Letters

The Lysenkoists

The article by Caspari and Marshak on "The rise and fall of Lysenko" (16 July, p. 275) contains, in my opinion, many errors of fact and logic.

I shall, however, comment on only one section, that entitled "Agricultural experiments."

In the first paragraph the authors refer to vernalization experiments. Without attempting to refute the design or performance of the experiments, or the interpretation of the data, they dismiss the results as failing to live up to their "spectacular claims," especially in light of the agricultural success of classical genetics. The fact that this may be so reflects in no way on the theoretical implications of vernalization, unless one expects economic success to be a measure of theoretical validity.

In the second paragraph the authors say that it is "quite possible" that vegetative hybridization results in chimeras. They do not note that the papers on these experiments and comments by advocates of the theory of vegetative hybridization specifically deny that chimera formation could explain the transformations at sites remote from the grafts and claim further that there is a fusion of heredities rather than a juxtaposition of genetically different tissues. Nonetheless, Caspari and Marshak go on to say, "Such 'chimeras,' also observed in the West, have, however, never been found to perpetuate hybrid characters in any experiments carried out outside of Russia." Glavinic [see *Rep. Intern. Hort. Congr. 14th* (1955), vol. 1, p. 440; *Proc. Intern. Congr. Genet. 10th* (1958), p. 98] and Sinoto [ibid., p. 269] both reported such changes in experiments performed outside the U.S.S.R.

In the last paragraph, the authors, commenting on the Lysenkoists' blood-injection experiments, say that results such as those reported could be due to genetic impurity and that "the Lysenkoists did not supply sufficient information to permit evaluation of their results." I refer them to a detailed and extensive literature by the Geneva and Gif-sur-Yvette groups [*Compt. Rend.* **245**, 448 (1957); **247**, 1049 (1958); **248**, 2519 (1959); **250**, 211 (1960); **255**, 781, 1030 (1962); **256**, 4501 (1963); and *Biol. Med. Paris* (1963)].

E. KAELLIS

4 Homestead Lane,
Roosevelt, New Jersey

Kaellis's remarks raise technical questions in biological experimentation which we intentionally avoided in our paper. We tried to emphasize the intellectual and historical basis of the difference between Lysenko's view of heredity and that prevalent in the Western world. In this connection, we tried to describe briefly the types of experiments carried out by Lysenko and his followers in support of their theories, and it seemed necessary to point out that they are being greeted with skepticism by Western geneticists. We felt that a review of the literature and an evaluation of the experiments and their criticism were beyond the scope of our paper.

To take up the second point first, reference should be made to the thorough and critical evaluation of grafting experiments outside the Soviet Union by D. S. Dean [*J. Heredity* **53**, 215 (1962)]. It appears that these experiments do not form a homogeneous group of cases but that, besides chimeras, transfer of viruses and gene-controlled substances may be involved. Sinoto [*Proc. Intern. Congr. Genet. 10th* (1959), vol. 2, p. 262; *Novant' Anni delle Leggi Mendeliana*, L. Geddà, Ed. (Rome, 1955), pp. 119–140] explains his own results by the assumption of "actants"—gene-controlled diffusible substances.

Our point in quoting the experiments on hereditary changes induced by blood transfusion was merely to indicate that they are regarded as equivalent to the grafting experiments in plants. If the experiments by Kushner and by the Swiss and French workers (the latter using DNA) on birds are valid, they would have to be regarded as cases of somatic transformation, as acknowledged by Russian as well as Western geneticists. It is no accident that work on transformation takes a prominent position in the Russian work in microbial genetics. The demonstration of somatic transformation in higher organisms is very difficult and requires elaborate control experiments to be convincing. Such controls are notably lacking in the experiments carried out in Geneva and Gif, and only the experiment of Szybalska and Szybalski [*Proc. Natl. Acad. Sci. U.S.* **48**; 2026 (1962)] on human cultured cell lines seems to meet these requirements. I believe that vertebrate pigment cells, because of their instability in individual development, form particularly unfavorable material for transformation studies.

The problem of vernalization and photoperiodism could not have been treated in our paper satisfactorily without going to excessive length. The plant physiological basis of these experiments seems to be well supported. The breeding experiments apparently involve crosses between different strains and selection for desirable recombinants, a simple Mendelian procedure. But this interpretation of the experiments would reject the Lysenkoist contention that they demonstrate a direct effect of environmental factors on heredity. From the Western point of view, the experiments are hard to interpret because it is not clear whether the starting material was genetically pure. On the other hand, because of Lysenko's denial of the existence of genes, the concept of genetic purity would be nonsensical from his own point of view. It appears, therefore, that for theoretical reasons the design of Lysenkoist experiments is in principle different from the design of experiments carried out under the assumption of the existence of genes, and that for this reason it is difficult to evaluate the experiments as described. The experiments themselves can easily be fitted into Mendelian genetic concepts; but in this case they do not give support to the alternative hypothesis.

Finally, I agree that lack of economic success "reflects in no way on the theoretical implications of vernalization." I do, however, suggest that it may have been an important factor in the recent change in the attitude of the Soviet government towards genetics.

A main point in the argument of our paper was that Lysenko would not have been able to achieve domination of Russian genetics, and to suppress Mendelian genetics, if he had not had the full support of the Soviet government. It is therefore important to try to analyze the arguments which led the government to endorse Lysenko's ideas officially for 20 years, and recently to withdraw its support. To anyone who has read the disputations of Lysenkoists and Mendelians in the 1940's, it is apparent that the validity and design of the experiments involved were only a minor point compared to political and ideological considerations.

E. W. CASPARI
Department of Biology,
University of Rochester,
Rochester, New York 14627

Caspari and Marshak compare the activities of Lysenko with the Scopes trial in Tennessee in 1925. Surely such a comparison serves only to soften the lethal implications of Lysenkoism. Lysenko was the guiding genius behind the purges of science from 1936 to 1948, in which some geneticists were known to have been put to death (see Garrett Hardin, *Nature and Man's Fate*). Vavilov, who was President of the Academy of Sciences, was arrested and sent to Siberia, where he died; others simply disappeared. The Scopes trial was an amusing sideshow comparable to the flagpole-sitting and related antics of the 1920's in the U.S.A. The defendant was fined $100; his conviction was reversed by the Tennessee Supreme Court; Clarence Darrow, who defended him, gained enormously in fame and reputation; and the uninterrupted study of genetics and evolution in the U.S.A. continued to move forward to new achievements in the laboratories of investigators such as Morgan, Muller, Bridges, Wright, and Dobzhansky. . . .

We learn from Caspari and Marshak that Lysenkoists are now willing to accept the existence of DNA as hereditary material, this being "a tribute to the remarkable developments which have taken place in Western genetics. . . ." By the same token, an acknowledgment of the existence of the moon might be interpreted as a tribute to the remarkable photographs taken by Ranger VII.

THOMAS H. JUKES
Space Sciences Laboratory,
University of California, Berkeley

. . . In their concluding paragraph Caspari and Marshak say, "The tragedy of Lysenkoism is that so much precious time has been lost for the biological sciences in the U.S.S.R." I cannot but disagree with this conclusion. The "tragedy of Lysenkoism" and of the conditions which permitted the rise of this "extraordinarily ambitious and ruthless scientific adventurer" is represented by the fate of Academician N. I. Vavilov who died in a Siberian labor camp for having the desire and determination to pursue the truth. I was a subject of a Communist-dominated state for several years and can attest that Vavilov's fate was shared by countless others, scientists and nonscientists, for the very same offense. Science cannot be evaluated without considering the man who participates in it, benefits from it, or suffers from it. For us the tragedy is a human tragedy first and a scientific one second; only to Soviet officials may it appear to be an economic one. . . .

It would have been desirable to include in the article a list of references. During my comparatively short stay in the U.S. I have noted the existence of several useful publications on this subject without specifically searching for them. These include Theodosius Dobzhansky's translation of one of Lysenko's pamphlets, *Heredity and Its Variability* (King's Crown Press–Columbia University Press, 1946); Julian Huxley's *Heredity East and West: Lysenko and World Science* (Schuman, New York, 1949); A. G. Morton's *Soviet Genetics* (Lawrence and Wishart, London, 1951); and C. Zirkle's *Death of a Science in Russia* (University of Pennsylvania Press, 1949); and *Evolution, Marxian Biology, and the Social Scene* (University of Pennsylvania Press, 1959). . . .

MIHALY BARTALOS
School of Medicine,
Howard University, Washington, D.C.

Financing Key Ideas

I note with great interest the editorial by Wolfle ("The productive environment for innovation," 30 July, p. 501) in which he reports on a Defense Department study of the conditions that have led to the most successful research achievements. It seems to me significant that in most cases funding for the development of the "key idea" was not readily available, and that "the company or university paid the expenses from its own funds, or borrowed money intended for related work or other activities. . . ." It seems to me that in such a situation a very careful evaluation of the need and the idea would first have been made so as to provide the retrospective justification for the diversion of these funds, and I suspect that the results were much less costly than they would have been if obtained with funds specifically allocated to them under a contract. I wonder how this factor could be built into our funding systems? . . .

MARTIN B. BERKE
Reinhold Book Division,
430 Park Avenue, New York 10022

Fertility Experiment Recalled

Popular interest in multiple human births, and especially in the recent instances of quadruplets, quintuplets, and even one set of stillborn sextuplets born to women treated with gonadotrophic hormones FSH (follicle-stimulating) and HCG, (human chorionic), prompts me to report on our experience nearly 25 years ago with cats. Our problem was not dissimilar to the present human one, though we were only trying to increase the number of cat fetuses for our experiments and harbored no thought of abetting a human population explosion.

Many cats fail to come into estrum in the laboratory at the expected season, and their infertility is related to their failure to develop ripe follicles and release ova. We reasoned that a little of the new Fevold-Hisaw follicle-stimulating hormone, followed by luteinizing hormone (LH), might correct this condition. We had had partial success with pregnancy urine and serum extracts, especially when administered during the season of expected estrum (*1*).

A graduate student, R. F. Becker (now at Duke University) was dispatched to Boston to learn how to prepare the hormones from sheep pituitary glands, after which we went into the business of cat-fetus production on a scale limited only, we hoped, by the availability of mature female cats from Chicago alleys and a few virile males. We opened Pandora's box!

Multiple births are the rule in cats, the normal range being two to six, rarely seven, kittens per litter. No fewer than eight fetuses were found *in utero* or were born of 12 hormonally treated cats. The maximum number was 20 em-