home after service in Africa. It likewise affords shelter to a considerable number of sailors of all nationalities, whose health it watches over carefully. The institution, which occupies 2½ acres of land in the city of Antwerp, has placed side by side the part that is purely colonial and the various scientific departments: the bacteriologic laboratory, the chemical laboratory, the museum of hygiene and the sanitary service. The theoretical instruction in tropical pathology is combined with the clinical instruction. There is a small hospital of fifty beds with all the modern comforts. The new institute has taken over a triple function; it is a university, laboratory and hospital combined. It is designed to furnish graduate instruction to physicians planning to practise in the Belgian Congo, to provide instruction for missionaries, sanitary officers and nurses who expect to serve in Africa, and, finally, to carry on scientific research.

THE German Association of Men of Science and Physicians (Gesellschaft Deutscher Naturforscher und Ärzte) is modifying its policy with the view of overcoming excessive specialization. It is proposed, according to *Nature*, to emphasize the problems common to many branches of science and medicine, and to promote discussion on these common topics on the widest possible basis. The annual meeting of the association will last not more than three and a half days. The council will only arrange the general sessions, the main group and joint sessions and popular evening lectures. The general sessions will be devoted to topics in which some definite results have been reached or to problems of immediate importance. If discussion does not follow these addresses by selected speakers, the same theme may be handled more freely in joint sessions of sections. *Nature* states that the council will abandon the attempt to arrange meetings of the separate sections, leaving them to deal individually with the local committee. It has been the custom for some years past that allied and associated societies should meet at the same place, and either before or after the formal meetings of the association. This custom is to be continued at the next meeting in Hanover (September 16–20) and with the help of the local committee. An innovation is the *Zweckverband* of German scientific and medical congresses, the aim of which is to maintain contact between these congresses so that whilst specialization goes forward they shall not be shut off from each other. The purpose of this union is to publish the dates, places and programs of these congresses.

DISCUSSION

PROTECTIVE RESEMBLANCES IN INSECTS—EXPERIMENT AND THEORY

IN 1912 the writer reviewed[1] experiments designed to test the theories of protective adaptations. In a recent paper[2] Professor E. B. Poulton discusses experiments made since that time, which, it is apparent, have been gradually improved, the improvement consisting in an increased approximation to natural conditions. This being the case, as freely admitted by Poulton (p. 37), why not accept the evidence as to the feeding habits of animals under entirely natural conditions, namely, that obtained by analysis of the contents of stomachs collected in the wild? This is the same question asked by the writer in 1912, and it remains in full force, for as I showed then (p. 361) "Animals accept in captivity articles of food they . . . do not eat in the wild . . . [and] animals reject in captivity articles of food which are . . . eaten by wild members of the species [hence] the experiments are not trustworthy guides to behavior under natural conditions."

Naturalists have it continually drilled into them that the experimental method is the only scientific procedure. So Poulton speaks of "solid grounds" to be obtained by "test in captivity" (p. 36). The matter at issue, however, is the natural feeding habits of animals, and these constitute the only basis upon which theories of protective adaptations can legitimately be proposed. As no reliable knowledge of natural feeding habits can be obtained by experiment, this is certainly one field where use of the vaunted experimental method has no application. The final test of the theory of protective adaptations is not apparent preferences at all, as the experimenters seem to think, but data (the more comprehensive, the better) as to what animals actually eat in a state of nature.

Poulton charges McAtee with ignoring various published records in his 1932 paper,[3] but they are all "exotic" from the point of view of a treatise devoted chiefly to the "Food Habits of Nearctic Birds" and in which the general and intentional omission of foreign material is explicitly called to attention (p. 145). Poulton's arguments about "looper" caterpillars have been sufficiently replied to in an article being published at about this same time in the Proceedings of the Entomological Society of London.

My critic says he is unwilling to follow "McAtee's controversial methods," but he does very well by indirection, as in the use of a quotation about "Sir Oracle," and he makes a thrust in calling for "if only

[1] *Proc. Acad. Nat. Sci.*, Philadelphia, pp. 281–364.

[2] Attempts to disprove the theories of warning colors, mimicry and protective resemblance in insects, Ve Congrès Intern. Entom., pp. 33–44, 1932 (1933).

[3] *Smiths. Misc. Coll.*, 85 (7), pp. 1–201.

it were possible—a little humility," which if well grounded would be devastating. The shoe is on the other foot, however, as McAtee has the humility not to attempt to explain everything about organisms, outranking in this respect all the selection theorists. In this very paper Poulton proceeds to explain why some birds devour the excrement of their nestlings, saying "there can be little doubt about the reason." There is great doubt about the reason, however, in the minds of naturalists who have observed that some birds do this, others do not, that those which sometimes do it at other times do not, that, in fact, the phenomenon is subject to all the variations, gradations and contradictory indications that characterize the behavior of organisms in general.

Attempts to draw hard-and-fast lines where none exist, to trammel within the limits of hypothesis where all is free, and to formulize where everything is protean, characterize the writings of selectionists. "Humility" does not put forward strained "explanations" for every detail of organic appearance and behavior, and in all truth "humility" can scarcely be claimed as a leading trait of definers, explainers and asserters of "natural laws." So it seems to the writer and so it has seemed also to others.

> It has been pointed out over and over again that the explanations of science never amount to more than the enumeration of the conditions under which the events in nature take place. With ultimate explanation, science does not deal.[4]
>
> It should not be forgotten that all evolutionary phenomena are fundamentally inexplicable.[5]
>
> From the peculiar nature of the case no causal explanation of evolution is possible.[6]

Probably the single factor that is most to blame for unwarranted theorizing by selectionists is their taking entirely too seriously the so-called "struggle for existence." This trait is exemplified in the International Entomological Congress paper by Poulton, when he speaks of "ants—successfully attacked as they are by enemies of many and varied kinds, yet holding their own in all the habitable parts of the earth, and providing models for well-nigh innumerable mimics belonging to diverse groups of insects, as well as many in the Arachnida" (p. 44).

Now ants in many places number millions to the acre, while birds, upon predation by which, selectionists set so much store, number about a pair to the acre. Other ant enemies may be more numerous, but they do not number millions. Effects can not be attained through selection except by a very high degree of elimination by selective agents, and certainly in the case of ants it is obvious that this does not and can not occur. It is universally admitted that the grand checks upon insect numbers are climatic, hence indiscriminate so far as protective adaptations are concerned. The aggressions of parasites and various other natural enemies also are not conceivably affected by the characters alluded to as protective adaptations.

In this typical case of explanation of ant resemblance, we have not only overemphasis on the struggle for existence but also the usual attempt to draw lines where none occur in nature. Theorists point out that certain ant mimics by virtue of some assumed advantage live among ants, but they ignore the more numerous insect and other guests of ants that do not resemble their hosts. Ant-resembling creatures do not all live with ants, some of them (*Gelis* spp.) occurring in far northern regions where there are no ants. Some ant mimics may be predators upon ants but others (various Cerambycid and Anthicid beetles) neither prey upon nor live with the ants. The assumed advantage of ant resemblance in protecting its possessors from predators is a very tenuous one, as ants admittedly are freely eaten by a great variety of enemies. The "protection" really amounts to the swarms of ants taking the brunt of predation from the much less numerous "mimics," and this is merely a consequence of their relative numbers, an advantage the rarer forms have anyway, regardless of their appearance.

If ant mimicry is not necessary to existence among ants, if it occurs without any possible relation to ants, and if it can act as a protection only as a result of numerical ratios, it would seem not only unnecessary but erroneous to invoke a theory of mimicry through natural selection to account for it.

A theory should account for all the facts within its scope, and as that of mimicry evidently does not, it should be discarded. The theory of mimicry is a fair sample of selection theories relating to protective adaptations, all of which agree in failing to explain the diversity of "adaptations" in identical environments, the similarity of "adaptations" in diverse environments, the survival for apparently indefinite periods of organisms with varying degrees of "adaptation," in other words their survival apparently whether "protected" or not, or if all are held "protected," the continued maintenance of average populations of all more or less predatory organisms.

Scientific theories do have an indefinite tenure of existence; based on knowledge available at the time of their conception, they must be modified with the progress of knowledge. Protective adaptation theories are no exception to this rule, and the more critically they are examined in the light of present knowl-

[4] Otto Glaser, *Am. Nat.*, p. 727, 1912.
[5] Wm. Patten, *Sci. Mon.*, p. 521, Dec., 1932.
[6] J. H. Woodger, "Biological Principles," p. 471, 1929.

edge of the world of nature (not of experiment), the more insecurely founded they appear to be.

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U. S. BIOLOGICAL SURVEY

THE IMPORTANCE OF DIAMETER AS A FACTOR IN MYELINATION

IT is believed that the close relationship existing between diameter of the axone and whether or not it shall possess a myelin sheath has not received sufficient attention. Due to the present interest in the physiology of nerve fibers, a brief presentation of the main facts seems to be desirable.

The bearing of diameter on myelination has been stated by Vogt,[1] who says that myelination of the tracts is not related to the acquisition of conducting power, but to the number and size of the component fibers. Speidel[2] states that myelination is preceded by a characteristic thickening of the axone. Ranson *et al.*,[3] speaking of the vagus rootlets proximal to the nodose ganglion, say that the number of fine myelinated fibers varies considerably in different animals, and further when the number of fine myelinated fibers is large the number of unmyelinated fibers is correspondingly reduced. In another part of the same paper they say that the unmyelinated fibers are always smaller than the myelinated fibers; however, the exact nature of the relationship is not emphasized.

From observations carried on in this laboratory, the author has become convinced that in mammals there exists a critical diameter, above which all axones are myelinated and below which all are unmyelinated. This diameter is tentatively stated as 1.5 μ, including the myelin sheath. Myelinated axones of 1 μ diameter can be seen in cross-sections of nerves containing large numbers of small fibers, but teased preparations show these to be constricted portions of slightly larger axones. These constricted regions are found principally opposite the sheath cell nuclei, but occur in other places in some of the fibers. Preliminary measurements indicate that a critical myelination diameter also occurs in birds, reptiles and amphibia, but that the value of the constant may be slightly less in birds and slightly greater in the cold-blooded animals. The relationship holds only for true neurones; *i.e.*, the neurosensory cells and protoneurones are excepted.

The above principle is based on measurements of the diameters of fibers in the dorsal and ventral roots of the cow, cat and rat. The thoracic ventral roots of the cat and rat contain two distinct groups of fibers—the somatic efferent and preganglionic efferent. The somatic motor group is entirely myelinated and the preganglionic efferent is partially myelinated in both animals. In the cow both groups are completely myelinated, and the fibers of each group are distinctly larger in this animal. In the dorsal roots of the cat and rat three fiber groups may be distinguished. In the rat the smallest group approaches the myelination limit and is believed to be partially unmyelinated. In the cat the smallest group is distinctly beyond the myelination limit, corresponding to the preganglionic efferent group in the cow as to size. In the dorsal root of the cow a fourth group of myelinated fibers has appeared. This group is entirely below the myelination limit in the cat and rat, and gives rise to the C wave in oscillograph records. In all three animals the smallest myelinated fibers are 1.5 μ, while the upper limit of size varies, the largest fibers occurring in the cow, the smallest in the rat.

Two other related principles of nerve fiber size are suggested: (1) Nerve fibers myelinate in the order of their appearance as specifically stainable axones and the first fibers to myelinate are the largest fibers in the adult; (2) the order of fiber size for the various functional groups is the same in different mammals. The two principles are known to hold true for all components of the spinal roots, for the median longitudinal bundle, the rubrospinal fasciculus and the corticospinal tracts. No exceptions to these rules are known to the author.

Given the above principles, the following are some of the known facts readily explained.

(1) The variations from animal to animal in the amount of myelination of the pre- and postganglionic visceral efferent fibers.

(2) Variations in the time of myelination of the tracts in the various species.

(3) The distal excess of myelinated fibers in the trunk over the sum of those in the spinal roots and the differences in the amount of this excess in different spinal regions and animals.

(4) Differences in the time of cessation of myelinated fiber increase in the spinal roots and nerves.

This statement is offered in advance of *in extenso* publication, because it is believed that a point of view is offered which will be valuable to many engaged in research on nerve fiber physiology and histology.

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[1] O. Vogt, *Neurol. Centralbl.*, 27: 137, 1908.
[2] C. C. Speidel, *Jour. Exp. Zool.*, 61: 279, 1932.
[3] S. W. Ranson, J. O. Foley and C. D. Alpert, *Am. Jour. Anat.*, 53: 289, 1933.

A NEW TERM FOR THE YOUTHFUL STAGE OF FORAMINIFERAL SHELLS[1]

IN the terminology of foraminifer shells no generally acceptable or appropriate name exists for the

[1] Published by permission of the Director, U. S. Geological Survey.