Comments and Communications

Resignation of Professor Muller From Academy of Sciences of the USSR

The following letter, dated September 24, 1948, was sent by H. J. Muller, of Indiana University, Nobel Prize winner and past president of the Genetics Society of America, to the President, the Secretary, and the Membership of the Academy of Sciences of the USSR:

"In February 1933 the Academy of Sciences of the USSR sent me a diploma, signed by its venerable President, Karpinsky, and its Secretary, Volgin, stating that I had been elected a 'Corresponding Member.' In accepting this election, I realized that it was a signal honor, inasmuch as your Academy had a long and most distinguished tradition of scientific achievement and integrity, and was still maintaining its high standards and, in fact, greatly expanding its valuable work. Although for nearly a decade I have not been sent your publications, I must presume that I am still on your rolls, since I have received no information to the contrary.

"The deep esteem in which I have held your organization in the past makes it the more painful to me to inform you that I now find it necessary to sever completely my connection with you. The occasion for my doing so is the recently reported series of actions of your Presidium in dropping, presumably for their adherence to genetics, such notable scientists as your most eminent physiologist, Orbeli, and your most eminent student of morphogenesis, Schmalhausen, in abolishing the Laboratory of Cytogenetics of your most eminent remaining geneticist, Dubinin, in announcing your support of the charlatan, Lysenko, whom some years ago you had stooped to take into your membership, and in repudiating, at his insistence, the principles of genetics. These disgraceful actions show clearly that the leaders of your Academy are no longer conducting themselves as scientists, but are misusing their positions to destroy science for narrow political purposes, even as did many of those who posed as scientists in Germany under the domination of the Nazis. In both cases the attempt was made to set up a politically directed 'science,' separated from that of the world in general, in contravention of the fact that true science can know no national boundaries but, as emphasized at the recent meeting of the American Association for the Advancement of Science, is built up by the combined efforts of conscientiously and objectively working investigators the world over.

"In Germany too it was the field of genetics, that of my own specialization, which was subjected to the greatest perversion, as I pointed out in publications and lectures gotten out both shortly before and during several years after the Nazi coup. And in the USSR the prescientific obscurantism of Lysenko, supported by the so-called 'dialectical materialism' represented by Prezent, with their faith in the inheritance of acquired characters, must lead inevitably, and indeed by the admission of some of their adherents, to the same dangerous Fascistic conclusion as that of the Nazis: that the economically less advanced peoples and classes of the world have become actually inferior in their heredity. The Nazis would have the allegedly lower genetic status a cause, while the Lysenkoists would have it an effect, of the lower opportunity of the less fortunate groups for mental and physical development, but in either case a vicious circle is arrived at, which objective geneticists do not concede. Objective geneticists, on the contrary, having established the existence of a separate material of heredity, which is not influenced in any corresponding way by modifications of the phenotype, or bodily characteristics of organisms, recognize the fallacy of judging the hereditary endowments either of individuals or of whole groups simply by outward appearances. Especially is this the case when, as with human mental traits, there are very variable environmental influences, such as differences in tradition, education, nutrition, etc., which have pronounced and systematic effects upon the development of these characters.

"In truth, genetics is so fundamental and so central to all fields of biological science, and even of social science and philosophy, that the excision of its established principles from the body of science as a whole cannot but result in the eventual debilitation and falsification of our understanding of things in general. Even the physical sciences must in the end be adversely affected by the admission of the naive and archaic mysticism of Lysenko, Prezent, and their group into the vacuum left by the removal of genetics, for processes must then be invoked which are contradictory to the workings of matter.

"Under the circumstances above set forth, no self-respecting scientist, and more especially no geneticist, if he still retains his freedom of choice, can consent to have his name appear on your list. For this reason I hereby renounce my membership in your Academy. I do so, however, with the ardent hope that I may yet live to see the day when your Academy can begin to resume its place among truly scientific bodies.

"The importance of the matters here at issue—including that of the authoritarian control of science by politicians—is in my opinion so profound that I am making this letter public."

On "Animal Hypnosis"

In reference to W. T. Liberson's paper, "Prolonged Hypnotic States With 'Local Signs' Induced in Guinea Pigs" (*Science*, July 9, pp. 40–41) I would like to call attention to the following points:

The basis of the theory of animal hypnosis is the fact that many animals may be "hypnotized" by retaining them in uncomfortable positions. Granted that there are many phenomena that may be reproduced in the lower animals that closely resemble hypnosis, the fact remains that this theory is quite generally accepted as being erroneous. Both Verworn and Hull have *proved* quite

conclusively that "animal hypnosis" due to placing animals in unaccustomed and inconvenient situations is a *tonic recumbency reflex.* Verworn assumed that this inferred that there was no psychological basis for "animal hypnosis." In this belief he was incorrect; however, in his assumption that this was not a true state of hypnosis, he was entirely correct, as was later confirmed by Clark Hull.

Dr. Liberson has produced an interesting paper on tonic recumbency reflexes but is extremely misleading in his terminology.

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Dr. True raises the question of terminology in regard to my note published in the July 9 issue of *Science.* The term "animal hypnosis" which I used has been employed in the past by a great number of scientists, among them Steininger, Babak, Reisinger, Rijland, and Pavlov, without implying the identity of this condition to human hypnosis. The term "tonic recumbency reflex" suggested by Dr. True is not satisfactory, first, because the animals I studied often presented clonic and agitated reactions, and second, because this term does not imply the presence of concomitant psychological phenomena which Dr. True, himself, believes to be a factor associated with immobilization. The suggested term is, furthermore, irrelevant in regard to the phenomena which I described, as they may be observed in an animal in the upright position. His contention that retaining the animal in an uncomfortable position or "placing animals in unaccustomed and inconvenient situations" is the basis for "animal hypnosis" is not supported by my experiments, at least as far as prolonged hypnotic states are concerned. Indeed, I showed that by the mechanisms of conditioning and frustration the prolonged "hypnotic" reactions may be observed when the animal is put on one side and fail to appear when it is put on the opposite side in the same uncomfortable position.

The importance of the training apparently completely escaped the attention of Dr. True. It is the alleged impossibility of forming an hypnotic habit in the animals which was one of the decisive arguments of Hull and others to deny this state the name of hypnosis, while one of the main findings reported in my note dealt precisely with the possibility of training a prolonged animal hypnotic state.

When the same name is applied to a phenomenon described in both man and animals, this term is never equally applicable to both situations. We speak of intelligence in considering simple maze problems for low mammals as well as in discussing the masterpieces of human genius. In the same way, we should not expect the term "animal hypnosis" to have the same characteristics as "human hypnosis." As this term has been equally applied to man and to animals, the purpose of a scientist is to determine the underlying mechanisms of these states in various species and to reveal their similarities and differences. The question of terminology then becomes secondary.

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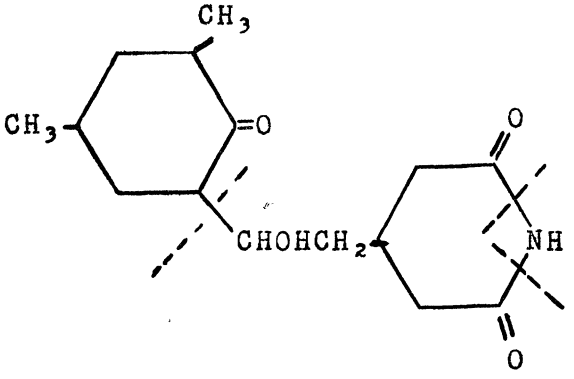
The Structure of Actidione, An Antibiotic From *Streptomyces griseus*

Recently Ford and Leach (*J. Amer. chem. Soc.*, 1947, 69, 474; 1948, 70, 1223), of the Upjohn Research Laboratories, reported the isolation of a new antibiotic substance from *Streptomyces griseus* which they named "Actidione." This interesting material is highly active against almost all yeasts but is relatively innocuous to other microorganisms. Subsequently, Leighty and Fortune, of these laboratories, also isolated this substance from streptomycin residues, and we undertook a study designed to elucidate the chemical structure of the new antibiotic.

Actidione has the empirical formula $C_{15}H_{23}NO_4$. It has one hydroxyl and only *one* ketonic group. Evidence based on various chemical degradations and transformations, together with that derived from physical studies, leads us to propose the structural formula I for Actidione.



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On alkaline hydrolysis the molecule is split, as indicated by the dotted lines, into three products: (1) ammonia, (2) a fragrant, optically active ketone ($C_8H_{14}O$) which was identified as 2,4-dimethylcyclohexanone, and (3) a 7-carbon acid fragment, presumably 3,3-propionaldehydediacetic acid. This latter substance has not as yet been obtained in a pure condition, but solutions containing it, upon mild oxidation, yield methanetriacetic acid $\left(HOCOCH_2CH\begin{matrix}CH_2COOH\\CH_2COOH\end{matrix}\right)$. Oxidation of Actidione yields a diketone, dehydroactidione ($C_{15}H_{21}NO_4$), which on alkaline hydrolysis is degraded to ammonia, 2,4-dimethylcyclohexanone, and methanetriacetic acid. Electrometric titration of the antibiotic shows the presence of a weakly acidic group with a $pK = 11.2$ (glutarimide likewise has a $pK = 11.2$). Catalytic reduction gives dihydroactidione ($C_{15}H_{25}NO_4$), which reacts with diazo-