Turkish and Finnish races. Zoologically, the Yenesei is important as being the line of demarcation between the faunas of eastern and western Siberia. And if the expedition is able to reach Alashan and neighboring regions, there will be valuable geographical work to be done and problems regarding the desiccation of central Asia to be solved.

## *UNIVERSITY AND EDUCATIONAL NEWS*

BY the will of Isaac C. Wyman, of Salem, Mass., a graduate of Princeton College, who died on May 18, most of his estate is bequeathed to Princeton University, to be used in whole or in part for a graduate school. Mr. John M. Raymond, of Salem, Mass., and Professor Andrew F. West, dean of the Graduate School were named as trustees. The daily papers estimate the value of the bequest to be from $2,000,000 to $10,000,000.

THE Jefferson Medical College of Philadelphia, has received a gift of $60,000 from Mrs. Maria Gross Horwitz, daughter of the late Professor Samuel D. Gross, the eminent surgeon, to endow the "Samuel D. Gross Chair of Surgery."

ASSISTANT PROFESSOR J. G. JACK will conduct a Field Class at the Arnold Arboretum, Harvard University, on Saturdays during the spring and early summer, to assist those who wish to gain a more intimate knowledge of the native and foreign trees and shrubs which grow in New England.

DR. E. J. WILCZYNSKI, associate professor of mathematics in the University of Illinois, has accepted a similar position in the University of Chicago.

DR. J. W. YOUNG, assistant professor of mathematics in the University of Illinois, has been appointed head of the department of mathematics in the University of Kansas.

MR. EDWARD M. WELLISCH, of Cambridge University, has been appointed assistant professor of physics in Yale University.

THE following appointments have been made at the School of Mines of the University of Pittsburgh:

A. E. Ortmann, Ph.D., professor of physical geography.

P. E. Raymond, Ph.D., professor of invertebrate paleontology.

S. L. Goodale, A.M., E.M., assistant professor of metallurgy.

L. K. Acker, Jr., E.M., instructor in mineralogy and geology.

G. T. Haldeman, E.M., instructor in mining.

Earl Douglass, A.M., M.S., instructor in vertebrate paleontology.

H. B. Meller, instructor in mining.

Dr. A. B. Wallgren, lecturer on first aid to the injured.

Alexander Silverman, lecturer on glass manufacture.

W. F. Fischer, E.M., assistant in petrography.

N. L. Estabrook, assistant in mineralogy.

J. B. Keller, assistant in assaying.

The year has been extended to four terms of 12, 12, 11 and 10 weeks each, so that a student can complete his course by working any three of the terms each year. He may also complete his work and graduate in three years if he takes four terms a year. A student, as heretofore, in this school can substitute a year of practical work done under the school's direction for one year of the usual class and laboratory work, and in this way graduate in three years. Some thirty-five thousand dollars worth of material has been added to the equipment during the past year.

MR. C. L. BOULENGER, of King's College, Cambridge, has been appointed to the lectureship in zoology at Birmingham University rendered vacant by the resignation of Mr. Leonard Doncaster.

## *DISCUSSION AND CORRESPONDENCE*

### WEISMANNISM, A CRITICISM OF DIE SELEKTIONSTHEORIE[1]

A NEW publication from the pen of August Weismann naturally must excite curiosity among biologists, not so much with regard to possible new ideas and theories, but rather with reference to the question how far the author has corrected and modified his old views in order to do justice to the numerous

[1] "Die Selektionstheorie." Eine Untersuchung von August Weismann. Jena, 1909. 70 pages, 1 plate and 3 text figures.

and serious objections to them advanced by various adversaries.

The title of the present booklet might suggest that Weismann had the intention of doing something like this, for his conception of the principle of selection is one of his chief peculiarities, which has been most vigorously attacked. But perusing this book, we find that not the slightest attempt has been made to discuss seriously these objections. Here and there a feeble show is made, as if he had paid attention to them, but generally he discusses only minor points, and avoids the most essential criticisms, those which, when admitted as correct, would inflict the finishing blow to that particular type of evolution-theory known as *Weismannism*. And further, a peculiar feature of the present book is that in certain cases Weismann admits that his critics are right in a particular point, but that he nevertheless insists in maintaining his old position and his old views about this point. We occasionally have come across this way of arguing in informal discussions with persons belonging to the weaker sex, but never, as far as we can remember, in a scientific treatise which pretends to be serious.

The whole book is an eulogy on selection and its power to *create new things*. Weismann believes, if this is admitted, then there will be no difficulty whatever in understanding the origin of the whole organic world, and consequently also the origin of new species by natural selection will be clear. He claims that he stands, in maintaining this view, upon the original standpoint of Darwin. *But Darwin never said that new species are created by natural selection.* Indeed, there is the title of Darwin's book, "The Origin of Species by Means of Natural Selection," and it must be confessed that, reading the title alone, it might be interpreted this way. But there are some people who have a habit of looking more closely at things, books especially, and when they began to read Darwin's book, they found out that there is a distinction of two processes within the whole great process of evolution: the one is the *transformation of species,* that is to say, the change of *one* existing form of life into *one* other one, and the other is the *differentiation of species,* that is to say, the dividing of *one* existing form into *two* or *more* other ones. The latter process is strictly the *origin of new species,* or, as it has recently been called, the process of *speciation.*

For the first process, the transformation of species, Darwin introduces the three factors: *Variation, Inheritance* and *Natural Selection,*[2] and treats of them in the first five chapters. But incidentally he also discusses the second process, the origin of new species. He does this chiefly in the fourth chapter, where he talks of the divergence of character.[3] As the writer has shown elsewhere,[4] Darwin feels a little uneasy about this point. Nevertheless, he gives a tentative answer, and this is, that new species originate, if they are "enabled to seize on many and widely diversified places in the polity of nature,"[5] or, "if (they) become fitted for . . . different habits of life or conditions."[6] This is exactly what by subsequent writers has been called *separation, isolation, bionomic separation,* and for which possibly the best term is "*ecological segregation.*" And I hope by mentioning these words Weismann may recollect that they are intended to express something, and that they are supposed to have a definite place within the evolution-theory. In fact, the working out of this principle is the most essential improvement added by subsequent writers to Darwin's theory.

The above distinction between transformation of species (Umwandlung der Arten) and the origin of new species (Entstehung neuer Arten) has been exposed again and again, has been discussed at such a length that it has actually become tiresome to have to repeat it. Any child should be able to see the point. But Weismann evidently fails to do so. All his previous writings, and also the present book, are, with reference to the dis-

[2] See summary at end of chapter IV., p. 102 ("Origin of Species," American ed., 1884).

[3] *Ibid.*, p. 86.

[4] *Pr. Am. Phil. Soc.*, 35, 1896, p. 175 ff.

[5] "Origin of Species," p. 87.

[6] *Ibid.*, chapter VII., p. 169.

tinction of these two processes, a maze of confused ideas, and he most obstinately continues to transfer the factor of selection, which Darwin introduced for the first process, and to apply it to the second process (speciation). Of course, in the writings of Weismann it is hard to quote a passage where he does this clearly and unmistakably, since in this respect clearness is altogether lacking, but, in the present book, it is easily seen that he actually intends to apply the principle of selection to the formation of new species by his reference to the mutation theory of de Vries. Of course, de Vries makes the same fundamental mistake. The mutation theory, as should be evident to everybody, deals preeminently and emphatically with the question of speciation; at any rate, de Vries claims that it does, if he wants to explain the *origin of* NEW *species by mutation,* and consequently it can not at all come into conflict with Darwin's principle of selection, which is intended only as a factor in the transformation process. Nevertheless, Weismann (as well as de Vries) regards the mutation theory as opposed ("Einwurf," p. 7) to the selection theory! Any one who expresses views like these demonstrates only that the true Darwinian theory is not understood by him, and that he has not the slightest idea of what the meaning and significance of de Vries's experiments are. As has been demonstrated elsewhere,[7] de Vries himself did not understand the bearing of his experiments upon the evolution theory in general, and, consequently, made the most serious mistakes in their interpretation.

This misunderstanding of Darwin's theory explains why Weismann so stoutly maintains that selection may *create new things: he needs some explanation for the origin of new species.* But this idea of his has been criticized so often that he is forced to pay at least some attention to the attacks, and, indeed, he admits that selection can not do anything without the material, with which it is to work, being furnished by variation: a number of writers have called his attention to this, and have reminded him that, if this is so, it is not logical to say that natural selection, by killing the unfit variations, "creates" new ones, but that the word "preserves" should be used. This objection is absolutely well founded, as everybody will grant, and Weismann has been cornered by it so completely, that no other escape remains for him but to say that this objection is "nonsensical" (sinnlos, p. 81). Further comment is unnecessary.

In his treatment of the "Lamarckian principle" and the causes of variation, Weismann shows the same lack of understanding, or, if not, a rather vicious tendency to distort facts and ideas. The Lamarckian principle, in its widest sense, which is also accepted by Darwin, says that the variations which are transmitted to the offspring are *caused* by the environment. It is true, Lamarck himself discussed "chiefly" (hauptsaechlich) use and non-use of parts as cause of variation. But Weismann admits, by using the word "chiefly," that there *are* others, and he surely ought to know that Darwin and subsequent writers have enlarged this principle so as to regard all reactions of the body to environmental factors as variations in this sense (acquired characters). In the present booklet, however, Weismann restricts the Lamarckian principle strictly to "use and non-use," and then, of course, it is easy for him to show that in particular cases quoted by him the Lamarckian principle does not apply.[8]

His chief argument against the Lamarckian principle is that we are to entertain "strong doubts" (p. 6) against the cooperation of this principle, and that the transmission of acquired characters is "hard to

[7] See SCIENCE, 23, May 11, 1906, p. 746; 24, August 17, 1906, p. 214; 25, February 1, 1907, p. 185.

[8] *This is a beautiful illustration of Weismannian logic.* On page 6, line 10, he uses the word "chiefly" (hauptsaechlich) in this connection, while almost immediately below, on the bottom line of the same page, "functional" variations (by use and non-use) become the "only ones" (allein), which constitute the Lamarckian principle. This surely justifies what we have said above on his tendency to distort things.

imagine" (kaum vorstellbar). There is hardly a single paper of Weismann on evolutionary subjects which does not assure us of this. But the reviewer has not seen in any one of them a clear statement what these doubts are, and his personal power of imagination, which surely has the same convincing force as Weismann's, is entirely adequate to admit this theory. Weismann's opinion to the contrary and his idea of "germinal variation" is a working hypothesis pure and simple, and should be used only as such; but the two opposite views should never be used as evidence against each other, and this is what Weismann does again and again, also in the present book. *The Lamarckian principle is wrong, because it is in conflict with the Weismannian theory of the germ plasm, and the latter is correct, because, since the Lamarckian theory is wrong, it is the only way to explain evolution.* This is practically the essence of Weismann's argumentation: a schoolboy's blunder against logic.

On the other hand, Weismann purposely overlooks the recent experimental evidence for the inheritance of acquired characters, furnished now by quite a number of biologists. He knows, at any rate mentions, only two of them, Semon and Kammerer, and says that, according to Pfeffer, those of the first are "incorrect" (nicht richtig), and that he is going to show that those of the latter can not be regarded as convincing. The reviewer is much afraid that this latter demonstration will rest upon something like Weismann's argument, which intended to show that his own experiments on *Polyommatus* do not furnish support for the Lamarckian view. As to the latter, I beg to compare what I have said some time ago with regard to this matter,[9] to which I have to add nothing, and which clearly shows that Weismann's conception of the Lamarckian principle is entirely wrong, in fact that he does not at all understand what the essential point in it is.

We may summarize our conclusions as to the Weismannian theories and the Weismannism as follows: In the beginning, Weismann proposed his theory of the germinal variation, and the subsidiary theory of the all sufficiency of natural selection in opposition to the current view of the inheritance of acquired characters, without positive support, but chiefly on account of the supposed insufficiency of the latter view. At that time it was a working hypothesis as well as the other theory. In his subsequent writings Weismann tried to strengthen his position, but he was forced, first of all, to abandon his idea of the "amphimixis" as the cause of germinal variation, and further he introduced his theory of the "germ plasm" and its variation, and, in close connection with the latter, his theory of inheritance.

By the theory of the "variation of the germ plasm" he *changed his original views of "germinal variation" in a fundamental way,*[10] a fact which was never acknowledged by him, and further, in connection with this, he was forced to admit facts which are strongly in favor of the Lamarckian principle, which, however, he denied by the argument: *if Lamarckian inheritance can be explained by the germ-plasm theory, there is no Lamarckian inheritance.*

His special views on the principle of selection, although attacked repeatedly and disproved, in part as incorrect, in part as illogical, were always maintained and defended by him, but only at the risk and to the detriment of sound logic. At the present time, in the booklet reviewed here, he is upon the old standpoint; he has not considered valid objections to his views, and has passed over the most serious in silence, repeating again and again his old blunders and absurdities.

This has gone on too long. *Weismannism* has become a term characterizing not only a particular brand of Darwinism "made in Germany," but also a particular kind of loose and illogical reasoning, which we are not wont to regard as a product of German universities. This harsh criticism would not be necessary but for the fact that Weismannism has become a scientific "creed"

[9] See *Biol. Centralbl.*, 18, 1898, p. 153, and Science, 23, June 22, 1906, p. 950.

[10] See *Biol. Centralbl.*, 18, 1898, p. 139 ff.

among a certain class of biologists, and, in consequence, has delayed progress in biology for a considerable time. Weismann alone is responsible for the discredit into which the Lamarck-Darwinian view of the causes of variation has fallen: the latter has become unfashionable and "not up to date." Thus biologists were and are to a certain extent afraid of looking at evolutionary questions under the assumption that the "inheritance of acquired characters" might possibly be correct, and failed to do, what was most needed, to prove or disprove this view by the way of experiment. Fortunately, at the present time, conditions seem to improve: observations and experiments are being made which have a distinct bearing upon this question, and we may say that unexpected results are forthcoming which tend to show that the Lamarckian principle, which is also Darwin's view of the origin of transmissible variations, should be reckoned with. We only hope that this spirit of emancipation from a scientific dogma may prosper and flourish, and true progress will be assured.

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## NOTE ON THE MARKING SYSTEM IN THE ASTRONOMICAL COURSE AT COLUMBIA COLLEGE, 1909–10

AFTER the first half year's work in the introductory astronomical course at Columbia had been finished, a test was made to ascertain the precision with which marks were assigned after the mid-year written examination. The answer books as handed in by the students were arranged in alphabetical order and each fifth book selected. In this way eleven answer books were obtained, representative of the class as a whole and chosen entirely without bias.

These eleven books were then marked by the following six professors of astronomy: Professor John M. Poor, of Dartmouth; Professor F. R. Moulton, of Chicago; Professor Wm. Beebe, of Yale; Professor O. M. Leland, of Cornell; Professor S. A. Mitchell, of Columbia; Professor Harold Jacoby, of Columbia.

No professor was permitted to see the marks assigned by the others; all were instructed to let the mark 10 represent that degree of proficiency which may be expected reasonably from a competent student who works hard; and 6 was to be considered a pass mark. No attention was to be paid to neatness, spelling, etc.; the marks were to be assigned upon astronomical proficiency only. The following table contains the results, the names of the professors being replaced by letters of the alphabet so as not to make public which professors gave the highest or the lowest marks.

| Book No. | A | B | C | D | E | F |
|---|---|---|---|---|---|---|
| 1 | 9 | 9.0 | 8.5 | 7.2 | 9 | 7.3 |
| 2 | 7 | 6.6 | 7.0 | 5.9 | 6 | 6.5 |
| 3 | 9 | 9.0 | 8.8 | 7.2 | 8 | 8.0 |
| 4 | 10 | 9.4 | 9.9 | 8.0 | 10 | 9.2 |
| 5 | 7 | 6.2 | 6.7 | 5.8 | 7 | 5.9 |
| 6 | 10 | 9.8 | 9.6 | 7.6 | 10 | 9.5 |
| 7 | 6 | 5.8 | 6.3 | 4.6 | 7 | 5.4 |
| 8 | 9 | 9.3 | 9.7 | 8.0 | 9 | 8.8 |
| 9 | 8 | 5.7 | 9.0 | 6.7 | 10 | 8.7 |
| 10 | 10 | 8.5 | 9.1 | 6.2 | 9 | 9.0 |
| 11 | 9 | 9.0 | 9.5 | 6.1 | 8 | 9.0 |
| Average | 8.5 | 8.3 | 8.6 | 6.7 | 8.5 | 7.9 |

The professor in the column D, whose average mark is 6.7, appears to have taken 5 instead of 6 as his pass mark; he explained in a letter that only one of the students should fail to pass in his opinion, although he assigned three marks under 6.

Making due allowance for this circumstance in the case of professor D, there is a very close accord in the marks given by the various professors. It would appear that the students have attained a very high average in their work, and that the marking system is more precise than some of its critics would have us believe. Possibly this may be due to the fact that astronomy is an exact science.

For the information of other teachers, the examination paper is appended.

HAROLD JACOBY

COLUMBIA UNIVERSITY,
April, 1910