The law states that nature preserves by generation what has been acquired by individuals by the influence of the circumstances to which their race has been long exposed: not by the influence of the circumstances to which they alone have been exposed in their own individual existences.

Leaving Lamarck's laws and doctrines entirely out of the question, if we define an acquired character as one which is determined by the "operation on the individual of given and

related quantities of external agencies," I am not aware that anyone has ever asserted that such a character is inherited, in the sense of being completely reproduced in the offspring without the operation of those external agencies. But I think there is reason to believe that if the same quantity of external agency acts on successive generations, it will produce more effect on the second than on the first, or, to use more correct language, that the effect in the second generation will be increased by a potentiality derived from the first. It is argued that the very possibility of the acquisition of new characters by the individual under new conditions is a proof that the old character had not become fixed and congenital after the action of the old condition on thousands of successive generations. But this is an illustration of the difficulty of completely expressing the problem in abstract language without reference to particular cases. If we consider the case of the pigmentation of the skin of the flounder, we find experimentally that exposure to light of the lower side for some years produces some pigmentation, but not so much as that on the upper side exposed in the individual for the same time. The action on the two sides in the individual being thus equal, or even greater on the lower side, how are we to account for the difference in favour of the upper? Evidently the congenital potentiality of the two sides is different. The old character has then become fixed and congenital to a certain very important degree. If no effect were produced by the action of light on the lower side of the individual, there would be no evidence that the congenital difference in the two sides had been produced by the difference in the relation to light repeated in countless successive generations. On the other hand, if the equal exposure of both sides produced equal pigmentation in the same time, this would be evidence that the difference in the pigmentation under normal conditions was not a congenital character at all. But as the facts stand, the only conclusion which is in accordance with them is that the congenital difference between the two sides is due to the gradual accumulation of slight effects on the congenital potentiality of the germ consequent upon the action of light in the individual. I could mention many other similar instances, which I think do constitute a reason for "associating the somewhat superficial and late responses of the parts of a growing individual to normal or abnormal forces of its environment with that more subtle and profound disturbance which is permanent and affects the potential character of the germ."

I am far, however, from supposing that all specific, generic, or morphological characters are due to the direct action of the environment in the soma, and equally far from admitting that every one of these characters has a part to play in the struggle for existence.

J. T. CUNNINGHAM.

Plymouth, November 30.

THE distinction between the "acquired characters" of Lamarck and the other "responsive characters" which follow the "influence of the normal environment" is, I venture to think, not very important. The two kinds of characters are indeed admitted by Prof. Lankester to be "of the same order," and their essential unity is clearly shown when we attempt to trace the history of evolution as Lamarck conceived it.

The first increase in length of the neck of the giraffe or swan was no doubt, according to Lamarck, "an acquisition under new conditions of new character." But when the process had started, its subsequent stages could hardly be spoken of in this way. The effort of stretching, which was supposed to supply the condition for further increase, was then neither "new" nor "special and abnormal."

In the numerous discussions of the last seven years the term "acquired" has been employed to cover both classes of characters, and, indeed, the argument has chiefly turned on the effect of normal rather than abnormal and special conditions, because the evidence supplied by the former for or against hereditary transmission was so much more convincing than that supplied by the latter.

Although the term "acquired" is an unfortunate one, and has added many difficulties and obscurities which would have been avoided by the substitution of Prof. Lankester's term, "responsive," I think it would only increase the difficulties if it were now authoritatively maintained that, although the majority of instances discussed and the really crucial cases adduced are "of the same order" as acquired characters, they must no longer be called by this name.

I entirely agree with Prof. Lankester as to the mutual anta-