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THE WORK OF HUGO DE VRIES AND ITS IMPORTANCE IN THE STUDY OF PROBLEMS OF EVOLUTION.[1]

As Professor Osborn, in his 'From the Greeks to Darwin,' has given an account of the development of the theory of evolution in Europe, there is no necessity for me to repeat its history. However, I should like to remark in passing, that the speculations of the oriental philosophers, especially the early Hindoos, have not received in the occident the attention due them, and to express the hope that some scholar will present to us of the occident the results of the thinking of the eastern sages on these problems.

That existing species of animals and plants have been derived from preceding species by natural processes is now universally believed by biologists. The man most potent in establishing this belief on a firm foundation was Darwin. Here it is unnecessary to do more than allude to his doctrine of the struggle for existence and the survival of the fittest by means of natural selection. Although I shall not present the data that he accumulated, I desire

[1] An address delivered before the Biological Society of Washington, on February 3, 1906.

especially to call your attention to the great number of instances of the utility of structures and adaptation to environment adduced by him. There is a mass of evidence showing that organisms possess useful structures and are more or less in harmony with their environment. I, therefore, assume both the theory of evolution and that organisms are more or less perfectly adapted to their environment.

There are three hypotheses to account for the evolution of new species and their perpetuation. Each one of the hypotheses recognizes the struggle for existence and the influence of natural selection.

The first hypothesis is considered peculiarly the Darwinian, to which Weismann has added his theoretical supplement. According to this hypothesis, animals and plants are subject to fluctuating variations of small amount. Those individuals whose variation gives them an advantage over their competitors for life are preserved, producing a gradual amelioration. This process is continued until ultimately the descendants differ so widely from their progenitors that they are referred to a separate species. The Weismannian supplement denied the hereditary transmissibility of characters impressed on the individual by its environment, so that the adjustment of organisms to their environment would depend entirely upon the preservation of favorable fortuitous variations.

The second hypothesis, usually known as the neo-Lamarckian, called the dynamic by Dall, and that of direct causation by Nägeli, for many years the rival of the one just stated, undertakes to account for the phenomena of adaptation by assuming that the organisms are directly molded into harmony with their environment by external forces, or that adaptation may originate through conscious effort, and that changes in individuals so brought about are transmitted to their offspring. The principal defenders of this hypothesis in this country were Cope and Hyatt. Dall, although he proposed the term dynamic, I believe, never accepted Cope's hypothesis of archæsthetism, an idea first advanced by Lamarck.

The third hypothesis is that of de Vries. He admits the struggle for existence, and recognizes fluctuating variation. He, however, contends that this kind of variation will not give rise to a new species, basing this conclusion on a large amount of experimental data obtained from the breeding of plants. According to him all that can be done by selecting the best individuals is to ameliorate the race to a certain point, beyond which no progress can be made, and that so soon as the process of continued selection is abated, regression toward the average variation of the species or variety ensues. De Vries also denies the permanency of the effect of natural selection on the fluctuating variations of a species. Natural selection, he claims, can not accumulate variations of this type beyond a certain limit; and that if individuals of such a naturally ameliorated race be transferred to another area they will regress toward the mean characters of the species. Natural selection has preserved certain individuals of the species, but has not really changed it. From a study of the mode of the appearance of the permanent 'varieties' or 'species' of cultivated plants, de Vries was led to believe that new species do not originate either by the gradual elimination of unfavorable fluctuating variation or by the direct modifying influence of environment, but by sudden mutation—the offspring differing from their parents by distinct lacunæ, breeding true and showing no tendency to revert to the parental form. His acquaintance with cultivated plants led him, as was stated, to this conclusion. He then conceived the idea of searching in nature for a species in a state of mutation. The story of his discovery of mutating individuals of Lamarck's evening primrose, *Œnothera lamarckiana,* in the vicinity of Hilversum, Holland, need not be repeated. From specimens of this species he obtained by pedigree cultures seven new forms, six of which he has described as new species and one as a new variety. Besides this experimental evidence in favor of his hypothesis, de Vries compiled a large amount of data that gives additional weight to his conclusion.

These are the three principal hypotheses attempting to account for the origin of new species. I think that it is not necessary to give a special discussion of isolation as a factor in evolution, as it does not affect the validity or invalidity of any one of the hypotheses stated. Isolation is a passive, not an active factor; its importance, however, is beyond question.

On the leaf following the title page of 'Species and Varieties, their Origin by Mutation,' are three quotations:

'The origin of species is a natural phenomenon.'—Lamarck.

'The origin of species is an object of inquiry.'—Darwin.

'The origin of species is an object of experimental investigation.'—De Vries.

The history of any movement of thought is always from a greater or less indefiniteness to greater definiteness and precision. The solution of any complicated problem must be preceded by the analytical work that discovers the factors involved.

The following will indicate the tendencies and stages of such a movement:

1. The accumulation of data, largely through mere curiosity.

2. An attempt to discover some causal relation underlying the phenomena, and the propounding of an hypothesis.

3. The energetic accumulation of additional data, especially to controvert or sustain the hypothesis already propounded.

4. A critical reexamination of the accumulated data to discover if they are susceptible of a different interpretation, or if there may not be some previously undiscovered underlying principle.

5. The announcement of a different interpretation or a new principle will produce additional activity both in the accumulation of data and in the attempts to interpret them.

6. There will be recurrent periods of the accumulation of facts with reference to old theories, and a continual critical reexamination both of facts and of theories. These will lead to a more highly developed critical faculty and more refined methods of research.

Previous to Lamarck there were a number of zoologists and botanists energetically recording their observations, especially as a result of the classification and system of nomenclature proposed by Linnæus. Lamarck proposed the theory of evolution that has been revived by the neo-Lamarckian school. Darwin correlated the data previously accumulated, supplemented by a stupendous number of observations of his own, and gave us his 'Origin of Species.' The accumulation of data has continued, and the critical examination of previously announced conclusions becomes more acute. With this more highly developed critical faculty have come additional methods of investigating the problems of evolution.

De Vries's book, 'Species and Varieties, their Origin by Mutation,' contains not only a vast body of highly important facts, but is pregnant from cover to cover with suggestions regarding important lines of needed research. This volume is divided into six principal sections:

I. *The Introduction.*—This section contains a brief review of the leading theories of evolution and a statement of his own method of studying the problem.

II. *Elementary Species.*—According to de Vries the species of systematists are not single groups of individuals intergrading among themselves and separated by lacunæ from neighboring groups, but are aggregates of such units. He, therefore, speaks of 'systematic species' and 'elementary species,' an 'elementary species' being one of the units that go to make up the 'systematic species.' I think it very unfortunate that de Vries has introduced the term 'elementary species' as opposed to 'systematic species.' It clearly shows that he is not in touch with refined modern systematic work, for his 'elementary species' is the systematic species of all modern systematists with whose work I am familiar, while his 'systematic species' has no status. However, his account of how species established by the older systematists are now being subdivided into smaller and smaller units as our studies become more detailed and more exact, is very interesting. He discusses elementary species not only in nature, but also among cultivated plants, and gives valuable information regarding the mode of their selection among the latter, especially wheat.

III. *Retrograde Varieties.*—I think de Vries again unfortunate in his attempt to define a variety as an elementary species that has lost a character. According to the usually accepted definition of variety in this country there is intergradation with the typical form of the species: a variety would be represented by a secondary mode in the species curve. The inference from de Vries's definition would be that we can not have retrogressive species. It is well known in paleontology that not only single species, but whole groups of animals have undergone retrogressive evolution—the Baculites, for instance. In this section de Vries brings out a very important distinction between two classes of phenomena that have been characterized as atavism. Real atavism is defined as the reappearance of ancestral characters in a species that is pure bred. The other phenomenon, and the one that is usually designated atavism, is considered false atavism by him, and the name 'vicinism' applied to it. 'Vicinism' is due to the crossing of species or varieties. A plant the result of such a cross, though apparently a pure strain, may produce offspring of the type of the ancestor that it least resembles. This section contains much information on crosses, including a statement of Mendel's law.

IV. *Ever-sporting Varieties.*—The term 'ever-sporting variety' is used for those forms regularly propagated by seed, of pure, not hybrid, origin, but which 'sport in nearly every generation.' An interesting account is given of how he tried to obtain a pure striped and a pure red variety from a striped variety of snapdragon, known as *Antirrhinum majus luteum rubro-striatum,* which sports into red flowers, but he was not successful. The seed of red individuals, or even seed from self-pollinated red flowers in a raceme of mostly striped flowers, would produce a certain number of striped flowers. The seed from striped individuals always gave a percentage of red ones. It is interesting to note that stocks producing double flowers are grown from single-flowered individuals. These experiments are so interesting, and so suggestive of further research, that it is a great temptation to give a detailed account of them, and also to describe the experiments in trying to get a race of five-leaved clover, and those with polycephalic poppies and monstrosities. His experiments with double adaptations are immediately germane to the subject under discussion, and they can be more fully described. But before proceeding to a discussion of them, the factors that lead to the phenomena of ever-sporting should be analyzed. According to de Vries,

> The wide range of variability of ever-sporting varieties is due to the presence of two antagonistic characters which can not be evolved at the same time and in the same organ, because they exclude one another. Whenever one is active, the other must be latent. But latency is not absolute inactivity and may often only operate to encumber the evolution of the antagonistic character, and to produce large numbers of lesser grades.

On a subsequent page, he says of the characters:

> They might be termed alternating, if it were only understood that the alternation may be complete or incomplete in all degrees. Complete alternation would result in the extremes, the incomplete condition in the intermediate states. In some cases, as with the stocks, the first prevails, while in other cases, as with the poppies, the very extremes are only rarely met with.

De Vries says:

> Taking such an alternation as a real character of the ever-sporting varieties, a wide range of analogous cases is at once revealed among the normal qualities of wild plants. Alternation is here almost universal. It is the capacity of young organs to develop in two divergent directions.

These phenomena are illustrated by numerous illustrations drawn from those presented by wild plants. The water-persicaria, *Polygonum amphibium,* is the one first cited. This plant occurs in two forms, one aquatic and the other terrestrial. The aquatic plants, known as var. *natans,* "have floating or submerged stems with oblong or elliptical leaves, which are glabrous and have long petioles. The terrestrial plants (known as var. *terrestre* or *terrestris*) are erect, nearly simple, more or less hispid throughout, with lanceolate leaves and short petioles, often nearly sessile." These "two varieties may often be seen to sport into one another. They are only branches of the same stem grown under different conditions."

Numerous other instances of double adaptation are given. Those taken from alpine plants transferred to the lowland are in some respects the most interesting for our discussion. De Vries says: "It is simply impossible to decide concerning the real relations between the alpine and lowland types without experiments." Some experiments are given by which the factor determining the change in character was discovered.

In concluding his remarks on these phenomena, the statement is made:

> Useful dimorphism or double adaptation, is a substitution of characters quite analogous to the useless dimorphism of cultivated ever-sporting varieties and the stray occurrences of hereditary monstrosities. The same laws and conditions prevail in both cases.

Interjected into this chapter is a consideration of the 'Theory of Direct Causation,' first advanced by Lamarck, subsequently advocated by Nägeli, von Wettstein, Strasburger and other German botanists, also by Hyatt, Cope and others in this country. The instances of double adaptation, of course, do not support this theory. De Vries gives an account of some plants that grow in the Desert of Kaits, Ceylon. These plants, although they have apparently grown in the desert for many centuries, have not become of the desert type, still possessing a thin epidermis and exposed stomata; and were shown by Holtermann to lose the only desert character that they had, their dwarf stature, when grown on ordinary garden soil. These plants disprove the Nägelian contentions: (1) That extreme conditions change organisms in a desirable direction; (2) that the only change induced by the dry soil, decreased stature, was not hereditary.

V. *Mutations.*—Under this heading are

included the chapters treating of de Vries's work on the production of the peloric toad flax, double flowers and new species and varieties of *Œnothera*. He also presents his views, with his seven laws, on 'the origin of wild species and varieties.' The last four chapters are entitled Mutations in Horticulture, Systematic Atavism, Taxonomic Anomalies and Periodic Mutations. Interesting data are presented in each one of these chapters. The phenomena of mutation in horticulture and taxonomic anomalies are in general in line with the general thesis being defended.

The idea that the species are constant through extended periods of time, following which is a period of mutation with the production of new forms, those best accommodated to the environment surviving, is important.

Dall proposed the term saltatory evolution in 1877, and suggested periodic mutation. His conclusions, however, did not rest on observed experiments.

VI. The last section of the book deals with 'fluctuations.' An account is given of the means of the statistical study of variation. Quetelet's law is stated, etc. In this section it is contended that fluctuating variation does not exceed certain definite limits, and that in cultivation, without the appearance of desirable mutations, it is not possible to ameliorate a species beyond a fixed degree. Ameliorated races, without continual selection, regress toward the mean of the species. In the last chapter of the book it is maintained that natural selection can do no more toward the creation of new species by an accumulation of fluctuating variation than can artificial selection.

From this very defective review of de Vries's work, it will be seen that he has investigated a wide range of phenomena. His method has mostly been by experiment; his results are such as to compel a critical reexamination of the views current on the process of evolution. I shall hastily criticize the three theories stated in the beginning of this discussion.

1. *The Darwinian Hypothesis.*—According to researches into the variability of organisms, fluctuating variation is around a mean and never transgresses certain limits. It is not possible to ameliorate a particular species beyond a fixed degree, and it is, therefore, impossible radically to change it. If continued artificial selection is not practised, the ameliorated race regresses toward the mean of the species. It is contended by de Vries that the same is as true of wild species as of those in cultivation. If the criticism can be sustained, this hypothesis must be abandoned.

2. *The Dynamical Hypothesis.*—Dall, in his paper entitled 'On Dynamic Influences in Evolution,' said:

> Passing from these general considerations to those of a more special character, the contention of Weismann that 'not a single fact hitherto brought forward can be accepted as proof' of the transmission of acquired characters demands attention.

In reply he says:

> If the dynamic evolutionist brings forward an hypothesis which explains the facts of nature without violence to sound reasoning, that hypothesis is entitled to respect and consideration until some better one is proposed or some vitiating error detected.

Some years ago, while a student at Harvard, I had the opportunity to attend a symposium on the hereditary transmission of acquired characters. The principals in the discussion were Professor Poulton, of Oxford, and Professor Alpheus Hyatt. The data presented by Professor Hyatt were subsequently published in his memoir entitled 'The Phylogeny of an Acquired Characteristic.' After the discussion, my conclusion was that Professor Poulton had shown that all experimental evidence was against the transmission of acquired characteristics; while Professor Hyatt produced no direct evidence to sustain his contention. What he did, was to show that a body of facts were in harmony with the *assumption* that acquired characteristics were inheritable, but his facts were as fully in accord with the *assumption* of Professor Poulton. As Professor Poulton had a certain amount of experimental evidence and Professor Hyatt had none, I thought that Professor Poulton had the better of the argument, but he did not show that acquired characters could not be inherited.

Since hearing the discussion between those eminent men, I have made an effort to go over the arguments for and against the 'Theory of Direct Causation.' I have read much or most of what Spencer, Cope, Hyatt and Dall have written on the subject. Dall has stated the proposition fairly and unequivocally. The facts that they present can be explained on their fundamental assumption, but they produce no direct evidence that that assumption is correct. Nägeli, von Wettstein and Strasburger represent in botany what Cope, Hyatt and Dall represent in zoology. As has already been stated, de Vries has collated a mass of evidence, all of which is against the views held by Nägeli among the botanists. The positive evidence is against the 'dynamic theory' or the 'theory of direct causation.'

But I wish to repeat the words of Dall:

> If the dynamical evolutionist brings forward an hypothesis which explains the facts of nature without violence to sound reasoning, that hypothesis is entitled to respect and consideration until some better one is proposed or some vitiating error detected.

It should also be remarked, that while Weismann denies the inheritance of functional variation, causing atrophy or hypertrophy of a part, he admits that climate may produce hereditary changes by acting on the germ-plasm. He, however, does not commit himself to the belief that such does occur.

I should like to enter into a full discussion of the effects of temperature on the pupæ of *Polyommatus phlœas* in influencing the color of the wings of the adult, but time does not permit. However, according to Weismann there is a critical period, after which the raising or lowering of the temperature does not affect the wing color of the adult. Weismann has pointed out that in southern Europe the golden-winged spring brood is derived from the pupæ of the dark-winged summer brood; while the dark-winged summer brood is derived from the golden-winged spring brood. An increase in temperature does affect the wing coloration, but there is no evidence to show that a permanent hereditary change is wrought. In fact the evidence is contrary to such an assumption. The period of sensitiveness to temperature in these butterflies is comparable to the period of sensitiveness discovered by de Vries in developing polycephalic poppies. Although the polycephalic poppies fluctuate in the number of converted stamens from almost 0 to 150 and over, de Vries found no instance of heads without indications of pistillody of the stamens, and in no instance were all the stamens transformed into pistils. The relative number of converted stamens, however, is largely determined by physical conditions; abundance of plant food and a sunny exposure are essential for the best results. The point of similarity between these experiments is that de Vries discovered in developing poppies and Weismann in developing *Polyommatus* a period of sensitiveness to external conditions. After this period is passed, varying physical conditions do not affect the fully developed adult.

Weismann's butterflies belong in de Vries's category of 'ever-sporting varieties,' and are comparable to the water persicaria and polycephalic poppies.

Weismann speaks of Nägeli's experiments on *Hieracium*. He says:

> Many climatic varieties of plants may also be due wholly or in part to the simultaneous variation of corresponding determinants in some part of the soma and in the germ-plasm of the reproductive cells, and these variations must of necessity be hereditary. Temperature, and nutrition in its widest sense, affect the whole body of the plant —the somatic as well as the germ-cells.

De Vries shows that the species of *Hieracium* studied by Nägeli exhibit the phenomena of double adaptation. There is no evidence that this attribute is originated by the direct influence of physical environment.

Although we have ample grounds for doubting the validity of the assumption of the adherents to the dynamic theory, we can not yet refuse their hypothesis respectful consideration.

3. *The de Vries Mutation Hypothesis.*—This hypothesis rests upon a negative and a positive basis. The former is the negation of the ability of the two preceding hypotheses to account for the origin of species, affirming that fluctuating variation is only between definite limits with reference to a mean, and that environment does not directly modify species. The positive element is the observation of new forms arising from older ones by mutation. Each of these conclusions of de Vries is open to challenge. (1) Have sufficient data been accumulated to justify our discarding the hypothesis that new species may originate by the gradual accumulation of variations that tend in a certain direction? (2) Is the evidence submitted sufficient to warrant the permanent rejection of the dynamic hypothesis? (3) Are his supposed mutations really mutations? The parentage of his *Œnothera lamarckiana* is not known. May not his new *Œnotheræ* be hybrids of some kind?

These different hypotheses present different explanations of phenomena assumed to be true by each one. I think that they render necessary a more critical analysis of the biological facts cited to substantiate each one.

At the last meeting of this society Dr. Merriam presented a paper, 'Is Mutation a Factor in Evolution?' His facts were that various regions are inhabited by subspecies of mammals or birds in accordance with their differences in physical conditions, and that the transition zone between two regions is occupied by intergrading forms. Take, for instance, two adjacent areas presenting different physico-geographic characters: one subspecies would be found in one area; in the other area, another subspecies. The physical conditions in going from one area to the other do not change abruptly, but gradually. The intermediate zone is not only intermediate in physical characters, but is occupied by individuals that are intermediate in their characters between the subspecies of the two different areas. As Dr. Merriam quoted the hammer and anvil simile of Dall, we are, I think, justified in placing him in the category of the dynamic evolutionists. His conclusions were:

1. There is evidence of the intergradation between species.

2. The direct influence of environment is the principal factor in the production of new species.

3. In the higher vertebrates there is no evidence of the origin of species by 'mutation.'

As to Dr. Merriam's statements regarding the classification and distribution of the animals discussed in his communication, we can say nothing, for all of us know of the many years that he has spent collecting and studying them and plotting their distribution with reference to geographic conditions. But I think his explanation of the phenomena open to question.

I will admit that Dr. Merriam's explanation of his facts may be true, but he did not convince me of its correctness any more than Professor Hyatt convinced me of the correctness of his interpretation in his 'Phylogeny of an Acquired Characteristic.' His facts seem just as plausibly explicable on the basis of the Darwinian hypothesis or that of de Vries. According to the former those variations tending to give the species an advantage in the struggle for its life would be preserved, while other variations would be eliminated. This preservation of certain individuals and the elimination of others would cause divergence in the characters of the occupants of the respective areas. In the intermediate zone, as there would not be definite selection, there would not be distinct differentiation of type.

The de Vries hypothesis will explain them just as well. The forms occupying the respective areas may have originated by mutation, and the intermediate zone may be occupied by hybrids.

The evidence in favor of none of these hypotheses is conclusive.

The facts presented by Dr. Merriam are a necessary foundation for the recognition of the factors involved in the problem, but they do not solve the problem. I should like to know:

1. Something concerning the stability of the characters of the forms inhabiting the different areas.

In this connection the following questions may be asked:

(*a*) Is the difference observed between the individuals occupying the different areas caused by the direct influence of physical environment? If the difference is caused by such influence, is the change so wrought only superficial or is it hereditarily transmissible? The feathers of birds exposed to strong sunlight are of lighter color than those of birds living in areas in which the light is weaker. From characters of this kind we can infer that the specimens exhibiting them lived under certain conditions. Do changes of this kind extend to the gametes of the individual or are they only somatic changes, enabling us to infer that an individual lived under certain physical conditions, similar to the inference drawn from seeing a man with certain scars on his face, viz., that he has attended a German university?

Dr. Dall, in his review of Gulick's 'Evolution, Racial and Habitudinal,' says concerning the theory of segregation advocated by that author:

> To justify final acceptation an hypothesis must not only be capable of accounting for the facts, but it must be shown to be the only one by which they may be adequately explained. It is also necessary to determine how far the animals in question have arrived at that state of equilibrium which we recognize by the name of species. If, as has been held by some authorities, the small color groups are really only of a temporary nature, and liable to immediate change upon subjection to modified environment, then the author's hypothesis, while losing none of its truth, is not a contribution to the evolution of species so much as to the physiology of color variation.

(*b*) Should the differences be gametic in origin, *i. e.*, not induced by the physical environment, is the selection between divergent variations of one species; or is it between two different species?

I could present a series of observed phenomena in the Madreporaria parallel to some of the data presented by Dr. Merriam in his discussion of the distribution of mammals and birds. These instances could be drawn from several genera, but those from *Turbinaria* are especially *à propos*. Mr. Pace has carefully studied these corals in the Torres Straits; he, however, discretely remarks:

> It will now be my endeavor to show that the variations of a turbinarian colony from the primitive cup-shape—the 'crateriform' type of Bernard—can be readily explained by reference to the

> conditions under which the coral has grown; though it by no means follows that heredity plays no part in determining the form of the growth assumed by the corallum under any particular conditions, and it may well be that the *tendency* toward one type rather than another is inherited; *this, however, can only be established by experiment.*

I have italicized the words: 'this, however, can only be established by experiment.'

2. Are the intermediate specimens in the intermediate area actually intermediate in character or are they hybrids?

The following known occurrence of hybridization taken from de Vries suggests that a similar phenomenon might occur in the intermediate areas described by Dr. Merriam. *Rhododendron intermedium* is an intermediate form between the hairy and the rusty species from the Swiss Alps, *R. hirsutum* and *R. ferrugineum,* the former growing on chalky, and the other on siliceous soils. Whenever these types of soil occur in the same valley and these two species approach one another, the hybrid *R. intermedium* is produced, and is often seen to be propagating itself abundantly. As is indicated by the name, it combines the essential characters of both parents.

De Vries says:

> It is not to be forgotten, however, that all taxonomic distinctions, which have not been confirmed by physiological tests are only provisional, a view acknowledged by the best systematists. Of course the description of newly discovered forms can not await the results of physiological inquiries, but it is absolutely impossible to reach definite conclusions on purely morphological evidence. This is well illustrated by the numerous discords of opinion of different authors on the systematic worth of many forms.

Until various physiological tests of the kind referred to by de Vries have been made, more than an hypothetical explanation of the facts presented by Dr. Merriam is impossible.

I now wish to reiterate my opinion as to the importance of the work of de Vries. The great value of his work consists in having shown that 'The origin of species is an object of experimental investigation,' and having furnished guidance not only as to *what* experiments should be made, but as to *how* they should be made.

Davenport in his last report to the president of the Carnegie Institution says:

> The factors of evolution are three—variation, inheritance and adjustment. Studies may be made on any one of these factors or all three together; as a matter of fact, they can hardly be studied wholly independently.

The discussion to follow will cover in its range each of these factors.

As I have opened the discussion it might be expected that I should furnish specific data bearing upon these questions. I can furnish instances that I have gleaned from the writings of de Vries, Weismann and others, and those recently published in Science, but all of these rightfully belong to others; I have, however, cited some of them. Out of my own studies I can produce evidence in favor of the general theory of evolution, I can present phylogenies of genera and species that, I think, will stand the test of rigid criticism, I can furnish examples of the adaptation of structures, I can also show instances of variations in accordance with varying physical conditions, but I do not know a single fact relating to the Madreporaria that would aid in forming a definite conclusion regarding the origin of variation or the means by which adjustment is affected—I repeat, 'or the means by which adjustment is affected,' for the expression 'natural selection' is mostly used to raise a cloud of mental dust behind which we escape into our ignorance.

I should like to say that the controlling influences that govern the distribution of corals are being studied as assiduously as possible by several men, and the subject has been given a certain amount of attention by nearly all recent students of zoophytes. We are obtaining more information on the physical determinants in the distribution of these organisms, but no one will be able to furnish more than an hypothetical explanation of the facts now accumulating until the conclusions are tested by experiments. Corals that grow in shallow water are fortunately easily experimented with, and I have hoped that the officials of the Carnegie Institution might undertake some work with them. Dr. C. Montague Cooke, of Honolulu, has told me that he intends undertaking a series of experiments on the reefs on the south coast of the Island of Molokai. Probably within a few years it will be possible to present definite data from the *Madrepora* on the questions now especially under consideration.

T. Wayland Vaughan.

SCIENTIFIC BOOKS.

Les tremblements de terre. Par F. de Montessus de Ballore. Paris, Libraire Armand Colin.

In Vol. IV., 1900, of *Beiträge für Geophysik,* Major de Montessus published a tabular statement of the seismicity of the various portions of the earth, divided into provinces. In the computation, 131,922 earthquakes were used and 10,499 epicenters; numbers far exceeding what had been compiled by preceding systematists taken all together. It was the work of many years, and from the mass of evidence distributively grouped he drew certain important conclusions. They were briefly as follows: (1) In a group of adjacent seismic regions, the most unstable (*i. e.,* most affected by quakes) are those which present the greatest differences of topographic relief. (2) The unstable regions are associated with the great lines of corrugation of the terrestrial crust. (3) Rapidly deepening littorals, especially if they border important mountain ranges, are unstable, while gently sloping littorals are stable, especially if they are the continuations of flat or slightly accidented coastal plains. (4) Though it is possible to indicate regions which present both volcanoes and earthquakes, there is no proof of interdependence between seismicity and volcanicity in general. While there are earthquakes which are certainly of volcanic origin, the one phenomenon does not necessarily imply the other. These views have been borne out and have been generally adopted by seismologists in the period of six years since they were promulgated.

But de Montessus seems to have been unwilling to let the matter rest. The inferences he drew in 1900, indeed, have not been abandoned. They, however, express the relations of seismicity to topography, and not to the causes of earthquakes, which were the real objects of his grand research. He has, therefore, taken up the subject anew, rearranged his facts, added to their number and made new generalizations from a geological as well as a topographical standpoint. And the new generalizations are of even greater interest and more striking than those of 1900. These are set forth briefly in the introductory chapter of the publication before us.

According to this analysis, earthquakes occur about equally, and almost exclusively, in two great circles or zones, which make with each other an angle of 67°. These zones are (1) the Mediterranean, or Alpine-Caucasian-Himalayan, which includes 52.57 per cent. of the quakes, and (2) the circum-Pacific Andean-Japanese-Malayan, which includes 38.51 per cent. of the quakes. These two zones coincide with the two most important lines of relief of the earth's surface. The poles of these great circles are situated 45° 45′ N., 150° 30′ W., and 35° 40′ N., 23° 10′ E., respectively.

This relation, which so far is purely geometric, calls for a geological interpretation, which may be read at once on the geological map of the world. The zones which include the seismic regions coincide exactly with the geosynclinals of the mesozoic age as they are figured by Haug in his well-known memoir, 'The Geosynclinals and the Continental