Treasurer—Professor H. E. Summers, of Iowa State College, Ames.

Secretary—Professor L. S. Ross, of Drake University, Des Moines.

The meeting in 1907 will be held at Drake University.

The following program was presented:

M. F. Arey: 'A Review of the Development of Mineralogy.'

H. W. Norris: 'The Carotid Arteries and their Relation to the Circle of Willis in the Cat.'

N. Knight: 'A Study of Dolomite and Magnesite with special reference to the Separation of Calcium and Magnesium.'

Bruce Fink: 'Ecological Notes from an Illinois Esker.'

J. Fred Clark: 'The Disparity between Age and Development in the Human Family.' (Illustrated by pronounced cases due to thyroid malformations.)

L. H. Pammel: 'Some Diseases of Rocky Mountain Plants.'

John L. Tilton: 'An Attempt to illustrate Tides and Tidal Action.'

J. E. Todd: (*a*) 'More Light on the Origin of the Missouri River Loess,' (*b*) 'Some Variant Conclusions in Iowa Geology.'

W. S. Hendrixson: (*a*) 'The Action of Bromic Acid on Metals,' (*b*) 'Logarithmic Factors for Use in Water Analysis,' (*c*) 'A List of Chemical Periodicals in Iowa.'

T. J. Fitzpatrick: 'The Liliaceæ of Iowa.'

L. Begeman: 'Mutual Induction and Internal Resistance of a Battery.'

Bruce Fink: 'Lichens and Recent Conceptions of Species.'

T. E. Savage: 'Some Unusual Features of the Maquoketa Shale in Jackson County, Iowa.'

A. T. Erwin: '*Amelanchier alnifolia* and its Cultivated Forms.'

Paul Bartsch: 'The Iowa Ornithological Literature of the Nineteenth Century.'

Walter J. Meek: 'A Study of the Choroid Plexus.'

Frank F. Almy: (*a*) 'The Effect of Pressure on Lines in the Spectrum of Iron,' (*b*) 'A Simple Demonstration of the Doppler Effect in Sound,' (*c*) 'The Physical Laboratory of Iowa College.'

G. E. Finch: 'A Portion of the Iowan Drift Border in Fayette County, Iowa.'

B. A. Place: 'The Relation of the Motor Nerve Endings to Voluntary Muscle in Amphibia.'

Fred J. Seaver: 'Notes on the Discomycete Flora of Iowa.'

Charles R. Keyes: (*a*) 'Lime Creek Fauna of Iowa in Southwestern United States and Northern Mexican Region.' (*b*) 'Geology of the Corinth Canal Zone.' (*c*) 'Alternation of Fossil Faunas.'

J. E. Guthrie: 'The Collembolan Eye.'

J. M. Lindly: 'Flowering Plants of Calcasieu Parish, Louisiana.'

F. A. Brown: 'Some Contributions to Madison County Geology.'

O. M. Oleson and M. P. Somes: 'Flora of Webster County, Iowa.'

K. E. Guthe: 'Electrical Units.'

H. P. Baker: 'The Holding and Reclamation of Sand Dunes by Tree Planting.'

L. S. Ross: (*a*) 'The Food of Subterranean Crustacea,' (*b*) 'Number of Bacteria in Des Moines School Buildings.'

B. O. Gammon: 'Cladocera in the Vicinity of Des Moines.' Presented by L. S. Ross.

D. W. Morehouse: 'Photographic Accessories of Drake University Equatorial.' Introduced by L. S. Ross.

C. O. Bates: 'Municipal Hygiene—Part II., Milk.'

B. Shimek: (*a*) 'Notes on Certain Iowa Trees and Shrubs,' (*b*) 'The Loess of the Missouri Bluffs.'

J. A. Udden: 'Cyclonic Distribution of Precipitation.'

L. S. Ross,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE MUTATION THEORY AGAIN.

Certain objections to the mutation theory of de Vries have called forth the wrath of Professor C. S. Gager, and he emphatically demands that this theory should be thoroughly understood before we discuss it.[1] With more zeal than discretion he affirms that this lack of understanding is shown in two recent articles published in Science, one of which has the present writer for its author;[2] he calls these articles a display of mental density, claiming that the views of de Vries have been misrepresented; but with reference to my own paper he only succeeds in demonstrating that he in turn has entirely failed to grasp the

[1] De Vries and his critics, in Science, July 20, 1906, p. 81 ff.

[2] Science, May 11, 1906, p. 746 ff.

essence of my views, and further, that other publications of mine on this and kindred subjects, which are absolutely necessary for the proper understanding of my views, are unknown to him.

The chief purpose of my article was to object to de Vries's conception of mutation and elementary species. If I object to these terms, of course, I do not accept them, and since I have given reasons for believing that they are wrong, the only appropriate rejoinder to this would be to show that my reasons are no good. Instead of this, Gager is satisfied with the vague and superficial statement that it is impossible to satisfactorily define the concept of species, neglecting entirely what I have written on this topic previously,[3] and, further, he spills a good deal of ink in reiterating de Vries's contentions.

Gager says:[4] "*When a careful worker says that he obtained a given form that breeds absolutely true, and which, for reasons fully explained, he calls an 'elementary species,' by means of a certain definite and clearly explained kind of variation which he defines and names 'mutation,' let us not refer to him as 'claiming to' have done so, or to the mutant as 'seeming to' breed true.*"

Here we have a concise statement of de Vries's claim, namely, that *he obtained a form that breeds true by means of mutation.* I have said[5] that de Vries claims '*that mutants are species.*' But if de Vries calls a form that breeds true an elementary species, *he obtained species by means of mutation;* and if the product of the process of mutation is a mutant, of course, *a mutant is obtained by means of mutation,* and, consequently, *mutants are (elementary) species.* Thus it is evident that my expression of de Vries's claim is *absolutely correct* and identical in its meaning with that given by Gager, *and his allegation that I have misunderstood de Vries is entirely unwarranted.* It rather seems that Gager himself has not fully understood what de Vries says, at any rate, that he was not aware of the true meaning and import of de Vries's theory. This is due, in part, to the fact that de Vries himself was not conscious of the logical consequences of his views, he belonging to the class of writers who are oblivious of the most fundamental principles of evolution.[6]

However, as I have endeavored to show, *I do not accept* the view that *mutants are species,* or *that de Vries obtained a form that breeds true by means of mutation.* In opposition to this I say, *that he obtained such a form by means of selection and segregation out of a certain kind of variation (mutation).* Indeed, Gager endorses also the latter view[7] by an emphatical 'Exactly!' and asks: 'why the dissenting critique?'

This plainly shows that, for Gager, these two phrases are identical, namely, that de Vries *obtained species by means of mutations,* and that he *obtained them by means of selection and segregation out of mutations.* Possibly my mental density comes in here; but I can not help it; I must regard these two phrases as having a *different* meaning, and this is the reason for my 'dissenting critique'; de Vries *never* said anything that might be interpreted in the sense of the second sentence.

That de Vries's view is wrong I have demonstrated by pointing out that the mutants actually did not breed true before he started his experiments, and very likely they would not have bred true if he had not taken them under his care. They began to breed true, not because they were mutants, formed by the process of mutation, but because he introduced two factors, which were absent previously, namely, selection and segregation. 'Pedigree-culture is the method required,' as Gager[8] quite correctly insists, but apparently without knowing of what it consists. The essential factors in pedigree-culture are selection and segregation, and pure strains are only ob-

[3] *Pr. Amer. Philos. Soc.*, 35, 1896.

[4] *L. c.*, p. 89.

[5] *L. c.*, p. 746.

[6] See Ortmann, SCIENCE, June 22, 1906, p. 947 ff.

[7] *L. c.*, p. 87, in the form: '*If de Vries had claimed that species might be made out of mutations*' (Ortmann, p. 747), namely, as is said in the same paragraph, but carefully omitted by Gager, *by means of selection and segregation.*

[8] *L. c.*, p. 86.

tained after a number of generations, as is seen in de Vries's experiments.

This is so evident, that it is simply astonishing that Gager is not capable of seeing it, even after attention has been called to it, and that he does not see that this is an important part of my objections, namely, *that it is selection and segregation that make mutants breed true.* Indeed, he asks:[9] "Where, from cover to cover, of 'Species and Varieties,' is any other claim made?" Please look at the title: 'Species and Varieties, their Origin by Mutation'; is this identical with 'Species and Varieties, their Origin by Selection and Segregation'? In my opinion, the *first* phrase means that by the process of mutation species (elementary species, which breed true) are made; the *second,* that selection and separation make them breed true. The *first* means that the quality of breeding true was created by the mutation process, before de Vries made the experiments, and (as de Vries says in the text) that the latter were undertaken only in order to *test,* to ascertain the existence of this quality; while the *second* means that the quality of true breeding was created, not by the process of mutation, but by the subsequent processes of selection and segregation. If Gager can not see the difference, I am sorry for him; or should it again be a case of mental density on my part?

If we remove this fundamental fallacy out of de Vries's theory, that it is not the process of mutation, but that of selection and segregation, which makes species breed true, nothing remains but the view that mutation is a peculiar kind of variation, which alone may start the species-making process, or which alone is apt to finally produce true breeding forms. A part of my article is written with reference to this possible claim, although I know very well that de Vries did not make it separately,[10] but always in connection with the first claim, that it is the process of mutation which produces true-breeding, elementary species.

[9] *L. c.*, p. 87.

[10] That I did not intend to represent this as de Vries's view, but only as a possible modification of it, is clearly seen in SCIENCE, June 22, 1906, p. 950, foot-note 9.

Gager[11] quotes my sentence,[12] but omits the introductory and important words: 'aside from the above claim.' This, of course, affords him a chance to show that I have misunderstood de Vries. But if we exclude the only test for the elementary species, that they should be true-breeding forms, as unsatisfactory, no other difference remains between fluctuating variation and mutation, but the degree or amount of deviation from the original type, the one being represented by 'small steps,' the other by 'sudden leaps'; and I must repeat that I am unable to draw a line between them. If Gager[13] again points to de Vries's definition of mutation (that it causes true breeding), I hardly can call this a fair criticism or a fair understanding of my views, after I have expressly excluded this criterion.

In this last instance, and in a few others, Gager directly distorts what I am saying. I have said[14] that 'in the beginning of the experiments, they (the mutations or mutants) were throwing off additional mutants.' Gager[15] omits the words 'in the beginning of the experiments,' and quotes the sentence as if it was clearly implied that I meant to say that '*all* the mutants were throwing off additional mutants.' In fact, I did not mean to say 'all,' for this would not correspond to the facts; and the words 'in the beginning of the experiments' are essential for the proper understanding of the whole paragraph, and of my contention that the mutants did not breed true, namely, *in the beginning:* they bred true *later on, in consequence of the experiment,* which was the point I wanted to bring out.

Further, when I say[16] that 'the breeding of domestic races has *always* been regarded as a process analogous to the one in nature by which new species are produced,' 'always' does not mean: by everybody and at all times. If it is hinted at by Gager[17] that I possibly might have intended to include Linnaeus or

[11] *L. c.*, p. 87.

[12] *L. c.*, p. 747.

[13] *L. c.*, p. 88.

[14] *L. c.*, p. 747.

[15] *L. c.*, p. 86.

[16] *L. c.*, p. 747.

[17] *L. c.*, p. 87.

other pre-Darwinian writers, this is a display either of mental density or of something worse, for he understood quite well what I meant, as is seen by his own use of the word 'always' further on.[18]

My two main contentions are: *that de Vries's conception of elementary species is inadequate*, and *that elementary species breed true, not because they are the product of a peculiar kind of variation, called mutation, but because they have been subject to the processes of selection and separation.* These essential points in my criticism have been overlooked by Gager, and he is content to say, with regard to the first one, that nobody, except makers of dictionaries, knows what a species is. With regard to my second contention, he fails entirely to see that it is intimately connected with the first one, and has made no attempt to demonstrate that mutation is capable of producing true breeds *without the help of selection and segregation,* and that the latter two factors *do not play an essential part in de Vries's experiments.* For the rest, he only points to de Vries's definitions of terms, which I reject; he points to the facts represented by the experiments, which I accept, but consider unsatisfactory and incomplete; and he points to the value of the experimental method as the only one that is apt to decide questions of evolution, which I positively deny. Experiments are valuable, but they should be properly understood, and should be correctly explained. The interpretation of his experiments given by de Vries is faulty, although the experiments themselves are indisputable facts; and the fallacy is due to his ignorance of the fundamental laws of evolution, and to his incorrect conception of the term species: with the latter his theory stands and falls.[19]

I hope that this will be sufficient, even to Gager, to define my standpoint, and, if any further discussion should be considered necessary, that it will take up the essential points of my views, and not merely repeat the arguments of de Vries. Gager has done only this, in a way which clearly lacks understanding of what I really object to. If he further would consider the rule, not to throw stones at people out of a glass house, and observe the necessary fairness to others, this would make the discussion a more pleasant and profitable one.

A. E. ORTMANN.

CARNEGIE MUSEUM, PITTSBURG,
July 23, 1906.

[18] *L. c.*, p. 88, foot-note 65: 'Since the process has been recognized and described.'

[19] SCIENCE, June 22, 1906, p. 948.

SPECIAL ARTICLES.

HERBARIUM TYPE SPECIMENS IN PLANT MORPHOLOGY.

THE close relationship existing between the different branches of botany and the dependence of these various branches upon each other make it very important that every precaution should be taken by the workers of each branch to make their specialty as helpful as possible to all other divisions of the subject. With the advancement of each phase of the subject the points of relationship become more prominent and the necessity for the preservation of records, specimens, etc., becomes of greater and greater importance.

Between no two branches of botany is the necessity of cooperation greater than between taxonomy and morphology. The taxonomist has long recognized the importance of type specimens and large herbaria have been brought together and maintained at great expense where these types may be preserved and studied to the best advantage. The morphologist has probably in most cases preserved his microscopic specimens, but in how many cases has the morphologist prepared herbarium specimens of the species on which he is working? This custom may and probably is followed by many workers, but it is also true that many morphologists have not only neglected to preserve type material but in many instances have not even taken the precaution to have their determinations verified by specialists in taxonomy.

If morphological botany is to add anything to our knowledge of taxonomic botany, it appears to the writer that herbarium specimens should be carefully prepared, properly labeled,