are not yet back to their former bad condition.

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SYSTEMATISTS AND GENERAL BIOLOGISTS

MAY I endorse the suggestion by Dr. L. O. Howard?[1] He says that he does not know whether determination of species is important to the experimental embryologist. When, as zoological recorder for Echinoderma, it was my duty to read a large number of papers by those workers, I formed the opinion that it certainly was important, and wrote:[2]

> It is well to urge on those gentlemen the need for an accurate determination of the material with which they work. They are too much inclined to infer the universal from the particular, and to overlook the fact that species and even local races differ from one another in their reproduction and development, just as much as in their habits and perhaps more than in their structure.

This plea was strongly supported by Viguier.[3]

Accurate discrimination of species is no less necessary for the field naturalist. J. H. Fabre, always ready to gird at the museum worker, had to confess that he had confused under the one name *Eumenes pomiformis* three species of mason-wasps, so that it was not possible for him "to ascribe to each of them its respective nest" (I quote from the selection just published under the title "The Wonders of Instinct," London, Fisher Unwin).

Most geologists have by this time learned that, for lack of the precautions advocated by Dr. Howard, many of their fossil lists are not worth the paper they are printed on. Recent advances in stratigraphical geology are almost entirely due to the keener appreciation of minute specific differences.

In a word, every kind of biologist should find in the despised taxonomist a valuable, indeed an indispensable, ally; and in our museums he should recognize a depository where the evidence for his conclusions may be preserved for future generations of workers.

LONDON F. A. BATHER

2 *Zool. Rec.*, for 1901.

1 SCIENCE, January 25, p. 93.

3 1903. *Ann. Sci. Nat. Zool.*, ser. 8, Vol. 17, p. 71.

SCIENTIFIC BOOKS

Lectures on Heredity. By H. S. JENNINGS, Ph.D., LL.D., Johns Hopkins University; OSCAR RIDDLE, Ph.D., Department of Experimental Evolution, Carnegie Institution; and W. E. CASTLE, Ph.D., Harvard University. Delivered under the auspices of the Washington Academy of Sciences, Washington, D. C. 1917. Pp. 82. Bound in buckram, 50 cents.

This is the second annual series of lectures presented before the Washington Academy of Sciences and reprinted in collected form from the *Journal* of that academy.

The study of genetics has become so highly specialized that workers in the different fields have ceased, except in rare instances, to make a serious effort to coordinate their work with that of others.

Dr. Jennings's classical work on the nature of variations in lower organisms deals with one of these highly specialized branches, and students of other branches should appreciate the service rendered by Dr. Jennings in his painstaking comparison.

"Having satisfied myself as to the nature of the variations that arise in the creatures that I have studied, I have looked about to see what other workers have found; and to determine whether any unified picture of the matter can be made."

After claiming that the idea of genotypes must be admitted as a general condition, the author concludes that this result "is not final, that it does not proceed to the end."

In a uniparental organism, from which all question of the recombination of existing diversities is eliminated, Dr. Jennings finds that "the immense majority of the hereditary variations were minute gradations. Variation is as continuous as can be detected."

The points at issue between the "genotypic mutationists" and the upholders of gradual change are clearly and concisely stated. Setting aside the question whether the evidence held to support the gradual change theory is conclusive or not, he proceeds directly into territory of the mutationists and shows that the "multiple allelomorphs" found in *Droso-*

phila supply in themselves all the evidence that even an extreme selectionist should ask. To explain why large changes are observed as well as small ones it is suggested that we may be "witnessing the disintegration of highly developed apparatus instead of its building up."

Dr. Castle's contribution also deals with the controversy over the nature of evolutionary change, whether continuous or discontinuous. He, however, is less inclined than Dr. Jennings to resolve the differences between the two schools into differences in the use of terms. After allowing for the effects of the confusion in terminology, he sees "two contrasted sets of ideas," which he arranges under the headings *Darwin* and *DeVries*.

Students of paleontology, geographical distribution and classification are shown, in general, to favor the belief in gradual evolution and the efficacy of selection. The opposite view, that of discontinuous variation and stability of new forms, is held by a majority of the students of experimental breeding. Support of the former view by one who has done such thorough work as an experimental breeder must have great weight.

From the point of view taken by Jennings, it would seem that in contrasting the mode of origin and the stability of new types Castle is himself open to the criticism of using the term "type" ambiguously. If by a new type is meant anything new, most geneticists would range themselves with DeVries, but if a new type is something more comprehensive, DeVries and his followers must stand alone.

The crux of the difference between Castle's views and those of practically all classes of mutationists would seem to be in Castle's holding that selection determines in some measure the range of variation in subsequent generations. Confining the question to inherited variations, does the selection of extreme variations form new centers of distribution? To do so, it would seem that small variations must be more numerous than large ones, an assumption which would be questioned by most mutationists, including Morgan.

Although cases were encountered in Dr. Castle's own work in which selection gave no tangible results, in many characters progress was rapid and continuous, with no indication of other than physical limits, and it is held that in the smaller mammals, which Dr. Castle has studied intensively for many years, there are few characters which can safely be referred to the agency of perfectly stable genes.

That selection does, from a practical standpoint at least, produce results is abundantly shown by Dr. Castle's work. If by the use of refined reasoning his critics are able to show that change in a single Mendelian character is not the only possible explanation of the results, these critics may then be referred to Jennings's results with *Difflugia*.

Dr. Riddle has here brought together in a concise and readable form the results of his extensive experiments on the nature of sex in pigeons and has coordinated these results with the work of other investigators.

Sexual differentiation is interpreted as the expression of quantitative differences in the rate of protoplasmic activity; the more active metabolism resulting in males, the less active in females. Many lines of evidence are presented, all of which are consistent with the view that preponderance of one or the other sex is conditioned by the rate or level of metabolism. These various lines of evidence show the following characteristics to be associated with the female sex, all of them being associated also with a low level of metabolism: Large size of yolk, low per cent. of water in the yolk, high per cent. of stored material, high total stored energy, exhausted physical condition of the mother, age of the mother.

In crosses the percentage of males increases with the width of the cross to the point of infertility. Since males are characterized by a more active metabolism, there is an agreement between these results and the commonly observed increased vigor of hybrids.

Assortive mating of gametes and differential death rates are fully considered and neither is found to furnish a possible explanation of the controlled sex ratios.

Not only can the sex of pigeons be changed but it can also be accentuated. The females hatched from the second egg of the clutch, laid in the autumn by overworked birds, are more pronounced females than the normal females of the species. This is evidenced by the persistence of a right ovary in such birds. In normal female pigeons the right ovary has completely degenerated in the week-old squab.

The literature reviewed gives evidence of a relation between rate of metabolism and sex in a great variety of animals, varying from worms to man. With sex viewed as an expression of differentiated metabolic activity, its independent origin in diverse groups of organisms ceases to be a stumbling block, being no more remarkable than that the same color should have originated independently in different groups. The work reported is confined to the animal kingdom and it should be of interest to determine whether in diœcious plants there is a corresponding differentiation in the rate of metabolic change.

Dr. Riddle's work would seem to call for discussion by those students of genetics who place the distribution of the chromosome in a causal relation to sex, since his results directly challenge this interpretation. It is shown that in some cases at least sex is determined before the segregation of the chromosomes, a fact which would seem to make chromosome number a characteristic rather than a cause of sex.

Furthermore, the challenge extends to all Mendelians, for if "one hereditary character (sex) is modifiable, is of a fluid, quantitative, reversible nature," surely the alternative nature and stability of other characters come in question.

It is worthy of note that all three investigators, though working in widely separated fields and approaching the problem of evolution from very different angles, conclude that evolutionary change is, in effect at least, a gradual process that is not beyond the power of man to man influence.

G. N. COLLINS

SPECIAL ARTICLES

THE RÔLE OF CATALASE IN ACIDOSIS

IF an inorganic acid, such as hydrochloric. be administered to an animal, it is neutralized by the alkalies of the blood and tissues; if an organic acid be administered, it is oxidized, unless the oxidative processes of the animal are defective, as in diabetes, in which case the organic acids are neutralized, as are the inorganic. This neutralization of acids leads to a depletion of the "alkaline reserves" of the body, which produces the condition known as acidosis. By the term acidosis is meant the impoverishment of the tissues and blood in alkalies. In very severe cases of diabetes, the animal is not able to burn sugar and can burn fat and protein only as far as β-oxybutyric and diacetic acids and acetone, hence the tissues of the diabetic would become acid in reaction were it not for the fact that the acids formed in this disease are neutralized by the alkalies of the tissues. Since this neutralization leads to a depletion of the "alkaline reserves" of the body in severe cases of diabetes and since acidity of the tissues is incompatible with life, the animal dies. From the foregoing it is readily understood how the intravenous infusions of sodium bicarbonate are helpful in overcoming the coma of diabetes. Besides diabetes, it is known that acidosis occurs in "surgical shock," in anesthesia, and in starvation. It is also known that in these conditions oxidation is decreased and that the accumulation of the resulting incompletely oxidized substances, acid in nature, are responsible for the acidosis. The present investigation was carried out in an attempt to find an explanation for the decreased oxidation with resulting acidosis in the conditions mentioned.

Diabetes.—Pancreatic diabetes was produced in dogs by extirpating the pancreas. Sugar appeared in the urine a few hours after the operations. About two weeks later, when the animals were in a moribund condition, they were killed and the blood vessels were washed free of blood by the use of large quantities of 0.9 per cent. sodium chloride, as was indicated