

was paid; I have never asked for reimbursement. This case is not mentioned for the purpose of excusing my own delay, but as affording an explanation of why university professors are sometimes slow, and to suggest at the same time that slowness may not be quite as bad as haste when that haste brings forth slipshod results.

Here again the slowness of the professors is liable at any moment to be made an excuse for invading our fields of operations. Thus the whole tendency of the survey's policy of haste is towards more haste and poorer work.

I acquit Mr. Walcott of any intention to discredit professors by his policy. With his intentions, however, we have nothing to do; it is with his methods and results that we are concerned. We can not discredit the source of instruction and keep the instruction efficient. This wholesale discrimination against the universities can have no other results than those mentioned: discredit to the professors, eventual loss of efficiency and a corresponding reaction upon the universities and upon science.

Finally, if this policy were confined to the geological survey proper there would be less to fear from it. But unfortunately the geological survey has expanded far beyond the legitimate fields of geologic work. Forestry, irrigation, water-supply, reclamation and engineering have been added to its functions, and it has occasionally looked longingly toward the Coast and Geodetic Survey, and I know not what more besides. This expansion, under Mr. Walcott's policy, simply increases the field of its possible powers of demoralization for education and science.

Furthermore, this same policy is already being put into active operation in the Carnegie Institution, and Mr. Walcott is now a candidate for the position of secretary of the Smithsonian Institution, where he could be counted upon to put it in still further practise. With a great overgrown national bureau already committed to this policy and with these two endowed institutions of research under similar control, the university professors of the sciences in this country and the universi-

ties themselves are face to face with a serious problem.

In connection with this question I have frequently been reminded that the geological survey has come to be a great scientific trust; that trusts and trust methods are in the air, and that there is little hope of success in fighting them, especially in view of the support commanded by the millions of dollars they receive every year. Very true; but there is also in this same air protest, rebellion and resentment against these high-handed methods, and especially so when they are paid for out of the national treasury.

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November 22, 1906.

#### EVOLUTION (COOK) AND MUTATION (WAAGEN).

Dr. O. F. Cook recently has published<sup>1</sup> a reply to my criticism of his views published some time ago,<sup>2</sup> but it only evades the main point at issue, and introduces, in its stead, a new topic, which had not entered the discussion before.

While I practically said, with reference to a previous article of Dr. Cook's,<sup>3</sup> that his distinction between 'evolution' and 'speciation,' although correct, is not new, and objected to the term 'evolution,' he meets this with the rejoinder, that there is a distinction between 'heterism' and 'evolution'; and since he regards this distinction as new, as a progress in science, he claims the right to use the old term evolution in a new, restricted sense.

However, also this new point in the discussion does not justify Dr. Cook, for it is *not new to science*. 'Evolution,' as he understands it, has been often classed with 'variation,' as I have also done in my previous article. Nevertheless, as Dr. Cook maintains, there is a distinction between 'evolution,' the 'progressive transformation of species' in time, and 'heterism' (or variation proper) of coexisting individuals. But in this sense 'evolution' is absolutely identical with

<sup>1</sup> SCIENCE, September 7, 1906, p. 303.

<sup>2</sup> SCIENCE, April 27, 1906, p. 667.

<sup>3</sup> SCIENCE, March 30, 1906, p. 506.

what Waagen<sup>4</sup> called 'mutation,' a term that has been accepted and frequently used by paleontologists (for instance Neumayr,<sup>5</sup> W. B. Scott<sup>6</sup> and also the present writer<sup>7</sup>). The same idea is implied in Eimer's term 'orthogenesis,'<sup>8</sup> which has also been used by subsequent writers.<sup>9</sup> Thus, 'other terms being available,'<sup>10</sup> there is absolutely no need to accept the confusing change of the meaning of the word 'evolution.'

Dr. Cook holds, that 'evolution' (correctly, according to the rule of priority, 'mutation,' Waagen) is made possible by two factors, 'heterism' (the normal diversity of individuals) and 'sybaxis' (interbreeding = amphimixis, Weismann). This is by no means correct, for it is unintelligible how interbreeding of different individuals should be able to result in a 'progressive change' of the species. Amphimixis only serves to maintain the average of a number of individuals, which vary around this average,<sup>11</sup> and, moreover, it is not an absolutely necessary factor in evolution (old sense), which is evident at once, when

<sup>4</sup> 'Die Formenreihe des Ammonites subradiatus,' in Benecke's 'Geogn. Palæontol. Beitr.,' 2, pp. 179-256.

<sup>5</sup> 'Die Staemme des Thierreichs,' 1, 1889, pp. 60, 61.

<sup>6</sup> 'On the Osteology of Mesohippus and Leptomeryx, with Observations on the Modes and Factors of Evolution in the Mammals,' in *Journ. Morphol.*, 5, 1891, p. 387, 388. 'On Variations and Mutations,' in *Amer. Journ. Sci.*, 48, 1894, p. 355.

<sup>7</sup> 'Grundzüge der marinen Tiergeographie,' 1896, pp. 30, 31. 'On Natural Selection and Separation,' in *Pr. Amer. Phil. Soc.*, 35, 1896, p. 176.

<sup>8</sup> 'Die Artbildung und Verwandtschaft bei den Schmetterlingen,' 2, 1895, p. 3. 'Orthogenesis der Schmetterlinge,' 1897, pp. i-xii.

<sup>9</sup> For instance, quite recently by Paul Sarasin, in *Verh. deutsch. zool. Ges.*, 1906, p. 137.

<sup>10</sup> Ortmann, in *SCIENCE*, l. c., p. 668. 'Transformation' might also answer; it corresponds to the German 'Umwandlung,' as used by Pfeffer.

<sup>11</sup> See Ortmann, in *Biolog. Centralblatt*, 18, 1898, pp. 146-149. (In opposition to Weismann's view of the action of amphimixis; see Weismann, 'Amphimixis,' 1891.)

we consider that there are organic forms, which lack sexuality.

As to the real causes of mutation (Waagen) I refer to my discussion of this subject in a previous paper,<sup>12</sup> which in the main accepts the views set forth by Pfeffer in a special treatise on this question.<sup>13</sup> According to this, mutation of a species, its transformation in time, is the evolutionary process restricted to a single species; it includes all factors of evolution (old sense) except one, segregation, which, when added, causes the splitting up of one species into two or more.

With reference to my belief that acquired characters are transmitted, and that all variation is due to environment, Dr. Cook maintains that 'heterism,' the diversity of co-existing individuals, is 'normal,' that is to say, an innate quality of organisms, and he asserts that there are no facts which support the doctrine that conditions of environment cause this diversity. I recommend to him to read again the last two paragraphs of my article,<sup>14</sup> and further, what I have said on these topics elsewhere,<sup>15</sup> for he has not paid attention to these arguments. The thesis propounded by Dr. Cook, that 'individual diversity persists in spite of uniformity of conditions,'<sup>16</sup> may be met by the antithesis: *if the environment remains uniform, perfect uniformity of individuals will result.*<sup>17</sup> But since it is practically impossible that any two individuals grow up under precisely the same conditions, a diversity of individuals results. This is a fundamental law. It is, however, not an innate, inexplicable quality of organisms, but is due to the external conditions, which continuously are changing and varying,

<sup>12</sup> *Pr. Amer. Phil. Soc.*, l. c.

<sup>13</sup> Pfeffer, 'Die Umwandlung der Arten, etc.,' in *Verh. Naturw. Ver. Hamburg* (3), 1, 1894.

<sup>14</sup> *SCIENCE*, l. c., p. 669.

<sup>15</sup> *Biol. Centralbl.*, l. c., p. 153. *SCIENCE*, June 22, 1906, p. 951.

<sup>16</sup> *SCIENCE*, l. c., p. 306.

<sup>17</sup> See Davenport, in *SCIENCE*, November 25, 1904, p. 704: 'Two individuals of the same family develop alike only under the same conditions of environment.' See also Copeland, in *Bot. Gaz.*, 38, 1904, p. 426.

and thus a uniform response of individuals to environment is rendered impossible.<sup>18</sup>

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September 22, 1906.

#### DISCONTINUOUS VARIATION.

DR. JORDAN'S kindly comment upon my Wood's Hole lecture would place me at a great disadvantage if I were inclined to be controversial (*SCIENCE*, 24: 399, 1906). This, however, is farthest from my purpose, and in fact is unnecessary since I find myself in agreement with him on most of the points brought up.

Too much emphasis can not be put upon some of the statements reiterated or advanced by Dr. Jordan, and he has certainly performed a very useful service by approving the principle that the students of a group, or of any phase of life are more likely to acquire better first-hand knowledge of it, and to render truer interpretations of the facts obtained than other writers. As vividly obvious as this may be, it has been necessary to be unpleasantly insistent upon it upon various occasions during the last few years, and to reassert that botanists are better fitted by opportunities and training for the comprehension of the nature and behavior of plants than any other class of naturalists. The dictum of Darwin so aptly quoted has never been more strictly applicable than at the present time, and an intimate and accurate acquaintance with a large number of species constitutes a very important share of the competency for profitable study of evolutionary problems. Whether or not the investigator publishes his descriptions is purely incidental.

On the other hand, it is not to be granted (and happily it is not) that a keen critical

<sup>18</sup> See Brooks, 'Heredity and Variation: Logical and Biological,' in *Pr. Amer. Philos. Soc.*, 45, 1906, p. 75. 'The species is \* \* \* in that reciprocal interaction between the living being and the natural world, of which it is a part.' This paper of Brooks is quite interesting and important. Although largely written in terms rather unusual in the discussion of evolutionary matters, it brings out ideas, which, if I correctly understand them, are essentially in agreement with my own views.

sense in nomenclature, or a zeal for the acquisition, making or conservation of type specimens forms a suitable equipment for the intelligent consideration of genetics, although when coupled with detailed studies in life-histories, cultures and field observations, the activities in question become of very great value in this connection.

During the last few years I have had occasion to discuss the species-idea with several of the more active systematic botanists, and find that the theoretical conceptions of species formulated by them vary widely, although overlapping in many essential points in all instances. Now these differences of opinion by no means lead me to deprecate species-making, or to distrust the value of the species erected by these workers, although known to apply differing standards. This knowledge and this confidence are shared by the general botanical public. The difficulty in delimiting in so many words the difference between continuous and discontinuous variation is of a similar character, and was recognized by myself in the earliest review of the work of de Vries on the subject published in an American journal in the following words: "From the reviews and discussions which have already been made of de Vries' papers it is to be seen that the greatest misunderstanding which may likely arise in the consideration of his results will be that founded on the error of confusing fluctuating variability and mutability." In this as well as in the consideration of species it is found that our difficulties disappear when we deal with concrete examples, especially if embraced in a pedigreed culture. To be able to examine a number of organisms in the field and determine which are continuous and which are discontinuous variants is not possible, for numerous reasons, although many botanists have assumed to do so.

What the actual origin of *Oenothera Lamarckiana* may have been can not be said: we can vouch for the actual origin of but few species. If records are to be trusted, however, it was in cultivation in the Paris Garden a century ago. My own breeding experiments have included a number of crosses involving various combinations of species from eastern