NORMAN A. WOOD: 'Twenty-five Years of Bird Migration at Ann Arbor.'

NORMAN A. WOOD: 'The Bird Life of Ann Arbor, Michigan, and Vicinity.' (By title.)

E. H. FROTHINGHAM: 'Notes on the Birds of the Michigan Forest Reserve.'

R. A. BROWN: "A Topographical Study of the Birds of the 'Overflow,' at Ann Arbor, Mich."

CHAS. C. ADAMS: 'An Ecological Survey of Isle Royal, Lake Superior.'

OTTO MCCREARY: 'The Ecological Distribution of the Birds on Isle Royal.'

MAX M. PEET: 'The Fall Migration of Birds on Isle Royal.' (By title.)

Professor Walter B. Barrows, president of the academy and of the club, gave his presidential address before the academy, on 'Facts and Fancies in Bird Migration' in the new lecture room of the physical laboratory on Thursday evening.

A business meeting was held in the afternoon in the office of the curator of the university museum. The following officers were elected for 1906–7.

President—Walter B. Barrows, Agricultural College.

First Vice-president—J. Claire Wood, Detroit.

Second Vice-president—Edward Arnold, Battle Creek.

Third Vice-president—Norman A. Wood, Ann Arbor.

Secretary—Alexander W. Blain, Jr., Detroit.

Treasurer—Frederick C. Hubel, Detroit.

Editor of the Bulletin—Walter B. Barrows.

Associate Editors—Wm. H. Dunham, Kalkaska; R. A. Brown, Kalamazoo.

The meeting adjourned to meet at the Detroit Museum of Art on May 4, 1906.

ALEXANDER W. BLAIN, JR.,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE FALLACY OF THE MUTATION THEORY.

DR. C. H. MERRIAM has lately pointed out[1] that *mutation* in de Vries's sense is not a species-forming factor, and that it is rarely, if at all, observed among living animals. Major T. L. Casey objects[2] to this sweeping condemnation of de Vries's theory, and believes that there 'may be a good deal' in the latter.

I only can endorse Merriam's view, and want to go on record as condemning even more emphatically the mutation theory for the following reasons:

De Vries claims that the process of mutation forms new species, and that the individual mutations (mutants) *are* species. In order to demonstrate this, he has made a number of experiments, in which he tries to show that the mutations breed true, and he uses this fact as a *test* for the specific value of the mutations. No other test is admitted, or even mentioned, by him.

This shows at a glance that de Vries's conception of the term species is all wrong, that he does not know what constitutes a species, in spite of his lengthy discussion of this term. Of course, it is generally admitted that species should breed true: but this is also a necessary character that belongs to the concept of variety. What distinguishes species from varieties is the fact that a species is not connected by intermediate or transitional forms with the most closely allied species. This latter principle is the one made use of exclusively (if possible) by systematists, botanists as well as zoologists. In many cases, indeed, it can not be used on account of the insufficiency of our knowledge; but under such conditions new species are always described with the tacit understanding that the demonstration of the existence of intermediate forms will reduce them to the rank of varieties.

De Vries has failed entirely to take notice of this fundamental principle, and to show that his elementary species and his mutations are *not* connected by intermediate forms with each other. But looking over the instances introduced by him, we see that such intermediate forms are recorded by de Vries himself, and I know from personal experience that such are present among several of the polymorphous genera mentioned by him (*Viola, Draba*).

Further, according to the experimental records on *Œnothera,* given by de Vries, I can not see how he is in a position to main-

[1] SCIENCE, February 16, 1906, p. 241, chiefly pp. 256 and 257.

[2] SCIENCE, April 20, 1906, p. 632.

tain that the mutations have bred true. They surely did not do this in the beginning of the experiments, since they were throwing off, in each generation, additional mutants, and it was only after some time that de Vries succeeded in obtaining a relatively pure strain.

Consequently, de Vries's contention that mutations are species is not supported at all by his experiments; whatever they are, *they are not species, since they do not show the characteristic features of such.*

However, if de Vries had claimed that species might be made out of mutations, nothing could be objected to this view; but this is no new idea. Similar experiments have been undertaken by animal and plant breeders, and a large number are on record. In fact, the breeding of domestic races has always been regarded as a process analogous to the one in nature by which new species are produced. But the main features of this process in nature as well as under domestication are *selection* and *segregation.* This is exactly what de Vries has done with his mutations: he selected and segregated them (preventing crossing), and thus he imitated nature's way, and finally obtained more or less pure strains, which are analogous to natural species. But before he began this process of selecting and segregating, the mutations were by no means species, but only varieties.

Aside from the above claim, de Vries further maintains that it is the mutations, and not the variations, that give origin to new species, and he thinks that there is a fundamental difference between them. However, I have been unable to see where he draws the line between variations, constituting small steps, and mutations representing sudden leaps, and I do not think that he has solved the old sophistic problem of how much must be added to a small thing in order to make it a large one. His discussion of unit-characters does not offer any help in this respect, since in many cases he confesses himself that he does not know what should be regarded as a unit-character.

Mutations are by no means as frequent as de Vries would fain make us believe. He concedes himself that he had considerable trouble in finding a fit object for his experiments, and, indeed, among living animals and plants in the wild state, mutations in de Vries's sense are extremely rare, and in this respect I agree entirely with Merriam's contention, not only with reference to animals, but also to plants. True mutations, that is to say, variations which represent sudden leaps, are found chiefly among domestic forms, and this fact, I think, is well established; and the form that finally furnished the material for de Vries's experiments, *Œnothera lamarckiana,* is a domesticated, a garden form, and not a native species of Europe. It is true, it lately has become a habit with some biologists to hunt for mutations in nature, but the search has been quite unsuccessful, for the so-called mutations in part do not at all represent sudden leaps; in part it was not considered worth while to investigate whether the sudden leaps discovered were connected with the original form by transitions or not.

Paleontological evidence for the former existence of mutations should be excluded from the beginning, since it is in the very nature of paleontological facts to be fragmentary, and, in this connection, it is well to call attention to the former use of the term *mutation* by paleontologists (Waagen, Neumayr, W. B. Scott); it means just the opposite of de Vries's mutation, namely, a change during phylogenetic development, which is characterized by slow, small, almost insensible steps. For this we possess *positive* proof.

Thus we arrive at the conclusion that de Vries has not made good his claim that mutations are species, since his conception of species is defective. If he should change his view, and claim that species could be made out of mutations, he would be right, but then it is the selection, and chiefly the *segregation,* that has this effect; and further, this would be no new theory. If he claims that it is mutation as distinguished from variation that starts the species-forming process, we must point out that mutations are rare in nature, that there is no sharp line to be drawn between mutation and variation, and that mutation has always been regarded as a special form of variation (sporting, halmatogenesis). Consequently, nothing is left of de Vries's mutation theory but the bare facts represented by his experiments, which, indeed, are valuable for the study of variation, but belong to a class that was already known to Darwin when he wrote his 'Origin of Species' and 'Variation under Domestication.' For the rest, I do not see that there is anything in the mutation theory which might advance our general knowledge of the factors cooperating in evolution.

A. E. ORTMANN.
CARNEGIE MUSEUM, PITTSBURG, PA.,
April 26, 1906.

MISREPRESENTATIONS OF NATURE IN POPULAR MAGAZINES.

FROM the numerous and conspicuous mistakes made by the popular magazines when treating of geographical and geological subjects it would appear that there is occasion for more careful editing by men conversant with scientific affairs.

Many of the mistakes are more than simply inaccuracies of statement or occasional exaggeration. They are often the most conspicuous thing in the magazine.

Take, for example, the finely colored full-page picture in the *Century* (Vol. XLVII., p. 553) entitled 'Sulphur Deposits at the Crater Vesuvius.' The fact is that there are no sulphur deposits at Vesuvius. Not only are there no deposits, but even a trace of sulphur is difficult to find. Unless the volcano changes its chemistry to accord with the *Century* there will be none from this last eruption. The artist evidently mistook the lava which had been bleached by chlorine to be sulphur; the editor allowed the mistake to pass; and all who gain their idea of Vesuvius from that source will have much to unlearn when they hear the facts.

The Outing Magazine, edited by men who have more than an indoor acquaintance with nature, begins this year with a frontispiece (January number) entitled 'Bridger was the first man to gaze on the Great Salt Lake' and represents Bridger standing on the shore while his horse, with nose deep in the lake, is eagerly drinking! We have seen many wonderful bronchos, but never one that could drink the water of Salt Lake.

A well-written article in *McClure's* (Vol. XXV., p. 504) is illustrated by many pictures of the Grand Cañon of the Colorado. The coloring was evidently done by one who had never seen the region. It entirely misrepresents the cañon and must have annoyed the artist. But even the drawing gives a wrong impression of the greatest of cañons, just as would a picture of Broadway or of State Street which represented the high buildings sloping towards each other across the street. There are no narrow gorges in the cañon such as those pictured. This style of illustration is a recurrence of the type of picture furnished by Egloffstein in 1857 for the Ives Report published by the United States government. It was hoped that misrepresentations of that character would end with that century.

Nature is as interesting and impressive as are exaggeration and misrepresentation. A picture may have the educational value of many pages of sentences, since it so readily catches the eye. Many people will see a picture, while few read the text. Consequently it is important that pictures should represent facts and it behooves the popular magazines to have not only careful literary, but scientific editing as well.

A. R. CROOK.

ALLUVIAL SLOPES.

ONE of the commonest topographic features of the western part of the United States, particularly of the arid west, is the characteristic sloping plain which fringes the flanks of the mountain ranges and is formed by coalescent alluvial fans. Many terms have been used to denote this sloping plain, among which are: alluvial slope, alluvial apron, alluvial piedmont plain, compound alluvial fan, wash apron, débris apron, detrital slope, wash plain, out-wash plain, foot slope, aggradation plain, boulder wash plain and others. It seems desirable that such a typical feature should bear a more specific appellation. The consensus of opinion of the geologists of the United