marks were illustrated by a number of lantern slides taken during the field courses given by him at Axton, in the Adirondacks, for the Cornell School, and at Milford, Pa., for Yale University.

The third paper, 'A Record of the Black Rat in Virginia,' was read by Mr. William Palmer. He noted the occurrence of an isolated colony of the black rat (*Mus rattus*) on the top of a Virginia mountain, Peaks of Otter, in Bedford County, at an elevation of 3,875 feet. The specimens collected are not quite typical. Probably but few individuals now exist in and about an old store at the summit.

The 421st meeting was held on December 1, 1906, President Knowlton in the chair and about fifty persons present.

General T. E. Wilcox remarked on the unusual abundance of quail and the cottontail rabbit in New York a few miles south of Utica.

Dr. Evermann informed the society of the recent death of two naval officers to whom biological science is much indebted, Lieutenant Franklin Swift, retired, of the steamer *Fish Hawk,* and Lieutenant-Commander Leroy M. Garrett, of the *Albatross.* Lieutenant Swift died on November 10, at Charleston, S. C., of typhoid fever, and Lieutenant-Commander Garrett was washed overboard 500 miles northwest of Honolulu on November 21, while the *Albatross* was returning with the great collections of the trip to Japan. These officers have commanded these research vessels during some of their most important work and are in large part responsible for the excellent results obtained.

Dr. L. O. Howard presented the first paper, on the subject 'Polyembryony and Fixation of Sex.' This paper was published at length in Science, December 21, 1906.

The second paper consisted of an illustrated lecture by Mr. John W. Titcomb, on 'Principles and Methods in Fish Culture.' He explained the underlying principles of artificial propagation as applied chiefly to salmonoid fishes, described in detail the methods and manipulations concerned and illustrated every point by lantern-slide pictures, showing apparatus, operations and the fishes themselves in all stages from the egg upward. He commented on the relation of fish culture to various natural sciences. The long and interesting series of illustrations included pictures showing the inauguration of fish culture by the speaker in Argentina, South America.

M. C. Marsh,
Recording Secretary

Discussion and Correspondence

Polyembryony and Sex-determination

In an extended review in the last number of Science (December 21, 1906), Dr. Howard has emphasized the astonishing and valuable results of the recent work by Marchal ('98, '04) and Silvestri ('05, '06) on the spontaneous polyembryony of certain parasitic Hymenoptera. He has quoted Bugnion's discussion of the bearing of this work on sex-determination but has not called attention to the fact that in the light of Silvestri's work this view may need revision.

As stated, Bugnion, '91, in the course of his work upon Encyrtus had noted that as a rule all of the individuals emerging from one host belong to a single sex. At the time, Bugnion thought that this "should be attributed to an occasional parthenogenesis, the caterpillars giving birth exclusively to males having been those which had been pierced by a non-fertilized *Encyrtus.*"

This conclusion, which was a logical one in view of the data then at hand, Bugnion discards completely since the appearance of Marchal's work. He believes that the phenomenon must be "a natural consequence of polyembryony, and that one would expect the sexes to be separated in this way wherever the embryos come from the division of a single egg."

While the latter clause is undoubtedly true, the possibility of the facts being explained on the basis of parthenogenesis is by no means excluded. Bugnion, in his work, did not observe the oviposition. Marchal presents no evidence that parthenogenetic development does not take place. In fact, he purposely leaves the question open, as '04, p. 298, "Le

cause de cette détermination reside-t-elle dans la fécondation pour le sexe femelle, et dans l'absence de fécondation pour le sexe mâle, il se peut qu'il en soit ainsi, mais le fait n'est pas démontré."

On the other hand, Silvestri very definitely determined that in the case of *Litomastix* the parthenogenetic development does take place and that, as in the bees, the fertilized eggs always give rise to females, the unfertilized to males. Until further observations have been made it would seem unsafe to discard Bugnion's earlier hypothesis that the observed facts regarding the preponderance of one sex or the other in *Encyrtus* are to be likewise explained.

WM. A. RILEY

VARIATION OR MUTATION?

SYSTEMATIC zoologists are not likely to be hasty in endorsing the dogma of de Vries in respect of individual variations, or 'fluctuations' in his terminology: "* * * they may be proved to be inadequate even to make a single step along the great lines of evolution, in regard to progressive as well as retrogressive development."[1]

There are two methods of approach to the part played by mutations and individual variations in the development of specific characters: the comparative, in use by taxonomists, and the experimental, at the hands chiefly of embryologists.

The argument for individual variation from the comparative side was well presented by Dr. C. Hart Merriam, in his vice-presidential address before the American Association, and that for mutations from the experimental side, with equal clearness, by Professor Davenport, in SCIENCE of November 2, although he does not take the extreme view of de Vries.

Now, both systematist and experimenter will admit the absence of any exact means of determining what may or may not have been originally a mutation in such cases, for instance, as slight discontinuity observed under nature where there is no knowledge of the race history—for when Davenport asks: "But will it not be often impossible to say whether a new-appearing quality is truly new or old?"[2] no one can deny him. The statistical method, though it be fondly looked on as a universal solvent, can give no help here, for it points out only the end facts, not their causes, and there seems to be no resource but in the balanced judgment of competent observers. Therefore, when one so qualified as Dr. Merriam states his opinion that in more than a thousand species and subspecies of North American mammals and birds, he does not find one which appears to have arisen by mutation, he records a conclusion of great weight. Essential agreement with Merriam results from a similar examination of North American scaled reptiles.

The measure established is that a species or subspecies to be rated as a possible mutant must be separated from its nearest known congener by at least one indivisible character. This, I believe, accords with the standard set by de Vries, as well as with that of Professor Davenport. It might be claimed by extreme mutationists that monotypic genera, appearing to be related to a species of another genus occupying the same range, have arisen by mutation, but in these cases there is rarely valid evidence on either side, and as either view must be an assumption, they are not considered in this examination. If we are to reach a general rule of probability it must be through cases determined upon reasonable grounds.

I have followed Professor Cope's last descriptive list of Nearctic reptiles, not by any means from complete agreement with it, but for the reason that the analytic method favored by him left few variants unnamed.

Among lizards, Cope says of the genus *Sceloporus:* "I recommend it as an excellent *pièce de résistance* for those persons who do not believe in the doctrine of the derivation of species." This thought may be borrowed and extended to include the whole list of Nearctic lizards, and addressed to all who require evidence of the derivation of species by minute gradations, for nowhere else, perhaps, are they more general. There is no room here for mutations.

[1] 'Species and Varieties,' p. 18, 1905.

[2] SCIENCE, September 22, 1905, p. 370.