anterior end of the antherozoid. The antherozoids of Muscineæ (Musci and Hepaticæ), and those of Characeæ, have only two very long and slender cilia attached in the same position. The structure and mode of development of these organs are almost identical in these two classes.

In the Fucaceæ, on the other hand, which may be taken as the highest type of Algæ with ciliated antherozoids, the structure of the antherozoid is altogether different. It is a naked cell, not enclosed in a cellulose-wall, with cytoplasm, nucleus, and pigment-spot; the two cilia both spring from a spot in close proximity to the eye-spot, although one of them is attached to the body of the antherozoid for a portion of its length.

The paper was illustrated by diagrams taken from the beautiful work on the 'Development and Structure of Antherozoids,' by M. L. Guignard; and the importance of the above facts was pointed out in support of the view that the Characeæ are more nearly related to the Muscineæ than to the true Algæ.

---

6. *A Hybrid Desmid.* *By* ALFRED W. BENNETT, *M.A., B.Sc., F.L.S.*

The author described the occurrence of what he regards as a hybrid form between *Euastrum crassum*, Ktz. and *E. humerosum*, Ralfs.

FRIDAY, SEPTEMBER 13.

The following Papers and Reports were read:—

1. *Discussion on Acquired Characters.*

(a) *On the supposed Transmission of Acquired Characters.* *By* E. B. POULTON, *F.R.S.*

(b) *Feasible Experiments on the Possibility of transmitting Acquired Habits by means of Inheritance.* *By* FRANCIS GALTON, *F.R.S.*

Feasible experiments have yet to be designed that shall be accepted as crucial tests of the possibility of a parent transmitting a *congenital* aptitude to his children, which he himself possessed, *not congenitally*, but merely through long and distasteful practice under some sort of compulsion.

The requirements are to eliminate all possibility of parental or social teaching, to bring up all the descendants in the same way, to make simultaneous experiments on many broods during many generations, and, lastly, to economise time, money, and labour. This list of requirements points with emphasis to experimenting on creatures that are reared from eggs, as fowls, fishes, and moths. *Fowls*—The largely extending practice of hatching eggs in incubators for commercial purposes, and the varied aptitudes of poultry, make them very suitable subjects. Birds are said to have an instinctive dread of various insects; hence mimetic insects, that are really good for food, are avoided by them. Do such insects exist, and could they be easily reared, which poultry would avoid at first, though experience would soon teach them to like and to eat greedily? Similarly as regards sounds and cries, which would frighten at first, but afterwards be welcomed as signals for food, &c. Would the stocks of two breeders, one of whom adopted such experiments as these and the other did not, differ in instinct after many generations? *Fish*—The experiment (quoted by Darwin) of Mobius with the pike, using a trough of water divided by a glass plate into two compartments, in one of which was the pike and in the other were minnows, was mentioned as an example. The pike after dashing at the minnows many times, and each time being checked and hurt by the glass plate, during some weeks, finally abandoned all attempts to seize them, so that when the plate was removed the pike never afterwards ventured to attack

the minnows. The question, then, is, whether fish reared for some generations under conditions which compelled them to adopt habits not conformable to their natures would show any corresponding change of instinct. Of course each generation would be reared in a separate tank from its parents. *Moths*—Experiments have been made for the author by Mr. Frederic Merrifield with the *Selenia Illustraria*, which has two broods yearly. They are being made for quite another purpose, but have already shown the ease of breeding hardy moths on a large scale when the art of doing so is well understood. All larvæ are fastidious in their diet, but it may well be that certain food which they would not touch at first would after a while be greedily eaten, and be found perfectly wholesome.

Experiments on the lines here suggested ought to show the proportion of cases in which acquired aptitudes of several kinds are *certainly not* inherited. They might also, perhaps, show that in a small proportion of cases they *certainly are*. Thus limits would be fixed within which doubt remained permissible. The object of this paper is to invite experts to discuss the details of the most appropriate experiments for doing this.

---

2. *The Palæontological Evidence for the Transmission of Acquired Characters.* By Professor Henry F. Osborn.

As a contribution to the present discussion upon the inheritance of acquired characters I offer an outline of the opinions prevailing among American naturalists of the so-called Neo-Lamarckian school, and especially desire to direct attention to the character of the evidence for these opinions. This evidence is of a different order from that discussed in Weismann's essays upon 'Heredity,' and while it cannot be said to conclusively demonstrate the truth of the *Lamarckian principle*, it certainly admits of no other interpretation at present, and lends the support of direct observation to some of the weightiest theoretical difficulties in the pure *Selection principle*.

1. We regard natural selection as a universal principle, explaining the 'survival of the fittest' individuals and natural groups, and as the only explanation which can be offered of the origin of one large class of useful and adaptive characters. We supplement this by the Lamarckian principle as explaining the 'origin of the fittest' in so far as fitness includes those race variations which correspond to the modifications in the individual springing from internal reactions to the influences of environment. There is naturally a diversity of opinion as to how far each of these principles is operative, not that they conflict.

2. If both principles operate upon the origin of the fittest we should find in every individual two classes of variation, both in respect to new characters and to modifications of the old: *First, chance variations*, or those which, with Darwin and Weismann, we attribute to the mixture of two diverse hereditary strains; these may or may not be useful; if useful they depend entirely upon selection for their preservation. *Second, variations which follow from their incipient stages a certain definite direction towards adaptation*; these are not useful at the start; thus while, as they accumulate, they favour the individual, they are not directly dependent upon selection for their preservation; these we attribute to the Lamarckian principle.

My present purpose is to show that variations of the second class are of an extent and importance not suspected previous to our recent palæontological discoveries, and that the Lamarckian principle offers the only adequate explanation for them.

3. The general theory as to the introduction and transmission of variations of the second class may be stated as based upon the data of palæontology—the evolution of the skeleton and teeth.

(1) In the life of the individual, adaptation is increased by local and general metatrophic changes, of necessity correlated, which take place most rapidly in the regions of least perfect adaptation, since here the reactions are greatest. (2) The main trend of variation is determined not by the transmission of the full adaptive modifications themselves, as Lamarck supposed, but of the disposition to adaptive