LYSENKO'S "GENETICS"

A Review

ABOUT a decade ago biologists outside the U. S. S. R. began to hear reverberations of a strange campaign against genetics waged by professor T. D. Lysenko, now member (academician) of the Academy of Sciences of U. S. S. R., director of great research laboratories, and holder of important administrative posts. Since then, the opinions of Lysenko have been mentioned rather often in American newspapers and popular magazines. Some of Lysenko's speeches, together with those of his opponents, have been re-published in English translation. It may seem strange, then, that American geneticists, and with few exceptions those of other lands outside the U. S. S. R., have either ignored the controversy entirely or at any rate refrained from answering Lysenko's challenges.

Two causes explain this forbearance. First, it did not seem altogether fair to criticize the opinions of a man whose main writings are accessible to only a minority of geneticists who are able to read Russian. This cause is now removed by the publication of a translation of Lysenko's *Heredity and its Variability,** in which the author gives a systematic presentation of his views held as late as 1943-1944. The views expressed in this volume thus represent the author's conclusions after he had reached his present highly responsible position.

The second cause has been a reluctance to criticize adversely the scientific output of the U. S. S. R. This consideration weighed especially heavily with those Americans who are friendly to Russian science, and who justly desired to avoid anything that would prove in the least mischievious to the scientific or general relations between the two countries. It is the considered conviction of this reviewer that the above reluctance is misapplied, and particularly so when a scientific theory has strayed so far from the truth as that of Lysenko so obviously has. Nations, like individuals, may justly resent off-hand derogations of their domestic affairs by casual acquaintances. But to withhold well-considered and constructive criticism is unbecoming to scientists, and least of all to friends among scientists. Such an attitude in reality implies a subtle disrespect, not unlike a distainful decision to overlook an obviously bad musician in a mediocre orchestra. Now, science in the U. S. S. R., and particularly the science of genetics, does not merit such treatment.

Lysenko opens the attack on the "Mendelian-Morganian" genetics in the very first paragraph of his book: "In all reference and text books on genetics, heredity is understood as reproduction of like by like. In my opinion, such a definition gives little basis for understanding the phenomenon of heredity." The author is obviously unaware of the fact that in modern genetics heredity is conceived of as transmission of a norm of reaction of the organism to its environment, and not as mere similarity of the externally visible structures of the parents and offspring. Geneticists "were and are using methods and approaches which do not permit them to learn anything about the essence of heredity." These "methods and approaches" are hybridization of different organisms, and they are faulty because hybridization leads to "mixing their breed through crossing." The author should have recalled that the main tenet of genetics is precisely that no mixing of the hereditary elements, genes, takes place in hybrids. To him, heredity is "the property of the living body to require definite conditions for its life, its development and to react definitely to

*Lysenko, T. D. Heredity and its Variability. Translated by Th. Dobzhansky. King's Crown Press, 65 pp. Price $0.50. New York, 1946.

various conditions." The word "conditions" is used throughout the book in an unusual sense, meaning food and other materials from which the body is constructed, as well as the states of the environment (such as temperature) in which the organism lives. With the word "conditions" so understood, is this definition anything more than a restatement of the view held by the "Mendelian-Morganians" that heredity determines the responses of the organism to its environment?

The chromosome theory of heredity is declared a fraud: "The cytogeneticists draw a picture of fertilization by looking through a microscope at a slide on which lie cells in various stages of development. They draw all they can see, and what they can not they invent by making guesses in accord with the Mendelian-Morganian theory of heredity." To suppose that heredity is transmitted through chromosomes "is completely unacceptable to a biologist. It does not agree with the sexual process, and in general is unlike any biological process taking place in the living body." On the other hand, it is acceptable to the author, but probably to few other biologists, to think that when sex-cells unite "they assimilate each other," this assimilation being alleged to be the essence of the sexual process.

Having thus administered a coup de grace to the chromosome theory, the author proceeds to tackle Mendel's laws. He is generous enough to admit that segregation of characters has been found in many hybrids, and "this was observed also in Gregor Mendel's experiments with crossing peas." But to think that this segregation should occur in a ratio of 3:1 "is basically wrong even for pea hybrids, including the factual material obtained by Mendel himself. The progenies of different hybrid plants varied even in Mendel's experiments much beyond the ratio 3:1. Thus, in the offspring of one plant there were 19 yellow and 20 green seeds, and of another plant—only a single green for 30 yellow ones." The author seems to be completely unfamiliar with the theory of probability, which explains such deviations from theoretically expected ratios, and he is surely unaware of the fact that several years ago an eminent authority on biological statistics made an analysis of Mendel's numerical data and found that they fitted Mendel's theory better than might be expected from the operation of chance. Or, perhaps, the author would regard this as clinching his argument for the rejection of Mendel's theory? In one of his published speeches, the author asserts that he challenged "Mendelian-Morganian" geneticists to produce data showing 3:1 segregation in any species, and that such data do not exist. He has not consulted the early genetic literature which is replete with such data. Nowadays, it is true, the regular 3:1 segregations often seem too trite to warrant publication. However, students taking elementary courses of genetics including laboratory exercises usually have the opportunity to observe such segregations in their own materials.

Lamarck Redivivus

In this book, Lysenko does not confine himself to debunking "Mendelian-Morganian" genetics; he offers what he thinks is a new theory of heredity, albeit many readers will recognize in this theory some nineteenth century notions, long ago and for good reasons discarded in scientific biology. He starts with the statement that a living body builds itself "from the conditions of the external environment," which it "assimilates" and transforms into itself. Now, the organisms "selects" from the environment the material it needs, and this selectivity is determined by its heredity. So far, so good. But then we read that "the external conditions, when they are included within, assimilated by the living body, become thereby internal, and not external, conditions, i. e., they become particles of the living body, and for their growth and development they in turn demand that food and those conditions of the external environment, which they

were themselves in the past." "The growth of separate parts and granules of a living body requires the same conditions of the external environment, through the assimilation of which the organism originally built these parts and granules of its body." The body or its organs, cells and "granules" may fail to find in their environment the materials ("conditions") which they "select" for their reproduction. If so, these organs may either fail to develop altogether, or they may be "forced to assimilate qualitatively or quantitatively unusual conditions." And when a "body assimilates, perforce, certain elements of the soil solution which it received for the first time, these elements become biochemically included in the body, and now become necessary for the growth and the development of the altered body." In such a way, "alterations of the living conditions which enforce changes in the development of plant organisms are the cause of alterations of heredity." Lest some readers suspect that the sentence just quoted is a sheer *lapsus calami,* it must be emphasized that the notion expressed therein is repeated over and over again, and, as a matter of fact, represents the central idea of the book under review.

One more quotation will suffice to make this abundantly clear: "the elements of the non-living nature from the environment which surrounds the plant are transformed into integral parts of the living body through a frequently forced assimilation, become living elements, and acquire the property of heredity. The developing living body will in future generations require these external conditions for the reproduction of its like."

It is incredible that as late as 1944 anyone could not only urge acceptance of the above "theory of heredity" but also delude himself into believing that this theory has in it the slightest bit of novelty. For it is nothing more than a naive Lamarckist creed, reminiscent of some nineteenth century versions of Lamarckism, particularly of that of Spencer, yet, alas, devoid of Spencerian finesse. Without being told so, we are being urged by the author to believe that acquired characters are inherited, and, more than that, that the inheritance of acquired characters is the basis of all genetics, if not of biology. Now, anyone who wishes to know why the hypothesis of the inheritance of acquired characters is discredited in modern biology can find the answers in elementary textbooks. In short, the discrediting is due to the fact that all critical experimental tests have given negative results, that all supposed indirect evidence can be better accounted for in other ways, and to the sterility of Lamarckism as a working hypothesis.

Darwin's Pangens "Rediscovered"

As if to make things more peculiar still, the author and his followers consider themselves Darwinists and carry their devotion to Darwin's name to a point bordering on fetishism. Indeed, Darwin receives some deep obeisances in the book under review, whereupon Darwin's theory of natural selection is quietly ignored. This does not prevent Lysenko from discussing in detail another of Darwin's theories, namely the "provisonal hypothesis of pangenesis," and this without acknowledging Darwin's authorship. According to Lysenko, every body part produces substances (pangenes?) which eventually become lodged in the sex cells and become integral parts of the latter. "The degree of the transmission of changes depends on the degree to which the substances of the altered body part are included in the chain of processes which lead toward the formation of the reproductive sexual or vegetative cells." Now, Darwin himself had no illusion as to the strength of the evidence on which the hypothesis of pangenesis rested, as the adjectve "provisional" clearly shows; and this hypothesis was demonstrated to be invalid already in the nineteenth century. All of which is either unknown to the author or else is ignored by him.

The Experimental Basis

Lysenko's proposal amounts, then, to no more and no less than scrapping of

all genetics and much of general biology built during the last half-century or more, and adopting a homespun concoction of Lamarck and Spencer, with some Michurin added for flavor. The least conspicuous place in the book under review is occupied by presentation of the facts which should induce modern biology to adopt so startlingly reactionary a course. More precisely, no facts at all are presented in such a way that one could critically evaluate them, but several astonishing experiments of the author and his disciples are casually alluded to. Thus, seeds from fruits of a white-fruited tomato variety grafted onto a red-fruited variety are said to have produced some red-fruited plants. The progeny of the latter is said to have "consisted of a majority of red-fruited plants, but approximately 20%-30% of the plants had white fruits." Such "vegetative hybrids" are said to have been obtained between tomato varieties with two-chambered and with many-chambered fruits, between prostrate and erect varieties, early and late varieties, etc. The offspring of a tomato grafted onto a nightshade is said to have changed in all characters, acquiring, among other things, "much improved taste qualities" and an early maturity. The author assures us that "after making such experiments any geneticist still believing in the correctness of the foundations of Mendelism will see not only the incorrectness of this theory but its harmfulness in practical application of breeding and of seed management." It is as easy as this.

"Many genetically spring varieties have been derived from winter vareties" of wheat by means of nothing more complicated than growing them under different temperature conditions. "We have already obtained several winter varieties of wheat and barley from spring varieties through education, through the influence of the external environment. These forms are no less winter resistant, and frequently even more resistant than the most resistant varieties known." This "education" consists in exposing spring varieties to low temperature for several generations. It is no wonder that having obtained such results the author entitles one of the chapters in his book "The liquidation of the conservatism of the nature of organisms." As a fitting climax, the following experiment is mentioned. "At the I. V. Michurin Central Genetic Laboratory, K. E. Enikeeva has crossed the American 16-chromosome plum 'Cheresota' with Michurin's 48-chromosome plum, variety 'Renclod Reforma.' The 16-chromosome variety was used as the maternal, and the 48-chromosome one as the paternal form. The plant obtained from the cross had the habit of the paternal form, including the 48 chromosomes. All these experiments show clearly the diversity of the biological process of fertilization which does not fit at all the cytogenetic mould invented by the Morganians." This is because "the fusion of two sex cells is a process of assimilation, a process of mutual consumption," and "depending upon which one of the sex cells assimilates its partner to a greater extent, the hybrid embryo will deviate to different degrees toward the breed of the assimilating sex cell."

Why Geneticists Scoff

What can one say about these experimental results? The author's claims are, to put it mildly, improbable. It is to be hoped that these experiments will be repeated by critical observers, and the truth or falsity of the author's claims will be established beyond doubt. The kinds of experiments mentioned in this book have all been made earlier by competent investigators, without, of course, producing the startling results alleged by the author. Thus, the offspring of grafted tomatoes and other plants never contained anything resembling his "vegetative hybrids." Winter and spring varieties of cereal crops, as well as cold resistant and non-resistant forms of cultivated and wild plants, have been extensively studied; these characters are genetically rather complex but examination of their inheritance disclosed nothing akin to the mutability claimed to

have been found by Lysenko and his collaborators.

Some people will probably wonder why geneticists do not rush to repeat these experiments. The answer is simple enough. The progress of science would be seriously disorganized if all scientists interrupt their work every time somebody publishes a dubious claim. Such claims are disposed of in due course. Admittedly, the history of science knows instances when claims first regarded as doubtful later proved to be valid and exerted an important influence on subsequent developments. One should not forget, however, that history is not as likely to record the vastly more numerous but less romantic instances when doubtful claims proved to be unfounded.

In any case, one can assert with complete confidence that genetic theories of Lysenko are invalid regardless of the final dispositon of his experimental claims. For the sake of argument, let us assume for a moment that "vegetative hybrids" are a reality, and that it is possible to obtain winter resistant wheat varieties merely by exposing their progenitors to cold. This would certainly not mean that Mendel's laws are abrogated, or that fertilization is "mutual consumption" of sex cells, or that acquired characters are inherited. Irrespective of whether the author has or has not discovered some facts of interest, his overall influence on science has been that of a thoroughgoing obscurantist. It is not Lysenko's fault that he has not succeded in wrecking genetics as well as the agricultural sciences in the U. S. S. R. We may be confident that the brilliant and active group of geneticists now working there will keep the U. S. S. R. in the forefront of scientific progress.

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PLANT LIFE OF THE PACIFIC

PLANT Life of the Pacific World, by Elmer D. Merrill* seems to me to be a truly amazing compendium of useful information. There is a note of authority about it which would lead any scientific man to conjecture that the author had spent many years in the Pacific area even if he did not know that Dr. Merrill grew up among its plants and built up a great herbarium in Manila. This was in the Bureau of Science, an institution for which he was largely responsible; now, alas, destroyed by the Japanese.

Dr. Merrill ranks at the top of the list of scientists who have studied the plants of the Pacific islands and his contributions to the systematic botany of that vast area are too numerous to be more than referred to here. As the Administrator of the great botanical collections of Harvard, and Director of the Arnold Arboretum his reputation is world-wide.

I am proud to have known Dr. Merrill since 1900. I met him when he was en route to Manila, on an Army transport just after the Spanish war to take up his residence in the Philippine Islands. For twenty-two years he labored there as few young men I have ever known have worked; day and night he spent in the field and among his specimens. He brought together a group of collectors and associates whose knowledge and enthusiasm has rarely been equalled anywhere in the tropical world.

As I recall him in those days, there was a fierceness about his interest in systematic botany that I came later to associate with other discoverers of new species of living creatures, insects, shells, reptiles or fossils, etc. I have sometimes wondered at the intensity of the drive

*MERRILL, ELMER D. Plant Life of the Pacific World. Pp. xv+295. $3.50. The Macmillan Co. New York. 1945.