

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.

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#### 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."<sup>1</sup>

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?"<sup>2</sup> 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.

<sup>1</sup> 'Species and Varieties,' p. 18, 1905.

<sup>2</sup> *SCIENCE*, September 22, 1905, p. 370.

In serpents, such variations as the presence or absence of certain head plates, or of a pair of dorsal rows of scales, are fairly common in many genera, but as a rule they do not transgress the obvious limits of specific variation, and unless combined with other differences they are not regarded as deserving of a name. Nevertheless, when they do transgress they fall within our definition of a mutation, for these characters are the indivisible units of repetitive series, and between their presence or absence there can be no intergradation. Among the species and subspecies enumerated by Cope, there are thirteen such cases which might possibly be allowed as mutations. But even granting them to be such, they seem to have failed signally in giving rise to new species, for nine of them are known only from the one type specimen each, and of the tenth, two examples only were collected more than twenty years ago, at the same time, in a well-settled part of Texas. The remaining three cases, of more or less established forms, have some claim to consideration. They are these:

The genus *Storeria* consists of three species, two of which, *S. dekayi* with seventeen rows of dorsal scales, and *S. occipito-maculata* with fifteen, occupy practically the same range from Vera Cruz north over most of the Austroriparian and eastern regions. There are slight color differences, fairly constant, but the difference in scale rows seems to be entirely so, and all herpetologists admit their specific distinctness. As there can be no gradation between fifteen and seventeen scale rows, which vary always in pairs, one or the other of these species, probably *S. occipito-maculata*, seems to have arisen from the other by a process which might be called mutation. It may be allowed that the differential characters are not adaptive.

In exactly the same way *Virginia elegans*, occupying a limited western portion of the range of *V. valeriae*, differs from it in having two more scale rows.

Finally, *Eutænia elegans atrata* (= *E. infernalis vidua* Cope) appears to be an offshoot of *E. elegans*, presenting a quite distinct color pattern and a tendency to a reduction of scale rows. According to Van Denburgh they are

found only on the coast slope of the peninsula of San Francisco, and the examples I have seen were collected promiscuously with typical *E. elegans*. The mutation in this case would lie in the distribution of color, for the reduction in scale rows is not fully constant. Whether they breed true is not known, but their scarcity renders it doubtful.

But we are now close to a mere matter of names, for in two, at least, of these cases variation and mutation approach each other so nearly that they come under the same definition, for the addition or subtraction of a pair of scale rows represents the lowest possible term in a variation series, and the name given to it is largely a matter of choice; yet beyond these cases no other evidence for the origin of specific characters by mutation is yielded by the examination. The conclusion is near to that of Dr. Merriam.

The value of the experimental method is not questioned by the doubt whether theoretical interpretation of the behavior of 'unit characters' in the germ plasm has yet reached a stage of certainty sufficient to stand over against the body of evidence contributed by the comparative method, as to the minor rôle of mutations in specific development in vertebrates.

That mutants occur in feral animals is doubtless true, even much more widely than the cases of melanism and albinism cited by Professor Davenport, but it does not yet seem necessary to modify the opinion not long since expressed by me elsewhere<sup>3</sup>—"In so far as its occurrence under nature is concerned, every zoologist who has worked over many genera for purposes of taxonomy will probably admit that many of his most perplexing anomalies, which occur now and then as one or a few individuals which can not be exactly placed, are in the nature of mutations, but few, I imagine, will be disposed to allow that they find evidence that these are inherited. . . . there is little evidence that they have been starting points of new species."

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<sup>3</sup> 'Theories of Evolution since Darwin,' 1906.