clay, stone or metal (gold and copper ornaments only). The great majority of the clay vessels were evidently intended for mortuary purposes only. They testify to a 'highly elaborated technique and cultivated taste,' but do not include any types that come up to the best there is in Chiriqui ceramics.

Two types of ornamentation are particularly noticeable: (1) Incised geometric designs; and (2) punctate knobs resembling raised tattoo marks, or scarifications. The author observes 'that certain classes of ornament seem to have been allotted to certain classes of vessels.'

The ancient Guëtares of Costa Rica seem to have excelled in the manufacture of large multicolored bowls, a number of which have been reproduced in color, thus adding attractiveness to what even without them would be a superb series of plates.

This large quarto volume is published at the sole expense of Mr. Åke Sjögren, who has also given the collection on a part of which the work is based, to the Royal Ethnological Museum in Stockholm.

GEORGE GRANT MACCURDY.
YALE UNIVERSITY MUSEUM,
NEW HAVEN, CONN.

SCIENTIFIC JOURNALS AND ARTICLES.

The Botanical Gazette for June contains the following papers: K. M. Wiegand publishes an account of his researches on the conditions of buds and twigs in winter, his observations leading to many conclusions entirely in variance with accepted notions. S. Yamanouchi publishes a preliminary account of his investigation of the cytology of *Polysiphonia violacea,* showing definitely the alternation of generations. H. F. Weiss describes in detail the structure and development of the bark in sassafras. E. J. Hill gives an account of the distribution and habits of the common oaks of the Lake region.

WE learn from *The Botanical Gazette* that a new journal, bearing the title *Annales de Biologie Lacustre,* is to be published under the editorship of Dr. Ernest Rousseau, with the cooperation of a large board of editors. The first fascicle is announced to contain 192 pages, with figures and maps. Publication is to be in German, English, French and Italian. Each volume will contain 400 to 500 pages, and the subscription price will be twenty to thirty francs. The address of the editor is Musée Royal d'Histoire Naturelle, rue Vautier, 31, Brussels.

Science Progress, published from 1894 to 1898 under the editorship of Professor Bretland Farmer and the general direction of Sir Henry Burdett, has been revived under the name *Science Progress in the Twentieth Century.* The editors are Dr. N. H. Alcock, lecturer on physiology at St. Mary's Hospital Medical School, and Mr. W. G. Freeman, F.R.S. The journal is published quarterly by Mr. John Murray. The contents of the first number are as follows: 'A Science of Commerce and some Prolegomena,' by W. J. Ashley; 'Chloroform a Poison,' by B. J. Collingwood, 'Physical Geography as an Educational Subject,' by J. E. Marr; 'On the Occurrence of Prussic Acid and its Derivatives in Plants,' by T. A. Henry; 'The Solvent Action of Roots upon the Soil Particles,' by A. D. Hall; 'Some Notable Instances of the Distribution of Injurious Insects by Artificial Means,' by Fred. V. Theobald; 'The Blood-Platelets,' by G. A. Buckmaster; 'Some Recent Progress in Chemical and Structural Crystallography,' by A. E. H. Tutton; 'The Geological Plans of some Australian Mining Fields,' by J. W. Gregory; 'The Corn Smuts and their Propagation,' by T. Johnson; 'Nehemiah Grew and the Study of Plant Anatomy,' by Agnes Robertson; and 'The Utilization of Proteids in the Animal,' by F. G. Hopkins.

DISCUSSION AND CORRESPONDENCE.

DE VRIES AND HIS CRITICS.

THE followers of Darwin in the early sixties had two difficult tasks to accomplish. One was to induce people to give the theory of natural selection an unbiased consideration; the other, and more difficult one, was to get them to understand it.

"I have often found the most extraordinary difficulty," wrote Darwin to Carpenter,[1] "in making able men understand at what I was

[1] 'Life and Letters,' Vol. II., p. 18.

driving." And as the display of mental density increased with time, he wrote to Hooker,[2] "I am inclined to give up the attempt as hopeless. Those who do not understand, it seems, can not be made to understand."

The scientific world has largely learned the first lesson—that of open-mindedness—but two recent articles in SCIENCE[3] suggest that we have not learned the other.

It is not my intention here to pose as an advocate of de Vries, nor as a self-appointed interpreter of the mutation theory. The writings of its author are too clear to need any additional elucidation. But it is important, before we discuss the theory, to be sure that we thoroughly understand it. A perusal of the articles just cited makes it difficult to believe that their authors have given the writings of de Vries a very thoughtful reading.

Calling attention at the outset to the importance of understanding the terms used by de Vries, the author of the first article says: "What we systematists have been in the habit of calling spontaneous variations or '*sports*' he calls '*mutations*.'" This is doubtless one of the commonest, and at the same time most fatal, errors to a correct understanding of mutation. 'Mutations' and 'sports' are by no means synonymous terms, as used by de Vries, and, after he has devoted several pages to carefully defining these terms, and to showing that, while mutations are a kind of 'sport,' *not all sports are mutations*, it seems difficult to understand how one who had gotten his notions of the theory at first hand could persist in the former loose usage of the terms. The distinction drawn by de Vries is clear. For example, he says,[4] "in order to avoid confusion as far as possible, with the least change in existing terminology, I shall use the term 'ever-sporting varieties' for such forms as are regularly propagated by seed, and of pure and not hybrid origin, but which sport in nearly every generation." Citing the striped variety of the larkspur as the first illustration, he continues: "Such deviations are usually called sports. But they occur yearly and regularly,"[5] and for this and other reasons they are not mutations.[6] Of these later.

Again our critic tells us that, "What we call *individual variations* he calls '*fluctuations*.'" And yet on page 77 of 'Species and Varieties' we read, "From a broad point of view, fluctuating variability falls under two heads. They obey quite the same laws and are, therefore, easily confused, but with respect to questions of heredity they should be carefully separated. They are designated by the terms individual and partial fluctuation." Individual variation and fluctuation, then, are not synonyms, for some fluctuations are 'partial' and not 'individual,' and this difference is explained and illustrated in the following twenty-three pages.[7]

On page 242[8] we are told that de Vries "appears to have been carried away with enthusiasm over his discovery and jumps to the conclusion that species in general originate by mutation—*and in no other way!*"[9]

'Species and Varieties' does not profess[10] to treat at length the problem of hybridization, yet lecture IX. deals with the subject, and on page 266 the author refers to the fact that "Kerner von Marilaun pointed out the fact long ago that many so-called species, of rare occurrence, may be considered to have originated by a cross," and nearly a score of well-authenticated illustrative examples follow, with the sanction of de Vries as to the hybrid origin of the species.

"And has any reason been brought forward to justify—much less necessitate—a change in this belief?"[11] (*i. e.*, that differences in char-

[2] *L. c.*, Vol. II., p. 109.

[3] Merriam, C. Hart, 'Is Mutation a Factor in the Evolution of the Higher Vertebrates?' SCIENCE, February 16, 1906. Ortmann, A. E., 'The Fallacy of the Mutation Theory,' SCIENCE, May 11, 1906.

[4] 'Species and Varieties,' p. 310.

[5] *L. c.*, p. 11.

[6] Mutations are distinguished from other kinds of sports, among other ways, by being 'of very rare occurrence.' *L. c.*, p. 191.

[7] See also pp. 190 and 191, and lecture XXVIII.

[8] Merriam, *l. c.*

[9] Italics mine.

[10] Cf. p. 250.

[11] Merriam, p. 247.

acter can be explained by the slow and gradual accumulation of individual variations). The whole book, 'Species and Varieties,' and the two German volumes of 'Die Mutationstheorie' at once furnish, to one who understands them aright, more than sufficient reason that the old theory must, to say the least, stand the test of the most rigid re-examination, and the crucial test of verification by experiment.

And what, pray, has 'the practically unanimous *belief* of zoologists and botanists the world over' to do with the merits of a new theory? Did they not practically all, previous to Lamarck, believe in special creation? "The old saying, *Vox populi, vox Dei,*" wrote Darwin,[12] "as every philosopher knows, can not be trusted in science."

In the same paragraph, this: '* * * a species appears [*sic*] to have arisen in a slightly different way,' etc. (Of course it is an elementary species for which such a claim is made.) Why question the veracity of the author of the mutation theory by using the word 'appears'? When, since the publication of Darwin's 'Origin,' has a scientific fact been supported by a greater amount of experimentation, more carefully and more fully recorded, and not by one man only, but by several workers?

And if the critic asks, "How does it follow that 'then in truth Darwinism can afford to lose the individual variations as a basis'?"[13] the answer is: because the bottom does not fall out of the theory by such a loss. Other material remains for natural selection to work on. 'If it can be proved that a man eats beefsteak for breakfast'[14] it does not follow that, 'of course he could not have eaten bread.' But, if it were demonstrated by reiterated experiments that bread can do for a man all that beefsteak does, then the man 'can afford' to do without the steak. Or, to make a more relevant application, some men may breakfast on steak, and others secure the same ends with bread alone.

Referring to his extended field studies of plants, the critic says, "These studies have convinced *me* that with plants, as with animals, the usual way in which new forms (subspecies and species) are produced is by the gradual progressive development of minute variations."[15] Several pages of 'Species and Varieties'[16] are devoted to examples illustrating that 'long-continued selection, alone, has absolutely no appreciable effect' in changing the inner nature of a species or of a race, whereas there is experimental evidence of another factor by means of which such a change is accomplished. The beauty of experiment is that it convinces *all,* because given results may be produced by all alike at will, and 'experiments are a repetition of things occurring in nature.'[17]

When one says he has 'passed in review more than a thousand species,' we are constrained to ask, 'which, of at least three or more commonly recognized kinds of species?' Obviously, for example, the intergrading species of a genus like *Cratægus* (hawthorn), would prove nothing germane to the subject. And if the best result of this laborious examination of species is, 'without finding a single one which *appears* to have originated in this way,'[18] one recalls the fascinating account in 'Species and Varieties'[18] of 'the first experimental mutation of a normal into a peloric race.'[19] "The step from the ordinary toadflax to the peloric form is short, and it *appears* [note the word] as if it might be produced by slow conversion."[20] What a fine pseudo series one might arrange with the normal *Linaria vulgaris* and the *L. vulgaris peloria* at the extremes, connected by the ordinary fluctuating peloric variations![21] The beauty of de Vries's method is that it is possible 'to arrange things so as to be present when nature produces * * * these rare changes.'[22] In this way it is *known* (not a matter of opinion)

[12] 'Origin,' 6th Am. ed., p. 143, 1883.

[13] Merriam, *l. c.*, p. 243.

[14] Merriam, *l. c.*, p. 243.

[15] Merriam, *l. c.*, p. 243.

[16] P. 790 *et seq.*

[17] 'Species and Varieties,' p. 430.

[18] P. 464.

[19] 'Species and Varieties,' p. 473.

[20] 'Species and Varieties,' p. 465. Italics mine.

[21] Cf. also 'Species and Varieties,' p. 249.

[22] 'Species and Varieties,' p. 465.

that "The mutation took place at once. * * * No intermediate steps were observed. * * * Not a single flower on the mutated plant reverted to the previous type."

"According to *degree* of development at the time of their first appearance they [variations] may be designated *ordinary individual variations,* or *sport variations.* This is an old story."[23] Exactly! The new story is de Vries's very careful distinction between different kinds of sports.

And when one reads that "his [de Vries's] studies of plants have been mainly with species as modified by man rather than with species in a state of nature,"[24] one can not help wondering if lectures II., XII., XVI., XX. and XXIII. have been read at all.

And again when it is stated that "As a matter of fact, subspecies in nature do not occupy the same ground with the parent form, but an adjacent area," one can only wonder why a statement that may be quite true of a given group of animals (*e. g.,* birds) is made so general as to include the entire plant kingdom. In what sense, we may ask, is the term subspecies used? If by it is meant geographic variety,[25] then, of course, groups that do not occupy the same region do not occupy the same region. But the term is not used in this sense in systematic botany.

And if the mutation theory seems to any one 'burdened' 'with the additional requirement that in giving off new forms the old is not altered,' relief will come to him by indulging in a little pedigree-culture experimentation, and then digesting what 'Species and Varieties' has to say about the destruction of the unfit and the survival of the fittest (elementary) species in 'intra-specific selection.'[26]

Can it be possible that, after reading 'Species and Varieties' and the larger German work, the only source of an adequate explanation of interspecific gaps is unlimited and favorable field experience, and unlimited experience in handling specimens, etc.?[27]

"Inasmuch as sudden or sport variations are exceedingly rare[28] while slight variations are exceedingly common, does it not follow that the vast majority of species must originate from slight variations?"[29] Possibly, if it can be shown that the origination of new species is an exceedingly common affair, and a matter of frequent observation. So far as the records show, no one, up to the time of de Vries, had ever knowingly observed the origin of a new species or natural variety.

And when we read that "One might spend a lifetime in studying animals and plants in the interior of almost any of the faunal [and 'floral,' we suppose is to be understood] areas without encountering transitional forms or intergrades,"[30] it almost seems as though we were reading de Vries. The mutation theory not only offers a possible explanation of this fact, but would even lead one to expect such a condition.

Some ten years before the publication of 'Species and Varieties' Bateson inquired,[31] "Is it not then possible that the discontinuity of species may be a consequence and expression of the discontinuity of variation?" and then added:

> Upon the received hypothesis it is supposed that variation is continuous and that the discontinuity of species results from the operation of selection. For reasons given above (pp. 15 and 16) there is an almost fatal objection in the way of this belief, and it can not be supposed that all variation is continuous and also that the discontinuity of species is the result of selection. With evidence of the discontinuity of variation this difficulty would be removed.
>
> It will be noted that it is impossible to suppose that the perfection of a variety, discontin-

[23] Merriam, *l. c.,* p. 245.

[24] Merriam, *l. c.,* p. 247.

[25] Cf. the map, p. 256.

[26] 'Species and Varieties,' pp. 741, 744, 749, 751, 800, 801, 802, 805, 825.

[27] Merriam, *l. c.,* p. 256.

[28] Ignoring again the continuous sporting of 'ever-sporting varieties,' and the distinctions between different kinds of sports.

[29] In the northern states white people are more common than black. Therefore, all the people in the northern states have originated from the whites. Q. E. D.

[30] Merriam, *l. c.,* p. 256.

[31] 'Materials for the Study of Variation,' p. 62.

uously and suddenly occurring, is the result of selection. * * * This consideration of course touches only the part that selection may have played in the first building up of the type and does not affect the view that the perpetuation of the type, once constituted, may have been achieved by selection.[32]

To say the least, the declaration that 'so far as known,' the theory of the origin of species by mutation is 'not applicable in the case of animals,' seems a rather arbitrary statement in the face of the mass of contrary evidence that exists in recent literature.[33]

Considering the volume of evidence in zoology[34] that 'distinct and perfect varieties may come into existence discontinuously,' the question forces itself, 'may not the discontinuity of species have had a similar origin?' At least the strong "presumption is created that the discontinuity of which species is an expression has its origin not in the environment," which is continuous, "nor in any phenomenon of adaptation, but in the intrinsic nature of organisms themselves, manifested in original discontinuity of variation."[35]

By a strange chance the article by Dr. Ortmann, in SCIENCE for May 11, 1906, immediately precedes one entitled 'Misrepresentations of Nature in Popular Magazines.' The history of science furnishes all too many instances of misinterpretations of *scientific theory* in *scientific* magazines. We do not recall an instance in which Darwinism (Darwin's Darwinism) has been more twisted out of shape than has the mutation theory in this latest exposure of its 'fallacy.' It would be interesting to know if the mutation theory itself would meet with such an 'emphatic' and wholesale 'condemnation' as does this misinterpretation of it.

We are told that "de Vries claims that the process of mutation forms new species, and that individual mutations (mutants) *are* species." The *title* of the English volume is 'Species *and Varieties,* their Origin by Mutation,' and one has to read only as far as the ninth page to learn that the author intends "to give a review of the facts obtained from plants which go to prove the assertion that species and varieties *have* originated by mutation, and are at present not *known* to originate in any other way," and on page 16 "Retrograde varieties and elementary [note the adjective] species may *both* be seen to be produced by sudden mutations."

And why beat about the bush and say that de Vries '*tries to* show that mutations breed true'? Why not frankly acknowledge the fact, so magnificently established by twenty odd years of painstaking experiment, and verified by other workers elsewhere, that the variations classed by de Vries as mutants do breed true?

If it is really true that de Vries 'does not know what constitutes a species,' then, indeed, do we find our faith in his work thereby increased. Who indeed, except the makers of dictionaries, does 'know what constitutes a species'? The author of the mutation theory does know, however, that 'genera and species are, at the present time, for a large part artificial, or stated more correctly, conventional groups,' and that, 'every systematist is free to delimit them in a wider or in a narrower sense, according to his judgment.'

Is it possible that one who can write, "What distinguishes species[36] from varieties is the fact that the species is not connected by intermediate or transitional forms with the closely allied species," has ever come into contact with a group like the hawthorns (*Cratægus*), or *Aster,* or the violets in botany, or the earwig (*Forficula*) in zoology? Whether this distinction 'is the one made use of exclusively (if possible)[37] by systematists, botanists as well as zoologists,' is, we believe, quite open to question. It was a consideration of the earwigs (*Forficula*) and crab (*Carcinus*) that led Vernon to state: "It is obvious, indeed, that between two absolutely dis-

[32] Bateson, *l. c.*, p. 69.

[33] *E. g.*, Bateson, *l. c.*, Vernon, 'Variation in Animals and Plants.'

[34] Space forbids citation of specific illustrations.

[35] Bateson, *l. c.*, p. 567.

[37] The meaning of the parenthesis is not clear.

[36] Meaning the systematic groups of specific value.

tinct varieties or species, and between pure monomorphic forms, all intermediate stages may exist."[38] Even Darwin considered that species, in the early stages of their history, were connected 'by intermediate gradations.'[39] Bicknell[40] has well said, 'The current ruling, more especially in vertebrate zoology, which, in view of * * * intermediates reduces to subspecies widely diverse organic forms, may well be suspected of being artificial and of attaching a fictitious importance to the mere evidences of origin which chance, perhaps, has allowed to remain unobliterated.'

Farlow concludes his paper on species by a sage remark: "I think we shall agree that in discussing the work of botanists in other departments than our own, it would not be wise to exact a rigid conformity to our individual conceptions of species, etc." It is another critic of mutation who tells us[41] that 'at the peripheries or borders' of faunal areas 'intergrades' between species[42] occur.

One who says that "de Vries has failed entirely to take notice of this fundamental [!] principle," surely can not have read that "new species and new varieties are seen to be quite free from their ancestors, and not linked to them by intermediates,"[43] and that the links between *elementary* species "often *apparently* overlap and can only in rare cases be determined on the sole ground of field observations," but that 'pedigree-culture is the method required,' etc.[44] Has the critic read: "Transitions are wholly wanting, although fallaciously apparent in some instances," as they appeared to be to Merriam,[45] "owing to the wide range of fluctuating variability of the forms concerned, or the occurrence of hybrids and subvarieties."[46]

If it is stated that de Vries "has failed * * * to show that his elementary species and his mutations" (*other* mutations, we suppose is meant) "are not connected by intermediate forms with each other," then we can not believe the writer has read the lucid definition: "any form which remains constant *and distinct from its allies* in the garden is to be considered as an elementary species,"[47] and that 'their limits often *apparently* overlap,' but may be determined by the method of pedigree-culture, though seldom 'on the sole ground of field observations.' The experimental facts were the source of the definition.

'Such intermediate forms are (indeed) recorded by de Vries himself,' together with his clear explanation of their significance. Bateson wrote over ten years ago: "We are concerned not with the question whether or no all intermediate gradations are possible or have ever existed, but with the wholly different question whether or no the normal form has passed through all these intermediate conditions."[48] One of the greatest values of de Vries's work lies in the fact that he was present when the transition took place, and gives, not a theory at all, but the record of a fact observed again and again.

If one is unable to see how de Vries can maintain that the mutations have bred true, as is stated on page 539 of 'Species and Varieties,' he has only to repeat the experiment himself to be convinced; and if it is implied, as it clearly is,[49] that *all* the mutants "were throwing off, in each generation, additional mutants," then the facts as recorded by de Vries are ignored. That mutation, however, may be a constant character,[50] just as truly as the shape of a leaf that constantly varies

[38] Vernon, *l. c.*, p. 41.

[39] Darwin, *l. c.*, p. 426.

[40] Torreya, **6**: 94, 1906. Cf. also SCIENCE, February 16, 1906, p. 257, where Cook reduces three species and three varieties of *Utethesia* to one species *because of the intergradations.*

[41] Merriam, *l. c.*, p. 256.

[42] Or should we say 'subspecies'?

[43] 'Species and Varieties,' p. 18. 'Elementary' species plainly from the context.

[44] 'Species and Varieties,' p. 18.

[45] Merriam, *l. c.*, p. 243.

[46] 'Species and Varieties,' p. 249.

[47] 'Species and Varieties,' p. 12.

[48] 'Materials for the Study of Variation,' p. 42.

[49] Ortmann, p. 747.

[50] During the mutation period of the species, if the theory of mutation periods ('Species and Varieties,' Lecture XIV.) shall finally become established, and excepting, of course, the 'stray mutations' ('Species and Varieties,' pp. 704–706).

(fluctuating variation), is clearly brought out by the author of the mutation theory. And if, in speaking of 'de Vries's contention that mutations are species,' one has in mind the units of the systematist, then it must be insisted that such is not de Vries's contention at all, nor can such a statement be wrested from the book, 'Species and Varieties,' if the context is always considered.

"However, if de Vries had claimed that species might be made out of mutations, nothing could be objected to this view."[51] Exactly! Then why the dissenting critique? Read, on page 13 of 'Species and Varieties,' the following:

Linnæus himself knew, that in some cases all subdivisions of a species are of equal rank, together constituting the group called species. No one of them outranks the others; it is not a species with varieties, but a group consisting only of varieties. A closer inquiry into the cases treated in this manner by the great master of systematic science, shows that here his varieties were exactly what we now call elementary species.

And this on page 558: *"The first law [of mutation] is, that new elementary species appear suddenly, without intermediate steps."*

Finally (page 459), "Hence we have distinguished between elementary species and varieties proper. THE FIRST ARE COMBINED INTO SPECIES" (of the systematist), etc. Could a clearer statement be conceived?

There is, indeed, a sense in which this is 'no new idea,'[52] but the recognition of it, and of its bearing upon theories of descent seems to be not only new, but difficult at the present day of being clearly recognized and understood. De Vries does not claim the credit of originating the idea. He proposes to found his theory, in part, upon "a critical survey of the facts of agricultural and horticultural breeding, as they have accumulated since the time of Darwin."[53]

Says our critic: "The breeding of domestic races has always (!!) been regarded as a process analogous to the one in nature by which new species are produced." When, we may be allowed to ask, previous to the appearance of the 'Origin,' had such an idea been seriously or at all generally held. If we have not access to the original, we may even learn from 'Species and Varieties,' if we read carefully, that Linnæus looked upon species as the result of special creation.[54]

The failure to take some of his statements at their face value, or at least to distinguish clearly when he is recording a fact and when he is elaborating a theory, is a peculiar feature of the criticisms of de Vries, as in the present critique, where it is stated that he 'finally obtained *more or less* pure strains.'[55] An equally peculiar and persistent feature of these criticisms is the insistence with which the author's carefully defined terms, representing equally careful and long-needed distinctions, are entirely ignored, thus giving to his statements a wholly different color than they possess in the original. *E. g.*, "Before he [de Vries] began this process of selecting and segregating, the mutations were by no means species, but only varieties." Where, from cover to cover, of 'Species and Varieties,' is any other claim made? The point is that the author very carefully states *what kind* of varieties they are, and applies to this kind the term 'elementary-species.'

Thus again, in the next sentence, "de Vries further maintains that it is the mutations and not the variations, that give rise to new species, and he thinks [*sic*] that there is a fundamental difference between them." A clear conception of the mutation theory would have resulted in some such changes as the following in that sentence: "de Vries further *gives experimental evidence* that it is the mutations, *and not the other types of variation,* that give rise to new species, and *between these kinds of variation* there is a fundamental difference." The critic might have added here, also, the statement, 'but this no new idea,' and might have quoted from Bateson:

The existence of discontinuity in variation is, therefore, a final proof that the accepted hypothesis is inadequate. If the evidence went no further than this the result would be of use, though

[51] Ortmann, p. 747.

[52] Ortmann, *l. c.*, p. 747.

[53] 'Species and Varieties,' p. 9.

[54] 'Species and Varieties,' p. 34.

[55] Ortmann, *l. c.*, p. 747. Italics mine.

its use would be rather to destroy than to build up. But besides this negative result there is a positive result too, and the same discontinuity which in the old structure had no place may be made the framework round which a new structure may be built.[56]

And the following cautious statement from Darwin: 'We are led to conclude that species have *generally* originated by the natural selection, not of abrupt modifications, but of extremely slight differences.'[57] Their origination by the natural selection of the 'abrupt modifications' is rejected, among other reasons, because "we have *no evidence* of the appearance, or at least of the continued procreation, under nature, of abrupt modifications of structure."[58] Since the work of de Vries the last statement no longer holds true.

The critic "is unable to see where he [de Vries] draws the line between variations * * * and mutations." The following quotation seems written in anticipation of that statement: "*The relation between mutability and fluctuating variability* has always been one of the chief difficulties of the followers of Darwin.[59] The majority assume that species arise by the slow accumulation of slight fluctuating deviations, and the mutations are only to be considered as extreme fluctuations, obtained, in the main, by a continuous selection of small differences in a constant direction."

"My cultures show that quite the opposite is the fact. * * * Oscillating changes have nothing in common with the mutations."[60] "Mutations are going on in all directions, producing, if they are progressive" (they are not all alike), "something quite new every time. Fluctuations are limited to increase and decrease of what is already available."[61]

One point is stated with terse accuracy: de Vries has not only not 'solved the old sophistic problem of how much must be added to a small thing to make it a large one,' but has clearly demonstrated, in his lecture on 'The Origin of the Peloric Toad-Flax' that, so far as the origin of species is concerned, the problem is incapable of solution.

The discoverer of 'the fallacy of the mutation theory' tells us that mutations have been observed chiefly among domestic forms. If this were true, the suggestion is, not to reject mutation on this account, but to initiate extensive experiments among a wide systematic range of wild plants (and of animals also), and see if what now appears to be the case is in reality a general truth, or only an expression of limited experience.

We are told[62] that if de Vries should 'claim that species could be made out of mutations, he would be right.' Well and good. The quotation[63] has already been given. Elementary species 'are combined into species * * *.'

If the rarity of mutations[64] seems to be a stumbling-block toward accepting them as the material on which natural selection may operate, it will be wholesome to recall that the formation of new specific groups (species of the systematists) is far more rare in nature than in the writings of systematic botanists and zoologists.

Mutation has, indeed, 'always'[65] been regarded as a special form of variation, and so it is in 'Species and Varieties,' notably in lecture I., and throughout the entire book. But if 'Consequently, nothing is left of de Vries's mutation theory but the bare facts represented by his experiments,' science has been munificently enriched thereby, to say nothing of the new method of research in the study of evolution, entirely ignored by the above statement, and the contribution for which science is de Vries's greatest debtor.

"To my mind," says de Vries, "the real value of the discovery of the mutability of the evening-primrose lies in its usefulness as a guide for further work."

I may repeat, in closing, what was stated in

[56] Bateson, *l. c.*, p. 568, 1894.

[57] 'Animals and Plants Under Domestication,' p. 495. Italics mine.

[58] Darwin, *l. c.*, p. 495.

[59] 'Species and Varieties,' p. 7. Even Wallace had this difficulty. *Ibid.*

[60] 'Species and Varieties,' pp. 568–569. Cf. pp. 715, 718 and 459.

[61] 'Species and Varieties,' p. 719.

[62] Ortmann, *l. c.*, p. 747.

[63] From 'Species and Varieties,' p. 459.

[64] Ortmann, *l. c.*, p. 747.

[65] That is, since the process has been recognized and described.

the beginning. I have not attempted to defend the mutation theory of de Vries, but only to emphasize the fact that, before we criticize it, or lend to it either our dissent or assent, we must first understand it. The ink that was spilled in discussing misinterpretations of Darwinism far exceeds that poured out in recording constructive studies in evolution. Let us not make the same mistake and waste of energy in the present case.

The mutation 'theory' is still largely a working hypothesis. It is founded almost entirely upon experiment, and can be verified only by the same means. The beauty of it is that it is already reduced to a question of fact. For mere opinion and inference, and *a priori* impressions and prejudice, and inductions from field studies and comparative morphology there is absolutely no place. If one doubts the facts, let him repeat the experiments of de Vries and MacDougal and others. If he doubts that they represent a general truth, a fundamental principle in biology, then let him await the fullness of time, for it is by repeated experiment, among a wide range of groups, and *by experiment only,* that the general application must stand or fall.

And I bespeak also a candid acceptance of the facts, after they are clearly distinguished from the inferences. The latter are open to debate, but not so the former. And when a careful worker says that he obtained a given form that breeds absolutely true, and which, for reasons fully explained, he calls an 'elementary species,' by means of a certain definite and clearly explained kind of variation which he defines and names 'mutation,' let us not refer to him as 'claiming to' have done so, or to the mutant as 'seeming to' breed true.

Pregnant with significance as the mutation theory is for the systematic botanist and zoologist, its truth can never be established nor disproved by the methods of taxonomy. Comparative studies may offer worlds of evidence and multitudes of problems to test the hypothesis, but experimentation is the only possible means for the final solution.

How do species originate? A mass of facts suggests that the method is by the natural selection of fluctuating variations, combined with geographical isolation, influence of environment, and other factors. But, after all has been written, the undeniable fact remains that no one has yet ever actually observed the origin of a single species in this way.

On the other hand, the fact is just as undeniable that a definite and clearly defined type of variation, called an 'elementary species,' has been actually observed, not once, but often and by many, to arise by a process, equally well defined and definite, and known as 'mutation.' Mutations do furnish material for the operation of natural selection and all other influences that tend to establish a unit group known in taxonomy as a 'species.'

The case seems perfectly plain that the burden of proof rests with the adherents of fluctuation.

C. STUART GAGER.
NEW YORK BOTANICAL GARDEN.

SPECIAL ARTICLES.

A NEW FOSSIL SEAL FROM THE MARINE MIOCENE OF THE OREGON COAST REGION.

IN a bulletin recently issued by the Oregon State University, Professor Thomas Condon has given a description of an unusually interesting fossil pinniped, which was obtained by him from the Marine Miocene of the Oregon coast. It is, indeed, a happy coincidence that this nestor of Oregon geology should celebrate his eighty-fourth birthday by so interesting and important a discovery. This does not quite equal the record of the great chemist, Chevreul, before the French Academy, it is true, but it is one sufficiently rare in paleontology.

Professor Condon has kindly permitted me to make a careful study of this unique specimen, and I do not hesitate to pronounce it easily the most important find that has yet been made in this group. As far as I am aware the specimen represents an entirely new and hitherto unknown genus, intermediate in many respects between the sea lions and seals, with perhaps the most pronounced affinities with the latter, and at the same time exhibit-