with alkali and methyl iodide, the N-methyl derivative resulted in every case. With ethyl iodide, both O- and N-derivatives were obtained, while with the higher iodides the O-compound was the chief product. The pure N-alkyl compounds were prepared from the acyl anthranils, and the pure O-compounds from the corresponding chlorine derivatives and sodium alcholates. A large number of isomers were prepared and examined, both of nitrated and unnitrated quinazolines.

F. H. POUGH,
Secretary.

THE TORREY BOTANICAL CLUB.

THE meeting of May 8, 1906, was held at the American Museum of Natural History at 8 P.M., with President Rusby in the chair.

The scientific program was an illustrated lecture by Dr. Grace E. Cooley on 'Forestry.'

The lecture considered the relation of forests and forest products to man, and the consequent importance of an intelligent comprehension of the principles and economic bearings of forestry. The nature of various important species of trees was treated of from the standpoint of silviculture, treating the tree as an individual plant; forestry, considering tree groups, or forests; physiography, discussing the relation of trees to the landscape and physiographic processes, and also from the point of view of economics and esthetics. The historical development of the U. S. Bureau of Forestry was briefly traced from the early beginning when a few interested persons met regularly at the home of Mr. Gifford Pinchot for discussion and instruction until the present organization of the national forest service. Forestry in other countries was also alluded to, and its long recognition and advanced stage of perfection abroad, standing in contrast to its rather tardy development in the United States.

C. STUART GAGER,
Secretary.

THE CALIFORNIA BRANCH OF THE AMERICAN FOLK-LORE SOCIETY.

THE eighth meeting of the California Branch of the American Folk-Lore Society was held at Cloyne Court, Berkeley, Tuesday, April 17, 1906, at 8 P.M. Mr. Charles Keeler presided. The minutes of the last meeting were read and approved. Dr. J. W. Hudson, having been approved by the council, was elected to membership in the society. On motion, Charles Keeler, A. H. Allen and P. E. Goddard, previously appointed by the Berkeley Folk-Lore Club as a committee to report on the feasibility of making a special study of the folk-lore of Berkeley and vicinity, were elected to represent the California Branch and to secure the cooperation of the two societies in the undertaking. A report reviewing the work of the society during the first year of its activity, which closed with this meeting, was read by the secretary. Dr. H. du R. Phelan, captain U. S. Volunteers, gave the address of the evening on 'The Peoples of the Philippine Islands,' based on a sojourn of several years in different parts of the archipelago and illustrated with numerous ethnological specimens. At its conclusion Dr. Phelan's talk was discussed by the members. The acting president thereupon announced the conclusion of the first year of the society's existence and the meeting was adjourned. Forty-five persons attended the meeting.

A. L. KROEBER,
Secretary.

DISCUSSION AND CORRESPONDENCE.

FACTS AND THEORIES IN EVOLUTION.

WITH reference to the writings of Weismann, I wrote in 1896,[1] that he has constantly mixed up the origin of species and variations, and the origin of adaptive characters. This holds good also at the present time, and may be said of other writers. The confusion is partly due to Darwin's phrase: origin of species, which was intended to include the whole process of evolution; but we must bear in mind that the latter is composed of several distinct processes.

In a recent article in SCIENCE,[2] Dr. F. Wayland Vaughan gives a review of de Vries's

[1] *Pr. Am. Philos. Soc.*, 35, 1896, p. 191.

[2] SCIENCE, May 4, 1906, p. 681.

mutation theory, and although, in general, his remarks and criticism appear to me well supported, he does not emphasize enough the fact that de Vries has entirely wrong ideas with regard to the process of species-making (*speciation*), and that he confounds it with *variation*. Indeed, Vaughan points out (p. 684) that de Vries's conception of species (elementary species) is inadequate; but he fails to see that this is a vital part of the mutation theory, and that the latter stands and falls with it.

In addition, I should like to express here a few opinions, which differ slightly from those set forth by Vaughan, and which I shall try to substantiate in the following paragraphs. The *first* is, that I think the theories of Darwin and of Weismann to be fundamentally different, Weismann always having incorrectly understood Darwin's view; thus it is impossible to regard the theory of Weismann as a kind of an amendment to that of Darwin, and to oppose both to the Lamarckian view; the *second* is, that I believe that the inheritance of acquired characters is an assumption that is 'entitled to respect and consideration' (Dall) not only because it is apt to explain certain facts, but chiefly so because it is the only theory that is based upon sound philosophical principles, the alternative theory being logically deficient. Besides, there is a *third* point, to which I object, namely, that Vaughan claims that 'the great value of de Vries's work consists in having shown that the origin of species is an object of experimental investigation.' I do not need to discuss this here again, since I have shown lately[3] that de Vries's experiments have no relation at all to the making of species (speciation), but only to the question of variation, and that they belong to a class of experiments that was known long ago.

I. The Darwinian theory has always been misinterpreted by Weismann in so far as he claimed that the emphasis laid by him upon natural selection, the 'all-sufficiency' of the latter, is the original Darwinian idea. But a perusal of Darwin's writings shows that, although he emphasizes natural selection as a *new* principle discovered by himself, he does not mean to say that it is the *only* factor in evolution.[4] This is seen at once by the fact that three chapters (1, 2, and 5) of the 'Origin of Species' are devoted to another factor, variation, while the struggle for life and natural selection are treated in the chapters 3 and 4; and on p. 100 ('Origin of Species'), at the end of the fourth chapter, Darwin condenses his ideas upon half a page in a summary, mentioning *three* factors: *variation; struggle for life* (resulting in *natural selection*); and *inheritance.*

I have shown previously[5] that Darwin also perceived that another question was to be settled, that of the differentiation into species (*speciation*); but with regard to this his ideas were somewhat hazy ('Origin of Species,' chapters 12 and 13). In my opinion, this point in Darwin's theory is the one that needed further elucidation, and this lack has been supplemented by M. Wagner by his separation theory.

That Darwin has been correctly understood by others in so far as it was seen that evolution is influenced by different, independent factors, is clearly shown by the exposition of his views as given, for instance, by Haeckel. I remember well, almost a quarter of a century ago, when I attended Haeckel's lectures on general zoology, that he made it a special point to bring home the idea that evolution as a general process in nature is not a *theory,* but a *logical deduction* from three well-established *facts.* The same view is found in Haeckel's 'Natuerliche Schoepfungsgeschichte' (3d ed., 1872), where he mentions (p. 139) *inheritance* (Erblichkeit) and *variation* (Veraenderlichkeit)[6] as the fundamental properties of the organisms, to which should be added Darwin's principle of the *struggle for life* (p. 144).

The same *three* factors in evolution are mentioned by Davenport (quoted by Vaughan, *l. c.*, p. 690) as: *variation, inheritance* and

[3] SCIENCE, May 11, 1906, p. 746.

[4] See Ortmann in *Pr. Am. Philos. Soc.*, 35, 1896, p. 187, 190.

[5] *Ibid.*, p. 182.

[6] Haeckel uses variation and adaptation as synonyms (see *l. c.*, p. 197), which should be borne in mind.

adjustment, and it is probably better to use the latter word (or *adaptation*), if we want to emphasize that these factors are *empirical facts;* adjustment is a fact directly observed in nature, while the struggle for life is an inference drawn from other observations.

I am prepared to accept this view in its full meaning, namely, that we have to deal here with *facts, which may be observed in nature,* and the *logical consequence* of the operation of these facts is *evolution,* that is to say, the change of the organic world, or its transmutation. But this does not exhaust all the existing phenomena, for we observe in nature a *fourth fact,* namely, that the chain of organisms is cut up in species. This we may call, with O. F. Cook, *speciation,* and thus we obtain altogether *four facts: variation, inheritance, adjustment, speciation.* These four facts would satisfactorily explain the whole of the organic world, if the *causes* of each of them were known: the process of evolution, consequently, is undeniable, and our investigations should be conducted so as *to discover the causes of each of the main factors in evolution.* As we shall see presently, the discussion in evolution, and the differences of opinion have hinged chiefly upon this question of the causes of these facts, and while in two of them the causes are very clear, in the other two they are much disputed.

It is the chief shortcoming of some of the modern writers, for instance Weismann and de Vries, that they are oblivious of this fundamental idea of evolution, and the consequence has been an utter confusion in their views. For me it is simply past comprehension, how it was possible that the writings of Weismann and de Vries have come to be looked upon favorably, and to be regarded as worthy of serious consideration.

I have always regarded *segregation* (*isolation, separation*), as introduced by M. Wagner, as the *cause of speciation.* This is, in my opinion, the most vital improvement upon Darwin's theory, and it is not opposed to it, but rather an amendment or addition to it. In this line, I have done some work myself, chiefly by trying to show the real extent of the term segregation (Gulick). I shall not go into detail here,[7] and only want to point out that I consider speciation as fully explained by biological segregation. The latter is a *fact* which, although it has not been demonstrated in all cases, is now supported by a sufficient number of actual observations, and what is most important, a case that is opposed to it has never been found, namely, that two closely allied species occupy absolutely the same range under identical ecological conditions. Many other writers concur with me on this point, and I name, aside from M. Wagner, J. T. Gulick, G. Baur, D. S. Jordan, J. A. Allen, C. H. Merriam.

As the *causes of adjustment,* we are to regard the *struggle for existence* and *natural selection* consequent to it. Vaughan (*l. c.,* p. 690) objects to the use of 'natural selection,' and possibly rightly so, considering how this term has been abused, preeminently on the part of Weismann. But the real value, and the correct conception of natural selection has been indicated by G. Pfeffer in a paper[8] which generally seems to have escaped attention. If we use natural selection in Pfeffer's sense (not as the survival of the fittest, but as the survival of fit individuals), I do not see why this term should be objected to or discarded. The struggle for life, which causes natural selection, and consequently adaptation or adjustment, is a logical deduction from observations in nature, for we always see that more individuals are produced than finally can find place in the economy of nature. This has been amply demonstrated by Darwin and others, and thus the causes of adjustment are

[7] See my publications: Grundzüge der marinen Tiergeographie, Jena, 1896, p. 33. On Separation, and its bearing on Geology and Zoogeography (*Amer. Journ. Sci.,* 2, 1896, p. 63). On Natural Selection and Separation (*Pr. Amer. Philos. Soc.,* 35, 1896, p. 182). Isolation as one of the factors in Evolution (SCIENCE, January 12, 1906). A Case of Isolation without Barriers (SCIENCE, March 30, 1906).

[8] 'Die Umwandlung der Arten, ein Vorgang functioneller Selbstgestaltung' (*Verhandl. Naturwiss. Ver. Hamburg* (3), 1, 1894).

to be considered as well known, being represented by indisputable *facts*.

Inheritance is a *fact* which can not be denied, but the *causes* of inheritance are unknown. However, we possess *theories* with regard to it, one of which is Weismann's germ-plasm theory. I am not going to discuss this here. The latest investigations on the minute processes in fertilization, as well as experiments on heredity, go far to advance our knowledge as to the causes of inheritance, but at present it is impossible to say to what end they finally may lead.

Variation is antagonistic to inheritance, and is also a *fact*. For a long time its *cause* seemed to be plain, and Darwin held the opinion that it is due to *changes of environment,* and he believed at the same time that changes thus produced might become hereditary ('Origin of Species,' in the very beginning of chapter 1, p. 5; further on, p. 8, and then again in chapter 5, p. 103). In this respect Darwin was entirely upon the standpoint of Lamarck, who was the first to express the idea of evolution in consequence of inheritance of acquired characters, chiefly by use and nonuse (here we have the recognition of *two* principles: inheritance and variation). Later, a different opinion began to prevail, namely, that acquired characters, such as are due to external stimuli, are not transmitted, and that only *variations of another class, which have a different cause,* are inherited. These are the so-called 'spontaneous,' 'germinal' or 'congenital' variations. This view was chiefly defended by Weismann, although he was not the first to propose it. Finally, de Vries maintains that it is *mutation,* and not variation that is inherited,[9] or more correctly that it is only a *certain form of variation* that is transmissible (connected with the species-making process), namely, that which is represented by sudden leaps.

Thus we see that the main dispute was with reference to the *causes of variation,* and we can distinguish *three* chief theories, which do not entirely correspond to the scheme given by Vaughan.

1. Dynamic theory (Dall). Evolution is started by variation due to external stimuli; these variations are transmissible to the offspring.

In this general view, we have to distinguish a development in four steps, each representing an improvement upon the older ideas, but not being contrary to them.

(*a*) Lamarckian view: two factors are recognized—variation and inheritance. Variations are called adaptations.

(*b*) Darwinian view: three factors are recognized—variation, inheritance and natural selection (struggle for life). Variations are not always adaptations, but may be disadvantageous. The struggle for life disposes of them. A fourth factor (speciation) is also indicated by Darwin, but not clearly recognized.

(*c*) Wagnerian view: addition of the fourth factor segregation (separation) as producing speciation.

(*d*) Pfeffer's correction of Darwin's conception of natural selection.

2. The view that not all variations are caused by external stimuli, and that not all variations are transmissible, but only those that are due to 'inner' causes. This view was held formerly by Weismann, but is now abandoned by him practically, although not professedly. This view is at present often called the Darwinian hypothesis, but wrongly so.

3. The view of de Vries. He also contends that only a certain class of variations is transmissible, that is to say, may start the formation of new species. This class is what he calls mutations. As to the causes of mutation de Vries is noncommittal.

I, for my part, accept the dynamic theory with all its amendments. I decline to consider the two other views, the third for reasons set forth recently.[10] I have also given my reasons for rejecting Weismann's views,[11] but it might be well to condense here again, why I believe that the theory of the transmission of acquired characters possesses a better title

[9] This is all that remains of de Vries's views after they have been stripped of their most obvious fallacies.

[10] SCIENCE, May 11, 1906.

[11] *Biolog. Centralblatt*, 18, 1898, p. 139 ff.

to respect and consideration than that of Weismann.

II. Vaughan claims that there is no experimental evidence for the transmission of acquired characters. This is not so, there *is* evidence. For instance, the experiments of Weismann with *Polyommatus phlœas,* quoted by Vaughan, *are* evidence, when properly interpreted.

With reference to the latter, I have said:[12]

What the Lamarck-Darwinian theory maintains is that external stimuli acting upon an individual may produce changes in its characters, and that these changes are transmissible, *i. e.*, may reappear in subsequent generations. But this is now exactly the view of Weismann. To quote his example, in the butterfly *Polyommatus phlaeas,* increased temperature (external stimulus) effects darker color (change of character), and Weismann further believes that this character (dark color) may reappear in subsequent generations in consequence of the increase of temperature.

For the *hereditary transmission* of such acquired characters Weismann has his own theory, *but this theory does not deal any more with origin of transmissible variations, but is a theory of inheritance* (*l. c.,* p. 155).

I think this settles the point: we see that characters reappear in the offspring that have been acquired by the parents. Observations to this effect are known, and, furthermore, I believe that *all* variations are due to external stimuli, and that *there are no variations due to so-called inner causes alone.* For there is a grave logical error in the latter assumption (*l. c.,* p. 144). The conception of spontaneous variation implies that a certain class of causes does not act in variation, namely, the causæ efficientes. Now, every process in nature must have three kinds of causes: causæ materiales, causæ efficientes, and causæ finales. The exclusion of the second class, while only the first and third are admitted, renders this assumption illogical: we need a causa efficiens, or external stimulus. That is to say, no germinal or spontaneous variation is thinkable, unless there is an external stimulus. Each and every variation must be consequent on an external stimulus, which necessarily precedes it in time.

An objection often made in cases where the transmission of acquired characters seems probable is that the acquired character again disappears in subsequent generations, after the external influence has ceased, that is to say, that the variations revert to the original form. Of course, this should happen. As I understand the dynamic theory, its claim is that external influences permanently change organisms only when they remain permanent in their action, and that it takes time, and, if the expression is permitted, effort on the part of the environment to render any change more or less stable. But just this latter effect is due to inheritance, and repeated inheritance only is able to fix a character to such a degree, that it in turn obtains the necessary inertia to be classed with the stable, that is to say, inherited, characters, which offer a certain resistance to additional changes of environment. In this respect, J. A. Allen's remarks are pertinent,[13] where he emphasizes the *simultaneous and permanent action of external conditions upon large numbers of individuals.* A change in the external conditions must act upon a multitude of animals, and they all must vary, and if they are more or less uniform in organization, they must vary in the same or a similar direction. This is the real starting point for any transformation that is to become permanent. I do not believe that in nature single chance variations (due to unusual stimuli acting but once) ever become the parents of a similarly changed offspring, but I think it is always a large number of specimens, in fact practically all that live under the changed environment that begin to vary: the environment simply forces them to do so. This fact, and we have evidence for it (see Allen, *l. c.*), goes far to furnish direct proof for the action of external stimuli in variation, and the phrase 'pressure of environment' introduced by C. H. Merriam[14] for this fact, the permanent and irresistible application of certain external forces upon a multitude of organisms, expresses this identical view. This pressure, generally, does not stop again after

[12] *Biolog. Centralblatt,* 18, 1898, p. 153.

[13] SCIENCE, November 24, 1905, p. 667.

[14] SCIENCE, February 16, 1906, p. 244.

it has once begun, and thus a permanent change is brought about. If we consider this, then the objection that sometimes the changes of the organisms have disappeared after the normal conditions had been reestablished, does not hold good; in fact, this was to be expected (compare Naegeli's experiments with *Hieracium;* also de Vries's experiments furnish examples).

This way of looking upon the 'pressure of environment,' as producing a certain tendency to vary in a definite direction, easily explains it that we *have* evidence of definite variation. M. M. Metcalf[15] is inclined to believe that such instances are in favor of the assumption of the action of inner causes; but I do not see why this should be so. A repeated or constant action of the same external stimulus should produce in any organic form the tendency to react upon this stimulus in a definite way. This has been called *orthogenesis* by Eimer. Such cases are known, and I do not hesitate to attribute them to a permanent action of the same external force upon a multitude of individuals. Of course, as soon as this process is well started, inheritance begins also to play a part, and it is this latter factor that finally firmly establishes the new characters.

As to the value of experiments in the study of variation, I want to call attention to the difficulty in interpreting the facts, when such experiments are made under artificial and unnatural conditions, as, for instance, in the botanical garden, or with domesticated forms. Here it is apparent that such a complexity prevails, not only a few, but a large number of conditions being different from those in nature, that the experiment becomes a bewildering maze. In my opinion, experiments should be made in close touch with nature, changing, if possible, only one or a few of the conditions, so that we may be able to record the effects of each single changed factor in the environment. But I do not believe that this is an easy task. On the other hand, we should bear in mind that nature has made and is making these experiments for us: the process of variation is going on continuously, and the effects of former variation are seen in nature, and may be studied in the shape of the actually existing variations, varieties and species, and their relation to the environment (ecology). This work naturally falls within the scope of the systematist, and is largely field work; specimens of this kind of work have been furnished by Merriam, Allen and others, and the modern ecological researches are just what is wanted. But we must confess that so far we have only the beginning of this study, which should be encouraged and enlarged. For *ecology* teaches us what the different types of environment are, and how the different elements in the environment affect each other, and how changes of environment may effect changes in the organization of the different forms of life dependent on it.

A. E. Ortmann.

Carnegie Museum, Pittsburg, Pa.,
May 28, 1906.

[15] Science, May 18, 1906, p. 787.

SPECIAL ARTICLES.

CORPUSCULAR RADIATION FROM COSMICAL SOURCES.

In my address[1] before the Physical Society, I gave an account of observations made several times daily since May 9, 1905, in a search for the possible occurrence of an ultramundane radiation. The work was there summarized as follows:

> Using the most sensitive condensation method, *i. e.*, that depending on the depression of the limiting asymptote of non-energized, dust-free air, no change of the quality of scrupulously filtered atmospheric air has thus far been detected. * * * Naturally (ions) would vanish during the slow passage of air through the filter, but fresh ions should be reproduced within the fog chamber by the same agency which generates them without * * *. Probably, therefore, the coronal method is as yet inadequately sensitive to cope with the variations of the small nucleations specified.

The ions, which are relatively large nuclei, withdraw much of the available moisture which would otherwise be precipitated on the colloidal nuclei of dust-free air. Hence the size of the terminal corona is diminished.

[1] *Physical Review*, XXII., p. 105, 1905; also p. 109 on 'radiant fields.'