

cision thus gained and the natural character of the classification proposed were pointed out.

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THE AMERICAN CHEMICAL SOCIETY. NEW YORK  
SECTION

THE third regular meeting of the season of 1906-7 was held at the Chemists' Club, 108 W. 55th Street, on January 11.

The Nichols medal, awarded annually for the best paper read before the New York Section, was presented to Howard B. Bishop for his paper 'On the Estimation of Minute Quantities of Arsenic.' Favorable mention was made of the paper of E. H. Miller and J. F. Thompson on the 'Silver Platinum Alloys' and of the papers of F. B. Power and Frank Tutin on the 'Chemical Examination of *Æthusa Cynapium*' and on the 'Chemical and Physiological Examination of *Chaillitia Toxicaria*.'

The rest of the evening was devoted to a symposium on the pure-food law by Messrs. H. W. Wiley (address read by chairman), Virgil Coblentz, R. Z. Doolittle and M. D. Foster. Further discussion followed, in which Messrs. Wm. J. Schieffelin, Albert Plaut, J. B. F. Herreshoff and L. L. Watters took part.

C. M. JOYCE,  
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#### DISCUSSION AND CORRESPONDENCE

##### FACTS AND INTERPRETATIONS IN THE MUTATION THEORY

THE foremost champion of de Vries's mutation theory in this country undoubtedly is Dr. D. T. MacDougal, and he has largely contributed to the popularity of this theory. In a recent article<sup>1</sup> he takes up certain objections made by various writers, and attempts to show that they are without foundations or opposed to the known facts. But the criticism of the objections made by C. H. Merriam, D. S. Jordan and the present writer fails to convince, and only serves to demonstrate that the vital points have been misunderstood.

<sup>1</sup> 'Discontinuous Variation in Pedigree-Culture,' *Pop. Sci. Monthly*, 69, Sept., 1906, pp. 207-225.

Before I try again to give a review of my objections to de Vries's theory, I shall prove in detail that MacDougal's criticism of them, as well as of those of Merriam and Jordan, is unsatisfactory. It may appear as presumptuous, when I take it upon me to talk in behalf of the latter two gentlemen, who are well able to take care of themselves,<sup>2</sup> but I may be excused on the ground that I hold precisely the same views, and am thus defending my own opinions.

I. MacDougal first takes up Merriam's contention, that the study of geographical distribution of animals shows no evidence of 'mutation' (in the sense of saltation or discontinuous variation), since there are gradual transitions, which point to a progressive development of minute variations. This is not admitted by MacDougal, because he maintains (p. 209) that 'once a mutant has appeared, no evidence of its distribution can be taken to account conclusively for its origin.' Jordan has answered this in the article just referred to. But there is yet another aspect. Merriam did not express any view as to the *origin of mutation (saltation)*; he only wanted to bring out the fact that mutations, in the sense of discontinuous variations, seem to be extremely rare in nature, which is indicated by the fact that, morphologically, varieties and even species are often very close to each other, and that, if there are cases where a discontinuity is apparent, a closer investigation of the distribution, not only of the supposed mutant, as MacDougal puts it, but of the mutant and its allied forms, reveals the existence of intermediate forms.

With reference to this latter case, I should like to make a few additional remarks. Granted the existence of a connecting form between two extremes, which appear to fulfill the morphological requirements of mutation,<sup>3</sup>

<sup>2</sup> See Jordan's rejoinder in *SCIENCE*, September 28, 1906, p. 399.

<sup>3</sup> We always are to remember that, strictly speaking, there is no morphological difference between fluctuating variation and mutation; the latter can only be recognized by experiment, according to de Vries, and also MacDougal. Thus it is not correct to talk, as MacDougal does, of

the question is, whether this connecting form represents a stage of development from one extreme to the other, or, as the de Vries school supposes, a hybrid between the extremes. This question, in my opinion, may be settled in many, if not in all, cases, by the facts of the geographical distribution. If the overlapping area of the ranges of the two extremes is occupied by an intermediate form associated with the two original forms, hybridization is indicated; if, on the contrary, in the intermediate area the intermediate form is exclusively represented, the latter very likely marks a connecting step in the development from one to the other extreme.

I am much tempted to illustrate this here by an example I have discovered in the distribution of certain species of river crawfishes (group of *Cambarus propinquus*). But it would take up too much space to give all the facts. It will be presented to the public in due time, in fact, the paper is just now going through the press. Suffice it to say that both of the above cases are illustrated in this example, but that the final decision was possible only after a thorough investigation of the geographical distribution of these forms had been made, by researches covering the whole of the state of Pennsylvania, and parts of Maryland, West Virginia and Ohio.

MacDougal further says (p. 209) that "a number of zoologists have assumed to speak of the distribution of plants, with apparently no basis except 'general information' to the effect that closely related species do not have the same habitat. This has been variously put, but the general meaning is as given." On page 210 he talks of this as 'the idea

mutation as a synonym of discontinuous variation. A mutation may be due to discontinuous variation, and, as de Vries declares, generally is, but not always. In this respect, MacDougal, in the article referred to, certainly goes beyond de Vries. If this is borne in mind, it is clearly seen that Merriam's objection does not affect at all de Vries's theory as a whole, but only the part of it that says that discontinuous variation or saltation is a necessary or frequent attribute of mutation (in the sense of creating the faculty to become a true breeding form).

\* \* \* that closely related species do not occupy the same region.' I believe I am included in this number of zoologists, although I never expressed such an idea; but since I do not know of any other zoologist who did, I think MacDougal refers to something similar I have said. I expressed it thus:<sup>4</sup> "two closely allied species never occupy absolutely the same range under identical ecological conditions." This, however, does not exclude the possibility that *parts* of their different ranges may overlap, and that in certain regions they may be found together, and, moreover, a case in point has been described by me.<sup>5</sup> Thus it is evident that MacDougal has misunderstood me, and that he battles against a fancied idea, which, indeed, is disproved by the instance given (*Opuntia fulgida* and *mammillata*), and to which I am able to add numerous other examples of plants<sup>6</sup> as well as animals. But this does not influence my contention, that *closely allied species of plants or animals never possess precisely the same geographical range*.

With reference to Jordan's opinion that *Enothera lamarckiana* might be a hybrid, which is also held by the present writer, MacDougal thinks that it possibly might be a good, natural species. This question, however, is not essential for the interpretation of de Vries's experiments. The suggestion that it might be a hybrid, or the fact that, in Europe, it is an escaped garden-form, is advanced only to explain the remarkable variability of it. De Vries, assuming that it is as good as a natural species, tries to account for its wonderful capacity to throw off mutants, which is not generally found among natural species, by believing that there may exist, in any species, a time or period of especially vigorous and frequent mutation. But con-

<sup>4</sup> SCIENCE, June 22, 1906, p. 949.

<sup>5</sup> SCIENCE, March 30, 1906, p. 504.

<sup>6</sup> For instance, *Orchis ustulata* and *tridentata*, two closely allied species, but of quite different aspect, grow side by side (forming hybrids) upon the meadows of the valley of the river Saale, near Jena, Germany; on a certain hillside near Jena, *Ophrys muscifera* and *aranifera* grow together (also forming a hybrid).

sidering the fact that, in Europe, *Oenothera lamarckiana* is surely an escaped garden-form, and that it actually has been there under the care of florists for a long time, the possibility that it might be found in the wild state in America does not affect in the least degree the very tempting assumption that its strange behavior is due to cultivation with all its inherent and accessory incidents, exactly as is the case in other garden-forms.

MacDougal regards it as a specimen of 'literary license' and 'inaccuracy' (p. 213), when I say that de Vries entirely failed to take notice of the principle that the discovery of intermediate forms serves to show that two supposed true breeding forms do not possess the rank of species as understood by the taxonomist, and that he also failed to show that his so-called elementary species are not connected by intermediate forms. I do not see where the 'license' and 'inaccuracy' come in, for it is a fact that de Vries nowhere reported that he went over the whole area of any of the species used or referred to by him, and tried to ascertain whether there are anywhere such forms. Indeed, he reports that on rare occasions he found something which might be taken for connecting forms, but he never searched carefully and conscientiously for them. On the other hand, I know positively from my own experience that such forms do exist at least in some of the elementary species discussed by de Vries: I found them myself in nature in the group of *Viola tricolor* and *lutea*, and saw them in the case of *Draba verna* in de Bary's laboratory.<sup>7</sup>

As regards my 'estimate of the futility of experimental methods' and my 'mistrust' of them, MacDougal (p. 213) has not understood my standpoint. I have never said anything that might be construed as if I 'mistrusted' experiments or believed them to be 'futile,' on the contrary, I fully agreed that experiments ought to be made, but warned against too great complexity and improper interpretation.<sup>8</sup> I chiefly called attention to the complexity of conditions offered in cultures in the

botanical garden, which is met by MacDougal (p. 213) by the statement that it is not the case, that 'domesticated races' have resulted from the 'effects of tillage.' But, disregarding the fact that 'tillage' is only one of the many factors contributing to the peculiar features of environment in the garden, I never said that the effect of tillage (or any other environmental factor) is the production of 'domesticated races.' I attribute to the environment the power to influence 'variation,' but in order to obtain 'domesticated races,' that is to say, forms which breed true, I always insisted that pedigree-culture is necessary. In nature the analogous process, selection and segregation, leads to the formation of species.

II. It is possibly well to present here again my objections to de Vries's mutation theory, and, to further the correct understanding of my views, I shall try to represent the matter in a somewhat different form, emphasizing chiefly what are the undoubted facts, and what are their interpretations on the part of de Vries and on my part.

My first and fundamental contentions are:

1. *De Vries's conception of 'elementary species' is inadequate.* There are, indeed, forms in nature which have a tendency to breed true, but which are not isolated from other forms, but these forms should not be called species. They have been called, for instance, by Darwin,<sup>9</sup> 'varieties,' and are distinguished by this quality from 'variations.' On the other hand, there are in nature true 'species,' characterized by the fact that the tendency to breed true is fully developed, and that there are no connecting links any more with allied forms: they are separated from the latter. This character furnishes a good definition for the term 'species,' which ought to be the taxonomic species. This, however, does not mean that in every case it should be easy or even possible to distinguish sharply between a variety and a species, since there are actual cases of transition in nature. Yet at the present state of our knowledge, the insufficiency of the latter alone prevents in many cases a final decision.<sup>10</sup>

<sup>9</sup> Darwin, 'Origin of Species,' p. 33.

<sup>10</sup> See *Pr. Amer. Philos. Soc.*, 35, 1896, p. 191.

<sup>7</sup> See also: Stone, W., in *SCIENCE*, May 4, 1906, p. 701, with reference to *Viola*.

<sup>8</sup> *SCIENCE*, June 22, 1906, p. 952.

2. *The essence of de Vries's experiments, pedigree-culture, consists of 'selection' and 'segregation.'* This becomes most evident in MacDougal's description of his methods as given in the paper under discussion (p. 214 f.), and anybody may see at a glance that MacDougal, as well as de Vries, did nothing that has not the purpose of selecting certain variations and their seeds, and of segregating (separating) them from disturbing influences.

Granting that these fundamental views are correct, we may now look at the bare facts represented in de Vries's experiments, without any attempt at explanation or theoretical speculation. The following two stand out prominently:

1. *By pedigree-culture de Vries succeeded in making certain variations breed true.*

2. *In other cases of variations he did not succeed.*

*The first sentence tells an old story.* The same has been done since times immemorial, and scientific investigation has taken notice of this fact since the time of Darwin. The process is now rather well understood, that is to say, with reference to the essential features of the action required of man: they are selection and segregation. De Vries did not change the old method in the slightest degree, he only introduced additional precaution and refinement in detail, taking particular pains to insure the full efficiency of these two factors by carefully excluding all possible interference with them. In addition, he was the first to keep proper scientific records of what he was doing.

*The second fact, on the contrary, is new,* and it is the point in de Vries's experiments which needs explanation and a theory. Why is it that certain variations did not breed true under de Vries's hands, although they were treated exactly like those belonging to the first group? The general belief, up to this time, was that any variation might be transformed into a true breeding form by proper treatment.

De Vries's explanation of this fact is given in his mutation theory. Believing that his experiments are conclusive, and that, since he himself did not succeed in cases of the second group, *nobody would be able to do so*, he pro-

pounds the theory that there are actually certain variations, in which selection and segregation (pedigree-culture) are impotent to produce true breeding, and, consequently, that there are two classes of variations, the one of which he calls 'mutation,' which produces forms which respond to the effort of the breeder, the other in which the art of the breeder has no effect, and which he calls 'fluctuating variation.' Then the first is, of course, all important for the species-forming process, while the other is of no consequence. There is no saying, with respect to any particular variation, whether it may belong to the one or to the other class, before the actual test (pedigree-culture) has been made, although it seems that mutations often or generally differ from fluctuating variations in the degree of deviation from the original form. This is the essence of the mutation theory.

The above conclusion and theory would be perfectly correct, if the proposition was correct that it is actually impossible to make certain variations breed true. But just in this point, I believe, de Vries is wrong, since his experiments were not conducted in such a way as to absolutely preclude the possibility that even so-called 'fluctuating variations' may be successfully transformed into true breeding forms. We always are to bear in mind that it is at least thinkable that a particular form may be bred true only under particular conditions, under conditions which are congenial or essential to its very existence. To present an imaginary example: a plant species may possess a peculiar variation, which is due to lack of direct sunlight (shade-form). Suppose this shade-form is cultivated according to de Vries's method in the botanical garden, in beds where it gets its full share of sunlight. I never believe, in such a case, that pedigree-culture will succeed in making this shade-form breed true, since always the conditions of environment will have the tendency to paralyze the effort of the breeder. If, however, this particular shade-form is bred in the shade, under proper environment, the attempt possibly may not be in vain.

This is only an example to illustrate what I think may be correct. Every plant breeder

knows that many of our garden forms will come true only when treated in a certain way. These various ways, including everything that comes under the head of gardener's 'tricks,' are familiar to the professional and amateur,<sup>11</sup> and their results are regarded among laymen as due to a 'lucky hand.' Pure strains of seed, of course, obtained by pedigree-culture, are the first condition, but pedigree-culture is not all of the secret, since the proper handling of the seeds is also material, as well as the observation of certain 'tricks' with the growing plants, *which have no other object but to furnish the congenial environment to the object.*

I think that any one who has ever done actual garden work, trying to raise particular strains of flowers or vegetables, will understand what I mean by these 'tricks.' This essential element is obviously lacking in de Vries's experiments: he uniformly bred all his mutants 'in the botanical garden,' and 'in well-manured soil,' and apparently also under the same conditions of climate, season, subsoil, insolation, etc., that is to say, under a uniform set of ecological conditions, such as are generally found in a botanical garden. Indeed, it has been questioned that a change of these conditions may influence the true breeding of a strain, but without sufficient reason, since such an assumption is surely unwarranted as long as the question has not been actually tested in a scientific way. The necessity, in certain cases, to observe certain 'gardener's tricks,' in order to get the best results in raising particular races, strongly favors the opinion that environment actually has something to do with it, and scientific experiments with this in view should be made by all means. Where de Vries succeeded in breeding true his 'mutations,' the environment of the botanical garden was not averse to the experiment, and in this connection it is suggestive that his chief success was attained with *Oenothera lamarckiana*—an es-

caped garden-form, to which apparently the botanical garden was congenial.<sup>12</sup>

Finally, in order to define my standpoint as precisely as possible, and in order to obviate unnecessary discussion of minor and irrelevant points, I shall condense everything I have said into five questions, and if anybody wants to challenge my propositions, I ask him to do so in terms as laid down here.

1. *Does the 'elementary species' of de Vries correspond to Darwin's conception of 'variety,' and is my definition of 'species' ('taxonomic species') acceptable?*

2. *Are selection and segregation the essential features in pedigree-culture?*

3. *Are de Vries's experiments, aside from their greater accuracy and refinement, essentially identical, in their method, with those of the earlier breeders, as, for instance, recorded by Darwin?*

4. *Is it advisable that breeding experiments should be repeated with due regard to environment, before a final judgment is to be pronounced, and are de Vries's experiments defective on this point?*

If the answer to these four questions is 'Yes,' then my contentions are recognized as well supported, and the answer to the next question should also be 'Yes'—

5. *Should the validity of de Vries's mutation theory be doubted, since he makes an unwarranted distinction between two kinds of variation, which further experiments possibly will prove to be identical?*

If, however, anybody should be inclined to answer 'no' to any or all of these questions, I ask him to give reasons for so doing. I have given my reasons for answering them in the

<sup>12</sup> I call attention to Jordan's account of some of Burbank's experiments (in *Pop. Sci. Monthly*, January, 1905), where also the influence of the environment in the production of variation is repeatedly emphasized, chiefly on pages 205 and 206. Possibly, if Burbank's attention is called to it, he may be able at once to quote instances where the true breeding of a certain strain depends largely on the environment offered. This, of course, should be the general rule, but it can be clearly observed only in such cases where a particular feature of the environment is known to be responsible for a particular variation.

<sup>11</sup> For those who have no practical experience in gardening, the study of a few items in Bailey, 'The Cyclopaedia of American Horticulture,' will give an idea of the immense variety of these tricks.

affirmative; consequently, it should be demonstrated that my reasons are no good. Nobody ever attempted this, and when arguments were given purporting to be opposed to my ideas, these invariably were not my views but only what the critic fancied to be my views.<sup>13</sup>

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October 4, 1906

#### SPECIFIC NAME OF *NECTURUS MACULOSUS*

IN the last number of the *American Naturalist* (Vol. XLI., January, 1907, pp. 23-30) there is an elaborate paper by Professor F. C. Waite under the above title, in which he shows that the name employed there has the priority over *N. maculatus*, the term most commonly adopted by anatomists. Towards the end of the paper (p. 27) he makes the following statement: "In the past ten years although many papers have been written on *Necturus*, two only have, as far as I know, used the correct nomenclature."

I wish to say that the 'correct' name was pointed out and the proper references given by the late Dr. G. Baur as early as 1897 (*Zool. Bull.*, I., p. 41). Since then it has been employed by various systematists. Thus the name *N. maculosus* is used in the eighth edition of D. S. Jordan's 'Manual of the Vertebrate Animals of the Northern United States,' 1899, p. 175, in which I tried to bring the nomenclature up to date. It has since been used, both in this journal (*SCIENCE*, N. S., XI., 1900, p. 555) by Fowler, and in the *American Naturalist* (XL., 1906, p. 159) by Stone.

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January 14, 1907

#### THE DEFINITION OF SOLID AND FLUID

TO THE EDITOR OF *SCIENCE*:—The point I have raised (October 26) as to the definition of solid and fluid seems quite timely in view of the discussion going on between Hoskins and See, and the letter of Mr. Willcox (November 9). Note the use of the term 'solid' in one, of 'substance' in the other, of the

two definitions of rigidity cited by Hoskins. Their difference seems to be as to whether it is proper to speak of the rigidity of a fluid or a gas. The real question of fact, how much the interior of the earth yields to a certain variation of pressure, has not thus far entered the discussion.

Again, Mr. Willcox defines fluid and solid quite other than was suggested by me and the line between as the curve of the plastic yield point.

His definition is quite tenable, if we agree to it, may be made as exact, and fits quite as well the Latin derivation of the word fluid, but I am not sure that it agrees as well with usage or is as practical. We could then speak of no substance as solid or fluid without knowing under what pressure it is. Whether a body were solid or fluid would then depend not merely on the state of the body itself, including its temperature, but also on its surroundings—the pressure. We cannot, then, as he writes me, 'properly refer to any substance as a plastic solid.'

The earth's interior would be classed as a fluid, and not, as has been lately common, on account of its high rigidity, as solid.

The one point which is not quite clear, as he brings it in parenthetically, is whether the plastic yield point, and so his definition, depends on the time or rate of application of pressure. I judge not, according to the molecular theory which he adopts (dear to T. Sterry Hunt) that there are three states of molecular aggregation, solid, fluid and gas, and that the solid molecules are heavy and complex aggregates of the liquid molecules, as these are in their turn of the gas, and that sufficient temperature and pressure will break up the large solid molecules.

The definition which occurred to me, that a fluid is a body that can not rest under stress, *i. e.*, in a strained condition, is, however, just as definite and draws just as sharp line as that of Mr. Willcox. We may express it in his terms thus—a fluid has a temperature such that its plastic yield point is reached even at zero pressure. The relative content of the two concepts can be expressed graphically thus.

<sup>13</sup> See also my reply to Gager's criticism in *SCIENCE*, August 17, 1906, pp. 214-217.